Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1	4	0	0	0	0	The format and style of the executive summary of this chapter is not consistent with other chapters. For example - there are no bolded main sentences, there are no square bracket references to relevant chapter sections, and the certainty and confidence language used in this chapter is not consistent with other chapters - i.e. in this chapter uses ratings such as 'robust evidence' and 'high agreement'. These are meaningless as there is no definition for them. Consistency must be achieved with certainty language across chapters. [Government of Australia]	Agreed, ES formatted to be consistent with other chapters
4-2	4	0	0	0	0	Many statements in the Chapter text are not adequately referenced and statements in the Executive Summary are not traceable to the Chapter sections so readers can get more information and references. For example, a statement in the Executive Summary on mass loss from the Antarctic Ice Sheet: (The average ice-loss from Antarctica was 65 ± 33 Gt yr–1 over the period 1993–2010, p.4, line 25-27) does not state a reference to the relevant Chapter section. Further information on this cannot be easily found in the Chapter text, and when the rate of ice loss is mentioned briefly in the Chapter text, there are no publication references provided to support the statement. This lack of traceability needs to be corrected. [Government of Australia]	Agreed, ES formatted to be consistent with other chapters, and traceability enforced
4-3	4	0	0	0	0	This chapter's executive summary has a good narrative style, which should be encouraged in other chapters. [Government of Australia]	Noted. But rewrite undertaken to address other comments
4-4	4	0	0	0	0	Throughout this chapter the choice of literature cited seems strange. There is a clear frequent tendency to cite a single recent technical research paper when introducing an important aspect of the cryosphere system, where some reference to the historical work and context would be appropriate. This might be acceptable (if capricious) if AR5 is seen as merely an update to be read in conjunction with the AR4. This approach to citation (which gives the unfortunate appearance of a lack of diligence) leads to endless reinvention of older work, and seems inappropriate in a work of review which should synthesise and inform the readers. [Government of Australia]	Disagree. Focus on up to date papers is encouraged, technical
4-5	4	0	0	0	0	Throughout this chapter the terminology "updated from REF XXXX", appears. It is not uniformly clear how this updating (by the chapter authors themselves?) connects to the cited publications. [Government of Australia]	Authors of the original papers have done the updating? Methods have been reviewed, only the data has been updated. Terminology agreed with Co-chairs
4-6	4	0	1			Treatment of Uncertainty: Chapter 4 does not yet use the uncertainty terminology consistently across all sections and in some statements/sections it is completely lacking. In some instances, the amount of evidence and/or level of agreement is stated, but the next step now needs to be taken so that a confidence level is assigned. Where appropriate, and the confidence is sufficiently high, a quantified likelihood statement can be made. All uncertainty terminology should be italicized. Please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed, likelihood and confidence language updated and made consistent
4-7	4	0	2			Sign of change: Please clarify your sign convention for reporting direction of change and be consistent in the style you use to report rates of loss (positive or negative). We have tried to highlight examples in our comments where confusion arises, such as where a 'loss' is described with a '-' value (e.g., a "loss of -0.55 mm" which is in fact a gain). We suggest you introduce early on in your chapter the convention you will be using. [Thomas Stocker/ WGI TSU, Switzerland]	We will check and use consistent,"change" has a sign, "rate" has a sign, but "loss/gain" do not have signs
4-8	4	0	3			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Noted, consistency addressed
4-9	4	0	4			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to	Same as 4-1

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	
4-10	4	0	5			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
4-11	4	0	6			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
4-12	4	0	7			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. We will only use conventional "acronyms" and defined abbreviations. But generally, try to avoid acronyms. And delete all acronyms/abbreviations s in ES.
4-13	4	0	8			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
4-14	4	0	9			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
4-15	4	0				Comment on Chapter: Great compilation, readable to a non-glaciologist, and a couple of timely papers in yesterday's Science. Congratulations..Peter [Peter Barrett, New Zealand]	Noted
4-16	4	0				In the Chapter 4: Observations: Cryosphere generally I still (like mentioned in the FOD) 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]	Rejected. This chapter assesses changes in observed snow, and with the exception of long-term records of SWE (which are discussed already), these topics lie outside the purview of this chapter.
4-17	4	0				Add a figure: the latest Figure from http://psc.apl.washington.edu/wordpress/wp-content/uploads/schweiger/ice_volume/BPIOMASIceVolumeAnomalyCurrentV2_CY.png, also found as Figure 2 at http://psc.apl.washington.edu/wordpress/research/projects/arctic-sea-ice-volume-anomaly/. The title of the figure is "Total Arctic sea ice volume from PIOMAS showing the volume of the mean annual cycle, the current year, 2010 (the year of previous September volume minimum), and 2007 (the year of minimum sea ice extent in September). Shaded areas indicate one and two standard deviations from the mean." [CELSO COPSTEIN WALDEMAR, BRAZIL]	Rejected - this is a chapter on observations and we avoid modelling results where possible
4-18	4	0				Insert the figure cited on comment 4 as "Figure 4.5" [CELSO COPSTEIN WALDEMAR, BRAZIL]	Same as 4-17
4-19	4	0				the same from the comment No 18 [CELSO COPSTEIN WALDEMAR, BRAZIL]	Do not understand comment
4-20	4	0				the same from the comment No 21 [CELSO COPSTEIN WALDEMAR, BRAZIL]	Do not understand comment
4-21	4	0				the same from the comment No 24 [CELSO COPSTEIN WALDEMAR, BRAZIL]	Do not understand comment

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-22	4	0				While the bulk of the material presented in the Arctic sea ice sections is qualitatively correct, many of the specific numbers and references given (where fact-checking was performed) contained numerous errors, omissions and inconsistencies. Given that IPCC Reports are scrutinized minutely for errors once they are issued, a thorough check of all references and cited values should be performed before this document is released to the public. Please note that while many instances are noted in the comments, not all of the references given or numbers quoted were checked. [Government of Canada]	Responses made to the specific commnets in the sections on sea ice.
4-23	4	0				An overall very complete report. Congratulations to all authors. There are many "submited article, be sure that each of them will be published on time to accomplish IPCC deadlines. [Government of Chile]	Noted. Only "in press" articles used in Final draft
4-24	4	0				The estimates of the mass balance data over the ice sheets needs some revision, as the values differ by about 20% for Greenland and 10% for Antarctica from Shepherd et al., 2012 (a comparison experiment). The mass balance assessment is one of the major results in this chapter and hence differences between both assessments are crucial and the origin of differences needs to be discussed rigorously. In particular, averaging in the mass balance estimate by the IPCC itself, as well as Shepherd et al., 2012, are using updated mass balances in the averaging procedure potentially several times. The values of the number of glaciers in the new global glacier inventory needs to be revised and in particular explained on which assumptions, e.g. minimum size, it is based and in which direction future updates will move, meaning is it an over- or underestimation, with respect to debris covered glaciers and snow covered ground. The current state of the ice sheets and the processes involved could from my point of view be enlarged, allowing to put the focus more on the state of the art of understanding processes taking place right now and causes for changes. This is in particular important, as Chapter 13 discusses the capability of models to represent those processes and the impact of that for projections of sea level change. So, I propose to enlarge this section for the benefit of delivering a profound basis for Chapter 13. Furthermore, the sea level estimates should be kept consistent between Chapter 4 and 13 - which is not the case up to date. The sections on snow cover and lake ice are incomplete and should be revised in order to cover a broader range. A couple of points can be found below. At many places references are given as 'update from x'. In all these cases the reviewers cannot assess the correctness of the numbers. I suggest that in these cases, the authors who made the update are contacted in order to assure that the numbers are correct. [European Union]	The IMBIE results are not included in the IPCC average presented, but are directly compared with that. Differences between the two estimates, which are within the uncertainty, are discussed. One reason for a difference is the different treatment of Greenland peripheral glaciers. The SOD included valus from a draft version of IMBIE - those have been updated to the published values. Numbers have however been double checked. Noted - the number of glaciers is added Disagree - the allocation of space in the chapter was carefullty judged for balance between elements of the cryosphere. Same as 4-5
4-25	4	0				At some locations the number, volume or pages are missing - this still needs to be checked further. Comiso 2011 needs to be changed (see above), Rignot 2011c is missing. [European Union]	Accept the references have been revised and will be checked in final editing
4-26	4	0				The extreme warming event over Greenland 2007 and 2010 and the extreme warming event in 2012 mentioned in chapter 1 (page 1-13) are not included in this chapter 4? [European Union]	Accepted - mention of these events added to relevant section
4-27	4	0				The information contained in the Likelihood table (Table 1.1) and Confidence figure (Figure 1.12) is critical to interpreting conclusions throughout the document. This information should be repeated in the front of the SPM, the TS and each Chapter and the terminology should be applied consistently. [Government of United States of America]	Noted - consistency of usage addressed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-28	4	0				Numerous instances of key parameters missing error bars were noted throughout the chapter. While many examples are noted in the following comments, it is suggested that the authors check all figures for appropriate application of error measures. [Government of United States of America]	Noted, estimates of uncertainty are quoted where available/appropriate.
4-29	4	0				Numerous instances are noted of terminology use that echos the official terms for liklihood and uncertainty. Several instances are noted below, the the authors are encouarged to consider the use of terms such as "very likely", "certainly" and "almost certainly". [Government of United States of America]	Agreed - consistency in coded language addressed throughout
4-30	4	0				This is a generally well presented and comprehensive chapter, with a very readable style. However, it is somewhat repetitive in places and contains too many 'old' references (pre-2007). It would benefit from all, rather than some, of the main sections having their own conclusion sub-sections e.g. for Arctic and Antarctic sea ice. Information is included on the extent of surface melting of the Greenland ice sheet, but not on the record minimum low in Arctic summer sea ice extent, in 2012. The sections on Arctic sea ice should be updated to include 2012 figures. The FAQs address two key questions on the cryosphere, however, there may be a case for including a third FAQ, on ice loss from the Arctic and Antarctic ice sheets, as this topic is also commonly raised. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted Accepted - Additions made Accepted - timeseries extended
4-31	4	0				The chapter compares latest findings with AR4 and also provides good linkage to the WGII report, in relation to the impacts of changes to the cryosphere. There is a degree of repetition of information on several topics (e.g. river and lake ice; and on ice sheets and sea level rise) between some main sections. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted - repeptition reduced as far as possible
4-32	4	0				The chapter is somewhat unbalanced. This is partly attributable to the fact that there is more information available on some topics than others. For example there is quite a bit more information about sea ice than about some of the other components of the cryosphere. For example the details about the Odden sea ice feature can probably be deleted. [Dorothy Hall, United States of America]	Rejected - balance of chapter was pursued from the outset. Emphasis on sea ice reflects recent research and substantial changes. Odden feature is signficant
4-33	4	0				Some strong statements are not supported by references and I have noted, below, where this happens. Also I question the validity of using only one (perhaps controversial) reference to cover some major topics. There are usually multiple, independent references cited for important topics which is a good thing. [Dorothy Hall, United States of America]	Noted - traceability of statements is addressed
4-34	4	0				The units should be more standardized. For example, in places change in number of days per decade is used as a unit and in other places change in number of days per year; this is just one example. Another example is on Figures 4.1.3 and 4.1.4 where cm/yr and m/year are both used. [Dorothy Hall, United States of America]	Agreed - consistency in use of units is addressed throughout
4-35	4	0				Suggested Additional References Hall, D.K. and G.A. Riggs, 2007: Accuracy assessment of the MODIS snow-cover products, Hydrological Processes, 21(12):1534-1547, DOI: 10.1002/hyp.6715. Hall, D.K. and D.A. Robinson, in press: Global Snow Cover, in Satellite Image Atlas of Glaciers (Williams, R.S., Jr. and J.G. Ferrigno, eds.) USGS Professional Paper 1386-A. Hall, D.K., R.S. Williams, Jr., S.B. Luthcke and N.E. DiGirolamo, 2008: Greenland Ice Sheet surface-temperature, melt and mass loss: 2000 – 2006, Journal of Glaciology, 54(184):81-93. Dewey, K., and Heim, R., 1983: Satellite observations of variations in Southern Hemisphere snow cover, NOAA Technical Report NESS, 87, USA. Nghiem, S.V. D.K. Hall, T.L. Mote, M. Tedesco, M. Albert, K. Keegan, C.A. Shuman, N.E. DiGirolamo and G. Neumann, 2012: The extreme melt across the Greenland ice surface in 2012, GRL, in press. Romanov, P. and D. Tarpley, 2001: Snow cover extent over South America derived from passive microwave and visible/infrared observations, 11th Conference on Satellite Meteorology and Oceanography, Madison, WI, USA, 15–18 October 2001 (American Meteorological Society), pp. 19–22.	Noted - addition of references is considered. new references added on the extreme melt year 2012 (Nghiem, et a., 2012) and the surface albedo change (Box et al., 2012)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Romanov P. and D. Tarpley, 2003: Automated monitoring of snow cover over South America using GOES imager remote sensing data. International Journal of Remote Sensing, 24(5), 1119–1125. [Dorothy Hall, United States of America]	
4-36	4	0				In relation to melting, reports of Ming et al., 2008 and Menon et al., 2010 need to be included. In general, role of Black carbon (BC) in snow melting needs to be highlighted. Refs: Menon S., Koch D., Beig G., Sahu S. Fasullo J. and Orlikowski D., 2010. Black carbon aerosols and the third polar ice cap, Atmos. Chem. Phys. Discuss., 9, 26593-26625. Ming, J., Cachier, H., Xiao, C., Qin, D., Kang, S., Hou, S., Xu, J., 2008. Black carbon record based on a shallow Himalayan ice core and its climatic implications. Atmos. Chem. Phys. 8, 1343–1352. [Umesh Kulshrestha, India]	Noted
4-37	4	0				Scenario of the Himalayan Glaciers is very important, it needs to be included [Umesh Kulshrestha, India]	Noted
4-38	4	0				Kulshrestha (2012) has reported a complete review of global warming scenario. Ref: Kulshrestha U.C. Global warming-Present status of research and future strategies. J. Ind. Geophys. Union, Vol.16, No.4, pp. 143-160. This study highlights important aspects of global warming in Indian perspective. [Umesh Kulshrestha, India]	Noted, but this study is more likely relevant to WGII than this assessment, where glaciers are discussed in terms of specifically defined regions, and no more locally than that.
4-39	4	0				For the most part, a very well-written an organized chapter [Christopher Little, United States of America]	Noted
4-40	4	0				Suggest intense efforts to rationalize scope of work and conclusions with Chapter 13. Seems like many sections of Ch 13 could be shifted and/or consolidated in Ch 4. [Christopher Little, United States of America]	Noted - but efforts to keep Ch13 and Ch4 indiividually coherent require some repetition. Increased cross-referencing provides better coherency
4-41	4	0				[No comment - merely entry on blank line to ensure that processing doesn't abort at this point.] [John McLean, Australia]	No comment required
4-42	4	0				In general, this chapter is the data presentation and does not integrate the cryospheric changes into a larger framework related to climate change. In Executive Summary (P 4-3, Line 3-9) the importance of the cryosphere as a signature of climate variability is mentioned. Further, in Introduction (P 4-6, Line 3-12), the sensitivity of all components of the cryosphere to climate change is stated. Above all, the chapters of the cryosphere in the IPCC reports are meant to present the cryospheric change as a part of the on-going climate change. Of sections 4.2 Sea ice , 4.3 Glaciers and 4.4 Ice sheets, section 4.4 Ice sheets is the best written. Section 4.4 is the only section in which an effort has been made to relate the ice sheet change to possible causes including climate changes (Section 4.4.3). It is desirable to have at least a section in each Section sea ice and Section glaciers, discussing the relationship between the cryospheric changes and climate change. As for Section 4.2, there is sufficient climatic data, especially temperature and wind accumulated in marginal arctic seas and the Central Arctic Ocean to allow for the interpretation of sea ice changes. Interpretation of the glacier mass balance change is not possible if the discussion is restricted only to annual net balance as done in Sections 4.4.3 and 4.4.4. The annual net balance is made of two distinctly different processes and quantities, winter (reflecting accumulation) and summer (influenced by ablation) balances. By separating the annual net balance into these components, one can relate the net balance to changing climate. There are sufficient publications and data on these matters. The present accumulation of expertise in glaciology should allow the authors to go beyond the mere presentation of the observational results. Too many submitted papers that are not yet in press are quoted in critical places. Some of these submitted papers are not yet the consolidated results of the study. An over reliance on not-yet accepted papers reduces the reliability of the assessment report. A reliance on unpublished papers must be kept to a minimum. This chapter is restricted largely to the authors' own works, and needs information from wider research results to gain an objective viewpoint. 4.7 Synthesis is weak. More concrete and numerically certain information should be presented. As some readers are expected to read "Introduction" and "Synthesis" before deciding to read the entire chapter, it is of	Disagree - the remit of this chapter excludes both statements that pertain to projectionn (Ch13), and statements that include attributions (Ch10). The presentation of observations of change, without either type of comment limits degree to which the role suggested can be achieved. Taken into account: winter and summer balances are now mentioned. Noted - we attempt to include the most up to date research. No "submitted" papers cited in final draft. Noted - review of self-citation completed. Noted - synthesis is redrafted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						advantage to repeat certain important outcomes in the main text and the synthesis. [Atsumu Ohmura, Switzerland]	
4-43	4	0				In general my impression is that there is a considerable bias in terms of regions and reference from Europe and USA, including only very few information on other parts of the world. Although I am aware that there are certainly some challenges related with references from other parts of the world, I still would like to have commented on that, since this bias was mentiioned as somewhat problematic after AR4. A certain improvement in this regard should be visible in AR5. [Nadine Salzmann, Switzerland]	Noted - for glaciers, the regions are addressed rather similarly in terms of assessed losses. With regard to examples of improvements in process understanding, we use the most compelling references without regard to geographic source. For frozen ground, data paucity is a problem for many areas, and we're struggling to make representative statements for many regions. For snow, the same applies. Esp for Southern Hemisphere.
4-44	4	0				The Figures and their captions are often full of abbreviations, significantly complicating to understand content. [Martin Schneebeli, Switzerland]	Noted - captions revised
4-45	4	0				Excellent treatment of the polar regions. Only point I would note is that ther are quite a few references marked as 'submitted'. [Michael Sparrow, United Kingdom of Great Britain & Northern Ireland]	Noted - "in sub" papers will not make it to the final version
4-46	4	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on Ice sheets, Glaciers, and sea ice. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendael and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	Noted - but we avoid specific mention of programmes, as this looks like advertising
4-47	4	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on Ice sheets, Glaciers, and sea ice. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendael and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	Same as 4-46
4-48	4	1	1	100	70	This Chapter appears to be much improved over the earlier draft, and provides a good review [Robert Thomas, United States of America]	No response required
4-49	4	1	1	100	70	of what we know about the cryosphere. Inevitably, it is overly detailed, and most IPCC target audience would benefit from a clear, reasoably brief Summary near the beginning of the Chapter. Uncertainties are generally under-estimated [Robert Thomas, United States of America]	Noted
4-50	4	1	1	100	70	This Chapter appears to be much improved over the earlier draft, and provides a good reviewof what we know about the cryosphere. Inevitably, it is overly detailed, and most IPCC target audience would benefit from a clear, reasoably brief Summary near the beginning of the Chapter. Uncertainties are generally under-estimated [Robert Thomas, United States of America]	Same as 4-49

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-51	4	1	1			Overall a good chapter. Please note, this might be of little significance to the report, but the glaciers of the McMurdo Dry Valleys have not changed significantly in the past 50 years. I can provide references if interested. [Andrew Fountain, United States of America]	Noted - agree that these are not really significant on a global scale and are such an oddity as to require too much space to explain. The emphsis in AR5 is on global and regional assessments but leaves littel space for sub-regional to small scale details
4-52	4	1	14			“Antony Payne” should read “Anthony Payne” [Atsumu Ohmura, Switzerland]	Disagree - Tony Payne, says it's "Antony" (but thanks, all the same)
4-53	4	1	26	1	56	I think that after the introduction it would be more correct to put the seasonal snow, then the glaciers, ice sheets, frozen ground and finally the sea ice. [Mauro Guglielmin, Italy]	Disagree - ordering of sections reflects significance to policy makers
4-54	4	1	57	2	5	I will add one FAQ more: How active layer thickening may change CO2 and CH4 fluxes in the atmosphere? [Mauro Guglielmin, Italy]	Noted - but FAQs will not be added at this time
4-55	4	1		68		One general comment is that in many places the report includes submitted papers, this is not correct in an international journal and even less here, where only accepted results should be considered to create the report.Therefore I suggest to delete all the parts of the paper based only on submitted paper or to leave these if other references can support the statements. [Mauro Guglielmin, Italy]	Noted - submitted papers are excised from the final version
4-56	4	1		98		Overall comment - there are various grammatical and stylistic corrections needed which I will not spend time on here. Generally clearly written; there are a few awkward construcitons, but I will not comment on these unless they interfere with comprehension. [W. Tad Pfeffer, United States of America]	Noted - copy editing has been improved
4-57	4	1		100		Since natural decadal variability has at least as large contribution as anthropogenic forcing to regional climate changes, it will be more beneficial to the society if more efforts are given to initialized decadal prediction in future IPCC reports. [Joshua Xiouhua Fu, United States of America]	Noted - but the brief for this specific chapter was clear and does not include projection
4-58	4	1		200		11. This paragraph refers to the entire Chapter 4. Chapter 4 reviews some of the published information on the topic "Cryosphere". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	Noted - but the brief for this specific chapter was clear highlight the climate related changes in the cryosphere, without straying into projection or attribution. With respect, the validity of our PhDs does not require defence in this form.
4-59	4	1				I just reviewed the sea ice section of this chapter. The section provides a good overview of the current state of sea ice and its trajectory over the past few decades. [Donald Perovich, United States of America]	No response required
4-60	4	1				better representation of authors covering the greather Himalayan region would have been important, this region [Dorothea Stumm, Nepal]	Taken into account: respective literature and text were added
4-61	4	1				seems under represented (despite fewer publications) [Dorothea Stumm, Nepal]	See 4-60
4-62	4	3	1	3	1	The term "unrelated to climate" together with "calving" implies, that calving is in general unrelated to climate, which is not the case if ocean warming increases calving as later discussed, e.g. p25, l36. Clarify. [Olaf Eisen, Germany]	Noted, but the text of the bullets is modified significantly since the SOD, and this is now longer applicable.
4-63	4	3	1	5	33	The Executive Summary does not refer to sections in the main body of the report, which makes it more difficult to trace statements made in the ES. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Agree, section references added

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4-64	4	3	1	6	34	The executive summary lists evidence for warming permafrost, but does not mention the evidence for warming glaciers, as later mentioned on p4-24, line 1-2. I suggest to include evidence for warming ice in the executive summary as well, although there are fewer observation sites than for permafrost. [Olaf Eisen, Germany]	Noted. The evidence for this is rather limited and specific reference is not considered sufficiently important to be in the ExSumm
4-65	4	3	1			Executive Summary: As outlined in our general guidance, please add line of cite, linking each paragraph in the ES to the underlying chapter sub-section where the assessment is provided. Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed, section references added
4-66	4	3	1			Executive Summary: As outlined in our general guidance, please add bolding to the key statements. The first sentence (or two) of each paragraph should be bolded to highlight the key message. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed, bolding added
4-67	4	3	3			I'm sure it's not possible at this late stage to standardize the time periods of analysis, but in reading the executive summary it is striking that different variables are reported over different time intervals. This is obviously dependant on data record availability, but sea ice extent trends are report through 2011, sea ice thickness through 2009, glacier loss through 2009, Antarctic Ice Sheet changes through 2010, snow cover extent through 2010, freshwater ice through 2004, and permafrost through 2005. In the case of NH snow season length and active layer thickness it is not clear which time period the reported trends in the Executive Summary cover. The lack of up to date time series for some variables is troubling, and some effort should be made (at least in future assessments) to report trends up to a consistent end date across all elements of the cryosphere. Given the record setting lows in snow cover extent in June 2012 and sea ice in September 2012, it would be timely to update these data records, but I suppose this is unrealistic at this stage. [Chris Derksen, Canada]	Noted: standardization of the periods has been attempted wherever possible, but often the published data don't support it. However, all records will include 2012 where possible. Sea ice to be updated to end of 2012; snow ditto. Ice Sheets data only avaialbel to the end of 2011.
4-68	4	3	6	3	7	Change "Given the inherent temperature-sensitivity of all components of the cryosphere over a wide range of time scales," to "Given that all of its components are inherently temperature-sensitive over a wide range of time scales," [Sarvesh Garimella, United States of America]	Agreed, text modifed
4-69	4	3	14	3	14	Comment text: The sentence beginning "These new obs.." states that ice is being lost from many components of the cryosphere, but by implication not most. This understates the chapter's conclusions. I suggest replacing with a statement about the crysophere as a whole, eg along the following lines "These new observations confirm that the cryosphere is experiencing a net loss of ice," which matches well with the following clause "although there are significant differences in the rate of loss." [Peter Barrett, New Zealand]	Noted, text has been modified to strengthen statement.
4-70	4	3	18	3	18	add:The strong and significant decrease in Arctic sea ice extent, area and volume reported in AR4 has continued [CELSO COPSTEIN WALDEMAR, BRAZIL]	Noted, but volume is discussed separately, and was not strongly noted in AR4. Both changes are noted in ExSum but separated, since confidence is different
4-71	4	3	18	3	18	"strong and significant decrease" - these are not quantified. [Government of Australia]	Noted, uncertainty language has been corrected throughout.
4-72	4	3	18	3	27	Please report the information on the new record low in Arctic Sea Ice extent occurred in September 2012. Reference : http://nsidc.org/arcticseaicenews/2012/10/poles-apart-a-record-breaking-summer-and-winter/ [Sai Ming Lee, Hong Kong, China]	Noted. This is discussed in the chapter and may be highlighted in ExSumm
4-73	4	3	18	3	33	If possible, I would suggest to update the information given here with data that include the 2012 Arctic sea ice minimum. [Sebastian Gerland, Norway]	Same as 4-72
4-74	4	3	18	3	33	Suggest including error estimates with all percentages. [Government of United States of America]	Noted, uncertainty language has been corrected throughout.
4-75	4	3	18	3	33	Suggest to add the area of Arctic sea ice in 2012。 [Lei Huang, China]	Same as 4-72
4-76	4	3	18	3	33	The extent of sea ice in 2012 should be added here. [Jing Ming, China]	Comes up several times
4-77	4	3	18	3	33	Here and in Chapter 4.2, it would be nice to mention the minimum record of the Arctic ice cover in 2012 with	Same at 4-72

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						figures; may be the values of trends have changed too [Andrey Shmakin, Russian Federation]	
4-78	4	3	18	3		Didn't AR4 also report on thickness losses which have also continued? Biased opinion: A volume loss (e.g 80% of total arctic sea ice volume in September between 1979 and 2012 (update after Schweiger et al. 2011) could be suitable addition to this list of changes. [Axel Schweiger, United States of America]	Same as 4-70
4-79	4	3	18			Maybe it is worth mentioning the new summer 2012 record of low sea ice extent? [Etienne BERTHIER, France]	Same as 4-72
4-80	4	3	20	3	20	The expression of confidence used here (e.g., robust evidence in high agreement) is different from other chapters and the SPM. It would seem there should be consistency in how this is expressed such that the SPM and chapters are connected. [Government of Canada]	Noted, uncertainty language has been corrected throughout.
4-81	4	3	20	3	21	Sentence is not clear. Either change the first part of the sentence to read "The DECADALLY-AVERAGED extent of Arctic sea ice has decreased in every season since satellite observations commenced." Or remove "decadal" from the first part of the sentence so that it reads "The average extent of Arctic sea ice has decreased in every season and in every successive decade since satellite observations commenced" . [Government of Canada]	Noted - Exsumm text has been modified significantly.
4-82	4	3	20	3	22	Suggest revision to: "The average decadal extent of Arctic sea ice has decreased in every season and in every successive decade since satellite observations commenced in 1979". [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-83	4	3	21	3	21	Indicate start date of satellite observations [Jeffrey Obbard, Singapore]	Noted - Exsumm text has been modified significantly.
4-84	4	3	21	3	22	What do you mean with "overall trend in sea ice extent". If you give a number, the quantity should be defined accurately, especially in the executive summary. Reading such a sentence without thinking gives the impression of clear information, after thinking about you have no clear information left. This applies to the following remarks as well. [Heinz Blatter, Switzerland]	Noted - Exsumm text has been modified significantly.
4-85	4	3	21	3	22	What is meant by the 'overall trend in sea-ice extent'. Does this refer to the annual maximum sea-ice extent? Needs clarification. [Regine Hock, United States of America]	Noted. Overall means a combination of all seasons and not just summer minimum or winter maximum.
4-86	4	3	21	3	23	Data from 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Noted - Exsumm text has been modified significantly.
4-87	4	3	21	3	25	Is the decline in the coverage of perennial ice not the same as sea ice extent. The difference between the 2 statements of decline is unclear, and for an executive summary be clearer for a broad audience not familiar with technical terms. [Regine Hock, United States of America]	Noted - now used "extent of perennial ice…"
4-88	4	3	22	3	22	"is" is better to replace "has been" [Yongjian Ding, China]	Past tense used throughout
4-89	4	3	22	3	22	Time period also an issue for Arctic - here 1979-2011; P9, L34: Nov 1979 - May 2012 [Walter Meier, United States of America]	Noted - Exsumm text has been modified significantly.
4-90	4	3	22	3	22	My suggestion would be to use consistent periods for all of the passive microwave extent values [Walter Meier, United States of America]	Noted - Exsumm text has been modified significantly.
4-91	4	3	22	3	22	Also suggest using whole years, e.g., Jan 1979 - Dec 2011 to avoid any biasing due to seasonality [Walter Meier, United States of America]	Noted - Exsumm text has been modified significantly.
4-92	4	3	22	3	22	If possible, it would be nice to update through Dec 2012, particularly in light of the record low in the Arctic [Walter Meier, United States of America]	Noted - 2012 sea ice records added
4-93	4	3	22			The extreme record low of 2012 should be included. [Wilfried Haeberli, Switzerland]	See 4-92
4-94	4	3	23	3	24	The use of “perennial ice” in this context must be avoided due to the uncertainty of the definition, especially when it is followed by “multiyear ice”. Some use it for any thick sea ice (e.g. P 4-9, Line 14 and P-4-10, Line 35), while others use it in a more restrictive sense, as “sea ice older than two years” (Baker, B.B. et al. (Eds.), 1966: Glossary of Oceanographic Terms. U.S. Naval Oceanographic Office, Washington, D.C., 204 pp; Air University, 1955: Glossary of Arctic and Subarctic Terms. Air University, Alabama, 90 pp). Prominent	Rejected. Perennial ice is clearly defined. It is what is left after the melt season.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						glossaries published by WMO and Scott Polar avoid listing this word. Further, insert a period at the end of this sentence, before a new sentence with “Sea ice concentration” begins. [Atsumu Ohmura, Switzerland]	
4-95	4	3	23	3	25	Typo. Sentence needs a period. [Government of Canada]	Editorial
4-96	4	3	23	3	25	Perennial ice is not synonymous with the summer minimum extent (although the summer minimum extent is a reflection of the extent of perennial ice at the end of the melt season). This sentence should be corrected to "The largest changes of all are the decline in the coverage of perennial ice (ICE THAT HAS SURVIVED AT LEAST ONE SUMMER'S MELT, THE LOSS OF WHICH IS REFLECTED IN the summer minimum extent; –12.2 % per decade) and multiyear ice (ICE THAT IS more than 2 years old; –15.6 % per decade)." [Government of Canada]	Agreed
4-97	4	3	23	3	27	To avoid redundancy and focus on the relative amount of seasonal/multiyear ice, versus perennial/multiyear, revise to: “The largest changes of all are the decline in the minimum extent of the summer sea ice cover, which occurs in September; -12.2 % per decade. Sea ice concentration has also decreased and the rate of decrease in ice area has been greater than that in extent. Robust evidence shows decline in seasonal (1 year old) and multiyear (more than 1 year old) sea ice coverage, at rates of (give rates), respectively, with related decreases in ice thickness and in ice volume.” [Government of United States of America]	Accepted. Incorporated with revisions
4-98	4	3	23			Add:Ice coverage in summer 2007 reached a record minimum, with ice extent declining by 42% compared to conditions in the 1980s.Reference: Maslanik, J. A., C. Fowler, J. Stroeve, S. Drobot, J. Zwally, D. Yi, and W. Emery (2007), A younger, thinner Arctic ice cover: Increased potential for rapid, extensive sea-ice loss, Geophys. Res. Lett., 34, L24501, doi:10.1029/2007GL032043. The expression of results of Arctic ice loss solely through the percentage loss per decade is not enough. [CELSO COPSTEIN WALDEMAR, BRAZIL]	Accepted. Incorporated with revisions
4-99	4	3	24			"perennial ice" is confusing and misleading; "summer minimum extent" is clear and is widely used in the community (e.g., see NSIDC). Thus, delete "perennial ice" and just simply use "summer minimum extent" here. (this comment was repeated by several reviewers). [Government of United States of America]	It is not misleading if defined at the beginning. We want to emphasize that the summer
4-100	4	3	25	3	25	Change "decade)" to "decade)." [Sarvesh Garimella, United States of America]	Accepted
4-101	4	3	25	3	25	Change "decreased" to "decreased," [Sarvesh Garimella, United States of America]	Accepted
4-102	4	3	26	3	27	explanation of perennial ice is done in page 10. But it is slightly difficult to distinguish multi year ant perennial ice in this page 3. [Hiroyuki Enomoto, Japan]	Accepted distinction made clearer
4-103	4	3	26	3	27	This introductory sentence "Robust evidence shows … " should come right after the first sentence in this bulleted paragraph, not in the middle of the paragraph. [Government of Canada]	Noted sentence changed
4-104	4	3	26			The distinction between perennial ice (2 years old) and multiyear ice (more than 2 years old) is unnecessary and confusing. It would be better to just have first year and multiyear ice for the entire sea ice section. A two stage (first year and multiyear ice is a better aproach. [Donald Perovich, United States of America]	Noted. The distinction is clearly made by WMO and the American Meteorological Society (Glossaries). We follow this definition because it better fits the multiyear ice data derived from satellite sensors.
4-105	4	3	27	3	27	add after (multiyear sea ice coverage and decreases in ice thickness, and in ice volume): "The mean annual cycle of arctic sea ice volume over the 1979 -2011 period ranges from 28,700 km3 in April to 12,300 km3 in September. Monthly averaged ice volume for September 2012 was 3,400 km3. This value is 72% lower than the mean over this period, 80% lower than the maximum in 1979. In contrast to the reduction in ice extent, the 2011 to 2012 change in volume was in line with volume losses that occurred in previous years, with 2007 and 2010 losses being substantial greater." [CELSO COPSTEIN WALDEMAR, BRAZIL]	Noted. Incorporated some of the ideas. The maximum did not occur in 1979 as the reviewer claimed.
4-106	4	3	27	3	27	Reference of these latter comments 1 and 2 :PIOMAS (Pan-Arctic Ice-Ocean Modeling and Assimilation System) from Polar Science Center . Applied Physics Laboratory . University of Washington. USA.These texts were obtained from its page Arctic sea ice volume anomaly, version 2. http://psc.apl.washington.edu/wordpress/research/projects/arctic-sea-ice-volume-anomaly/ and the main bibliographic reference about how the data were obtained is Schweiger, A., R. Lindsay, J. L. Zhang, M. Steele, H. Stern, and R. Kwok, 2011: Uncertainty in modeled Arctic sea ice volume. Journal of Geophysical Research-Oceans, 116. doi:10.1029/2011jc007084, already cited in this chapter, page 12,line 6 (see the aforementioned	Noted, but modeling study is not within the scope of this chapter.

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						reference for details). Anothers references: Zhang, J., M. Steele, and A. Schweiger (2010), Arctic sea ice response to atmospheric forcings with varying levels of anthropogenic warming and climate variability, Geophys. Res. Lett., 37, L20505, doi:10.1029/2010GL044988 . This Polar Science Center has more than 180 scientific publications about cryosphere: see http://psc.apl.washington.edu/wordpress/publications/. You need to inform the amounts of total ice loss from a certain reference date, as for example, the beginning of the measurements from ice satellites, so readers have the real dimension of the high recent loss of recent decades without the need to make arithmetic calculating (decade) X (-x%per decade) . It was well done, for example, with the overall mean winter thickness at line 28 of page 3 of this chapter. [CELSO COPSTEIN WALDEMAR, BRAZIL]	
4-107	4	3	27	3	27	No comma after thickness [Sarvesh Garimella, United States of America]	Editorial
4-108	4	3	27	3	28	What do you mean with "overall mean winter thickness". If you give a number, the quantity should be defined accurately, especially in the executive summary [Heinz Blatter, Switzerland]	Noted. Removed "overall" and use only "mean winter thickness"
4-109	4	3	27	3	28	It does not seem appropriate to provide a quantitative estimate regarding the changes in the thickness of the Arctic sea ice cover (i.e. 1.8 m between 1978 and 2008), given the level of uncertainty in submarine and satellite-derived estimates. [Government of United States of America]	Noted, eithet deleted or provide error bars.
4-110	4	3	28	3	28	Change "With decrease" to "With decreases" [Sarvesh Garimella, United States of America]	Accepted
4-111	4	3	28	3	30	Typo. "With decrease in … " should be corrected to "With decreases in …" [Government of Canada]	Accepted
4-112	4	3	28	3	30	Extreme wind forcings contribute significantly to sea ice volume loss in all season by ice compression and dirft rate increase. Thus drift rate increase is a cause of ice loss, and is not simply a consequence of ice loss. [Government of United States of America]	Accepted. Elaboration made.
4-113	4	3	28	3	33	The reasons stated for the increase in drift are speculative and based on two papers. Therefore, it seems inappropriate to include this in the high level summary of the chapter. The statement indicating that the period of surface melt on perennial sea ice has increased by 6 days per decade over the period 1979-2010 is not included in section 4.2.2.6, “Time of Arctic Sea Ice Adv". Therefore, it should not be included in the high level summary of the chapter. Suggested revision: “Other significant changes to the Arctic Ocean sea ice include an increased rate of drift; a lengthening in the duration of the period of surface melt by 20 days over 30 years; and a nearly 2-month lengthening of the ice-free season in the region from the East Siberia Sea to the western Beaufort Sea over the period 1979-2011.” Upon further review and consultation during the USG review process, it was pointed out that these findings/conclusions may very well be supported by the text and references in Section 4.2.4 and Figure 4.7d. However, we wanted to highlight it for the authors for their consideration, nonetheless. [Government of United States of America]	Accepted. Period of surface melt will be included in both
4-114	4	3	28			"winter thickness decreased by 48% to only 1.89 m between 1980 and 2009": It is not transparent how and where this figure (48%) is obtained and what is its error bar? Is it derived from different kind of measurements at different location/coverage in different years with numerical interpolation/extrapolation across many missing years? Please clarify how the number is obtain so readers can understand the uncertainty. [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-115	4	3	28			change "to only 1.89 m" to "to 1.89 m": no context for "only" [Laurence Padman, United States of America]	Noted - Exsumm text has been modified significantly.
4-116	4	3	30	3	31	For greater clarity suggest " ... lengthening of 6 days per decade in the duration of the period of surface melt on perennial sea ice over the period 1979-2010, ..." [Government of Australia]	Noted - Exsumm text has been modified significantly.
4-117	4	3	30	3	32	In terms of consistency it is unclear why one lengthening change is quoted as a rate (days per decade) over a given period, while the second (for ice-free season) is quoted as total change over a period. [Government of Australia]	Noted - Exsumm text has been modified significantly.
4-118	4	3	30	3	33	Clarify whether this sentence, which reads "… of 6 days per decade … " should read as "… by 6 days per decade …" . [Government of Canada]	Noted - Exsumm text has been modified significantly.

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4-119	4	3	32			Please discuss what is meant by "ice-free"? Does it mean no ice at all physically, or does it just mean "not detectable by passive microwave data"? [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-120	4	3	35	3	35	what do you mean with "total sea ice extent"? Is it maximum sea ice extent? [Heinz Blatter, Switzerland]	Noted - Exsumm text has been modified significantly.
4-121	4	3	35	3	35	Perhaps this should be "statistically significant" rather than just "significant"? [Government of Australia]	Noted - Exsumm text has been modified significantly.
4-122	4	3	35	3	35	1.4%/decade for Antarctic sea ice trend quoted here is not consistent with later in the text, e.g. 1.1% on P17, L9 [Walter Meier, United States of America]	Noted - Exsumm text has been modified significantly.
4-123	4	3	35	3	36	Data from 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Noted - Exsumm text has been modified significantly.
4-124	4	3	35	3	36	ice extent' and 'sea-ice area'. What is the difference? [Regine Hock, United States of America]	Noted - Exsumm text has been modified significantly.
4-125	4	3	35	3	36	"small but significant" may lead to confusion, also, "sea ice extent" and "sea ice area" may be too complex and not necessary to use both. [Shichang Kang, China]	Noted. But sea ice extent and area are two parameters that have been used in many publications. We will provide a definition that might be easier to understand.
4-126	4	3	35	3	36	"small but significant" may lead to confusion, also, "sea ice extent" and "sea ice area" may be too complex and not necessary to use both. [Jing Ming, China]	will fix the problem indicated
4-127	4	3	36	3	36	"extent increases, and area has greater increase due to increase in concentarion" may be not clear as "area" and "extent" are not yet defined. [Hiroyuki Enomoto, Japan]	Accepted. Elaboration made
4-128	4	3	36	3	36	Time period for trends seems to vary in chapter - here 1979-2011; P14, L55: 1978-2012; P17, L9: 1979-2010 [Walter Meier, United States of America]	Noted. Used the same record length for the time series studies using PM
4-129	4	3	36	3	36	"due to an increase in concentration" ... It may be better to refer more clearly to sea ice concentration per unit area [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Used "increase in ice concentration per unit area…"
4-130	4	3	37	3	37	Refer to new study, as of Nov 2012 by British Antarctic Survey, published in Nature Geoscience - based on changing wind patterns and growth of ice in Antarctic. [Jeffrey Obbard, Singapore]	Noted. used the reference
4-131	4	3	43	3	43	A paragraph explaining why Antarctic sea ice is increasing is needed. The Chapter only states that it is increasing and not why. Some commenters may assume (in fact some already are) that sea ice in Antarctica is increasing due to 'global cooling'. See recently released publication Holland and Kwok (2012) 'Wind-driven trends in Antarctic sea ice drift' Nature Geoscience. [Government of Australia]	Noted. used the reference
4-132	4	3	44	3	44	Define glaciers as not including ice sheets? [Christopher Little, United States of America]	Accepted: text has been changed for clarification
4-133	4	3	44	3	49	Having said (line 44) that the total land-surface area covered by glaciers was not precisely known in AR4, is the new dataset of glacier outlines really accurate to 20 square kilometres? [Government of Australia]	Accepted: text has been changed
4-134	4	3	44	3	49	This summary points out that understanding area does not mean that we have resolved volume. This conclusion should be echoed more clearly in the main text. [Government of United States of America]	Accepted: text has been changed
4-135	4	3	44			"glacier" is not yet defined (to exclude ice sheet areas), so this is not clear [Laurence Padman, United States of America]	Accepted: text has been changed
4-136	4	3	46	3	46	No uncertainty on 739,820 ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted: text has been changed
4-137	4	3	46	3	49	The numbers are too precise to be believed, and their uncertainties should also be included. Please see Table P.18 for your reference. [Jing Ming, China]	Accepted: text has been changed
4-138	4	3	46			I suggest replacing 739,820 by 740,000. Unnecessary "precision" and to be consistent with the rest of the chapter. Also the 212,103 km3 could be replace by 212,000 km3 [Etienne BERTHIER, France]	Accepted: text has been changed
4-139	4	3	46			What is the error associated with 739,820 km2? [Government of United States of America]	Accepted: text has been changed
4-140	4	3	46			The outlines in some regions concern "glacier complexes" rather than "glaciers". This has implications for	Accepted: text has been changed

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						estimates of ice volumes and sea level equivalents (cf. next comment). [Wilfried Haeberli, Switzerland]	
4-141	4	3	47			The use of “perennial” of “perennial glaciers” must be avoided. [Atsumu Ohmura, Switzerland]	Rejected: the term used is 'peripheral' and correct
4-142	4	3	48	3	48	I could not find the figures 165,000 and 212,103 km3 in section 4.3 Glaciers. [Government of United States of America]	Taken into account: the respective bullet has been changed
4-143	4	3	48	3	48	Is "highly uncertain" necessary given that it's quantified in the same sentence? [Christopher Little, United States of America]	Taken into account: uncertainty language has been reconsidered
4-144	4	3	48	3	48	Why include volume here, rather than mass (since it's implied anyway with the conversion to SLE) [Christopher Little, United States of America]	Taken into account: the respective bullet has been changed
4-145	4	3	48			inconsistent units: 165,000 to 212,103 km3. These numbers should be presented in Gt to be consitent with those on page 4. [Government of United States of America]	Taken into account: the respective bullet has been changed
4-146	4	3	49			Please correct: 0.43 ± 0.06 m SLE: Huss, M. & D. Farinotti 2012. Distributed ice thickness and volume of all glaciers around the globe. Journal of Geophysical Research -- Earth Surface, 2012, 117. doi:10.1029/2012JF002523 [Luzi Bernhard, Switzerland]	Taken into account: the respective bullet has been changed
4-147	4	3	49			These estimates are still overly precise; the higher value is most likely a systematic overestimation because of the problems related to area-related thickness/volume estimates for glacier complexes instead of individual glaciers (cf. Huss and Farinotti 2012). The effect of ice below sea level is probably a few centimetres sea level equivalent: this obvious and non-negligible effect had been ignored so far and must now be correctly mentionenned (cf. Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion). [Wilfried Haeberli, Switzerland]	Taken into account: the respective bullet has been changed
4-148	4	3	52	3	52	Insert "shorten, " in front of "shrink" [Jacob Clement Yde, Norway]	Taken into account: the respective bullet has been changed
4-149	4	3	53	3	55	It is ambiguous. [Tao Che, China]	Taken into account: the respective bullet has been changed
4-150	4	3	53	3	55	The meaning is too vague. In fact, some glaciers are advancing, although most of them are retreating. Please clarify. [Jing Ming, China]	Taken into account: the respective bullet has been changed
4-151	4	3		5		The Executive Summary of Ch 4 is now very good. [Terje Wahl, Norway]	Thanks, However Exsumm text has been modified significantly.
4-152	4	3		17		I am only commenting on the sea ice portions of Chapter 4. That includes pages 3 to 17 (Executive Summary, Introduction, and Sea Ice sections); pages 45-48 (FAQ 4.2, and Synthesis); and Figures 4.1 through 4.7, Figure 4.25, and Figure 1 of FAQ 4.2. [Harry Stern, United States of America]	No response required
4-153	4	3		17		In general, the sea ice section is good. All the main points are covered adequately. [Harry Stern, United States of America]	No response required
4-154	4	3		17		My main concern is with the confusing terminology of “perennial ice” and “multiyear ice”. The authors define perennial ice as ice that survives the summer melt, and multiyear ice as ice that survives at least two summer melts. See page 3 lines 24-25, page 10 lines 36-39, and page 16 line 47. These definitions are different from common usage among sea ice scientists, they are different from the IPCC AR4 report, and they are not internally consistent. I will provide details, point by point, in the following 8 comments. [Harry Stern, United States of America]	Noted - definitions made clearer
4-155	4	3		17		1 of 8. Common usage is that multiyear ice is ice that has survived at least *one* melt season. Nearly every sea ice researcher in the business uses the “one melt season” definition, including all of the following works that refer to multiyear ice: Maslanik (GRL 2007, GRL 2011); Nghiem (GRL 2006, GRL 2007); Kwok (JGR 2004, GRL 2005, GRL 2006, GRL 2007, JGR 2009); Gloersen et al (“Arctic and Antarctic Sea Ice, 1978-1987”, NASA SP-511, 1992); Weeks and Ackley (Chapter 1 in “The Geophysics of Sea Ice”, NATO ASI	Noted. if common usage is wrong, it should be corrected. WMO and the American Meteorological Society Book of Glossaries define multiyear ice as ice that survived at least two summers. The products presented by Gloersen et al., Nghiem et al and Kwok

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						Series, Volume 146, 1986); Tucker et al (Chapter 2 in "Microwave Remote Sensing of Sea Ice", AGU Geophysical Monograph 68, 1992); and the on-line NSIDC "Cryosphere Glossary". In other words, a large body of publications over the past 20+ years considers multiyear ice to be ice that has survived at least *one* melt season. So the definitions in the present Chapter 4 do not conform with common usage. [Harry Stern, United States of America]	et al represents multiyear ice as defined here. The key problem is that the signature of second year ice has been observed to be different from that of multiyear ice and closer to that of first year ice. This is most evident in the Antarctic where the perennial ice is mainly second year ice which has basically first year ice signature during the subsequent winter. Some aircraft measurements indicated similar observations and a time series analysis done by Comiso (2006) for the Arctic also confirms this. It is amazing that this reviewer spent so much time and effort on this issue.
4-156	4	3		17		2 of 8. The National Academy of Sciences just came out with a report called "Seasonal to Decadal Predictions of Arctic Sea Ice: Challenges and Strategies". On page 13 of the report, multiyear ice is defined as "Ice that has survived at least one melt season". So the definitions in the present Chapter 4 do not agree with the National Academy of Sciences. [Harry Stern, United States of America]	Disagree. There should not be a problem as long as the term used is properly defined and especially if the plots presented correspond to the variable as defined.
4-157	4	3		17		3 of 8. The definition of multiyear ice in the IPCC AR4 report of 2007 is given in Section 4.1 of that document, on page 342: "Sea ice less than one year old is termed 'first-year ice' and that which survives more than one year is called 'multi-year ice'." The term 'perennial ice' is not used in the IPCC AR4 report of 2007. So the definitions in the present Chapter 4 are not consistent with previous IPCC usage. [Harry Stern, United States of America]	See 4-155
4-158	4	3		17		7 of 8. The word "perennial" means lasting for an indefinitely long time, or lasting throughout the entire year. "Perennial ice" is commonly used to mean the same thing as "multiyear ice". Nghiem et al (GRL 2006) is typical: "Arctic sea ice consists of two major classes: perennial or multi-year sea ice defined as sea ice that survives at least one summer ... and seasonal or first-year sea ice". The recently released report from the National Academy of Sciences (see comment 2 of 8 above) similarly equates multiyear ice with perennial ice, and first-year ice with seasonal ice (page 19). I agree with this usage. "Perennial ice" and "multiyear ice" should mean the same thing. [Harry Stern, United States of America]	See 4-155
4-159	4	3		17		8 of 8. I recommend that the authors define multiyear ice as ice that has survived at least one melt season. They may want to include a brief explanation that this breaks with WMO terminology but conforms with common usage among sea ice researchers, whose papers are cited in the present chapter. The term "perennial ice" can then be dropped, or used synonymously with "multiyear ice". The authors may still discuss ice that has survived at least two melt seasons by referring to it as ice that has survived at least two melt seasons. Figure 4.4 (page 75) should be re-labeled accordingly. [Harry Stern, United States of America]	See 4-155
4-160	4	3		17		Sea ice decline is given in units of percent per decade in many places, e.g. page 9 lines 33-40, page 10 lines 47-50, and other places. What is the base period on which the percent change is calculated? I can't find that information anywhere in the section. [Harry Stern, United States of America]	Noted. The base period is the mean over the satellite record.
4-161	4	4	1	4	9	This sentence is vague: "between 0 - 1% " may mean about everything, as it suggests that an undefined number of glaciers changed little or not at all. By contrast, the chapter refers to 0.1% - 1% (which rules out 0%) on line 40 of page 21, and figure 4.10 suggests that only one study was < 0.1%/yr, and that it was for a period ending before 2000 (and of course for a specific region). In addition, p44 L31 concludes that "in all mountain regions where glaciers exist today, glacier volume has decreased considerably over the past150 years". I would suggest a clarification in the ES, referring to 0.1% - 1% for the period 1960s - 2000s, and/or making clear how much regions showed substantial glacier area reduction. [Philippe Marbaix, Belgium]	Noted - Exsumm text has been modified significantly.
4-162	4	4	3	4	4	What is it has "robust evidence in high agreement"? The strong and significant decrease in Arctic sea ice extent and area or the fact that it has been accompanied by many other changes in the characteristics of the Arctic sea ice cover. What characteristics? [Government of Chile]	Accepted - uncertainty language modified to house style
4-163	4	4	3			Unit for 371 ±50 Gt/yr --> Gt yr-1 [Etienne BERTHIER, France]	Editorial

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4-164	4	4	4	4	5	There seems to be no logic reason why glaciers around the periphery of the ice sheets should be excluded when reporting global glacier ice loss. Either delete the last part of this sentence from ", or ..." or show how much of the global 2005-2009 glacier ice loss was due to glaciers located peripheral to the ice sheets. [Jacob Clement Yde, Norway]	Noted: peripheral glaciers are discussed in more detail in the text and respectively reflected in the ExSum
4-165	4	4	4	4	6	The first estimate clarifies use of all glaciers versus those far from large ice sheets, whereas the second estimate does not; clarify. [Richard B. Alley, United States of America]	Noted: peripheral glaciers are discussed in more detail in the text and respectively reflected in the ExSum
4-166	4	4	4	4	6	It is not clear why those two estimates are compared here and how the first one (2005-2009) has been derived. Is it also a multi-method estimate? Maybe one sentence enumerating the different methods would be welcome [Etienne BERTHIER, France]	Taken into account: the respective bullet has been changed
4-167	4	4	4	4	6	lack of clarity - loss rates presented as applicable to a given period (as quoted) do not "amount to ice loss", they are still rates. [Government of Australia]	Taken into account: the respective bullet has been changed
4-168	4	4	4	4	6	前后两句应合成一句，或者给出区间或整体说明，在Summary中不应该列出太多数字。 [Shichang Kang, China] the two sentences could be merged into one senrence, and /or just showing the interval or giving a explanation is enough, since too many figures should not be in the summary	Taken into account: the respective bullet has been changed
4-169	4	4	4	4	6	In the Executive Summary, which should appeal to a wide community, it is not advisable to quote two different numbers on a major climate parameters (global glacier net mass balance, -371 Gt/yr "or" - 262 Gt/yr) without further explanation. A non-expert may draw the conclusion better not trusting any of these two numbers. [Helmut Rott, Austria]	Taken into account: the respective bullet has been changed
4-170	4	4	4	4	6	If the glaciers in the periphery of the ice sheets are included in the numbers for glacier loss, it has to be made clear that these glaciers are not considered in the ice sheet mass balance, in order to avoid double accounting [Helmut Rott, Austria]	Noted: peripheral glaciers are discussed in more detail in the text and respectively reflected in the ExSum
4-171	4	4	4	4	6	Can these ranges and 2 different time spans be consolidated into a single statement, as has been done in the first draft of the SPM. This avoids highlighting at this level the results of a single study. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: the respective bullet has been changed
4-172	4	4	4	4	7	Suggest the authors include a statement pertaining to the treatment of ice dynamics and its relative role alongside melt in modulating mass changes in the small glaciers. This would allow a reader to assess how the glaciers are different from the ice sheets. What is the current best estimate of the fractional contribution from dynamics? If no new number exists, Meier and all made assumptions similar to Greenland, Burgess said 30% for Arctic Canada. It is known that dynamics are less of a contribution to mass loss than for ice sheets, - which should be pointed out as the best guess and a serious knowledge gap. Suggest that the document recognize that progress is underway on this problem, albeit at a slower rate than for the ice sheets, but a good first step has been taken by figuring out how many calving glaciers exist in each region. [Government of United States of America]	Taken into account: the respective bullet has been changed
4-173	4	4	4	4	15	In this paragraph, first mass loss is discussed, then area changes and (L11-L15) again ice loss is considered. Re-organize? Or split into two paragraphs 1/ area, length change and disappearance of glaciers and 2/ mass balance on the long term with a focus on the well-observed 2005-2009 time period. Currently the logic of the paragraph is a bit hard to follow [Etienne BERTHIER, France]	Taken into account: the respective bullet has been changed
4-174	4	4	4	4	15	Why only the recent data is used but no eariler data? [Tao Che, China]	Taken into account: the respective bullet has been changed
4-175	4	4	4	4	15	Why only citing the most recent data, but ignoring the earlier results? [Jing Ming, China]	Taken into account: the respective bullet has been changed
4-176	4	4	4	4	23	The numbers for global ice loss seems to be in discrepancy with the ice loss from the Greenland Ice sheet in the next paragraph. [Government of NORWAY]	Taken into account: the respective bullet has been changed

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4-177	4	4	4			I suggest including the englacial warming measured/inferred at very high altitude sites (Alps, Andes) in the executive summary. See Chap 4, Page 24, Line 1 [Etienne BERTHIER, France]	Rejected: the information is only based on local observations and cannot make it into the Executive Summary
4-178	4	4	9			'Regionally' is not clear. Do you refer to part of some mountain ranges here? Maybe 'locally' would be best? [Etienne BERTHIER, France]	Taken into account: the respective bullet has been changed
4-179	4	4	10	4	10	On p. 21, lines 55-56, it is stated that more than one hundred glaciers have disappeared in the last 30 years, not "Several hundred" as stated here. [Government of United States of America]	Taken into account: the respective bullet has been changed
4-180	4	4	10	4	10	"several hundred glaciers" should be more detailed in the number. Considering the studies done in Tibetan Plateau, this statement is too misleading, which should be deleted here, or in stead, presenting the change in area and volume. The current number change is meaningless, e.g. a larger glacier can generate more smaller glaciers after its shrinkage. And the number judgement is too uncertain [Jing Ming, China]	Taken into account: the respective bullet has been changed
4-181	4	4	11	4	13	This statement is not true neither for NW Himalayan glaciers, stable or even gaining mass in the nineties [Azam et al., J. Glaciol, 58 (208), 315-324, doi:10.3189/2012JoG11J123, 2012.; Vincent et al, The Cryosphere Discuss., 6, 3733-3755, 2012] nor for Karakoram glaciers, presently stable [Gardelle et al, Nature Geosc., DOI: 10.1038/NGEO1450, 2012; Kaab et al, Nature 2012]. Since these glaciers represent large ice masses, I beleive that the sentence should be re-written to take these specific features into account. [Patrick WAGNON, Nepal]	Taken into account: the respective bullet has been changed
4-182	4	4	13	4	13	Comment text: It's not clear if the "a slight decline" is absolute or relative to the "increases in ice loss" earlier in the sentence. If the latter then a clearer wording would be "a slight reduction in rate". [Peter Barrett, New Zealand]	Taken into account: the respective bullet has been changed
4-183	4	4	13	4	13	There are more than two studies that indicate higher ice loss in 1920-1940 than today. Change "Two" to "Recent". [Jacob Clement Yde, Norway]	Taken into account: the respective bullet has been changed
4-184	4	4	13	4	15	Any evidence about other glaciers to bring forward here ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account: regional details are now included but space for further details is limited
4-185	4	4	15	4	15	"medium evidence medium agreement" - what do these rankings mean? [Government of Australia]	Accepted - uncertainty language modified to house style
4-186	4	4	17	4	17	Include some statement about snowfall in Greenland [Christopher Little, United States of America]	Noted: See Buchart et al. 2012 Climate of the past: low certainty confidence in an increase in precipitation in recent decades in Greenland
4-187	4	4	18	4	19	Comment text: Comparisons in ice loss would be more easily understood if averages for successive intervals were presented, rather than averages for a shorter and a longer intervals from the same starting point. Why not present the data in terms of two periods 1993-2004 and 2005-2010, so that the rate of change can be more clearly seen? I note it's done in some parts of the chapter but not others. [Peter Barrett, New Zealand]	Rejected. These are the agreed periods with Chapter 13 Sea Level Rise to maintain consistency.
4-188	4	4	21	4	21	It could be useful to specify: increased surface melt and runoff (about 60%) and increased glacier discharge (about 40%):….(source: 4.4.2.2.1; p. 29, line 50/51) [Government of Germany]	Accept. Percentages have been added to the text.
4-189	4	4	21	4	21	increased surface melt and runoff, and increased glacier discharge': mixing the terms runoff and discharge in the same sentence may be confusing for readers familiar with their meaning in hydrology. Suggested change: "increased surface meltwater runoff, and increased ice discharge. (Glacier discharge is understood as the runoff (water) from glaciers. What is meant here is ice discharge). [Regine Hock, United States of America]	Accept
4-190	4	4	21			Can you be more specific and indicate the partionning of the mass loss between change in SMB and change in ice discharge? [Etienne BERTHIER, France]	Accept. See comment 4-188.
4-191	4	4	25	4	25	The exression of "Robust evidence high agreement" in line 25, however, is "robust evidence, in high agreement", it is better use the same expression [Yongjian Ding, China]	Accepted.
4-192	4	4	25	4	26	It seems inconsistent to use the likelihood terminology here (very likely) for ice mass change in Antarctica,	Accepted.

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						while the paragraph above for Greenland does not make this next step, and only refers to robust evidence and high agreement. Surely if you can provide a quantified likelihood terminology for Antarctica you could also for Greenland. [Thomas Stocker/ WGI TSU, Switzerland]	
4-193	4	4	30	4	30	It could be useful to add: In the near-absence of surface runoff and long-term change of snowfall, Antarctic long-term changes in grounded ice mass are almost entirely explained by increased glacier speed (source: 4.4.2.3.1; p. 31, line 28/29) [Government of Germany]	Accept
4-194	4	4	31	4	31	"spanning" or "covering" not both. [Government of Australia]	Accept
4-195	4	4	31	4	32	Grammar is odd, and may want to quantify that there are no significant changes. [Richard B. Alley, United States of America]	Accept
4-196	4	4	31	4	32	Comment text: "spanning" is redundant. Consider replacing this sentence with something like "Reconstructions of snowfall [from observations?/modelling] now cover the period 1979-2011, strengthening the conclusion from AR4 that the total snowfall in Antarctica is not changing." [Peter Barrett, New Zealand]	Accept
4-197	4	4	31	4	32	Page 4 line 31 to 32. The is sentence should be caveated in the sense that the no change in snow fall is relative to the noise in the signal.... so the noise or natural variability should be worked into this sentence. Maybe ...do mot suggest any detectable change in total snowfall relative to natural variability in Antarctica. It could be expresses as 0 mm plus minus confidence level. [Nathaniel Lee Bindoff, Australia]	Accepted - change to appropriate uncertainty language.
4-198	4	4	31	4	32	There's something wrong with this sentence - the word "spanning" seems to need to be deleted. [Adrian Simmons, United Kingdom]	Accept
4-199	4	4	31	4	32	What does "spanning, now covering" mean ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accept
4-200	4	4	31	4	32	This statement that there has been no change in total snowfall in Antarctica is difficult to reconcile with the statement coming from the ES of Chapter 2 (SOD page 4, line 41) which suggests snowfall in Antarctica has been increasing (albeit with low confidence). [Thomas Stocker/ WGI TSU, Switzerland]	Raise with Chapter 2 - the chapter 2 use is not appropriate for the papers cited. (Chapter 2.5.1.3)
4-201	4	4	34	4	52	This material is not reflected in the text. I.e., we could not identify the same formulations of these three statements in the Executive Summary in the text of Section 4.4. [Government of United States of America]	Accept.
4-202	4	4	34			"some outlet glaciers": To how many many outlet glaciers does this statement refer? What percentage of "some outlet glaciers" compared to the total number of outlet glaciers? [Government of United States of America]	Accept.
4-203	4	4	35	4	36	It is unclear whether the glaciers that "slowed down following acceleration" decreased to slower speeds than prior to acceleration. It is only stated that "none have returned to their conditions prior to 1990s." [Government of Australia]	Accept.
4-204	4	4	37	4	39	Two concerns with this statement: first, the Ant Peninsula changes have not be linked (strongly, anyway) to sub-ice shelf changes. Secondly, the evidence for former grounding of ice under PIG (Jenkins 2010) makes the linkage between ocean forcing in recent decades and the ongoing retreat fuzzy, and that's where the largest changes in flow have occurred. Citing this as being the case in West Antarctica seem to suggest that these flow changes are widespread, when they remain relatively localized. I know that this is the message of Pritchard et al 2012, but I think the Ch 4 authors need to consider whether this truly indicates robust evidence and high agreement. [Christopher Little, United States of America]	Accept.
4-205	4	4	42	4	43	The term "climate warming" is popular but not scientifically correct and should better be avoided in IPCC reports: Climate is defined as a statisticial average of meteorological conditions and as such can neither "warm" nor "cool" or even "improve", "deteriorate" or "have an optimum". Terms like climate change, global warming, atmospheric temperature rise, etc. are preferable. [Wilfried Haeberli, Switzerland]	Accepted. Global replacement for all occurrences of this phrase.
4-206	4	4	42	4	43	This 'very likely due to climate warming' is an attribution statement, which would be expected to come from the	Rejected in part. Phrase "climate warming" replaced.

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						Chapter 10 assessment, and not from Chapter 4. Suggest removal. [Thomas Stocker/ WGI TSU, Switzerland]	However, attribution of ice shelf change has been kept in the text. (As appeared in AR4).
4-207	4	4	42	4	43	"Climate" is defined as the weather averaged over a long period, e.g. 30 year, and, hence, cannot "warm". Please specifiy what the ice shelve retreat and collapse is very likely due to, e.g. atmospheric warming. [Michael Zemp, Switzerland]	As 205.
4-208	4	4	42			began decades ago. Better to indicate when stated in the future "decades ago" can be different from whta the authors mean now. [Government of Chile]	Accepted. Replace phrase "decades ago" with "late 20th century"
4-209	4	4	43	4	43	Atmospheric warming? Or do we include ocean in there? [Christopher Little, United States of America]	As 205.
4-210	4	4	44	4	45	Be clear about whether this statement refers to West Antarctica, if East Antarctic ice shelves are stable, as stated in the next sentence. [Government of Australia]	Accepted. "However" moved in second sentence to clarify the text.
4-211	4	4	44	4	46	Strange wording here -- not sure what to make of this statement. Are many other ice shelves also virtually certain to be thickening? And is the certainty associated with the measurement or the processes underlying the thinning? Also, thinning and stable are not exactly in opposition as indicated by this statement. [Christopher Little, United States of America]	As 210.
4-212	4	4	45	4	46	The Ross Iceshelf is not in East Antarctic. [Jing Ming, China]	Accept. Setence rewritten.
4-213	4	4	45	4	47	This implies that the Amery ice shelf is also stable, which we don't know right now. However, given the amount of melting occuring on the Amery, it could also be metastable right now. Room for interpretations should be excluded here. [Olaf Eisen, Germany]	Accept. Sentence changed - "stable" replaced by "not changing".
4-214	4	4	46	4	47	"are likely stable at present": for what time span? can you really know this? [Heinz Blatter, Switzerland]	As 213.
4-215	4	4	50			The 1.7 mm/yr contribution from Land Ice mass is not consistent with the values found for the same time period in Table 13.1 of the Sea Level Rise chapter (by the way, in this table 13.1 is it really disturbing to have only peripheral glaciers of the ice sheet included in the "glaciers" column) [Etienne BERTHIER, France]	Accept. All numbers cross checked between chapters 4 and 13.
4-216	4	4	52			Should not the reference be to Chapter 13 where the sea level budget is examined? [Etienne BERTHIER, France]	Rejected. This chapter provides the numbers to Chapter 13.
4-217	4	4	54	4	54	add "(NH)" after "Northern Hemisphere" [Yongjian Ding, China]	Editorial - agreed
4-218	4	4	54	4	54	avoid acronym SCE [Regine Hock, United States of America]	Editorial - agreed
4-219	4	4	54	5	7	Please provide estimates of uncertainty in the numbers stated here. [Government of United States of America]	Editorial - agreed
4-220	4	4	54	5	7	It would be nice to mention that snow mass has at the same time increased over large parts of Eurasia; detailed analysis is given in Callaghan et al (2011a, 2011b) - comprehensive studies not cited in the AR5. The papers are: Callaghan T.V. et al., 2011a. The Changing Face of Arctic Snow Cover: A Synthesis of Observed and Projected Changes. AMBIO, Vol. 40 (Suppl. 1), pp. 17–31. DOI 10.1007/s13280-011-0212-y; Callaghan T.V. et al., 2011b. Multiple Effects of Changes in Arctic Snow Cover. AMBIO, Vol. 40 (Suppl. 1), pp. 32–45, DOI 10.1007/s13280-011-0213-x [Andrey Shmakin, Russian Federation]	Noted - Exsumm text has been modified significantly.
4-221	4	4	55	4	55	The statement that most snow cover changes occurred in the 1980's does not really agree with the text later in this bullet, or the results in Figure 4.19. While it's true that average SCE for March and April has not changed appreciably since 1990, the strong reductions in May and June SCE (i.e. primarily Arctic snow) since AR4 are downplayed if the first statement of this bullet is that most snow cover changes occurred in the 1980's. [Chris Derksen, Canada]	Accepted. We were trying to escape from expressing all changes as linear trends, but in conjunction with comment 4-1383 we have reverted to linear trends in order to describe all months.
4-222	4	4	55	4	55	"most reduction" - Does this mean that more than 50% of change over the past 90 years has been in the 1980s. Or that the 1980s was the decade with the most changes compared to all the other decades ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. See response to comment 221.
4-223	4	4	55			"most reduction occurring in the 1980s": How much? How many percent? [Government of United States of	Accepted. See response to comment 221.

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						America]	
4-224	4	4	57			The earlier period in “compared with the period 1922-1970” is too incompatible to compare with the data of 1970-2010. The earlier data are also not homogenous compared with those of the satellite era. [Atsumu Ohmura, Switzerland]	Accepted. See response to comment 221.
4-225	4	5	1	5	1	"5.3 days per decade" No uncertainty on this quantity ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Rejected? The original study cited did not compute uncertainties on the trend.
4-226	4	5	4	5	6	The snow albedo discription is not the observed fact. It is some simulated results. Therefore, these statement should not be put in this chapter. More details see Chapter 4.5.6, that paragraph shoulbe be replaced by some observed data and publish. [Tao Che, China]	Accepted in part. Text revised - the evidence is weak but is not based solely on modeling.
4-227	4	5	4	5	6	The albedo of snow cover is not observed but simulated, and the related content should be moved to other chapters. [Jing Ming, China]	See response to previous comment.
4-228	4	5	6	5	7	Page 6 line 6 to 7. I wondered if snow cover and snow water and permafrost in Southern Hemisphere could be handled as separate bullet. There are so many strong NH results, and the Southern Hemisphere changes in snow cover and snow melt etc records are so weak that there appearance in NH centric results takes away from NH results. [Nathaniel Lee Bindoff, Australia]	Accepted.
4-229	4	5	7	5	7	"no trends" - is no significant or no substantial trends meant here ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
4-230	4	5	8	5	11	The first set of stated values (210 [145 to 275] do not appear in Ch. 4 Table 4.5. Also, can you be specific about what is meant by 'based on different time windows since 2003' - does this mean that one is 2003-2009 and the other is 2005-2009 as listed in table 4.5? This doesn't match up with the values stated though. [European Union]	this comment erroneously refers to page 5 but should be page 4
4-231	4	5	10	5	14	Please provide some description of the "limited evidence" upon which these conclusions are based. Do these include large lakes, e.g., Great Lakes, Lake Ladoga? [Government of United States of America]	Accepted. Will add assessment of evidence.
4-232	4	5	10	5	14	Please provide estimates of uncertainty in the numbers stated here. [Government of United States of America]	Accepted. Will provide uncertainty estimates wherever possible.
4-233	4	5	11	5	11	Care needs to be taken with the use of the term 'acceleration' in the context of rates of change. Over what portion of the time series did this 'acceleration' occur? [Chris Derksen, Canada]	Accepted. Text will be revised.
4-234	4	5	12	5	13	The last sentence has nothing to do with river/lake ice, and should be deleted. [Jing Ming, China]	Rejected. Both river/lake ice and snow cover have a climatically significant seasonal cycle and the sentence compares them.
4-235	4	5	16	5	17	What do you mean with "permafrost temperatures"? Permafrost temperature is a variable temperature field, which does not change uniformly! Please specify where and when the change is up to 3 deg C. [Heinz Blatter, Switzerland]	Noted - Exsumm text has been modified significantly.
4-236	4	5	16	5	18	There is a problem with the magnitude of change given for permafrost temperatures. This may be partly due to errors in section 4.6.2 as some of the rates presented in Table 4.7 are not in agreement with those in the literature as for the most part the changes have been up to only about 2°C over the last 3 decades in the northern hemisphere (where records are long enough to examine trends over this period). The literature quoted in chapter 4 and also the SWIPA report summarizes change in permafrost temperature as typically between 0.5 and 2°C over the last 3 decades for the northern hemisphere. See below for additional comments related to this. [Sharon Smith, Canada]	Noted - Exsumm text has been modified significantly.
4-237	4	5	17	5	17	"up to 3C". Should not only highlight the upbound, but also give the complete range. [Jing Ming, China]	Noted - Modified to use language correctly
4-238	4	5	17			As before better to identify the period involvedin "past three decades". [Government of Chile]	Noted - Exsumm text has been modified significantly.
4-239	4	5	17			"up to 3oC" is this only the maximum. What is the range of the change, and its statistical distribution? [Government of United States of America]	See 4-237

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-240	4	5	18			"in some regions": what regions? How large are they relative to other regions? [Government of United States of America]	Disagree - cannot give detail in Ex Summ
4-241	4	5	19	5	20	"temperature increase for colder … than for warmer permafrost": this statement is only qualitative at best. Provide the quantitative assessment with estimates of uncertainty. [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-242	4	5	20	5	21	How is the "Russian European North" different from the "Russian North"? [Government of Canada]	Noted - Exsumm text has been modified significantly.
4-243	4	5	21	5	21	delete "where:" [Yongjian Ding, China]	Editorial
4-244	4	5	21	5	21	"where: where" - grammar. In fact, quite a number of typos in the chapter. Not listed here, available on request. [Olaf Eisen, Germany]	See 4-242
4-245	4	5	21	5	21	Remove the first "where" before the colon ("where: where") [Government of Canada]	See 4-242
4-246	4	5	21	5	21	"...where: where taliks…" should just be "where: taliks…" [Robert Kandel, France]	See 4-242
4-247	4	5	21	5	21	"...where: where taliks…" should just be "where: taliks…" [Robert Kandel, France]	See 4-242
4-248	4	5	21	5	21	"where: where.." [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	See 4-242
4-249	4	5	21	5	21	Delete ": where". [Jacob Clement Yde, Norway]	See 4-242
4-250	4	5	21			Can anything be said about the reduction in permafrost extent in km2 here? [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-251	4	5	21			Is it correct to have two “where”? [Atsumu Ohmura, Switzerland]	See 4-242
4-252	4	5	21			It is suggested to include the term talik in the glossary as it is a quite specific term. [Klaus Radunsky, Austria]	Noted
4-253	4	5	23	5	23	What boundary are we talking about here? Insert either "sporadic" or "discontinuous" in front of "permafrost". [Jacob Clement Yde, Norway]	Noted - Exsumm text has been modified significantly.
4-254	4	5	23	5	24	The concept of boundaries/limits, imagined as a line that can migrate,should be avoided because: (a) it is misleading, suggesting a rather homegenous phenomenon, and (b) because this usually cannor be defined or measured in a clear way. [Stephan Gruber, Switzerland]	Noted - Exsumm text has been modified significantly.
4-255	4	5	23	5	24	It should be clear that this shift in the permafrost boundary is observed for Russia, not the entire permafrost zone. [Sharon Smith, Canada]	Noted - text revised.
4-256	4	5	26	5	27	Focus should be on average values and changes, not a maximum value from a specific site ("increased by up to 90 cm"). Does a maximum value in active layer increase make sense at all, considering that "permafrost with a thickness of 10 to 15 m completely thawed..." (page 4, line 22)? Rephrase this sentence. [Jacob Clement Yde, Norway]	Disagree - specific examples give the scale of possible changes
4-257	4	5	27	5	28	There are also CALM sites with decreasing active layer sites, so the range spans from increasing through decreasing active layer thickness. [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-258	4	5	27	5	28	"tens of centimetres" sounds like less than 90cm to me, but presumably it does mean here from a few centimetres to 90cm - maybe better to give some numbers ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted - Exsumm text has been modified significantly.
4-259	4	5	27			"up to 90cm" is the only the maximum. What is the range of the change, and its statistical distribution? [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-260	4	5	29	5	29	avoid acronym ALT [Regine Hock, United States of America]	Noted - avoided in Ex summ
4-261	4	5	29			I would add the following sentence “ Even in an area where the air temperature remains stable in the last 15 years as the Ross area in Antarctica, the active layer thickness is increased of 1 cm/year (Guglielmin & Cannone, 2012). [Mauro Guglielmin, Italy]	Disagree - this is too much detail from an area where permafrost is largely insigifnicant in impact
4-262	4	5	31	5	32	Suggest the authors explain that this sentence pertains to areas not underlain by permafrost. [Government of	Noted - Exsumm text has been modified significantly.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						United States of America]	
4-263	4	5	32	5	33	The changes in Russia and China should not be emphasized in ES here, and this should be deleted. [Jing Ming, China]	Noted - Exsumm text has been modified significantly.
4-264	4	5	32	5	34	Page 5 32 to 34 I found this sentence confusing. Why does the active layer thickness decrease with warmer air temperatures? Are you able to use likelihood language here, or confidence language around change in thickness to calibrate this result. [Nathaniel Lee Bindoff, Australia]	Noted - Exsumm text has been modified significantly.
4-265	4	5	33			"about 32 cm" Please clarify. Is this statement based on one measurement? [Government of United States of America]	Noted - Exsumm text has been modified significantly.
4-266	4	6	3	6	12	The definition of the crysphere should also include partly snow covered land and for all the areas, hydrology and nature where ice, snow and permafrost occur. [Government of NORWAY]	Disagree - the definition explicitly includes snow. Hydrology and nature, are not part of the cryosphere.
4-267	4	6	3	7	33	Comment section 4.1. Excellent introduction with key concepts simply expressed, including use of units with the relationship between Gt of ice and water, and mm of sea level rise explicitly stated. [Peter Barrett, New Zealand]	No response required
4-268	4	6	4			why not 'ice' rather than 'water in the frozen state'? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Noted - we attempt to be clear, noting that the non-scientific reader might not realise that snow and frozen soil contain what scientist would say is "ice".
4-269	4	6	5	6	5	Ice caps are mentioned here, but nowhere else in the chapter apart from the titles of many referenced articles. It might be worthwhile to mention/justify here (rather than lines 8-12) on page 7) your choice of ice sheets to replace this term (also perhaps the term inlandsis). Also, the term ice shelves does not appear here, although it appears in many places in the chapter. [Robert Kandel, France]	Noted - The definition at the end of the introduction clearly states why "ice caps" does not appear later in the chapter.
4-270	4	6	7	6	7	The river and lake ice generally only survive from winter to spring in North Hemisphere, thus this sentence is not so certain [Yongjian Ding, China]	Reject - with respect, we believe the sentence is clear enough.
4-271	4	6	9	6	10	Comment text: The antiquity and history of the East Antarctic Ice Sheet is better known than these lines indicate, and a single reference to a modelling paper is inappropriate. Wording and references along the following lines are suggested. "on the other hand, the Antarctic ice sheet first formed as a dynamic feature 34 million years ago (Zachos et al., 1992; Barrett, 1996), with its East Antarctic component becoming relatively stable around 14 million years ago (Barrett, 2012, in press)." The latter paper provides a comprehensive review and resolves the controversy over East Antarctic Ice Sheet instability in Pliocene times in favour of its persistence for the last 14 Million years. It is referenced in AR5 Ch 5 and a copy is available from the TSU. References: Barrett, P.J. (1996). Antarctic paleoenvironment through Cenozoic times - a review. Terra Antartica, 3, 103-119. Barrett, P.J. (2012, in press). Resolving views on Antarctic Neogene glacial history – the Sirius debate.Earth and Environmental Science: Transactions of the Royal Society of Edinburgh. Uploading at same time as the comments are submitted. Zachos, J. C., Breza, J., & Wise, S. W. (1992). Early Oligocene ice sheet expansion on Antarctica, sedimentological and isotopic evidence from the Kergulen Plateau. Geology, 20, 569–573. [Peter Barrett, New Zealand]	Accept, updated Barret reference is now available to cite and is used.
4-272	4	6	10	6	10	It is not clear why a short computer modelling paper (DeConto and Pollard 2003) was regarded as the appropriate reference to 30 Million years of Antarctic ice sheets - rather than, for example, the references cited in that paper. [Government of Australia]	Same as 4-272
4-273	4	6	15	6	6	Fig. 4.1. caption: Why 'blue/grey' I only detect one color in the legend of figure 4.1 [Regine Hock, United States of America]	Figure caption revised
4-274	4	6	15	6	6	Fig. 4.1. caption: Why 'blue/grey' I only detect one color in the legend of figure 4.1 [Regine Hock, United States of America]	Same as 4-273
4-275	4	6	15	6	21	figure caption can be shortened [Government of Kenya]	Disagree - the sources of data need to be cited in the

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							caption.
4-276	4	6	15	6	22	Comment text: Fig 4.1 caption. Great fgure but some colours/contrasts not strong enough eg the yellow line for the average sea ice edge should be a stronger yellow (looks pale blue), and I cannot spot the "shaded area over land and permafrost shows snow cover", the limit shown by the black line is really all that is needed. [Peter Barrett, New Zealand]	Noted - Figure is revised
4-277	4	6	16	6	16	minimum extent' must refer to a time period which should be given here. [Regine Hock, United States of America]	Figure caption revised
4-278	4	6	16	6	16	Consider eventually updating 2011 with 2012 [Seymour Laxon, United Kingdom]	A nice refinement if possible
4-279	4	6	16	6	17	Unclear if 'minimum extent and 'location of the ice edge (15% concentration)' are the same thing. If so use the same terms. [Regine Hock, United States of America]	Figure caption revised
4-280	4	6	20	6	21	Figure caption 4.1: Sentence is highly confusing for broader audience. Suggested reformulation: The Greenland ice sheet and glaciers outside the ice sheet are shown in different colors. (or glaciers disconnected to the ice sheet) [Regine Hock, United States of America]	Figure caption revised
4-281	4	6	21	6	21	What is meant by "glaciers within the ice sheet"? Rephrase. [Jacob Clement Yde, Norway]	Figure caption revised
4-282	4	6	21			Terminology "glaciers within the ice sheets" is not very clear. Peripheral? [Etienne BERTHIER, France]	Figure caption revised
4-283	4	6	28	6	29	Why using multiple datasets and not the new first complete glacier inventory (Randolph Glacier Inventory) that has been compiled for the studies that are cited and used in following subchapters. [Regine Hock, United States of America]	Taken into account: only RGI are now used as a reference since all others are included there
4-284	4	6	29			The GLIMS database is to be cited as GLIMS (YEAR) and not as Zheltyhina (2005). The latest version is: GLIMS (2012): GLIMS glacier database. Armstrong, R., B. Raup, S.J.S. Khalsa, R. Barry, J. Kargel, C. Helm, and H. Kieffer (eds.), U.S. National Snow and Ice Data Center, Boulder, Colorado, USA: Digital media. Online available from: http://www.glims.org [Michael Zemp, Switzerland]	Rejected: all references are included in the RGI which is cited
4-285	4	6	31	6	38	remove second part of paragraph and / or greatly shorten it [Regine Hock, United States of America]	Disagree, this is important context for the entire chapter
4-286	4	6	31	6	38	I suggest to delet this paragraph, or at least to re-formulate it. The current formulation is written in a very popular style and in my view not at a level adequate for the AR5. [Nadine Salzmann, Switzerland]	Disagree, the second half of the paragraph makes an important point, and style should be accessible, so long as it is precise
4-287	4	6	31	6	38	It is important to note that some components can act as filters of the climate signal, i.e. filter out high frequency changes to allow better assessment of longer term trends (decadal to century scale). This is especially true for permafrost temperatures measured below the depth of seasonal variation. [Sharon Smith, Canada]	Noted, but the point is a subtle one, that would complicate a paragraph intended to be simple and clear
4-288	4	6	32			quotes around natural thermometers incorrect [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted - revised to house style
4-289	4	6	40	6	46	Ice sheet changes (particularly in Antarctica) may be a major contributor to sea level change, but also affect ocean circulation and marine ecosystems by their freshwater inputs to the Southern Ocean. [Government of Australia]	Accept, add phrase
4-290	4	6	41			"… physical, biological …": add chemical, ecological [Government of United States of America]	Rejected - suggested addition does not add to meaning or understanding of sentence
4-291	4	6	42	6	42	Suggest to add a reference to WGI AR5 Ch13. [Thomas Stocker/ WGI TSU, Switzerland]	Accept - add reference to chapter 13 (check other referenmce too!)
4-292	4	6	42			could reference chpt 13 as well as WG2 chpt 5? [Antony Payne, United Kingdom of Great Britain & Northern	See 4-292

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ireland]	
4-293	4	6	46	6	46	Maybe reference recent work (Oct 2012) on the effect of Arctic ice shrinkage on climate and Atlantic Multi-Decadal Oscillation in the UK/Northern Europe? See: Nature Geoscience 5, Pages: 788–792 Year published:(2012), Sutton & Dong. [Jeffrey Obbard, Singapore]	Rejected - this is not the place for such specific citation
4-294	4	6	46			Besides the frequently mentioned albedo effect, the effect of latent heat possessed by the cryosphere should be considered important. The direct effect of latent heat of melt is to shift the phase of climate change, and is especially important for sea ice. [Atsumu Ohmura, Switzerland]	Reject - the suggestion is understood, but this is a rather subtle point that would need significant space to explain. "latent heat" could not be used without explanation, etc. This section is meant to be easy to read by non-scientist
4-295	4	6	47	6	47	"ice albedo feedback effect" may be "snow and ice albedo feedback effect" [Tao Che, China]	Accepted
4-296	4	6	47	6	47	"ice albedo feedback effect" may be "snow and ice albedo feedback effect". [Jing Ming, China]	same as 4-296
4-297	4	6	47	6	49	It is not quite this simple - the ice content will be an important factor determining whether thawing of permafrost results in damage to infrastructure (the engineering design is also a factor). Better wording might be "Thawing of ice-rich permafrost may result in damage to vulnerable Arctic infrastructure". The implications for the carbon budget are also a bit more complex and it may be better to just say that changes to frozen ground could alter the carbon budget (and remove mention of methane release). [Sharon Smith, Canada]	Accepted - add "ice-rich"
4-298	4	6	48	6	49	The long-term destabilisation of steep slopes in cold mountain regions all over the world should also be mentionned here; this makes it clear that severe impacts from climate change on permafrost will not be limited to remote regions at high latitudes. [Wilfried Haeberli, Switzerland]	Noted - added if space allows. This section offers examples but does not pretend to be inclusive.
4-299	4	6	49			Suggestion: "atmospheric carbon budget" [David Bromwich, United States of America]	Disagree - "carbon budget" is about transfer of carbon between air, ocean, soil etc.
4-300	4	6	51	7	3	Paragraph can be better structured: put all glacier information together before jumping to sea-ice. Also there is nothing about permafrost. Has there not been any 'substantial progress' worth mentioning? [Regine Hock, United States of America]	Agreed, reordered paragraph, Added "glaciers" to line 52.
4-301	4	6	51	7	3	An important factor related to improvements since AR5, particularly for high latitudes/Arctic cryosphere is the International Polar Year. This involved a concentrated and coordinated effort with respect to Polar research of which a substantial component was focussed on the cryosphere. These efforts led to improved observations, development of new techniques etc. Related to this were integrated cryospheric synthesis at national scales such as that done for Canada (Derksen et al. 2012) and at the circumpolar scale such as AMAP assessment "Snow Water Ice and Permafrost in the Arctic" (SWIPA). These are important things that should be mentioned in the Introduction. Reference: Derksen, C., Smith, S.L., Sharp, M., Brown, L., Howell, S., Copland, L., Mueller, D.R., Gauthier, Y., Fletcher, C., Tivy, A., Bernier, M., Bourgeois, J., Brown, R., Burn, C.R., Duguay, C., Kushner, P., Langlois, A., Lewkowicz, A.G., Royer, A., and Walker, A. 2012. Variability and change in the Canadian cryosphere. Climatic Change, 115: 59-88. [Sharon Smith, Canada]	Disagree, instructions are to avoid advertising of specific programmes of research, even of this large scale.
4-302	4	6	51			Write "..progress has been made throughout cryospheric research and monitoring." [Michael Zemp, Switzerland]	agreed, added "and monitoring"
4-303	4	6	55	6	55	Perhaps it should be mentioned here that sea ice thickness data aren't available for the Antarctic, and why this is so? [Government of Australia]	Disagree - this point is made in the relevation section but is too detailed to be discussed here
4-304	4	6	55	6	55	Is this a reference to the Arctic. If so, refer to 2012 data also. [Jeffrey Obbard, Singapore]	Actually, line 54. Agreed, add "arctic" and "2012"
4-305	4	6	56	6	56	Comment text: Useful to add in brackets how complete (90%) or (99%) [Peter Barrett, New Zealand]	Taken into account: text has been made clear on RGI
4-306	4	6	56	6	57	Write: "A nearly complete vector dataset of glacier outlines allows for better estimates of the total ice volume." Reasoning: The globally complete vector dataset of glacier outlines by Arendt et al. (2012) is rather a MAP than and INVENTORY (cf. definitions in Oxford Dictionary), or - using space agency terminology - rater a Level-1 than a	Disagree - we try to avoid jargon like "vector dataset" But replace "covers" with "includes" Rejected: the RGI is used as the most advanced (in

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Level-2 product. Using the term inventory for Arendt et al. (2012) is strongly misleading since there are two inventories available (WGMS and NSIDC 1989, updated 2012; and GLIMS 2005, updated 2012) and leads to strange conclusions such as the comparison of the claimed "inventory completeness" of almost 100% in AR5 versus 42% in AR4. The globally complete vector dataset of glacier outlines by Arendt et al. (2012) is a major advance especially for the modelling community but far away from a completed world glacier inventory (including all glacier entities with individual timestamps and related attributes such as on glacier length, area, elevation range, classifications). Reference: GLIMS (2005, updated 2011): GLIMS glacier database. Armstrong, R., B. Raup, S.J.S. Khalsa, R. Barry, J. Kargel, C. Helm, and H. Kieffer. 2011 (eds.), U.S. National Snow and Ice Data Center, Boulder, Colorado, USA: Digital media. Online available from: http://www.glims.org WGMS and NSIDC (1989, updated 2012): World Glacier Inventory. Compiled and made available by the World Glacier Monitoring Service, Zurich, Switzerland, and the National Snow and Ice Data Center, Boulder CO, USA. Digital Media. Online available from: http://nsidc.org/data/glacier_inventory/index.html [Michael Zemp, Switzerland]	coverage) inventory of glacier and glacier complex outlines
4-307	4	6	56			"covers now" - "now covers" [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-308	4	6				Figure 4.1: The color scheme is suboptimal since some categories have more than one color, for example can sea-ice just have one color? The label land glaciers is confusing. Is the distinction between ice sheets and other glaciers relevant here? Can glacier ice cover simply get one color? [Regine Hock, United States of America]	Accepted: Fig 4.1 has been improved
4-309	4	7	2			Change “rapid loss” to “rapid ice loss” [Harry Stern, United States of America]	Agreed
4-310	4	7	11			I would replace " For the largest glaciers" by "for the two largest glaciers". To make clear that only two ice sheets currently exist. [Etienne BERTHIER, France]	Agreed
4-311	4	7	13			could make it 'sea water' rather than 'water' this would then justify use of approximately otherwise looks odd [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Agreed
4-312	4	7	17	7	17	Table 4.1 - the legend for this table sates that the table shows 'cryospheric components, sensitivity to climate and potential impacts', however the table does not show 'sensitivity to climate' or 'potential impacts'. Suggest deleting these words. [Government of Australia]	Agreed - deletion made
4-313	4	7	17	7	17	The minimum volme given for Arctic sea ice (18000 km3) is inconsistent with an asumed mean thickness of 2.5 m P. 4 L. 33 and the lower limit on exten given on P. 8 L. 52 [Seymour Laxon, United Kingdom]	Noted
4-314	4	7	17	7	17	The title of Table 4.1 looks not closely relative to the contents, or it is too broad compare to the contents. [Jing Ming, China]	Same as 4-312
4-315	4	7	17	7	18	Table 4.1: Uncertainty for sea level equivalent of ice sheets and glaciers should be added (as these are essential numbers for discussion of future scenarios). [Helmut Rott, Austria]	Taken into account: Table 4.1 has been revised
4-316	4	7	17	7	33	This Table should perhaps split out the WAIS from 'Antarctic ice sheet' to ensure separation of estimates for WAIS and East Antarctic. Throughout this chapter it is generally not possible to separate West from East. [Government of United Kingdom of Great Britain & Northern Ireland]	Disagree, difficulties in defining WAIS and EAIS, and the fact that they are only mentioned here means we should not split them
4-317	4	7	17	7		Table 4.1 Arctic Ice Volume Range doesn't seem to be correct. Summer Arctic Ice volume is much lower. On the conservative side at least greater than 5x10^3 km^3 (e.g. Schweiger et al 2011). Using the volume range based on 2.5m and 3.0m thickness is a bit misleading [Axel Schweiger, United States of America]	Noted - XXX check this
4-318	4	7	17			The sea level equivalent given in Table 4.1 for glaciers is most likely too high - 0.4 would be more realistic (cf. the comment for page 3, line 49). [Wilfried Haeberli, Switzerland]	Agreed - 0.46 - 0.59 is given elsewhere
4-319	4	7	17			Table 4.1: wrong/inomplete citation. Thre area comes from Arendt et al, but not the volume. Where does that number come from ? [Regine Hock, United States of America]	Taken into account: Table has been revised

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4-320	4	7	17			Table 4.1: Rewrite the caption as "Cryospheric components, global surface coverage and potential contribution to global sea level rise." [Michael Zemp, Switzerland]	Same as 4-312
4-321	4	7	21	7	21	Table 4.1 caption: 'glaciers around Greenland and Antarctica' is not correct. The inventory includes glaciers IN Greenland [Regine Hock, United States of America]	Taken into account: Text has been revised
4-322	4	7	30	7	31	above floatation [European Union]	Disagree - this would not be correct
4-323	4	7		17		Section 4.2 is a limited assessment of the current state of our understanding of sea ice and a step back from AR4. It reflects a strong bias towards publications by the lead authors for this section, Comiso and Kwok, and fails to consider the myriad of papers by many other authors who have worked towards documenting the current state of sea ice. For example, one my favorite papers is Perovich et al. 2008; there is only one brief sentence on this paper. Beyond ice-albedo feedback, Perovich et al. 2008 also document the changes in top and bottom melt that is crucial for our attribution of change in Chapter 10. [Ignatius Rigor, United States of America]	Noted.
4-324	4	7				Table 4.1: Glaciers, sea level equivalent - in the text 0.46-0.59 is given (page 3, line 49) [European Union]	Noted. All figures will be revised and made consistent
4-325	4	7				Section 4.2 provides good background, widely describes the Arctic sea ice changes and less extensively the Antarctic ice changes. [Government of Poland]	Noted.
4-326	4	7				Table 4.1: Suggest the authors provide error estimates where available. [Government of United States of America]	Noted.
4-327	4	7				Section 4.2: This section mostly omits major surface observations systems such as the International Arctic Buoy Programme, Ice Mass Balance Buoys, etc., and range of results from IPY, which provide key crucial parameters for sea ice characterization, climate assessment, and understanding of sea ice change. [Government of United States of America]	Accepted. A reference will be cited
4-328	4	7				Section 4.2: This section omits albedo change associated with the Arctic sea ice regime shift from being dominated by multi-year ice to a new state dominated by first-year ice. This is crucial since difference in albedo between multi-year and first-year ice can increase solar heating with an equivalent to thinning the ice by 1 meter (Perovich and Polashenski, Geophys. Res. Lett., 2012). [Government of United States of America]	Accepted. reference will be noted
4-329	4	7				Section 4.2: This section omits temperature on sea ice surface or near sea ice surface, either measured by satellite sensors or in-situ sensors. Temperature is one of the most important parameters for climate assessment and understanding of sea ice change. [Government of United States of America]	Noted. Page limit is an issue and the topic belongs to Chapter 2.
4-330	4	7				Section 4.2: If the intention of this chapter is to provide a robust community consensus of the state of the Arctic sea ice cover, it misses the mark. Instead, it strongly reflects the opinion of a few (albeit well-recognized) experts in the field, with a heavy bias towards satellite-derived observations. More effort should be made to explicitly indicate the level of agreement, evidence and confidence of each reported observation, using the guidance provided to the lead authors. [Government of United States of America]	Rejected. Justification for rejection provided.
4-331	4	7				table 1 not clear how total %ages are derived from components [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Noted - figures are revised to make it clear
4-332	4	7				Table 4.1. In the section “Ice on Land”, in the column “Sea Level Equivalent”, in the row “Total”, the total is not the sum of the entries in the column. It should be 66, not 64.6. [Harry Stern, United States of America]	Noted. All figures will be revised and made consistent
4-333	4	7				Table 4.1. In the section “Ice in the Ocean”, in the column “Volume”, in the row “Total”, these numbers (37.7 to 40.2) are clearly not correct if the volume of ice shelves is 761, as indicated. [Harry Stern, United States of America]	Accepted.
4-334	4	8	9			"… wind and ocean currents drive the drift of individual pieces of ice (called floes)." Wind and currents can also impact pack ice in major ways. [Government of United States of America]	Noted. (see also snow box)
4-335	4	8	19	8	19	dependent on the air temperature.. And solar and thermal radiation….etc. Too fine of a point for summary?	Accepted - modified to read surface energy budget

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Axel Schweiger, United States of America]	and oceanic heat flux.
4-336	4	8	21	8	23	Repeating a comment I made to the first order draft; I still find it would be important to mention also superimposed ice along with snow-ice. Corresponding references are Onstott 1992 (Geoph. Monogr.), Kawamura et al. 1997 (JGR), Nicolaus et al. 2003 (Phys. & Chem. of the Earth), and Wang, C., Wang, K. et al. (revision subm. to Annals of Glaciology 62). [Sebastian Gerland, Norway]	Noted. Too fine a point.
4-337	4	8	27	8	29	The desalination process in multiyear ice occurs when summer melt water at the surface of the ice cover flushes the ice, rather than via melt pond formation. Practical evidence of this process is available in an ice camp, where fresh ice is collected from the top of multiyear hummocks (not the bottom of melt ponds) and melted to provide the camp with water. Suggest sentence be revised to read: “The salinity of the ice decreases as it ages, particularly for multiyear ice where melt water can form at the surface in summer and subsequently drain through and flush the ice.” [Government of United States of America]	Accepted - text revised. particulary MY ice where summer melt water at the surface flushes the ice. In this case, MY is deleted since first year and second year ice are actually the ice types which need desalination.
4-338	4	8	27			Typically "reject," not "eject" is used to describe the reduction of salt as the ice grows. [Donald Perovich, United States of America]	Accepted - text revised.
4-339	4	8	28			Melt pond formation on sea ice is mostly limited to the Arctic [David Bromwich, United States of America]	Accepted - text revised.
4-340	4	8	28			"particularly for multiyear ice." The impact of flushing is greatest in the first summer melt that the ice experiences. You could say "particularly during summer melt when melt ponds…" [Donald Perovich, United States of America]	Accepted - text revised.
4-341	4	8	29	8	30	Suggest this sentence be revised to read: “The salinity and porosity (total volume of brine and air pockets) of sea ice affects its mechanical strength, its thermal properties and its electrical properties – the latter being very important for remote sensing.” [Government of United States of America]	Accepted - text revised.
4-342	4	8	33	8	34	Comment text: For the sentence beginning "This is one of the reasons why.." I suggest continuing "changes in extent and thickness of sea ice are very different in the two regions." [Peter Barrett, New Zealand]	Accepted - text revised.
4-343	4	8	33	8	34	Poorly worded sentence. [Christopher Little, United States of America]	Accepted - text revised.
4-344	4	8	38	8	38	Would it be helpful to include a figure of Arctic Ocean basins? [Christopher Little, United States of America]	Rejected. See FAQ 4.1 Fig. 1.
4-345	4	8	38	14	23	Can a brief discussion of how frequently the various Arctic passages (e.g., northwest passage) have been open in the past and more recently be included here? [European Union]	Rejected.Not systematic monitoriing of all arcitc in published liter.
4-346	4	8	40	8	50	Move to previous section since it applies to both hemispheres? [Christopher Little, United States of America]	Accepted - text revised.
4-347	4	8	41	8	42	Suggest revising to: “Since the advent of satellite passive microwave imaging systems in 1979, …” [Government of United States of America]	Accepted - text revised.
4-348	4	8	43	8	43	"just a few days" seems odd. The data are every other day for 1978-1987, and daily since then. [Walter Meier, United States of America]	Accepted - text revised.
4-349	4	8	46			Typo hence [European Union]	Accepted - text revised.
4-350	4	8	48	8	50	Page 8, line 48 to 50. I think it is a mistake to use only one method for the sea ice extent and sea ice area. The report should be comprehensive and show the same sea ice indicators with multiple methods. To defend this approach, an appendix could be added with the multiple methods being used, and showing that they make no difference. The difference between methods is one of the largest sources of error, for example in sea level measurements and also in ocean heat content. I recommend the figures be shown with multiple methods rather than a single method. [Nathaniel Lee Bindoff, Australia]	Accepted - supplementary material is now introduced to compare the difference between the different methods. The bottom line is that the trends are consistent when using the different data sets.
4-351	4	8	48	8	50	This choice must be motivated. Furthermore, a thorough discussion on the uncertainties associated with the various algorithms used to retrieve ice concentration and extent from satellite measurements should be added. [Thierry Fichefet, Belgium]	Acepted - supplementary material is added to address this concern.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-352	4	8	48	8	50	We still feel this statement needs supporting with a table that provides evidence that the various procedures are generally consistent. It is fine that the assessment here is based on the single technique of Comiso and Nishio, but some quantitative comparison of how this technique compares with others is needed. What about the Hadley ISST dataset that extends further back to 1950 (see separate comment on this)? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - supplementar material on the different algorithms will be provided.
4-353	4	8	50			This chapter considers only 1 of the suite of widely used observed sea ice concentration datasets. This seems a bit like using only 1 GCM for future projections, and it strikes me as perhaps too narrow for this assessment. Isn't the Assessment Report a place where the range of widely used ice cover datasets would be compared, at least briefly? [Ian Eisenman, United States of America]	Accepted - supplementar material on the different algorithms will be provided.
4-354	4	8	50			"based on a single technique (Comiso and Nishio, 2008)." Why this technique and not another? How much difference is there between techniques? Does it substantially change conclusions about changes in ice cover if a different technique is used? [Donald Perovich, United States of America]	Accepted - supplementary material on the different algorithms will be provided.
4-355	4	8	52	8	52	Arctic sea ice cover is not seasonal, as there is some ice cover all year round (unlike in the Antarctic for the most part). It would be more correct to say "Arctic sea ice cover varies seasonally, with the average ice extent … ". [Government of Canada]	Accepeted. Change to Arctic sea ice cover varies seasonally
4-356	4	8	52	8	52	"average" over what period - entire timeseries I would guess, but should explicitly state for clarity [Walter Meier, United States of America]	Taken into Account. The climatology uses the average of all data.
4-357	4	8	52	8	53	The recent concern of sea ice reduction is often focused only on summer minimum. This is happning in the cetrral Arctic Ocean. As this section noted, winter extent reaches to far south in the sub-Arctic seas. Winter variation is occuring not in the Arctic Ocean but in the sub-Arctic seas. It may helpful to look into the meaning of the annual curve of sea ice exttent in the Northen Hemisphere, mentioning of "summer min in CentralArctic" and "winter max. in the sub Arctic" is useful. Arctic Ocean is full in winter with ice. In 2012 April sea ice extent was almost maximum in the last 10 years in this month, but Arctic ice has not increased as this is due to Bering Sea ice increase. [Hiroyuki Enomoto, Japan]	Taken into accoun. Trends for the maximum extent in winter is now cited.
4-358	4	8	52	8	53	Please, specify the time period over which the average has been computed. [Thierry Fichefet, Belgium]	Taken into account.It is the average over all available data.
4-359	4	8	52	8	53	Average over what time period? The two references given are 16 yrs. Apart -- didn't the average extent change over that many years? [Dorothy Hall, United States of America]	Taken into account. More current references cited
4-360	4	8	52	8	57	The arctic sea ice extent should be updated for 2012 based on NOAA's Arctic Report Card to be published early December 2012. Alternatively the current numbers should be connected to which year or years. [Government of NORWAY]	Accepted
4-361	4	8	53	8	54	This statement is not completely correct and should be stated as: "The summer ice cover is confined mainly to the Arctic Basin and to the Canadian Arctic Archipelago..." [Government of Canada]	Taken into account. Inserted Canadian Archipelago.
4-362	4	8	55			This statement, indicating that the ice cover at the end of the summer consists primarily of thick, old and ridged ice types, is misleading under current conditions. As the observations in this section show, there is a trend towards increased amount of seasonal ice making up the ice pack even at the end of summer. Suggest revising to read: “At the end of summer, a large portion of the Arctic sea ice cover consists of thick, old and ridged ice types.” [Government of United States of America]	Accepted - text revised ans suggeestions incorporated.
4-363	4	8	55			This assumes no first-year ice survives. Maybe add "and some first year ice that has survived the summer melt." [Ron Lindsay, United States of America]	Accepted - text revised.
4-364	4	8	56			Suggest revising from: “Interannual variability is largely determined by…” to “Interannual variability is largely EVALUATED by…” [Government of United States of America]	Rejected.
4-365	4	8				Section 4.4.2 contains a lot of details, but there is a lack of information, how changes of ocean temperature	Rejected - outside the scope of the chapter.

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						(Atlantic water) influences on the Arctic sea ice cover [Government of Poland]	
4-366	4	9	2			We have some concern that the observational dataset used in chapter 10 for the detection and attribution of changes in Arctic Sea Ice (Hadley ISST_ice) is not included in the Chapter 4 assessment. There may be very valid reasons for this, but some discussion of this dataset at least is needed. If there are concerns over the quality of this dataset then these should be communicated with Chapter 10. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. There are indeed some valid reasons and a discussion of this is now presented in a supplementary material.
4-367	4	9	4	9	31	These paragraphs describe 'significant' seasonal trends in ice extent, referring to Figure 4.2. Is the term 'significant' used here in a statistical sense, or simply to in the context of notable differences. Significance levels are not indicated in any of the panels of Figure 4.2. [Chris Derksen, Canada]	Accepted - text revised.
4-368	4	9	4	9	56	For consistency, all time series should be shown and discussed up to 2012. The trends should also be calculated until 2012. [Thierry Fichefet, Belgium]	Accepted - most of them are now up to 2012 with some exceptions.
4-369	4	9	4		5	I suggest rephrasing. Instead of "Figure 4.2 (derived from passive microwave data) shows both the seasonality of the Arctic sea ice cover, and the large decadal changes that have occurred over the last 32 years" I can be "Figure 4.2 shows the seasonality of the Arctic sea ice cover derived from passive microwave data, and reveals the large decadal changes that have occurred over the last 32 years. [Government of Chile]	Accepted - text revised.
4-370	4	9	4			The entire chapter 4.3 and in particular sections 4.3.2 and 4.3.3 require major revisions to improve structure, clarity and languaage/grammar. Many sentences are poor English and therefore difficult to follow. There are a number of spelling and other editorial errors which I will not point out separately. [Regine Hock, United States of America]	Taken into account: sections have been restructured and revised
4-371	4	9	4			The text would be easier to read for a broader audience if the number of acronyms in the text was reduced. Some acronyms do not seem necessary, either because they only occur a few times or they don't contribute anything substantial. For example for the audience of IPCC the acronym RGI seems unnecessary. It can in all cases easily circumventend by other more understandable terms. [Regine Hock, United States of America]	Taken into account: abbrevations have been reduced
4-372	4	9	5	9	5	"the last 32 years" should be "the last 33 years" because the period from 1979 to 2011 is 33 years. [Government of Japan]	Accepted - text revised.
4-373	4	9	7			What are "decadal changes"? Decadal trends? Maybe "Changes in decadal averages" is what is meant [Ron Lindsay, United States of America]	Accepted - text revised.
4-374	4	9	11	9	13	Section 4.2.2.1, Page 4-9, line 11-13, “Figure 4.2 also shows that the change in extent from 1979 – 1988 to 1989-1998 was significant mainly in spring and summer while the change from 1989-1998 to 1999-2008 was significant during all seasons.” The latter part of this statement (1989-1998 to 1999-2008) is not supported by the figure, which shows that the two are comparable in the early spring (April/May/June). [Government of United States of America]	Accepted.
4-375	4	9	13	9	15	Typos requiring revision: "thick" should be "thickest", and "survives" should be "survive". [Government of Canada]	Accepted - text revised.
4-376	4	9	13	9	15	The bracketed statement is not quite correct. Some thick seasonal ice often survives the summer melt (to then become perennial ice). The statement in the brackets should be changed from "(called perennial ice)" to "(predominantly perennial ice)". [Government of Canada]	Rejected. Perennial ice includes seasonal ice that survives the summer.
4-377	4	9	13	9	15	“The largest interannual changes occur during the summer when only the thick components (called perennial ice) survives the summer melt.” It is no longer true to say that only thick components of the ice cover survive the summer melt. Terminology is not used consistently: “Perennial ice”, as defined later, is not restricted to “the thick components”. Based on the authors' definition, thin first year ice that survives the summer melt is also “perennial”. [Government of United States of America]	Noted. Modified text to take into account the concerns of this reviewer.
4-378	4	9	13			"the change from 1989-1998 to 1999-2008 was significant during all seasons." The blue and gold curves in Figure 4.2 look very close together in April, May, and early June. The differences look very small. Are they really significant differences? Significant in what sense, statistical? [Donald Perovich, United States of America]	Accepted - rewording to refer to seasonal extremes.

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4-379	4	9	14			"perennial ice" is confusing and misleading; "summer minimum extent" is clear and is widely used in the community (e.g., see NSIDC, IICWG, NIC, etc.). Thus, delete "perennial ice" and just simply use "summer minimum extent" here. At the time of minimum sea ice extent, some ice may be newly grown (first-year sea ice) while some other ice areas may still undergo melting and part of those may not survive the melt later. [Government of United States of America]	Accepted -text revised.
4-380	4	9	14			Add "initially thick componenets" as the ice may be thin at the end of the melt. [Ron Lindsay, United States of America]	Accepted - text revised.
4-381	4	9	14			"when only the thick components (called perennial ice)." But according to your definition perennial is only 2 years old and multiyear is 3 and older. As stated before, please remove the added category of perennial and multiyear. Just use first year and multiyear. [Donald Perovich, United States of America]	Noted - text revised.
4-382	4	9	15	9	15	the two references of Comiso in 2011 is the same in the reference list,thus "Comiso,2011a" should replace with "Comiso,2011". This error also in line 37, line 40 in page 10, line 37 in page 15,line 8 in page 16,and figure capation of 4.4. [Yongjian Ding, China]	Accepted - text revised.
4-383	4	9	17	9	17	Changes have been large in the last three years' - state specifically what the three years were - by the time AR5 is published it would imply 2011-13. [Government of Australia]	Accepted - text revised.
4-384	4	9	17	9	17	"Changes have been large…" seems to imply that large changes occurred during 2009-2011, but I think the authors mean that the period has been much lower than earlier periods, as stated in the clause following [Walter Meier, United States of America]	Accepted - text revised.
4-385	4	9	17	9	18	“Changes have been large in the last three years: the average extent for 2009-2011 was less than in earlier periods in all seasons, especially summer.” This statement is not supported by Figure 4.2, specifically the statement that “less than in earlier periods in all seasons”. This figure shows that the 2009-2011 ice extent was comparable to 1999-2008 in the late fall (Nov/Dec) and late spring (April/May). [Government of United States of America]	Accepted - text revised.
4-386	4	9	17	9	18	"the average extent for 2009-2011 was less than in earlier periods for all seasons, especially summer." Figure 4.2 does show that 2009-2011 was quite a bit less in summer and somewhat less in winter. However, it looks very similar to 1999-2008 in March, April, and May. [Donald Perovich, United States of America]	Accepted - text revised
4-387	4	9	17	9	22	Clearly this should be updated to refer to the record minimum extent in September 2012. [Adrian Simmons, United Kingdom]	Accepted - data updated
4-388	4	9	17	9	22	This paragraph needs to be re-written in light of the new record low sea ice extent in summer 2012. [Harry Stern, United States of America]	Accepted - data updated
4-389	4	9	17			Include the 2012 record in the discussion. [Ron Lindsay, United States of America]	Accepted - data updated
4-390	4	9	18	9	19	This needs updating: the September pan-Arctic sea ice minimum extent in 2012 broke the record set in 2007 by a large margin. One could also mention in this paragraph that regional differences in the peripheral seas are important. In March 2012, both Davis Strait and the Bering Sea broke winter records for maximum ice extent (both the 30+year satellite record and the 40+year ice chart record). As a result, the change from maximum winter ice extents to summer minimum ice extents in the Arctic was also record-setting in 2012. [Government of Canada]	Accepted - data updated and text revised. Increases in extent in the Bering Sea and Davis Strait are refected in the winter and spring trends.
4-391	4	9	18			The text should acknowledge the all-time minimum in Arctic sea-ice extent that was reached in September 2012, significantly lower than the 2007 record minimum. [David Bromwich, United States of America]	Accepted - data updated
4-392	4	9	20	9	20	This needs to be updated to account for 2012 data. [Jeffrey Obbard, Singapore]	Accepted - data updated
4-393	4	9	21	9	22	Even if the 2012 new low record in September sea-ice occured after the deadline for the submission of papers, this largely recognized extrem record must be mentioned in this section. In that case, it must be mentioned that this record happened after the submission deadline. [Government of France]	Accepted - data updated
4-394	4	9	22	9	22	Consider adding 2012 ice minimum. [Seymour Laxon, United Kingdom]	Accepted - data updated

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4-395	4	9	23			After line 23, there could be a paragraph about the 2012 sea ice minimum. Or it could be handled in a separate section on 2012 that includes information on the Greenland melt event, too. The Greenland melt event is currently mentioned in the body of the report; should it be? [Dorothy Hall, United States of America]	Accepted - data updated and a paragraph is written. The Greenland melt does not belong in this section.
4-396	4	9	24	9	24	insert specific dates after 'the 32-year satellite record' [Government of Australia]	Accepted - specific dates are now indicated
4-397	4	9	24	9	26	Considering rephrasing as follows: "… consistent trends between monthly anomalies …" should be "…consistent trends in time series of monthly anomalies …" [Government of Canada]	Accepted - text revised.
4-398	4	9	24	9	26	It is unclear how the bracketed statement relates to the rest of the sentence because of how it is written. It would be clearer if the bracket read "(where monthly anomalies are defined as the difference between a monthly average and the 32-year average for that month)". [Government of Canada]	Accepted - text revised.
4-399	4	9	29	9	31	Figure 4.2 shows that ice cover changes in both the eastern Arctic Basin (near Russia) and the western Arctic Basin (near the Alaska coast and in the Canadian Arctic) are largest in summer and autumn, not in "winter and spring" as is currently indicated for the eastern Basin or "summer and spring" as is indicated for the western Basin. Changes in the peripheral seas, on the other hand, are largest in winter and spring. [Government of Canada]	Accepted - text revised.
4-400	4	9	29	9	31	“Ice cover changes are relatively large in the eastern Arctic basin in winter and spring, while in the western basin they are more pronounced in summer and spring”. This is not supported by the figure. Suggest revision to say: “Ice cover changes are relatively large in the eastern Arctic basin, particularly in winter and spring, while in the western basin they are more pronounced in summer and autumn”. [Government of United States of America]	Accepted - text revised.
4-401	4	9	30			“while in the western basin they are more pronounced in summer and spring” – this should be “summer and autumn” [Harry Stern, United States of America]	Accepted - text revised.
4-402	4	9	33	9	34	Note that this comment applies also to page 47, lines 8-11, FAQ4.2 Figure 4.1 and Figure 4.7 where different trends are presented for Arctic sea ice extent. Corrections may be required elsewhere as the decadal trend of -3.9% is cited in several instances. FAQ 4.2, Figure 1: No references are given for the source of the data presented in this figure. It appears that the background sea ice extents (grey areas on the maps) are from Comiso and Nishio (2008), but this doesn't appear to be the study from which the trend numbers (e.g. -3.9% per decade for the Arctic) come from. In Figure 4.7, the trend for Arctic ice extent is given as -4.6% per decade (with a reference to Comiso and Nishio, 2008), which is different from the trend presented in this figure (which has no reference). Upon consulting Comiso and Nishio (2008), the Arctic ice extent and area anomaly trends in this paper are actually given as -3.4% and -4.0% respectively, not -4.6%. That is three different sets of numbers: FAQ4.2 gives -3.9% per decade (no reference); Figure 4.7 gives -4.6% with a reference to Comiso and Nishio (2008) but this article does not contain such a number; Comiso and Nishio (2008) give -3.4% (extent) and -4.0% (area). Close attention should be paid to these inconsistencies with the appropriate corrections made. [Government of Canada]	Accepted - text has been revised and trend values are now consistent
4-403	4	9	33			Suggest revision: “From the monthly anomaly data, the overall trend in sea ice extent in the Northern Hemisphere …” [Government of United States of America]	Accepted - text revised.
4-404	4	9	34	9	34	Page 9 Line 34, are the rates of decline in ice extend including confidence intervals or some thing else. Could you please be clear what the error bars mean in this chapter. This comment is valid everywhere. [Nathaniel Lee Bindoff, Australia]	Accepted - add to text.
4-405	4	9	34	9	34	"% per decade" - what is the baseline period for the %? % relative to what? Suggest the period be the same used for the average [Walter Meier, United States of America]	Noted. The period is indeed the same as for the average.
4-406	4	9	34	9	38	The trend values presented in this sentence do not agree with those in FAQ4.2, Figure 1, to which the sentence is referring to. In this sentence +4% per decade is cited for the Bering Sea, while in FAQ4.2, Fig.1, the number for the Bering Sea is shown as +6.4%. In this sentence, -8% per decade is cited for the Greenland sea, while in the Figure the number is shown as -7.3%. The region with -8% in the Figure is Baffin	Accepted - the numbers are now consistent

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Bay / Davis Strait on the west side of Greenland. Also, in the Figure, there is an even larger decrease of -10.3% in the Barents Sea, which would seem to provide the more natural opposite extreme to the range in trend numbers than -8%. Clarification is required as to why this is not referred to instead. [Government of Canada]	
4-407	4	9	35	9	37	The relationship between the large spatial variability in the regional trends and the complex atmospheric circulation system is well accepted within the sea ice community. However, there remains lively debate about the role of the AO, particularly in the recent years of sea ice decline. There may also be some role from the ocean. Suggest deleting this sentence or revising to: “This large spatial variability is associated with the complexity of the atmospheric and oceanic circulation systems.” [Government of United States of America]	Accepted - text revised.
4-408	4	9	35	9	45	All of the percentages and unceratinties make this paragraph is difficult to read. It might be better presented as a table, with a descriptive narrative. [Donald Perovich, United States of America]	accepted. Attached season to each number
4-409	4	9	35			“+4% per decade in the Bering Sea” – this does not agree with FAQ 4.2 Figure 1, which shows +6.4% in the Bering Sea. “−8% per decade in the Greenland Sea” – this does not agree with FAQ 4.2 Figure 1, which shows −7.3% in the Greenland Sea. [Harry Stern, United States of America]	Accepted - text has been revised and trend values are now consistent
4-410	4	9	36	9	37	The phrase "the complexity of the atmospheric circulation system as influenced by the Arctic Oscillation" is a bit cumbersome. Would "variability of the atmospheric circulation, as manifested in the Arctic Oscillation" be better? Should a "for example" be inserted after "Arctic Oscillation"? [Adrian Simmons, United Kingdom]	Accepted -see above
4-411	4	9	37	9	37	There is a reference error, which is also found elsewhere in the text and in the References section (p.51, lines38-41). The references (Comiso 2011a) and (Comiso 2011b) both point to the same paper in the reference section. Clarification is required as to whether there should just be a single reference for this paper or in fact there are two different papers. Also, in the actual reference on p51, there is a typo: "doi:doi:10.1175/..." only needs one "doi". The reference contains a further error, while the submission date was 2011, the paper is actually published in a 2012 volume (Volume 25, Issue 4 (February 2012)). Thus, Comiso 2011 should be corrected to Comiso 2012. [Government of Canada]	Accepted - reference date has been corrected.
4-412	4	9	37	9	40	The cited seasonal trend numbers in extent and area are not found in the reference given (Comiso, 2011a). The correct reference needs to be provided here and added to the list of references at the end. It is likely the error noted in the comment above (regarding Comiso 2011a and 2011b both pointing to the same paper) is related. Similar numbers in Comiso and Nishio (2008), but it is possible this statement is referring to an updated version of this paper which is missing in the references. [Government of Canada]	Accepted - text revised to indicate that the numbers are difference because the data has been updated.
4-413	4	9	37			The Thompson and Wallace study is old. Most of the recent large trend in the Pacific sector is not associated with the AO. [Ron Lindsay, United States of America]	Accepted - text has been revised - see above
4-414	4	9	39	9	39	There is potential for confusion here. The meaning (months) of summer and autmin should be clarified. As read it would appear from thiis that downward trends are greater in summer than autumn whereas elsewhere it is stated (correctly) that the largest trand is in the end of summer ice. [Seymour Laxon, United Kingdom]	Accepted -take closer look, perhaps due to confusion in reading.
4-415	4	9	40	9	40	similar results were obtained…. Seems unecessary since lack of differences between procedures was stated earlier. There are a lot more citable references if completeness is desired [Axel Schweiger, United States of America]	sentences added - see above.
4-416	4	9	40	9	41	The Parkinson and Cavalieri 2012 paper referenced here (and listed on page 60 of the reference section) is for Antarctic sea ice, not Arctic. It is understood that the Arctic version of this paper is in preparation but is not available yet to the public. This sentence therefore should be omitted, if Antarctic numbers were erroneously cited for the Arctic, or the reference should be corrected if Arctic numbers were correctly cited but not from this Antarctic paper. [Government of Canada]	Accepted - check citations.
4-417	4	9	43	9	44	Isn't the trend in area larger in summer also due in part to low bias caused by surface melt and meltponds on the ice? [Walter Meier, United States of America]	Rejected -melt ponds are not an issue for passive microwave at the end of summer when the surfaces are dry.

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4-418	4	9	43	9	45	Suggest providing additional citations to support the conslusions provided in the last 2 sentences in this paragraph. [Government of United States of America]	Rejected - multiyear ice cover discussed below
4-419	4	9	43			What is the error bar for ice concentration in summer time? What is the effect of surface melt on ice concentration estimates from passive microwave in summer time? Surface melt on sea ice impacts passive microwave signatures and results have large errors. [Government of United States of America]	Accepted - quote Steffen and others
4-420	4	9	45			A discussion of sea ice volume trends is required, maybe here, but more likely at the end of section 4.2.2.4. See Schweiger et al (2011). Trends in volume are more consistent than area or extent as measured by the R^2 value of the linear fit (Lindsay et al., 2009) [Ron Lindsay, United States of America]	Noted. Take a look at the ice thickness section.
4-421	4	9	47	9	50	Correction required, as 2009-2011 does not constitute a 5-year average but rather three-year average. [Government of Canada]	Accepted - text revised.
4-422	4	9	48	9	50	This is the caption for Figure 4.2. On line 49, “five-year” should be “three-year” because it refers to 2009-2011. Also, following “from 2009 to 2011”, add “in black”. [Harry Stern, United States of America]	Accepted - text revised and data is updated to 2012.
4-423	4	9	49	9	49	a five-year average should be "a three-year average" because the period from 2009 to 2011 is 3 years. [Government of Japan]	Accepted - text revised.
4-424	4	9	53	9	55	This is the caption for Figure 4.3. On line 55, change “triangle fonts” to either “triangles” or “triangle symbols”. [Harry Stern, United States of America]	Accepted - text revised.
4-425	4	9	57	10	25	While these observations have a high degree of uncertainty, they do play the important role of putting the satellite data into context. That said, the section could easily be reduced to ½ its current length, summarizing the key content in one paragraph. For instance, in a high level document like this, the disucsion of regional studies seems overdone. More generally, why have a separate section for this longer record when it falls well within the scope of Section 4.2.2.1? Suggest combining sections 4.2.2.1 and 4.2.2.2. [Government of United States of America]	Rejected
4-426	4	9	57	10	25	Another example of extension of records beyond satellite record was the work of Tivy et al (2011) for the Canadian Arctic (a somewhat more recent reference than some of the ones provided). Tivy et al (2011) used archival records from the Canadian Ice Service to examine sea ice changes since the 1960s (an example of utilization of under exploited historical data sets).Reference: 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. Journal of Geophysical Research 116 (C03007). doi: 10.1029/2009JC005855 [Sharon Smith, Canada]	Rejected - regional study
4-427	4	9				Section 4.2.2.1: Provides a good summary of the current understanding, though downplays the long running debate related to various methods used to interpret the satellite imagery. While these various methods result in the same general conclusion (i.e. the sea ice cover is in decline), they do result in some variability in the quantitative details (e.g. values of the extent, area, concentration and related trends). Despite this debate, the observations of ice extent and concentration can be classified as robust evidence with a high level of agreement, resulting in high confidence. [Government of United States of America]	Noted. See above modificaitons. Supplementary material is now added to address the concern of this reviewer.
4-428	4	9				Figure 4.2 Trend maps are difficult to read and color choice not very intuitive. Spatial trend maps should have some indication of significance [Axel Schweiger, United States of America]	Noted.long standing issue.
4-429	4	10	3	10	5	Words appear to be missing from this sentence, clarify if should be written as "some based on regional in situ observations taken from ships or aerial reconnaissance (...) while others WERE BASED on terrestrial proxies (e.g., Fauria et al., 2010; Kinnard et al., 2011)." [Government of Canada]	Accepted - text revised.
4-430	4	10	4			Include a short discussioin of Polyak et al. (2011) in this section as well to show an even longer term context. [Ron Lindsay, United States of America]	check
4-431	4	10	5	10	5	Wonder why Polyak et al., Quaterny Science Reviews, 2010 isn't mentioned? [Walter Meier, United States of America]	check
4-432	4	10	8	10	9	Clarify whether this sentence should be written as: "A more comprehensive database compiled by Walsh and	Accepted - text revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Chapman (2001) WHICH covered the entire Arctic showed very little interannual variability until the last three to four decades." [Government of Canada]	
4-433	4	10	8	10	25	The way the muted pre-1970 interannual variability is discussed is strange. It's first stated that it's minimal, then we have to wait a while before it's discussed almost as an artefact. What am I supposed to take from this? [Christopher Little, United States of America]	say it's an artifact ot begin with.
4-434	4	10	9	10	11	The final sentence of this paragraph is not clear: "For the period 1901 to 1998, their results show a summer mode that includes an anomaly of the same sign over nearly the entire Arctic that captures the sea-ice trend determined from recent satellite data." Summer mode of what? Anomaly of what? [Chris Derksen, Canada]	Accepted - text revised.
4-435	4	10	9	10	11	The meaning of this statement is unclear as presented. Suggest revising as: "For the period 1901 to 1998, their results show a summer mode that includes an anomaly of the same sign over nearly the entire Arctic, with the greater magnitudes found in the peripheral seas and along the margins of the summertime ice pack. This mode reflects the spatial patterns in the summer sea-ice trends determined from recent satellite data." [Government of Canada]	check
4-436	4	10	9			The lack of variability is due in part to the lack of observations and the substitution of climatology. I don't think we can't claim a lack of variability in the earlier period based on these data. [Ron Lindsay, United States of America]	Accepted - text revised.
4-437	4	10	10			This sentence doesn't make sense to me. [Ron Lindsay, United States of America]	Accepted - text revised.
4-438	4	10	13	10	25	Before 1953, the longer timeseries is very incomplete with climatology frequently used, but after ice charts became more complete, so the results are more confident after 1953. Also could mention Meier et al., The Cryosphere, 2012, which homogenized the 1953-1978 record with the consistent passive microwave record. [Walter Meier, United States of America]	Accepted - text revised
4-439	4	10	13	10	25	In this review of "pre-satellite" sea ice data, it is surprising that the review paper, Poliak et al., Quaternary Science Review, 29 (2010), 1757, is not cited and commented. Indeed, this paper concludes that the current, observed ice loss appears to be unmatched over the last few thousand years, and not explainable by any of the knwon natural variabilities. This point is important to stress the critical character of the present decline. [Jérôme Weiss, France]	check
4-440	4	10	13		14	I might suggest a change from "Figure 4.3 shows an updated data set with longer time coverage (i.e., 1870 to 2011) that is more robust since it includes additional historical data (e.g., from Danish meteorological stations)", by" An updated data set with longer time coverage (i.e., 1870 to 2011) that is more robust since it includes additional historical data (e.g., from Danish meteorological stations) is shown in Figure 4.3" [Government of Chile]	Accepted.
4-441	4	10	13			More robust than what? Who updated the data set? Perhaps we should use the Meier et al. (2012) data that goes back to 1953? [Ron Lindsay, United States of America]	Noted. More robust than what was in Walsh and Chapman because of newly available data. Check Mier citation
4-442	4	10	16	10	18	The wording of this section isn't great - is the intention to emphasise a lack of trend or decadal variability over the 1870-1950 period, or is it interannual variability which is lacking in that period? [Government of Australia]	Noted. Modified text to take into account the concerns of this reviewer.
4-443	4	10	18		19	“An important factor that may have contributed to the lack of significant interannual variability during that period is the heavy use of climatology to fill gaps,…” Use of climatology cannot affect the interannual variability, it can affect only our ability to detect the interannual variability. Please modify the sentence accordingly. [Petr Chylek, United States of America]	Accepted - text revised.
4-444	4	10	21	10	23	This is a very odd sentence. Read literally it would implay that sea ice extent can change as a result of thre data used to monitor it which is non-sensical. At leat add the word (or replace "drastic") with "apparent" [Seymour Laxon, United Kingdom]	Accepted - text revised.
4-445	4	10	23	10	23	Consider including the figure from Kinnard and explain the methosd and uncertainties. [Seymour Laxon, United Kingdom]	Rejected. Using the record from 1870 is already problematic.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-446	4	10	23	10	23	I think a reproduction of a the 1400 year record from Kinneard would be useful at the expense of the somewhat redundant Fig 4.3 and 4.4 [Axel Schweiger, United States of America]	Rejected. See above
4-447	4	10	23	10	25	Sentence seems a little out of place here - seems to go with earlier proxy discussion. In general, it may be better to separate out the proxy data from the pre-satellite observations - i.e., in one paragraph discuss the pre-satellite observations followed by a paragraph on the proxy records. [Walter Meier, United States of America]	Accepted. Text revised
4-448	4	10	25	10	25	are likely very high.. This appears a little too dismissive. Kinnard et al. provide uncertainty estimate which provide the basis are a more deliberate statement. If there are questions about these uncertainty estimates then this should be discussed. [Axel Schweiger, United States of America]	Accepted. Sentence deleted
4-449	4	10	25			"Again the uncertainties of such studies are likely very high." Which studies? Is it just the Kinnard et al (2011) and Fauria et al (2010) studies, or does the Walsh and Chapman study also have high uncertainty. At what point does high uncertainty make it inappropriate for the IPCC report. [Donald Perovich, United States of America]	Noted. Sentence deleted, see above.
4-450	4	10	28	10	31	Figure 4.4 appears to show data up to February 2012 and the figure therefore cannot be from a paper published in 2011. Please explain or add a caveat that data have been expanded from the original publication. [Seymour Laxon, United Kingdom]	Accepted. Data has been updated to December 2012
4-451	4	10	35			Why is Greenland Sea excluded? [Ron Lindsay, United States of America]	Noted. Greenland Sea is not part of the Arctic basin which is the area of interest
4-452	4	10	35			As before I don't see the need for the separation of perennial and multiyear. What not combine perennial and multiyear and compare the combination to first year ice. That is the important contrast - first year ice and ice that has survived a summer (or more). [Donald Perovich, United States of America]	Rejected. The observed parameters are different and the perennial and multiyear ice are defined. See also the glossary.
4-453	4	10	36	10	36	Should the period 1979-2010 actually be 1979-2012 as is shown on Figure 4? [Government of Australia]	Accepted. Data has been updated and text revised
4-454	4	10	36	10	36	Change "2010" to "2011/2012". [Jacob Clement Yde, Norway]	Accepted - text revised.
4-455	4	10	36	10	37	Some report shows decrease of old ice. Figure 4.4 shows pararell decline of multi-year ice and perennial ice. Is this mean surviced first year ice has not changed? [Hiroyuki Enomoto, Japan]	Noted. First year ice that survives the summer is called second year ice and the change in their extent is not known.
4-456	4	10	38			4 of 8. The authors state that their definition of multiyear ice is from the WMO, and that is true. The WMO says that “old ice” is ice that has survived as least one melt season, and “multiyear ice” is ice that has survived at least two melt seasons. So the authors’ definition of multiyear ice conforms with WMO, but their usage of “perennial ice” does not conform with WMO. So the terminology cannot be justified by appealing to WMO definitions. [Harry Stern, United States of America]	Noted. Perennial ice was not defined by WMO but has been loosely defined as ice that survives the summer melt.
4-457	4	10	39	10	39	salty-> saline [Christopher Little, United States of America]	Accepted - text revised.
4-458	4	10	39	10	41	It would be clearer if "Generally, that ice …" were changed to "Generally, multi-year ice …" [Government of Canada]	Accepted - text revised.
4-459	4	10	43	10	57	It is not clear how "sea ice cover" is defined, and how this relates to extent, and area (which were defined earlier on page 8, lines 46-48. We suggest a definition of sea ice cover/coverage is also introduced earlier, where these other definitions are given. [Thomas Stocker/ WGI TSU, Switzerland]	accepted - text revised to refer to glossary
4-460	4	10	44	10	44	Should the period 1979-2011 actually be 1979-2012 as is shown on Figure 4? [Government of Australia]	Noted - 2012 is now added
4-461	4	10	47	10	50	Do the reported uncertainties in the trends include the measurement uncertainties or is this simply the uncertainty of the fit? Some discussion about the uncertainty in mulit-year trends seems indicated if those are used as a proxy for thickness. [Axel Schweiger, United States of America]	Noted. It is the uncertainty (one sigma) of the slope as derived using linear regression.
4-462	4	10	49	10	49	delete "Comiso," [Yongjian Ding, China]	Accepted - text revised.
4-463	4	10	51	10	51	For clarity considering changing: "The higher negative trend …" to "The greater negative trend …"	Accepted - text revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Government of Canada]	
4-464	4	10	51	10	51	Insert "updated from" in front of "Comiso". [Jacob Clement Yde, Norway]	Accepted - text revised.
4-465	4	10	55	10	55	For clarity considering changing: "The higher negative trend …" to "The greater negative trend …" [Government of Canada]	Accepted - text revised.
4-466	4	10				Section 4.2.2.3: This section is confusing in using different definitions of multiyear ice, which is given the same name but has different physical meaning. Also, problems are: (1) multiyear ice is derived from different type satellite sensors, (2) from passive microwave but with different methods based of passive brightness temperature signature versus ice drift tracking in time, or (3) within the same sensor with same method but multiyear ice signatures do not fit within the definition of surviving two summers. [Government of United States of America]	Rejected - these issues are addressed in Comiso (2012).
4-467	4	10				Section 4.2.2.3 Multiyear/Seasonal Ice Coverage: This is an important section, reporting on a key change in the composition of the Arctic sea ice cover. The story here is the significant increase in the relative amount of seasonal ice, replacing the older, thicker multiyear ice. I strongly recommend that the authors maintain a focus in reporting on trends in the relative amounts of multiyear and seasonal ice, versus multiyear and perennial ice. Seasonal ice and perennial ice are not the same. Further, the ability to distinguish between seasonal and multiyear ice is more robust and widely agree to than is the ability to distinguish between perennial and multiyear ice, as defined in this report. [Government of United States of America]	Noted.
4-468	4	11	2	11	5	5 of 8. The discussion of multiyear ice cites results from papers that consider multiyear ice to be ice that has survived at least *one* melt season – e.g. Nghiem et al (GRL 2007), Kwok (JGR 2004), Kwok (GRL 2007), and Kwok (JGR 2009). Thus the “two melt season” definition of multiyear ice put forward in Section 4.2.2.3 is inconsistent with the sources that are being cited in the same section, all of which use the “one melt season” definition. So it is not clear which “multiyear ice” type is being discussed, the “one melt season” type in the references or the “two melt season” type in the definition on page 10. [Harry Stern, United States of America]	Noted
4-469	4	11	3			Kwok, 2004: this paper looked at only 4 years 1999-2003, which show an overall multiyear ice increase (NOT decrease) with more multiyear ice in 3 out of the 4 years. Why are these 4 years relevant here compared to others? Why is the ice increase here relevant to the decrease of multiyear ice assessment in section 4.2.2.3? [Government of United States of America]	Accepted.
4-470	4	11	9			add: "Sea ice volume is an important climate indicator. It depends on both ice thickness and extent and therefore more directly tied to climate forcing than extent alone. However, Arctic sea ice volume cannot currently be observed continuously. Observations from satellites, Navy submarines, moorings, and field measurements are all limited in space and time. The assimilation of observations into numerical models currently provides one way of estimating sea ice volume changes on a continous basis. Volume estimates using age of sea ice as a proxy for ice thickness are another useful method." COMMENT: These texts were obtained from the page http://psc.apl.washington.edu/wordpress/research/projects/arctic-sea-ice-volume-anomaly/ and the main bibliographic reference about how the data were obtained is Schweiger, A., R. Lindsay, J. L. Zhang, M. Steele, H. Stern, and R. Kwok, 2011: Uncertainty in modeled Arctic sea ice volume. Journal of Geophysical Research-Oceans, 116. doi:10.1029/2011jc007084, already cited in this chapter, page 12, line 6. [CELSO COPSTEIN WALDEMAR, BRAZIL]	Rejected - these are discussed in respective subsections.
4-471	4	11	9			Add:Summer ice volume may be more sensitive to warming while summer ice extent more sensitive to climate variability.Reference:Zhang, J., M. Steele, and A. Schweiger (2010), Arctic sea ice response to atmospheric forcings with varying levels of anthropogenic warming and climate variability, Geophys. Res. Lett., 37, L20505, doi:10.1029/2010GL044988 . [CELSO COPSTEIN WALDEMAR, BRAZIL]	Rejected - the discussion is not appropriate here.
4-472	4	11	17			locations -> regions (each region in this study includes several locations) [Ron Lindsay, United States of America]	Accepted - text revised.
4-473	4	11	19	11	19	add "of" after "average" [Yongjian Ding, China]	Accepted - text revised.
4-474	4	11	23	11	31	If these studies use a density to convert to thickness, can thickness be used instead? [Christopher Little,	Rejected. Yes, but assumptions about densities have

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						United States of America]	to be made.
4-475	4	11	26	11	26	Attention is required as there may be an error in labelling figures. Figure 4.7 is referenced here before any references are made to Figures 4.5 (a 5-year IceSat ice thickness figure which is only referred to later on) or 4.6 (an Antarctic sea ice figure associated with the section following this Arctic section). Should Figure 4.7 therefore not be Figure 4.5? [Government of Canada]	rearrangment
4-476	4	11	26	11	26	There is a need here to be more specific in the reference to Figure 4.7. The bracket should be changed from (Figure 4.7) to (Figure 4.7b). Panel b in Figure 4.7 is the only one associated with the regression analysis of submarine data. [Government of Canada]	Accepted.
4-477	4	11	26	11	26	Figure 4.7 is refered to before Figures 4.5 and 4.6. [Jacob Clement Yde, Norway]	Accepted - figure moved.
4-478	4	11	26			It looks like Figure 4.7 is introduced before Figures 4.5 and 4.6. [Harry Stern, United States of America]	Accepted - figure moved.
4-479	4	11	27	11	28	Please, specify the region for which those numbers are valid. [Thierry Fichefet, Belgium]	Accepted.
4-480	4	11	28	11	28	The paper by Wadhams shows no change in mean thickness but a slight decrease in modal thickness but over a very limited area. The results do not really carry much authority compared with the satellite observations of ice type change. It could be argued that the change in thickness seen is not so signficant given the very limited geographical coverage (North of Greenland). [Seymour Laxon, United Kingdom]	Noted.
4-481	4	11	29	11	29	(Wadhams et al.,2011) replaced with "Wadhams et al.(2011), [Yongjian Ding, China]	Accepted - text revised.
4-482	4	11	34	11	35	Consider wording: "…a spatially comprehensive estimates …" to "…a spatially comprehensive estimate …" [Government of Canada]	Accepted - text revised.
4-483	4	11	34			I suggest to begin with Satellite altimetry tachniques…. And delete "It has been demonstrated that" and put a reference of it. [Government of Chile]	Accepted - text revised.
4-484	4	11	36	11	36	The bracket text is incorrect. Freeboard is not "(the floating portion of sea ice)". Freeboard is "(the height of sea ice above the water surface)". [Government of Canada]	Accepted - text revised.
4-485	4	11	37	11	38	Recommend that the reference to challenges be more specific with a suggested revision as follows: "The principal challenges to accurate thickness estimation VIA SATELLITE ALTIMETRY are in the discrimination of ice and open water, and in estimating snow cover thickness." [Government of Canada]	Accepted - text revised.
4-486	4	11	40	11	40	One comma too many, it should be "on the ESA ERS and Envisat satellites…" ESA is no satellite. [Robert Kandel, France]	Accepted - text revised.
4-487	4	11	40	11	40	One comma too many, it should be "on the ESA ERS and Envisat satellites…" ESA is no satellite. [Robert Kandel, France]	Accepted - text revised.
4-488	4	11	40			“Radar altimeters on the ESA, ERS and Envisat satellites” – ESA is not a satellite. [Harry Stern, United States of America]	Accepted - text revised.
4-489	4	11	41	11	43	Second half of sentence could be written more clearly. Suggestion: "…, the high variability and shortness of the time-series (1993–2001) indicate that the trend (which is in a region of mixed seasonal and multiyear ice) cannot be considered as significant" [Government of Canada]	Rejected. Original wording preferred.
4-490	4	11	41	11	49	"downward trend…cannot be considered as significant…no significant changes in eastern Arctic…show thinning." This section is confusing. What errors are associated with the data from submarines and the different satellites. Can we say with some certainty that thickness is changing, is not changing? [Donald Perovich, United States of America]	Accepted.
4-491	4	11	46	11	50	Reconsider placement of Figure 4.7: Figure 4.7 is mentioned before Fig 4.5 and inserted 6 pages after its first reference. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - figure moved.
4-492	4	11	48	11	51	There is a need here to be more specific in the reference to Figure 4.7, which here should be specified as Figure 4.7b. [Government of Canada]	Editorial.

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4-493	4	11	49	11	49	Figure 4.7 replaced with "(Figure 4.7c)" [Yongjian Ding, China]	Editorial.
4-494	4	11	50	11	50	Again, can this be updated to cover the 2012 minima? [Jeffrey Obbard, Singapore]	Accepted - text revised.
4-495	4	11	50	11	50	a large decrease in ice thickness due to the 2007 record minimum in summer is clearly seen… I actually don't see that in 4.7 or 4.5 but I may also not understand what this is supposed to say. Maybe needs some rewording. [Axel Schweiger, United States of America]	Noted - rewording
4-496	4	11				Section 4.2.2.4.1: (the following text reflects multiple reviewer's comments) Suggest the authors include a discussion of uncertainty in these measurements. Regarding submarine measurement: Different submarine tracks were at different part of the Arctic underneath different types of sea ice with different thickness in different years, and there are many missing years. Regarding satellite measurements: Ice thickness results are derived from different satellite covering different parts of the Arctic over different ice types at different time resolution and and revisit frequencies in different years. The authors need to distinguish between actual measurements and interpolation and extrapolation. It is unclear how a consistent uncertainty can be assessed here. [Government of United States of America]	Accepted. - add citations and discuss uncertainties.
4-497	4	11				Section 4.2.2.4: This section significantly overstates the current level of confidence in the detailed understanding of changes in ice thickness and volume. Given the challenge involved in estimating ice thickness using indirect techniques (e.g. converting submarine draft measurements to ice thickness, or satellite measurements of ice freeboard to ice thickness), this capability is very much a work in progress. There is considerable and widespread confidence in the observation that the thicker multiyear ice is thinning and that the overall volume is decreasing. The latter is recognized as being the result of the decrease in the thickness of the multiyear ice and, importantly, the increase in the relative amount of seasonal ice. There is significantly less confidence, within the community, regarding the quantitative accuracy of the reported trends. For instance, based on the work presented in Giles et al. (2007) and Kwok and Cunningham (2008), it can be show that errors in satellite-based estimates of ice thickness range from 0.57 m to 0.74 m. Suggest that the results reported in this section be characterized as high agreement, medium evidence. Giles, K. A., S. W. Laxon, D. J. Wingham, D. Wallis, W. B. Krabill, C. J. Leuschen, D. McAdoo, S. S. Manizade and R. K. Raney (2007) Combined airborne laser and radar altimeter measurements over the Fram Strait in May 2002, Remote Sensing of the Environment, vol. 111, pp. 182–194. Kwok, R., and G. F. Cunningham (2008) ICESat over Arctic sea ice: Estimation of snow depth and ice thickness, J. Geophys. Res., vol. 113, C08010, doi:10.1029/2008JC004753. [Government of United States of America]	Noted. However, the stated uncertainties are for a single retrieval but the Arctic scale estimates of thickness and volume are based on a large number of observations, which reduces the error. Additionally, assessment with submarine and upward looking sonar ice draft have provided a more realistic picture of the errors at 25km length scale of ~0.5 m. The figures have been revised to show this.
4-498	4	11				Section 4.2.2.4: The fact that the estimates of ice thickness from all of these methods combine to indicate an overall thinning of the thicker multiyear ice is the finding that can be legitimately classified as high agreement and robust evidence. Conclusions regarding the quantitative details (i.e. the specific amount of ice loss to the 10's of centimeters, which is comparable to the measurement error) is much less certain (high agreement, medium evidence). While this is a fine line, it is an important distinction. [Government of United States of America]	Noted.
4-499	4	12	2	12	4	I don't understand the volume loss numbers. The authors seem to be saying that the total volume loss during 2005-2008 (the four years since 2005) was 6300 km^3. Then they say 1237 km^3/year (October) – what does the "October" mean? Then they refer to the ICESat record (2004-2008) which is a different time period from "In the four years since 2005". Also, should the ice loss have a negative sign in front of it, or not? The authors write "a net loss of 6300" (no negative sign) and then "volume loss of -1237" (negative sign). I think there should not be a negative sign, since loss implies negative already. [Harry Stern, United States of America]	Noted. Text revised to clarify discussion.
4-500	4	12	2			"In the four years since 2005" – maybe this should be "In the four years following 2005" [Harry Stern, United States of America]	Accepted.

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4-501	4	12	7	12	7	There is no mention here of CryoSat-2. The land ice section mentions CryoSat-2 as a significant tool in land ice monitoring, the same is also true for sea ice thickness and volume. [Seymour Laxon, United Kingdom]	Accepted.
4-502	4	12	10	12	11	There is a reference error in the caption for Figure 4.5. Kwok (2009) in the list of references refers to a Fram Strait export paper, not an ice thickness paper. Upon investigation, this figure is found in Kwok and Sulsky (2010), Oceanography or alternatively in Kwok (2010), Journal of Glaciology. Neither of these is listed in the references. [Government of Canada]	Accepted.
4-503	4	12	13	12	13	4.2.2.4.2 should be '4.2.2.4.3" [Yongjian Ding, China]	Accepted - text revised.
4-504	4	12	13	12	13	Change "4.2.2.4.2" to "4.2.2.4.3". [Jacob Clement Yde, Norway]	Accepted - text revised.
4-505	4	12	13	12	27	Updated total sea ice thickness data collected 2007 and 2010-2011 with airborne electromagnetics for Arctic first and second year sea ice north of Svalbard are about to be published (Renner et al. in press, Annals of Glaciology 62, 2013). Corresponding information on modal total ice thicknesses could be added here. [Sebastian Gerland, Norway]	check this paper.
4-506	4	12	13			The section heading should be 4.2.2.4.3. Fix subsequent section headings. [Harry Stern, United States of America]	Accepted - text revised.
4-507	4	12	15	12	15	one is better than "another" [Yongjian Ding, China]	Editorial.
4-508	4	12	22	12	27	Citation does not exactly match the data presented in the reference of Haas et al. (2010). The following correction (changing "since" to "in") should be made: "Airborne EM measurements performed in the Lincoln Sea IN 2004 (Haas et al., 2010) within a latitudinal band between 83°N and 84°N showed some of the thickest ice in the Arctic, with mean and modal thicknesses of more than 4.5 m and 4 m, respectively. IN 2008, mean and modal thicknesses decreased to less than 4.4 m and 3.5 m,, which is most likely related to the narrowing of the remaining band of old ice along the northern coast of Canada." [Government of Canada]	Noted. This has been revised.
4-509	4	12	24	12	25	Inconsistency between "more than 4.5m" and "less than 4.6m" [David Bromwich, United States of America]	Accepted - text corrected.
4-510	4	12	24	12	25	The mean went from “more than 4.5 m” to “less than 4.6 m” – this is not necessarily a decrease! You can say that the ice thickness “changed” from one to the other, but you can’t say that it “decreased” from one to the other. [Harry Stern, United States of America]	Accepted - text revised.
4-511	4	12	28	13	2	There is no mention anywhere in here of the results of moored ULS for ice thickness trends and ice volume export. Some comment should be made even if the conclusion is that this dataset may be limited in some respects. [Seymour Laxon, United Kingdom]	Accepted.
4-512	4	12	31	12	37	Any robust results to relay in this paragraph? If not, since the subsections are so short, why not just combine into one section? [Christopher Little, United States of America]	noted.
4-513	4	12	33	12	35	Sea ice drift and deformation also depend on the internal stress in the ice, and on the coastlines. [Harry Stern, United States of America]	Noted. Internal ice stress is dependent on ice strength and coastlines are boundary conditions.
4-514	4	12	35	12	35	"On time scales of days to weeks, winds as well as the mechanical behavior of the ice cover are responsible for most…". [Jérôme Weiss, France]	Noted.. This was mentioned in the previous sentence.
4-515	4	12	36	12	36	insert ocean before surface [Christopher Little, United States of America]	Accepted - text revised.
4-516	4	12	37	12	37	delete "impacts" [Yongjian Ding, China]	Accepted - text revised.
4-517	4	12	37	12	37	Revision is required. Only one of the words "affect" or "impacts" is needed. The sentence should read either "Changes in ice drift affect surface heat and mass balance of sea ice." or "Changes in ice drift impact surface heat and mass balance of sea ice". [Government of Canada]	Accepted - text revised.
4-518	4	12	37	12	37	heat fluxes? [Christopher Little, United States of America]	Accepted - text revised to read 'energy balance'.
4-519	4	12	40			Should reference Rigor re: drifting buoy network. [Dorothy Hall, United States of America]	Noted -
4-520	4	12	40			1978 to 2011 is 34 years [Ron Lindsay, United States of America]	Accepted - text revised.

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4-521	4	12	40			The International Arctic Buoy Programme has been around since 1979, so 29 years should be changed to 34 years depending on the "date" of AR5. [Ignatius Rigor, United States of America]	Accepted - text revised.
4-522	4	12	40			“over the last 29 years” should be 33 years [Harry Stern, United States of America]	Accepted - text revised.
4-523	4	12	45	12	45	There is a need here to be more specific in the reference to Figure 4.7, which here should be specified as "Figure 4.7e". [Government of Canada]	Accepted - text revised.
4-524	4	12	45	12	45	Figure 4.7 is refered to before Figure 4.6. [Jacob Clement Yde, Norway]	Accepted - text revised.
4-525	4	12	49	12	49	About sea ice weakening: Gimbert et al., JGR-C, 117, C00J12, 2012, recently showed that the strengthening of sea ice inertial oscillations over the Arctic in recent years (from 2002), is not only the trivial consequence of simultaneous variations of ice thickness and concentration, but also resulted from a genuine mechanical weakening of the ice cover. [Jérôme Weiss, France]	Noted.
4-526	4	12	52	12	52	The intended meaning of this sentence would be clearer if "Sea ice export through the Fram Strait, together with growth and melt, is a major component of the Arctic Ocean ice mass balance …" were changed to "Sea ice export through the Fram Strait, in addition to growth and melt, is a major component of the Arctic Ocean ice mass balance …" [Government of Canada]	Accepted - text revised.
4-527	4	12	53	12	54	"Over a 31 year" should be "Over a 32 year" because the period from 1979 to 2010 is 32 years. [Government of Japan]	Accepted - text revised.
4-528	4	12	53	12	55	In the abstract of Kwok (2009), the mean annual outflow through Fram Strait is given as "706±113" and NOT "699±112". Further a period is required at the end of this sentence. [Government of Canada]	Accepted - text revised.
4-529	4	12	55	12	55	add "." before "But". [Yongjian Ding, China]	Accepted - text revised.
4-530	4	12	55	12	56	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. Therefore, the conclusion of this paper disagrees with the sentence currently written in the second order draft. [Jérôme Weiss, France]	Rejected. The results are based on extrapolation using limited data.
4-531	4	12	55			"no significant decadal trend in Fram Strait area flux in the satellite record." It might be interesting to comment on this in the context of the increased ice drift speed mentioned in the previous section. [Donald Perovich, United States of America]	Accepted.
4-532	4	12	56	12	56	"Decadal trends in ice volume…" [Walter Meier, United States of America]	Accepted - text revised.
4-533	4	12				Section 4.2.2.5.1: Consider including strength, specific dominant wind direction, persistence in wind direction, and the duration of strong wind speed and direction; wind strength alone is not sufficient and can mislead to erroneous conclusions. [Government of United States of America]	Accepted.
4-534	4	12				Section 4.2.2.5.2: Consider comparing with other results/measurements. Satellite method can't accurately account for extreme events, or where sea ice change is large such that images at two different times are decorrelated. Limitations should be noted. [Government of United States of America]	Noted. Results agree with increases in drift speed from buoy data.
4-535	4	13	2	13	2	shouldn't Smedsrud et al. 2011 who find an increase in export be discussed here? [Axel Schweiger, United States of America]	Rejected. The results are based on extrapolation using limited data.
4-536	4	13	17	13	19	A period is needed at the end of the sentence. [Government of Canada]	Accepted - text revised.
4-537	4	13	21	13	25	reference needed for the figures stated in lines23-beginning of line 25. [Government of Australia]	Accepted.
4-538	4	13	21	13	31	Repeating a comment I made to the first order draft; Nicolaus et al. 2010 (JGR) published findings about length of melting season in the high Arctic, and an intercomparison with satellite derived information on onset of melt. These findings are from the previous record low extent year 2007. I suggest to include a sentence or two on these results here. [Sebastian Gerland, Norway]	Rejected. Too short a record.
4-539	4	13	23	13	24	If sea ice advance is 26 days later and sea ice retreat is 35 days earlier, then the ice season duration is 61	Rejected. Days are counted differently,

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						days shorter, not 59. [Harry Stern, United States of America]	
4-540	4	13	25	13	26	This sentence repeats in words the numbers in sentence before -- any way to join them? [Christopher Little, United States of America]	Accepted - text revised.
4-541	4	13	25		26	In which area applies the 2-month lengthening? Either there is a specific region in which the 2-month are correct, or the number is wrong. The original text of Stammerjohn et al., 2012 says: Where Arctic sea ice decrease is fastest, the sea ice retreat is now nearly 2 months earlier and subsequent advance more than 1 month later (compared to 1979/80), resulting in a 3-month longer summer ice-free season. [European Union]	Accepted. This has been clarified.
4-542	4	13	33	13	34	Word "radiation" is missing from "shortwave radiation". Suggest changing to: "… increase absorption of incoming shortwave RADIATION and melt, ..." [Government of Canada]	Accepted - text revised.
4-543	4	13	34		36	I would move these sentences about the radiation to 4.2.2.3 [European Union]	Comment is unclear, "move" to where, or "remove"?
4-544	4	13	34			Maybe reference Perovich et al. (2007) who show the time of melt onset significantly impacts the total absorbed solar flux for the season [Ron Lindsay, United States of America]	Accepted - text revised.
4-545	4	13	34			“shortwave and melt, creating…” should be “shortwave and melt ice, creating…” [Harry Stern, United States of America]	Accepted - text revised.
4-546	4	13	46			It's not clear what the large-scale importance of the Odden is. [Donald Perovich, United States of America]	Noted: the Oden reflects the large-scale structure of the Ocean and is a cliamte indicator
4-547	4	13				Section 4.2.2.5.3: Results are only short term in a region of the Beaufort Sea primarily, in a partial time of each year, low resolution (missing smaller multiyear ice floes from divergence), and in summer when drift speeds are lower. Such results are not representative and cannot be stretched (interpolation/extrapolation) to conclude the relative effects of export vs in-situ melt. [Government of United States of America]	Noted. Discsssion revised.
4-548	4	13				Section 4.2.2.5.3: This presentation is biased in that it highlights small ice export from the Nares Strait in one year (2007) while omiting important ice export from the Fram Strait in many other years. [Government of United States of America]	Noted. There is more than one year of export estimates at the Nares Strait. And, the export at the Fram Strait was discussed.
4-549	4	13				Section 4.2.2.6: Define "ice free" [Government of United States of America]	Text not found in Section 4.2.2.6
4-550	4	13				Section 4.2.2.7: the Odden ice tongue is primarily irrelevant to climate assessment here. It is controlled significantly by the local seafloor feature defined by the Jan Mayan Fracture Zone and the Mohns Ridge. The seafloor is a geological factor, not a climatic factor. [Government of United States of America]	Noted: the local sea floor is not changing, but the occurrence and extent of the Oden; hence there is a signal either from the ocean or the cliamte.
4-551	4	13				Section 4.2.2.7: Polynyas are small, thin ice can grow in it quickly. What is the accuracy of ice thickness estimate in polynyas with passive microwave data (25-50 km resolution, land/ice contamination to 100 km around)? Why are those important for climate change? [Government of United States of America]	Noted: Polynyas are very important for the dep water formation; the occurrence frequency of polynya's are driven by oceanic and climatic forcings. Thin ice in small polynya's cannot be dervived with PM, but larger polynya's (i.e. North Water (10,000 km2) can.
4-552	4	13				Para. 1 under 4.2.2.7 - there is quite a bit of detail here on the Odden sea ice feature which could probably be deleted. [Dorothy Hall, United States of America]	Noted: see remark 5-550
4-553	4	14	2	14	2	Change "decreased by 462 km3" to "decreased from ... to ... km3" in order to give the reader an idea of the proportions we are talking about. The uncertainty of this estimate should also be mentioned. [Jacob Clement Yde, Norway]	Accepted: Text revised
4-554	4	14	2	14	3	“Sea ice production in the 10 major Arctic polynyas decreased by 462 km^3 between 1992 and 2007”. This statement needs some context – what percent decrease is that? [Harry Stern, United States of America]	Accepted: T4xt revised
4-555	4	14	3	14	3	Insert reference. [Jacob Clement Yde, Norway]	Accepted: Reference added
4-556	4	14	7	14	7	Define the use of the term "Fast ice" here instead (or as well) as on page 4-16, line 20 [European Union]	Accepted.
4-557	4	14	7			Please give an estimate of the total area of shorefast ice to provide a large-scale context. [Donald Perovich,	Rejected. Total coverage of fast ice not available at

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						United States of America]	this time.
4-558	4	14	14			“five different individual Siberian sites” but only four are listed [Harry Stern, United States of America]	Accepted. Text revised.
4-559	4	14	17	14	17	Please be consistent to use the minus signal when discussing any decrease or loss. Compared to page 12 shown where it reads "the large volume loss of -1237 km3 yr-1" (literally therefore a volume gain!), here no minus signal is used "thickness loss of 0.33 cm yr-1". However, on page 31 the minus signal comes back again as "a loss of -58xx Gt yr-1". [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
4-560	4	14	17	14	19	Statement regarding fast ice thickness trends in the Canadian Arctic requires updating and reference to more recent data. Long time series of end-of-winter ice thickness data for 3 stations in the Canadian Arctic (Cambridge Bay, Resolute Bay and Eureka; Canadian Ice Service: http://www.ec.gc.ca/glaces-ice/default.asp?lang=En&n=E1B3129D-1) reveal a small downward trend at Eureka, a small positive trend at Resolute Bay, and a negligible trend at Cambridge Bay (Melling, 2012 - his Figure 3.3; updated from Brown and Coté, 1992). None of the trends are statistically significant because they are so small. The Melling reference is: "Melling, H (2012) Sea-Ice Observation: Advances and Challenges, Chapter 3, in Arctic Climate Change - The ACSYS Decade and Beyond. Atmospheric and Oceanographic Sciences Library 43, doi 10.1007/978-94-007-2027-5_3. Springer Science+Business Media B.V. 464 pp." [Government of Canada]	Accepted.
4-561	4	14	22	14	22	should be "freeze-up"? [Walter Meier, United States of America]	Noted.
4-562	4	14	22			Perhaps use "freezup" instead of "freeze-on." [Donald Perovich, United States of America]	Accepted - text revised.
4-563	4	14	22			“freeze-on” should be freeze-up [Harry Stern, United States of America]	Accepted - text revised.
4-564	4	14	25	16	35	4.2.2.2 shows longer records of Arctic sea ice extent. Suggest the authors include a comparable assessment for Antarctic sea ice extent. There a few papers which address pre-1979 sea ice. The authors should also consider a discussion of the confidence level to attach to pre-satellite estimates based on these papers (even if the opinion may be "not sufficiently robust to make a conclusion"). Cotte and Guinet, 2007, Deep Sea Research, "Historical whaling records reveal major regional retreat of Antarctic sea ice", De La Mare, 1997, Nature, "Abrupt mid-twentieth-century decline in Antarctic sea-ice extent from whaling records", Cavalieri, Parkinson, and Vinnikov, GRL, 2003, "30-Year satellite record reveals contrasting Arctic and Antarctic decadal sea ice variability" shows some data for 1973-1977 that indicates that Antarctic sea ice was higher than present before a drop in the late 1970s... [Government of United States of America]	Noted. Please see section on pre-satellite records.
4-565	4	14	27	14	27	Please define specifically what 'more seasonal' means [Government of Australia]	Accepted.
4-566	4	14	27	14	29	Please, specify the time period over which the average has been computed. [Thierry Fichefet, Belgium]	Accepted.
4-567	4	14	29			The reference Comiso et al., 2011 is incorrect. The numbers are from Comiso, J. C. (2010), Polar Oceans From Space, 507 pp., Springer, New York. [European Union]	Accepted.
4-568	4	14	39	14	39	"from 32 years" should be "from 33 years" because the period from 1979 to 2011 is 33 years. [Government of Japan]	Accepted. Actually, it is now 34 years.
4-569	4	14	39	15	12	For consistency, all time series should be shown and discussed up to 2012. The trends should also be calculated until 2012. [Thierry Fichefet, Belgium]	Accepted
4-570	4	14	39		44	A reference is missing. [European Union]	Accepted. Reference provided
4-571	4	14	44	14	44	add the reference after the sentence [Yongjian Ding, China]	Accepted. Reference provided
4-572	4	14	46	14	46	Reference to Figure 4.5 should be Figure 4.6. Figure 4.6 is out of sequence and should be switched with the current Figure 4.7. [Chris Derksen, Canada]	Accepted.

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4-573	4	14	46	14	46	Replace "extent" by "concentration" and 'Figure 4.5" by "Figure 4.6". [Thierry Fichefet, Belgium]	Accepted
4-574	4	14	46	14	46	This should be Figure 4.6, not 4.5 as stated. [Government of Australia]	Accepted
4-575	4	14	46	14	46	Pls change: Fig. 4.6 b,c,d, and e [Government of Germany]	Accepted
4-576	4	14	46	14	46	"Figure 4.5" should be "Figure 4.6". [Government of Japan]	Accepted
4-577	4	14	46	14	46	Change "4.5" to "4.6". [Jacob Clement Yde, Norway]	Accepted
4-578	4	14	46	14	49	The sentence may wrongly lead to the conclusion that the alternating positive and negative trends around Antarctica are related to the Antarctic Circumpolar Wave while the timescale associated with the trend displayed in Figure 4.5 are very different from the ones associated with the Antarctic Circumpolar Wave. I would thus suppress any reference to the Antarctic Circumpolar Wave in this discussion of the trend of sea ice extent. [Hugues Goosse, Belgium]	Accepted. Text has been rewritten
4-579	4	14	46			Figure 4.6 not 4.5 [European Union]	Accepted.
4-580	4	14	46			“Figure 4.5 b, c, d, and e” should be Figure 4.6 [Harry Stern, United States of America]	Accepted
4-581	4	14	49	14	49	In the context of the ACW it is worth mentioning the work of White, W. B., and I. Simmonds, 2006: Sea surface temperature-induced cyclogenesis in the Antarctic circumpolar wave. Journal of Geophysical Research, 111, C08011, doi:10.1029/2004JC002395 which explores the ice-storm connection within the ACW [Ian Simmonds, Australia]	Noted. Other reviewers thinks otherwise.
4-582	4	14	49			How can a time-varying wave produce spatial patterns in the long-term trend? I don't think it is the ACW doing it. [Ron Lindsay, United States of America]	Accepted. Text has been rewritten
4-583	4	14	51			“positive trends are prevalent in the Weddell and Ross seas” in austral winter in Figure 4.6. I don’t see a positive trend in the Weddell Sea in Figure 4.6b – the Weddell Sea looks all white (no trend) to me. As for the Ross Sea, the positive trend seems to be well north of the actual Ross Sea. [Harry Stern, United States of America]	Accepted. Text has been rewritten
4-584	4	14	53	14	53	add the reference after the sentence [Yongjian Ding, China]	Accepted - a reference is now cited.
4-585	4	14	55	14	56	The stated trend value of 1.4 +- 0.2 does not have a citation. [Government of Australia]	Accepted - a reference is now cited.
4-586	4	14	55	14	56	The stated trend period (November 1978 to May 2012) is inconsistent with the stated trend period (of 32 years) within FAQ 4.2 and with the period stated in the SPM of 1979-2011 - consistency needed. [Government of Australia]	Trends values have been updated to December 2012 and they are now all consistent
4-587	4	14	55	15	2	Explain the difference between sea ice extent and sea ice area (open water free surface) at the beginning of the chapter as both dataset are expressed in millions of km2 but do not always vary concomitantly. [Government of France]	Noted. Definitions of ice extent and ice area are also now in the glossary.
4-588	4	14	55	15	7	In a new publication, Holland and Kwok (Nature Geosciences, 2012) connect Antarctic sea ice extent development especially to local winds, rather than temperature. This could be mentioned/cited here, and on page 46 (l. 50-57), and on page 47 (l. 23-26). [Sebastian Gerland, Norway]	Noted.
4-589	4	14	56	14	57	Why are seasonal trends used here? The seasonal trends in Antarctic sea ice hide very distinct monthly trends since the mechanisms across some seasons vary markedly - compare with the statement on the mechanisms on page 15, line 55. [Government of Australia]	Noted. Seasonal trends are used for conciseness and to give an overview about how the trends vary with season. Monthly trends can be approximately inferred from Fig. 4.6a.
4-590	4	14	57	14	57	The stated trend values are not in the cited report. Is the citation wrong or the trend values? [Government of Australia]	Accepted. The correct reference is Parkinson and Cavalieri (2012)
4-591	4	14				Section 4.2.2.8 Arctic Land-Fast Ice. This sections provides many details of ice observations without drawing any conclusions. [Government of United States of America]	Noted. It is not easy to make inferences since the record length of the data is only 9 years. This is also an observation chapter and making attribution is not

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							allowed.
4-592	4	14				Section 4.2.2.8: The authors are encouraged to make some assessment of the uncertainty represented by measurements from different locations over different time periods. [Government of United States of America]	Noted
4-593	4	14				Section 4.2.3 Antarctic Sea Ice: This section shows a myriad of different or opposing changes in different paramaters in different regions during different time periods. These do not seem to support any consistent trend for climate change assessment. What is the implication for Antarctic sea ice? [Government of United States of America]	Noted. There is a lot that we do not know about the variability of the Antarctic sea ice and an assessment is at best inconclusive.
4-594	4	15	2	15	2	Should this be "statistically significant trend"? [Government of Australia]	Noted. Yes, it is an statistically significant trend.
4-595	4	15	5			"Parkinson and Cavalieri (submitted)." Check to see if accepted by 15 March 2013. [Donald Perovich, United States of America]	Noted. Yes, it was accepted and published in 2012.
4-596	4	15	6	15	7	Similar comment to the one immediately above. "an atmospheric circulation pattern that is influenced strongly by the Southern Annual Mode and the Antarctic Circumpolar wave" could be replaced by "variability of the atmospheric circulation associated in particular with the Southern Annual Mode and Antarctic Circumpolar wave". [Adrian Simmons, United Kingdom]	Accepted. Text has been revised accordingly
4-597	4	15	10		12	The source is missing. The colour code reminds to Comisos colour bars, but I could not find the figure in one of his publications [European Union]	Accepted. Source is now provide.
4-598	4	15	11	15	11	Using the same description in Figure 4.2, "average values of" should be "a three-year average". [Government of Japan]	Accepted. The figure has been updated to 2012 and hence now a 4-year average.
4-599	4	15	16	15	19	It is not sufficient to mention the possible effect of reverse-bed slopes here. A more extensive discussion on effects of 2D-topography is necessary, like the one provided in chapter 13: p.38, l33-35, l43-45, p.39, l54. [Olaf Eisen, Germany]	Glaciers
4-600	4	15	32	15	32	Holland and Kwok has now been published. [Seymour Laxon, United Kingdom]	Accepted.
4-601	4	15	32	15	32	To derive a decadal trend from a 19-year serie is assuming [Martin Schneebeli, Switzerland]	Rejected - comment incomplete.
4-602	4	15	32	15	39	This paragraph should perhaps be re-written. Parts of it seem to be inconsistent, some statements are made without consideration of why they are included in this paragraph and sentence structure makes at least one sentence incomprehensible. [Government of Australia]	Accepted.
4-603	4	15	32		39	This paragraph needs to be revised entirely. The citations Comiso 2011 a, b are similar and wrong. The title given under 2011 a and b is published in 2012 and is one and the same publication. Comiso, J.C. (2012). Large Decadal Decline of the Arctic Multiyear Ice Cover Journal of Climate, 25, 1176-1193 10.1175/JCLI-D-11 -00113.1 [European Union]	Accepted.
4-604	4	15	32			"Holland and Kwok (submitted)." Check to see if accepted by 15 March 2013. [Donald Perovich, United States of America]	Accepted.
4-605	4	15	33	15	35	The way that these two sentences are written suggests some inconsistency. Are changes caused by local winds (atmosphere) or by changes in the Gyres (ocean circulation)? [Government of Australia]	Accepted. Text revised.
4-606	4	15	35	15	35	remove "in" at end of line [Walter Meier, United States of America]	Editorial.
4-607	4	15	46	15	46	delete "a trend of" [Yongjian Ding, China]	Accepted. Text changed.
4-608	4	15	53	16	2	Move to section 4.2.4 -- includes arctic [Christopher Little, United States of America]	Rejected.Discussion relates to both hemispheres and cannot be made until Antarctic data are presented (here). Minor change to text made.
4-609	4	15				What about Change in the Antarctic Ice duration shows contrasting regional patterns. Instead of "There have been contrasting regional patterns of change in the Antarctic ice duration" [Government of Chile]	Accepted. Text has been revised.
4-610	4	16	11	16	11	Does "SAM was nagative since the late 1970s" means negative trend of SAM or only late 1970s is negative?	Accepted. Text revised to clarify discussion.

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						[Hiroyuki Enomoto, Japan]	
4-611	4	16	12	16	12	Negative SAM: Less circum polar annular pattern and more meandaring of circulation? How it may relate polynya formation? [Hiroyuki Enomoto, Japan]	Accepted. Text revised to clarify discussion.
4-612	4	16	13	16	13	Weddel polynya was concerned in the former part of this section, but the latter part is concerning generally on polynya. It may confuse area and phenomena of ice production, export. [Hiroyuki Enomoto, Japan]	Accepted. Text revised to clarify discussion.
4-613	4	16	20	16	20	There is no citation for the comment that fast ice is more extensive in Antarctic than the Arctic. [Government of Australia]	Accepted. Statement removed
4-614	4	16	20	16	20	Is it "Landfast" or "Land-Fast"? [Government of Australia]	land-fast is used in glossary. Text now consistent
4-615	4	16	20	16	24	Confusing paragraph -- is fast ice just around E Ant? Surprised that it can really be that thick. Even if it can, is that the most important thing to say about it? [Christopher Little, United States of America]	Noted. The only recent published study is for E.Antarctica. Text makes no explicit comment about thickness. However, since fast-ice is a greater % of ice volume than of area, it is implicit that thickness of fast ice is greater than the average thickness.
4-616	4	16	20			The term "fast ice" is used extensively in the earlier section 4.2.2.8, so It is not necessary to begin section 4.2.3.6 with the words "Landfast ice (or "fast ice")". However, section 4.2.2.8 (Page 4-14, line 7) could be started in this way. [Adrian Simmons, United Kingdom]	Accepted and text revised.
4-617	4	16	26	16	27	It is unconvincing that "ocean swell and waves and strong wind events" can be regarded as climate-related processes. [Government of Australia]	Accepted and text revised.
4-618	4	16	26	16	35	Little is said about the interannual variability of Antarctic fast ice (cf Section 4.2.2.8 on Arctic fast ice). Hence, is a 9-year length of fast ice sufficient for establishing "significant changes", and if so, what is the significance of the changes? [Government of Australia]	Noted. There are no data on the thickness of Antarctic fast ice away from a very few near-coastal sites. (The same holds for Antarctic sea ice in general). The text already cautions that the record is only 9 years
4-619	4	16	32	16	35	Confusion between sectors of the Southern Ocean. As such the sentence and interpretations are confusing. The Indian sector spans the whole 30-160°E and the Pacific sector the 160°E to 60°W. As such, the 20-90°E should be named western Indian sector and the 90-160°E eastern Indian sector. [Government of France]	Accepted. The ocean names used are those in the original publication, but sectors now defined by longitude range only.
4-620	4	16	39		41	As previous comment [Government of Chile]	Unclear what this refers to
4-621	4	16	40	16	41	Should be "Evidence in...these changes is robust and in high agreement." Here is an example (one of many) where in accordance with the uncertainty guidance note, you seem to have a basis to be able to assign 'high confidence' to changes in sea ice, and add quantified likelihood statements. [Thomas Stocker/ WGI TSU, Switzerland]	Synthesis sector has been deleted
4-622	4	16	43	16	43	The term "the ice" should be replaced by "sea ice". [Government of Australia]	Section on Synthesis is deleted. Content is incorporated in the Chapter synthesis and Technical summary.
4-623	4	16	44	16	44	It states here that the overall trend in extent over 1979-2010 has been -4%. However in the SPM it says 3.9% during 1979-2011. Consistency needed. [Government of Australia]	Accepted, figures have been revised for consistency
4-624	4	16	44	16	48	Data from 2011 and 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Accept. Updated
4-625	4	16	44			Here in the "synthesis" section the Arctic sea ice trend is reported as -4% per decade during 1979-2010. But the sections above only mentioned a trend of -3.9% per decade during 1978-2012. This seems like it may confuse readers and would perhaps be simpler to use the same time period in all sections. [Ian Eisenman, United States of America]	Accept. Modified text accordingly.
4-626	4	16	44			p.4.3, 4.9 and elsewhere says -3.9% per decade so it should be consistent with that. [Dorothy Hall, United States of America]	Accept. Modified text accordingly.

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4-627	4	16	50			Section on decrease of Arctic Sea Ice. It could also be stated that during the cold and dark winters sea ice will continue to form for many years into the future. Thus an ice free Arctic Ocean would occur annually when the thinner winter ice is gone. [European Union]	Accept. Modified text accordingly.
4-628	4	16	51	16	53	"Declassified submarine…during 2009." Please provide a reference for this. [Donald Perovich, United States of America]	Accept. Cited references
4-629	4	16				Section 4.2.4: Suggest a discussion of uncertainty in the referenced measurements and error bars for the corresponding figures. [Government of United States of America]	Accept. Modified text accordingly.
4-630	4	17	2	17	3	Following comment #3 above: This decrease of resistance to wind forcing is not only the trivial consequence of decreasing concentration and thickness, but also the signature of a genuine mechanical weakening, as shown by Gimbert et al., JGR-C, 117, C00J12, 2012, from an analysis of inertial oscillations and a simple coupled upper ocean/sea ice dynamical model. [Jérôme Weiss, France]	Accept. Cited references
4-631	4	17	8	17	8	Explanation needed of why these sea ice characteristics are not known for Antarctica [Government of Australia]	Noted. There are actually patterns in Antarctica that are similar - warming in the Antarctic Peninsula and West Antarctica corresponding to negative trends of sea ice in the B/A seas and cooling in parts of East Antarctica corresponding to positive trends in sea ice in the Ross Sea.
4-632	4	17	8	17	10	Data from 2011 and 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Accept. Updated
4-633	4	17	8	17	10	Antarctic sea ice extent trend is given here for 1979-2010, whereas on page 14, line 55+, information is given until 2012 (which was updated relative to the first order draft). Trends differ slightly accordingly.Unless this is not done on purpose (there might be a difference in which trends exactly were calculated, in terms of means, increments etc.), I would suggest to update or extend the information (with also including data until 2012 where it is not done yet) so that it is consistent/more clear at the different places in the document. [Sebastian Gerland, Norway]	Accept. Updated
4-634	4	17	9	17	9	States here that increase in ice extent of 1.1% per decade between 1979-2010, but SPM states 1.4% per decade between 1979-2011. Consistency needed. [Government of Australia]	Accept. Modified text accordingly.
4-635	4	17	9	17	9	The trend and the dates should be consistent with those presented at 4-3-35/36 (and at 4-47-23/24). [Ian Simmonds, Australia]	Accepted
4-636	4	17	9			Here in the "synthesis" section the Antarctic sea ice trend is reported as 1.1% per decade during 1979-2010. But the sections above only mentioned a trend of 1.4% per decade during 1978-2012. This seems like it may confuse readers and would perhaps be simpler to use the same time period in all sections. [Ian Eisenman, United States of America]	Accepted
4-637	4	17	10	17	10	The statement "indicating an increase in concentration" does not seem to take into account the earlier statements within Section 4.2.3.1 and Figure 4.6 which clearly show that there has been an increase in concentration around parts of Antarctica. [Government of Australia]	ACcepted. Included in the discussion.
4-638	4	17	10			Here it says that Antarctic ice extent is increasing more slowly than area (indicating an increase in concentration). It should probably be mentioned that while this is true for ice concentration estimates derived using the Bootstrap algorithm (used in this chapter), it does not hold for all widely used ice concentration estimates: Parkinson and Cavalieri (2012, dpi:10.5194/tc-6-871-2012) use the NASA Team algorithm and find the opposite result (extent is increasing more rapidly than area). [Ian Eisenman, United States of America]	Disagree. The NT1 used in Parkinson and Cavalieri is not as reliable as the NT2 as decribed by Markus and Cavalieri. Further discussion on this is provided in the supplementary material.
4-639	4	17	24	17	24	Section 4.3: The major flaw of the section is its tendentious citation of refences especially on Chinese glaciers. [Jing Ming, China]	Taken into account: references have been added
4-640	4	17	24	25	43	1. Again and again we read that “glaciers are the main contributor to sea level rise on a global scale”. It is wrong statement on the at least three main reasons. a) Significant part of continental glaciation located in closed (not drained) river basins, which have no contact to the Ocean. In the Asia region the relative part of such glacier is more than 60%. b) The estimated melt water output from glaciers varies roughly between 0.05-	Rejected: Ths statement is widely reported in the peer-reviewed literature.

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						2.0% from annual runoff in the mouths of continental rivers connected to the Ocean. Prevailing values of coefficient variation for such rivers are in the range 0.1-0.15. No any dependence revealed between the Ocean level change and annual river runoff. Thus, glaciers runoff cannot be considered as main contributor to sea level. c) All calculations of glaciers contribution to the Ocean level does not include coefficient of glacial runoff, which is around of 0.5 and significantly diminish potential role of glaciers as contributor to the water balance of Ocean. [Vladimir Konovalov, Russian Federation]	
4-641	4	17	24	25	43	2. I closely familiar with Randolph Glacier Inventory v.2, 2012 (Arendt et al., 2012) and may say about it the followings. RGI v2 directory contains a number of serious shortcomings and obstacles to use in applications. a). Unsuccessful identification system of glaciers, which is incompatible with the same one accepted in WGI. b). Compared with WGI, in RGI v2 given only the total area of glaciers, which also includes the nunataks, that leads to an overstatement of net area of glaciation. c). The use of satellites with different image resolution and small-scale maps (1:1 000 000) has led to incompatible quality of boundaries and extent of glaciers in RGI v2. d). In cases where multi-images of glaciers are available it is not specified, to which time belong values of the area given in the RGI v2. e). Clear and detailed description is absent for techniques of automatic determination the boundary of glaciers and moraine cover on them. f). The influence of seasonal snow cover on the quality of glaciers interpretation is not studied in different years. Thus, calculations of glacier volume and other parameters based on area values from RGI v2 are rather doubtful. [Vladimir Konovalov, Russian Federation]	Noted: we are fully aware of all mentioned technical shortcomings of the RGI v2.0. Nevertheless the RGI is a substantial improvement over previously available datasets for global scale calculations (see 4-690).
4-642	4	17	24	25	43	3. Quality control and independent confirmation are absent as a rule to the regional and especially global results, obtained by different methods and models. [Vladimir Konovalov, Russian Federation]	Accepted: Uncertainty language has been added in order to reflect this issue
4-643	4	17	24	25	43	4. It is well known and true that direct i.e. field measurements on glaciers are very sparse and distributed spatially uneven (see Table 1). These data are suitable for local glaciological analysis, but they cannot provide statistically substantiated extrapolations to the global scale. Table 1. Spatial variability of glaciers area Fgl (excluding Greenland and Antarctic), and measurements of Bn, AAR (accumulation area ration), ΔL (length change). See SUPPLEMENTS [Vladimir Konovalov, Russian Federation]	Rejected: Such extrapolations are performed in the peer-reviewed literature e.g. Marzeion et al. 2012.
4-644	4	17	24	25	43	In the section 4.3 Glaciers are mentioned attempts of some authors (e.g. Radic and Hock, 2010) to come to the results in Global Scale by means of different manipulations (statistical upscaling) with the sources not full and reliable enough. It is evident, if basic information includes errors and uncertainties, all these features will be transferred into the calculated global volume of glaciers. The other example of ignoring the quality of initial data is modeling of the glacier contribution to sea level rise in 1800-2005 by using measurement of glaciers length (P. W. Leclercq, J. Oerlemans, J. G. Cogley, 2011). In the Guide to prepare USSR Glacier Inventory we see that the length have to be measured on large scale topographical maps along flowline located in the mid of glacier between its highest and lowest points. Estimated accuracy of the length values is 0.1 km. So, the determination of length requires qualified specialist, proper scale of map and unified method of identification the flowline. But in the considered case we have the followings.Fig 1. Temporal distribution of topo year in WGI, 2012. No data=49 193 or 37% of total number 132 890. Topo year - Year of the topographic maps used for measurements of glacier parameters. WGI – is World Glacier Inventory, prepared in World Glacier Monitoring Service. http://nsidc.org/data/docs/noaa/g01130_glacier_inventory/ see SUPPLEMENTS Figs 1-2 [Vladimir Konovalov, Russian Federation]	Part 1: Noted: This is the reason why we now use the RGI instead of statistical upscaling. Part 2: Rejected: We are well aware of the problems and uncertainties when measuring length changes.
4-645	4	17	24	25	43	As for unified method of identification the flowline, I quote below the Glossary of Mass Balance and Related Terms (Cogley, J.G., R. Hock, L.A. Rasmussen, A.A. Arendt, A. Bauder, R.J. Braithwaite, P. Jansson, G. Kaser, M. Muller, L. Nicholson and M. Zemp, 2011, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, UNESCO-IHP, Paris. Retreat Decrease of the length of a flowline, measured from a fixed point. In practice, when the retreat is of a land-terminating glacier terminus, the fixed point is usually downglacier from the terminus, that is, on the glacier forefield. The quantity reported is most often the amount	Noted: The mentioned study also refers to front variations, which are indeed different from length changes.

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						of retreat rather than the length itself. Advance is the opposite of retreat, that is, advance of the terminus Thus, we see that the quality and homogeneity of data on glacier length, which were used for modeling of the glacier contribution to sea level rise in 1800-2005, are not of high standard. However, this important component of modeling was not considered in paper P. W. Leclercq, J. Oerlemans, J. G. Cogley, 2011. [Vladimir Konovalov, Russian Federation]	
4-646	4	17	24	25	43	5. The content and style of description in section 4.3 Glaciers designed mostly for glaciologists and didn't took into account the needs and interests of hydrologists who calculate, model, and forecasts the long term river runoff, water resources and their management in boundaries of watersheds but not mountain regions or other large territories, delineated in Fig.4.8 Thus, socio-economic efficiency of section 4.3 Glaciers is nearly null. [Vladimir Konovalov, Russian Federation]	Rejected: This section is on cryospheric observations as reported in the peer-reviewed literature. Regional distinctions follow topo-climatological boundaries as reported in the literature. Socio-economic impacts are subject of respective Chapters and sections in the WGII assessment.
4-647	4	17	26	17	26	Disconnected' is not correct. The choice was made to include the glaciers 'weakly' connected to the ice sheet according to the classification by Rastner et al., 2012, to include in the domain covered in this chapter, i.e. glaciers. Just delete 'and disconnected from'. 'Outside' should suffice and is correct, as the definition what is 'outside' is left open. [Regine Hock, United States of America]	Taken into account: text changed to 'topographically disconnected from'
4-648	4	17	26	17	35	This is not completely consistent with the further part of the paragraph, and could benefit from some rephrasing. E.g., 'under the force of gravity … to regions with higher temperatures …'. This is not/only partially true for calving glaciers. Dry calving is not mentioned at all. Even if not important on a global scale, I think the first paragraph of section 4.3 should be precise in describing the range and complexity of processes. If some concrete suggestions are needed for this paragraph, contact me (Andi Kääb). [Andreas Kääb, Norway]	Taken into account: Due to given page lengths, we could not include all cases possible in this short overview. However, we have rephrased the text to consider dry calving.
4-649	4	17	26	17	41	This introductory paragraph should also contain a sentence or two about thermal regimes of glaciers (cold, polythermal, temperate); this is especially important in relation with contributions to sea level - atmospheric warming only causes meltwater to run off to the ocean where firn areas are temperate or where cold firn becomes temperate. The overly detailed explanation of ablation and accumulation processes, on the other hand, could be shortened here. [Wilfried Haeberli, Switzerland]	Taken into account: The text has been revised. The issues related to the conversion of SLE to SLR are described in Ch. 13.
4-650	4	17	26			In the first sentence you talk about perennial surface land ice mass, but later about glaciers. This is not the same. [Andreas Kääb, Norway]	Taken into account: text has been revised
4-651	4	17	26			need a better means of explaining that you are including the periperal ice masses of greenland and antarctica [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been revised
4-652	4	17	28	17	29	The description of firn as old snow lasting more than one year is inadequate, particularly in the context of this sentence. Please provide a more detailed explanation [Government of Australia]	Taken into account: The explanation has been revised.
4-653	4	17	28			definition of firn in () not very useful as repeats 'cumulate over several years'. Would be better to describe firn as intermediate state (density) between snow and ice [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been revised
4-654	4	17	29	17	29	For greater clarity suggest that "ice flows downslope" should be expanded to make it clear that the slope in question is the large scale (ice) surface slope, not that of the underlying bedrock. [Government of Australia]	Taken into account: text has been revised
4-655	4	17	29	17	30	The formulation can be misinterpreted that 'various processes of ablation' ONLY occur in the downslope regions where there are higher temperatures. This is a widespread misconception that should be avoided here. Ablation (and accumulation) occur across the entire glacier. Better perhaps: 'where ablation (loss ...) exceeds accumulation' or 'where net mass loss occurs'. [Regine Hock, United States of America]	Taken into account: text has been revised
4-656	4	17	30	17	30	Suggest the addition of "surface" to qualify "relief". [Government of Australia]	Taken into account: text has been revised
4-657	4	17	30	17	30	Maybe consider the word 'influence' instead of 'modify' [Nadine Salzmann, Switzerland]	Noted: see response to comment 4-658
4-658	4	17	30			The relief modifies atmospheric conditions ...: modify is a strong expression for this fact - too strong! [Luzi Bernhard, Switzerland]	Rejected: in some regions glaciers only exist due to topography (shading, increased precip., snow

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							accumulation, etc.). The influence of the relief can thus be even more than strong (i.e. modifies is rather weak).
4-659	4	17	30			the sentence starting 'The relief ...' adds little and is repeated later in paragraphy - drop. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text was rewritten
4-660	4	17	31	17	33	Accumulation/Ablation is in most regions mainly due to...: to times the exactly same diction:([Luzi Bernhard, Switzerland]	Noted: This is on purpose.
4-661	4	17	32	17	33	refreezing of liquid water also important in very high altitudes, e.g. high elevations in Himalayas where cold glaciers [Dorothea Stumm, Nepal]	Taken into account: text has been revised
4-662	4	17	32	17	33	are found (wording could possibly also be 'extreme elevations' to indicate e.g. elevations above 5500 m a.s.l. in [Dorothea Stumm, Nepal]	see 4-661
4-663	4	17	32	17	33	contrast to high elevations in North America or Europe.) [Dorothea Stumm, Nepal]	see 4-661
4-664	4	17	33	17	34	This sentence neglects the fact that frontal ablation does not only occur by calving. In fact there is increasing evidence starting with Motyka 2003, that much or the mass loss of marine terminating glaciers comes from subglacial melting and not calving. Change to: 'loss of ice by calving and subglacial melt of marine or lake terminating glaciers ... [Regine Hock, United States of America]	Noted: We are aware of this but cannot include all processes in this short general overview.
4-665	4	17	34	17	35	Comment text: Check these lines. Is sublimation really important in low latitude regions? I know it's important around the edge of the Antarctic ice sheet where it's too cold for melting but blue ice areas lose 30 or so cm a year. [Peter Barrett, New Zealand]	Rejected: sublimatioin is repeatedly reported to be a key process in mass balance of low latitude glaciers
4-666	4	17	35	17	36	Did you mean the difference and not the sum? Or do you consider “ablation” has being negative which would be a bit strange. [Etienne BERTHIER, France]	Taken into account: Text has been revised to clarify that ablation is counted negative
4-667	4	17	35			missing melt into the oceans directly [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Noted: Several details are not considered in this short introduction text due to space limitations.
4-668	4	17	36			dito comment No 5 [Luzi Bernhard, Switzerland]	Rejected: The modification of the energy and mass fluxes over glaciers by the relief has been described extensively in the peer-reviewed literature.
4-669	4	17	36			What about 'The related energy and mass fluxes are directly linked … but modified …' [Andreas Kääb, Norway]	Taken into account: text has been revised acordingly
4-670	4	17	36			here is a better place to say that the presence of a glacier influences local climate and may therefore trigger feedbacks [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Rejected: Glaciers influence climate only at a very local scale, beyond what is discussed here.
4-671	4	17	37			'indicators as' - 'indicators because' [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-672	4	17	38			Wording: for a non-native speaker it sounds as the glacier actively adjusts something, while it rather responds by loosing or gaining mass. [European Union]	Taken into account: Text has been revised.
4-673	4	17	38			adjusting the size'. Size is a bit unspecific. Volume, mass, area, length? [Andreas Kääb, Norway]	Noted: It is indeed not only the size that is adjusting so the text has been revised.
4-674	4	17	39	17	40	The reference to "[WGII]" and "Section 4.10" are confusing. To what are the authors refering? [Government of United States of America]	Accepted: text has been changed for clarification.
4-675	4	17	39			In my opinion here should be better clarify that the response time of a glacier is strongly dependent from its size with at least a couple of example. In the following text in fact sometimes the response of large glaciers is mixed with smaller ones and this could create confusion to the reader. [Mauro Guglielmin, Italy]	Rejected: Response times are indeed an important glaciological concept, but a description here is beyod the scope of this assessment
4-676	4	17	40	17	40	Does 'section 4.10' refer to WGII? I believe it is not correct to state that glaciers (as defined for the current section '4.3 Glacier' here ice sheets are not included) are a main contributor to global sea level rise. [Nadine	Taken into account: This has been clarified i.e. section 10 of the WGI report. Rejected: Currently, glaciers still

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						Salzmann, Switzerland]	contribute significantly to sea-level rise.
4-677	4	17	40	17	41	Comment text: replace "main" with "major" and add "section." to end of sentence. [Peter Barrett, New Zealand]	Ediorial
4-678	4	17	41	17	43	We wonder if the word 'extent' can be avoided in Section 4.3, given that it had a very well defined usage in relation to sea ice, but in the context of glaciers seems to be used interchangeably with 'area'. The section title uses '4.3.1. Current Glacier Extent...' but then the very next line begins with "The total area covered'. On line 41, it is suggested that "extent" is an indication of overall glacier state, but this seems a bit vague, and this sentence could perhaps be made more specific: "The determination of their overall state (area, volume and length) and their changes in time..... [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: Text has been revised saying 'Current area and volume of glaciers' now. Line 41 has also been revised accordingly
4-679	4	17	43	18	30	The Randolph Glacier Inventory for the Southern Andean region (Southern Andes) lacks of the minimum accuracy needed to account for the total glacier area and total number of glaciers, therefore it is difficult to estimate the glacier changes since AR4. This opinion is based on the discrepancy between the Randolph dataset for the Southern Andes (17) and the most updated glacier inventory published in Chile by the main governmental department in charge of this topic (DGA, 2009). Further comments are given below. [Andrés Rivera, Chile]	Noted: We agree that regionally quality issues existed in the RGI, but this does not change the substantial improvement over the dataset that was available for AR4. The new glacier inventory data for Chile were not made available in time for consideration in AR5.
4-680	4	17	45	17	45	not precisely known in AR4' would indicate that we know it now precisely. We have an imporved picture on area cover, but this is certainly still far from 'precise' -> suggest to replace the word [Nadine Salzmann, Switzerland]	Taken into account: Text has been revised
4-681	4	17	45			not certain that precisely is the right word here - implies that we do know total area precisely now which is not possible [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Text has been revised
4-682	4	17	46			related' calculations. More specific? [Andreas Kääb, Norway]	Noted: examples will be added
4-683	4	17	46			can omit spatially [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been revised for clarification
4-684	4	17	47	17	47	(Cogley,2009a) and (Radic and Hock,2010) replace as "Cogely (2009a) and Radic and Hock (2010)" [Yongjian Ding, China]	Editorial
4-685	4	17	47	17	47	delet 'vector': this is a detail, which is not important here [Nadine Salzmann, Switzerland]	Accepted: Another phrasing for has been used.
4-686	4	17	47	17	47	Here and elsewhere in the text the name "Radic" should be "Radić". [Jacob Clement Yde, Norway]	Editorial
4-687	4	17	47	17	51	Write "For AR5, a new globally complete vector dataset of glacier outlines was compiled from a wide range of data sources of variable levels of detail and quality (Arendt et al. 2012; version 2.0) (Figure 4.8, Table 4.2). The digital glacier outlines in this dataset refer to the past 50 years and have been widely used as a base in several studies assessed here." [Michael Zemp, Switzerland]	Accepted -text rewritten
4-688	4	17	47			Parentheses at references wrong [Andreas Kääb, Norway]	Editorial
4-689	4	17	47			Write "..was spatially extended by Cogley (2009a), Radic and Hock (2010), as well as by WGMS and NSIDC (2012)." Reference: WGMS and NSIDC (2012): World Glacier Inventory. Compiled and made available by the World Glacier Monitoring Service, Zurich, Switzerland, and the National Snow and Ice Data Center, Boulder CO, USA. Digital Media. Online available from: http://nsidc.org/data/glacier_inventory/index.html [Michael Zemp, Switzerland]	Rejected: We do not cite data bases if they are cited in the literature we use.
4-690	4	17	49	17	49	delete version 2.0. All other versions are also a significant improvement. The studies cited later in the text that use the RGI dataset are based on a variety of versions. Pointing out one of the versions here is misleading. [Regine Hock, United States of America]	Accepted
4-691	4	17	51			too much detail here can omit line start "glcier-covered areas ..." [Antony Payne, United Kingdom of Great	Accepted: Sentence has been rewritten (see 687)

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						Britain & Northern Ireland]	
4-692	4	17	52	17	52	Remove space before the coma in the parenthesis [Antoine RABATEL, France]	Editorial
4-693	4	17	52	17	52	remove first comma from: (modified from , Radic and Hock, 2010) [Dorothea Stumm, Nepal]	Editorial
4-694	4	17	52			Modified from Radic and Hock [European Union]	Editorial
4-695	4	17	52			wrong comma [Andreas Kääb, Norway]	Editorial
4-696	4	17	56	17	58	For assessing the overall mass depletion (sea level contribution) from glaciers and ice sheets it is essential to avoid double accounting. This requires to specify clearly which glaciers in the periphery of ice sheets are taken into account for ice sheet mass balance, and which are considered in the numbers for glacier mass change. Figure 4.1 does not allow a reasonable check on this (very coarse resolution). [Helmut Rott, Austria]	Accepted: The peripheral glacier issue has been revised and homonegized with the ice sheet section.
4-697	4	17	56			this repeats text from line 26 - a better definition on line 26 and this line ("calculations ...") can be omitted. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted
4-698	4	17	57	17	57	This sentence is not quite correct. The Bliss dataset does not include glaciers that are disconnected from the ice sheets if they are located on the mainland. It only includes the glaciers on the islands around Antarctica. Change to: 'refer to the glaciers on the islands in the Antarctic and subantarctic periphery. [Regine Hock, United States of America]	Accepted
4-699	4	17	57	17	57	Change "ice sheet" to "Antarctic ice sheets". [Jacob Clement Yde, Norway]	Accepted
4-700	4	17				Section 4.3 omits observations of precipitations, which are important to assess whether glacier decrease is simply due to more drought. [Government of United States of America]	Rejected: The importance of precipitation for glacier nourishement is mentioned at several places (L27, 30, 31, 32 etc.). The assessment of the reasons for glacier change are discussed in Ch 10 Detection and Attribution.
4-701	4	17				Section 4.3: There is a lack of consistent terminology in this section that is more than editorial, it obscures the meaningfulness of the text. This is especially important for ablation. Ablation includes all processes of mass loss, but is used in the chapter when a subprocess is intended (calving). When a specific subprocess is discussed, the specific vocabulary needs to be used. Otherwise the intent is lost. Other cases include reference to the glacier terminus as the front (iceshelf term) and the tongue. Neither of these words should be used for terrestrial glaciers. The use of glaciated and glacierized also needs attention. Consistency is imperative. [Government of United States of America]	Taken into account: The consistency of the terminology has been checked and reference to the Glossary was introduced.
4-702	4	17				Section 4.3: There is no description of the importance of various competing processes, nor the recent development of understanding of said processes. For example, this section should consider a statement about the importance of iceberg calving (not frontal ablation in general), progress made on this problem since AR4 and the remaining knowledge gap around the process. The same could be said about submarine melt. This is especially important since the next section acknowledges the efforts to date to inventory calving/tidewater glaciers but there is no motivation for the non-specialist to understand why this is being done. Additionally, the authors have partitioned measurement methods by dimension (1,2,3: length, area, volume) but give no motivation for doing this, nor is there a discussion of the reliability of these methods. All that is given is Table 4.3, where advantages/disadvantages are listed in a rather ad-hoc and vague way. This treatment may leave a reader without any tools to evaluate and interpret the results presented here. [Government of United States of America]	Calving glaciers and submarine melt: Taken into account: A statement on the importance of these processes has been added in ice sheets section. - Methods by dimension: Taken into account: We have now better introduced the motivation behind this separation (it is largely based on the strong differences in measurement techniques as shown in Table 4.3). - Table 4.3: Taken into account: The vague columns were replaced with quantitative accuracy information.
4-703	4	17				Section 4.3: The use of the word peculiarities to describe glaciers where dynamics are important seems unwarranted. A substantial component of global glacier mass balance is related to these so-called "peculiarities" and they should be discussed in as much detail as possible, not brushed off. The issue of resolving total mass loss versus mass loss as a result of direct climate forcing is one way this could be addressed and discussed. [Government of United States of America]	Rejected: We do not yet have sufficient process understanding to separate mass changes resuting from climatic vs non-climatic control. We have, however, for the first time tried to consider glacier ice below sea level as not contributing to sea-level rise after melt (see section 13).
4-704	4	17				Section 4.3: The layout and character of the ice sheets section (4.4) does a better job at conveying the state of	Taken into account: We have added more regional

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						the science and giving the reader the necessary insight to evaluate the assessment observations. The relative strengths and weakenesses of several central methods are described and then discussed over the various regions. Although this method would not directly transfer to the 18 glacier regions, while maintaining a reasonable length, some adoption of this structure would greatly enhance the readability and relevance of the glacier section of this document. A summary of how the measurements from each dimension (L, A, V) assess the overall change is suggested. For example, AK is experiencing lower than average area change (based on Fig. 4.10), but is among the largest mass loss regions (not evident in Fig 4.11). Summaries such as this would give insight into the available data and results. [Government of United States of America]	observations but have to focus on a few (maybe boased) examples as page limitations do not allow for a more in-depth discussion.
4-705	4	17				Section 4.3: The uncertainties provided in tables and figures need to be fully and consistently defined. The authors provide many quantitative estimates of uncertainty in the tables and figures of chapter 4. For example, table 4.4 lists specific mass change rates with ± uncertainty values and Figure 4.12 shows shaded uncertainty regions). Are these strictly estimates of precision (and if so, at what confidence level(s)), or do the uncertainty estimates include considerations of structural errors and unknowns (potential biases, such as might stem from attempts to "up-scale" results from restricted sampling)? Structural uncertainties are alluded to in the executive summary (p.3 line 55), which is all the more reason to explicitly define uncertainty as it is used in section 4.3. [Government of United States of America]	Taken into account: uncertainty language has been revised
4-706	4	18	1	18	23	Bahr and Radic (TCD 6, 763-770, 2012) show that global glacier volume may be underestimated by as much as 10% by assigning a lower size cutoff of 1 km^2. It would be appropriate to mention this here. [W. Tad Pfeffer, United States of America]	Rejected: the RGI widely contains these small glaciers
4-707	4	18	1	18	30	Comment re' Table 4.1: The volume estimates (km^3) should be specified as ice equivalent (i.e.) units. It is logical that these would, since this is a discussion of glacier volume and not sea level, but the sea level equivalent column (mm) may lead to some confusion since the conversion is 0.9*km^3/360 to get mm SLE. On a related note, the conversion is given in Chap 4 as 362 GT/mm (on page 7/line 13) while its given as 360 GT/mm in Chap 13. Not a big difference, but we should have our ducks coaxial. [W. Tad Pfeffer, United States of America]	Taken into account: A recently published paper by G. Cogley (2012) allows for a consequent use of 362.5. This has been adopted throughout Ch4 and also in Ch13.
4-708	4	18	1	18	30	Table 4.1 lists only the Huss & Farinotti volume estimate in terms of its sea level equivalent. The total volume of each of the three listed estimates is given in km^3 units the table, but by listing only the H&F SLE, the reader who simply wants to grab the number for total glacier sea level equivalent is likely to see the single value (427.7±57.8 mm) and think it's the agreed-upon value. While the H&F method is a good one, it has by no means displaced the other recent estimates. The divergence between these volume estimates is unresolved at this point. [W. Tad Pfeffer, United States of America]	Accepted: The table was revised considering all published values.
4-709	4	18	1	18	30	The reasons for the difference between the S2a and S2b volume estimates could be made clearer, either in Table 4.1 or in the text on line ca. 10-18. The difference between S2a&b is noted but not really explained; the main difference is that one used the scaling exponents of glaciers while the other used the exponent for ice caps. [W. Tad Pfeffer, United States of America]	Taken into account: text has been revised accordingly
4-710	4	18	2	18	2	comment text: delete "s" form "volumes" and replace "only available" with "available only" [Peter Barrett, New Zealand]	Editorial
4-711	4	18	2	18	23	It is clear that not all European glaciers can be covered in the analysis here. Can you give some justification for the selection criteria? [European Union]	Rejected: we only report about the 19 regions as delineated in the RGI (with slight changes to regions suggested by Radic and Hock (2010) but not about sub-regions or even individual glaciers.
4-712	4	18	2	18	23	The entire paragraph is confusing. Should be partially re-written. It is not clear why the text details the study by Huss and Farinotti and Radic et al, while others are only mentioned as reference (line 12). [Regine Hock, United States of America]	Accepted: section has been rewritten after final acceptance of cited papers
4-713	4	18	4	18	5	The IPCC report should be extremely careful with the use of direct volume-area correlations. This technique is	Noted: We have to consider for this report the results

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						popular but highly problematic and can even be seen as a misleading statistical data manipulation: glacier volumes are never measured but calculated from measured areas and thicknesses. Correlating volume with area indeed means to correlate a mathematical product (area times thickness) with one of the factors (area) from which it had been calculated - a fundamental thinking mistake. The corresponding autocorrelation of area (as contained in volume) with itself seemingly suppresses the very large scatter in the relation between the originally measured areas and thicknesses and thus provides a completely wrong impression concerning the quality of the available data and their interrelations. Statistically correct relations between the measured thicknesses and areas immediately show the very large scatter (roughly an order of magnitude around the mean: cf. Figure 8.5 in Cogley, G. (2012): The Future of the World's Glaciers, in: A. Henderson-Sellers & K. McGuffie (Eds.), The future of the worlds climate, pp. 197–222. Elsevier. As a consequence, area-related thickness/volume estimates can hardly define total volumes and sea-level equivalents within less than about plus/minus 25% lor even more (cf. Meier, M.F., Dyurgerov, M.B., Rick, U.K., O'Neel, S., Pfeffer, W.T., Anderson, R.S., Anderson, S.P. and Glazovsky, A.F. (2007): Glaciers dominate eustatic sea-level rise in the 21st century. Science, 317(5841): 1064–1067). Huss and Farinotti apply a far better (flux- and slope-dependent) technique. Their estimate is therefore more reliable but should still be corrected for the considerable amounts of ice below sea level (cf. comment about page 3, line 49 and Haeberli, W. and Linsbauer, A. (2012): Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion); the uncertainties calculated in their paper are probably still over-optimistic. [Wilfried Haeberli, Switzerland]	from the peer-reviewed literature. By explicitly showing the great range of values obtained from the methods applied, we demonstrate the large uncertainty that is inherent in these methods.
4-714	4	18	8	18	8	Another study using a new method is McNabb, B., R. Hock, S. O'Neel, L.A. Rasmussen, Y. Ahn, H. Conway, S. Herreid, I. Joughin, T. Pfeffer, B. Smith and M. Truffer, 2012. Using surface velocities to infer ice thickness and bed topography: A case study at Columbia Glacier, Alaska. J. Glaciol. 58(212), doi: 10.3189/2012JoG11J249. [Regine Hock, United States of America]	Noted: We have restricted our assessment to publications reporting methods that can be applied on a global scale.
4-715	4	18	9	18	10	better write: ... glaciers from the compilation by Arendt et al. (2012). [Dorothea Stumm, Nepal]	Editorial
4-716	4	18	9			of all 181500 glaciers' - the number of glaciers depend on the definition of minimum size of the glacier and the distance between two neighbouring glaciers, as well as the algorithm in the different software. Before a number is given, these criteria for the former two should be given. The number of glaciers also appears in Chapter 13 and the numbers should be cross-checked for consistency. [European Union]	Taken into account: The number of glaciers is indeed depending on the definition and thus highly variable. We have rounded the numbers to better reflect this uncertainty.
4-717	4	18	10	18	14	The following statement is made: "For a total glaciated area of about 740,000 km2, a volume of 171,600 km3 (0.43 m sea level equivalent, SLE) was calculated (Table 4.2). Other studies (e.g., Giesen and Oerlemans, 2012; Grinsted, submitted; Radic et al., submitted) used different ways of calculating the volume-area scaling, as well as different glacier areas and entity allocations, and obtained slightly different volumes." The sea-level equivalent of glaciers and ice caps of 0.43 m that is mentioned in the text is perhaps not “slightly different” from the 0.35±0.07m that is obtained by Grinsted (submitted) although it is only just outside of Grinsted's uncertainty range. [Government of Iceland]	Taken into account: The text has been revised.
4-718	4	18	10	18	23	Table 4.2: I was unable to reproduce SLE 427.7± 57.8 mm. The values I got for S1, S2a and S2b are, respectively, 431.4, 535.0 and 415.4. If one takes the mean ice volume of all three methods, one obtains 460.6. Of course, for this kind of uncertain estimation, one could just say 43 cm, but when numbers are given to the first decimal, one wonders how they were obtained. I personally think S1 is the most realistic evaluation. [Atsumu Ohmura, Switzerland]	Taken into account: The table was revised considering the range of values reported in the literature.
4-719	4	18	10			double reference [Andreas Kääb, Norway]	Editorial
4-720	4	18	14	18	14	Volumes are different than in table, and these numbers (in both text and table) have a high degree of precision -- are they that precise? [Christopher Little, United States of America]	Taken into account: The table was revised considering the range of values reported in the literature.
4-721	4	18	16	18	17	If Dyurgerov and Meier (2005) is taken into account, 785,000 km2 is the correct area, not 795,000 km2. The area difference between AR4 and AR5 (785,000 – 739,820 km2, 5%) may be a consequence of misleading inventories in some regions and therefore, not necessarily indicative of a lowering in global glacier extent as	Noted: The differences are likely due to larger regions not counted in the RGI (e.g. the Antarctic Peninsula). Many wrong RGI regions have been corrected.

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						was suggested in line 17. [Andrés Rivera, Chile]	
4-722	4	18	16			Maybe remind the reader how the volume were calculated in AR4. Area/Volume scaling also? [Etienne BERTHIER, France]	Taken into account: Text has been revised
4-723	4	18	17			"RH are in good agreement" with what? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been revised.
4-724	4	18	18	18	18	Since the sum of 12.2(Alaska)+11.8(Greenland)+18.6(Antarctic and Subarctic) is 42.6, "about 42%" might be "about 43%". In case of the effect of rounding errors, "about 42%" is okay. [Government of Japan]	Taken into account: text has been corrected
4-725	4	18	18	18	21	The rational for the category of 42 % of 'glacier covered area' does not make sense to me. Why are the 3 [Dorothea Stumm, Nepal]	Taken into account: The sentence was revised to be more clear.
4-726	4	18	18	18	21	areas [1], [5] [19] mentioned? They are not the biggest areas, they are not all polar, what is the [Dorothea Stumm, Nepal]	see 4-725
4-727	4	18	18	18	21	argument to choose these three areas? Even all mentioned regions don't make up the polar regions. [Dorothea Stumm, Nepal]	see 4-725
4-728	4	18	18	18	21	What about Iceland and North Asia? [Dorothea Stumm, Nepal]	see 4-725
4-729	4	18	18	18	21	The ice covered areas in Asian regions [13], [14], [15] are also significant, making up over 16 %. This area is often [Dorothea Stumm, Nepal]	see 4-725
4-730	4	18	18	18	21	referred to as Third Pole, also because of other 'ice reservoirs' like permafrost. [Dorothea Stumm, Nepal]	see 4-725
4-731	4	18	18			42% glacier cover: which reference area? [Andreas Kääb, Norway]	Taken into account: Text has been revised
4-732	4	18	19			I do not understand the rational in grouping Alaska with Greenland and Antarctica here and putting Arctic Canada in a second group. Why not grouping Arctic Canada with Greenland and Antarctica to isolate the three largest regions? [Etienne BERTHIER, France]	Taken into account: text has been revised
4-733	4	18	23	18	23	What other factors? [Regine Hock, United States of America]	Taken into account: minimum size
4-734	4	18	23			other factors: mainly disintegartion, right? Mention that, and perhaps also that disintegration is a function of time. [Andreas Kääb, Norway]	Taken into account: The other important one is minimum size considered (corrected)
4-735	4	18	26	18	26	replace 'for the 19 RGI regions' by 'for 19 regions'. This to reduce the number of unnessary acronyms and make the text easier to read. RGI does not add any essential information here. [Regine Hock, United States of America]	Editorial
4-736	4	18	26	18	30	Table 4.2 is incorrect on Icelandic glaciers. Correct values are Area= 11.100 km2, Volume = 3.600 km3; measured and SLE = 10 mm. Please note that these values come from extensive measurements and are far more reliable than the scaling methods used in the table. The appropriate values are described in Björnsson, H., and F. Pálsson. 2008. Icelandic glaciers. Jökull, 58, 365-386. [Government of Iceland]	Taken into account: The table has been revised. For consistency, only volumes obtained with a globally consistent method are reported.
4-737	4	18	26	18	30	The tidewater fraction in Iceland is not zero, since Breidarmerkurjokull calves into a lagoon affected by tides. Its area is 760 km2 which is 7% of the total area of glaciers in Iceland. Thus the correct fraction of tide water glaciers is 7% [Government of Iceland]	Noted: These fractions have been estimated with a rather rough model that does not recognize the channel of the lagoon to the ocean.
4-738	4	18	26	18	30	Table 4.2: I am not certain if the numericals in the table are good. It seems that too much was relied on satellite remote sensing in sacrificing more accurate terrestrial data. For example the number of glaciers and the surface area in Island are rather on a small side. I asked the person who is responsible for the Islandic Glacier Inventory, Dr. Oddur Sigurðsson at the Islandic Meteorological Office, and his answer is number of glaciers 300, total surface area is 11,079 km2. The difference might not appear large, but we want to use the observations inventory to compare in the future. We must have a very accurate listing. [Atsumu Ohmura, Switzerland]	Taken into account: The numbers in the table were revised using more accurate datasets.
4-739	4	18	26	18	30	In Table 4.2 error bars are largely missing. The two error bars specified (SLE, 13.5% and Volume, 12%) seem	Taken into account: The numbers in the table were

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						to be over optimistic, considering that the estimated Volume for S2a is 24 % higher, and the volume by Radic and Hock (2010, cited on line 18 of this page) is 40% higher. [Helmut Rott, Austria]	revised showing the range of values rather than statistical errors.
4-740	4	18	26			Why call the Alps as Central Europe, there is no reason both climatically and geographically. I proposed to call the area as “The Alps”. Pireneys will be not included but their contribution is negligible in terms of area and volume. [Mauro Guglielmin, Italy]	Rejected: The regions and their names are the product of intensive discussions in connection with the RGI and the published literature and should thus not be changed.
4-741	4	18	28			Unless we are mistaken, there is no mention in the text as to what the difference in methods are between S2a and S2b. Please add a line or two either in the text, or in the caption which describes clearly the difference in these methods. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The numbers in the table were revised showing the range of values rather than statistical errors.
4-742	4	18	30	18	30	In Table 2, for the region 16 (Low Latitudes) the glacier surface area is largely overestimated in the RGI. Rabatel et al., 2012 mention about 1900 km² for the glaciers of the tropical Andes which gather more than 99% of the tropical glaciers. This estimation has to be corrected. [Antoine RABATEL, France]	Taken into account: The RGI dataset was revised in this region.
4-743	4	18				Table 4.2: Pls specify in the head-row of Table 4.2: 'Percent of total global glaciarated area' [Government of Germany]	Editorial
4-744	4	18				The leftmost column "Nr." in Table 4.2 should be "No." since this column means the number. [Government of Japan]	Editorial
4-745	4	18				Caption, Table 4.2. Sugges that the caption indicate the study time spans so that this table can stand alone. [Government of United States of America]	Taken into account: The time span of the RGI input data is now given in the main text.
4-746	4	18				Table 4.2: a very important, and highly informative table, no doubt. However, there are problems with some numbers given for glacier areas for some regions. The glacier area numbers are taken from the Randolph Glacier Inventory (RGI) which is incomplete and incorrect in some regions, and it may not be possible to correct these errors until the final version of the report, even though a newer version of the RGI may be considerd. This problem has to be addressed in some adequate way. For region 16, Low Latitudes, the number given is approximately twice as big as it is in reality (ca 4000 versus 2000 km2, see also Rabatel et al. 2012, revised version in TC). If this report went out with this large error, it could cause major trouble to experts working in this region, institutions and, particularly, could seriously discredit IPCC. I therefore strongly recommend to add a sort of disclaimer, saying that the numbers provided are subject to some uncertainties. Or any other measure that is considered appropriate. The problems with these numbers from RGI should in my view not be an argument not to publish this information but caution should be given to the issue. [Christian Huggel, Switzerland]	Taken into account: We have analysed the error (seasonal snow was mapped) and corrected it.
4-747	4	18				Tab 4.2: Why not uncertainties given also for the regions? Else, explain why only given for the total. [Andreas Kääb, Norway]	Taken into account: The numbers in the table were revised showing the range of values rather than statistical errors.
4-748	4	18				Table 4.2: Should there be a SLE column for S2? [Christopher Little, United States of America]	Taken into account: The numbers in the table were revised showing the range of values rather than statistical errors.
4-749	4	18				table 4.2 define what is ment by tidewater fraction - ie glaciers that are in contact with ocean at some point along their perimeter? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The sentence was revised to be more clear.
4-750	4	18				Table 4.2: The RGI data set sums 32,400 km2 of ice in the Southern Andes. Considering the most recent works published about this topic in the area, there are only near 27,000 km2 of ice between Chile and Argentina (Rivera et al, 2008; DGA 2009; Naruse, 2006; DGA, 2009; UNEP, 2007; Casassa et al in press, Bown et al in press; Falaschi et al in press) therefore, the 20% discrepancy, means that the RGI data set added near 5,000 km2 of ice. We don’t know where this number is coming from. Later on in this review we will address this discrepancy in more detail. [Andrés Rivera, Chile]	Taken into account: Values in the table were revised based on a new assessment. To consider better datasets in the RGI, we need the vector outlines rather than the papers.
4-751	4	18				Table 4.2: The percentage of tidewater glacier was underestimated. All the tidewater glaciers are in the icefields of the region (Northern and Southern Patagonian icefields, Cordillera Darwin and nearby islands).	Underestimation: Noted: Numbers in the table refer to a simple model that is applied in a globally consistent

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						The tidewater represents near 23% of the total area of the Southern Andes (assuming 27,000 km2 as a total). Apart from them, near 38% of the Southern Andes are freshwater glaciers. By the way, why not including in the possibly anomalous behaviour the freshwater calving glaciers? In many cases these glaciers are also responding to deep lake fjords (Skvarca et al, 2002) in a similar way tidewater glaciers are responding to nearby fjords (Rivera et al, 2012). The freshwater glaciers number is larger than previously estimated due to the strong ice retreat into proglacial lagoons both at the western and eastern flank of the Andes (Loriaux and Casassa submitted, 2012). [Andrés Rivera, Chile]	manner to all glacier outlines. - Freshwater lakes: Taken into account: We have now better explained the differnt glacier types. Noted: We can only only consider submitted or in press studies that were made available to us in time.
4-752	4	18				Table 4.2: The RGI inventory in region 17 was based on imagery from the period 2000-2003 (since then, strong retreats have occurred everywhere). On the other hand the quality of the used images were not the best, because they included as glacier areas, too many temporal snow surfaces. This is the main explanation for the huge difference between our glacier area estimation and the one presented by the RGI. [Andrés Rivera, Chile]	Taken into account: Values in the table were revised based on a new assessment. To consider better datasets in the RGI, we need the vector outlines rather than the papers.
4-753	4	18				Table 4.2: In more detail, this RGI bias in Southern Andes appears to be non-systematic, ranging from minor (≤5%) up to very large differences. One example of the latter is a volcano located at 38ºS (Nevados de Sollipulli), where RGI included several isolated snow patches beyond the caldera rim. In a recent satellite image without temporal snow, this area is having near 12 km2 whilst RGI yields near 30 km2 ¡¡¡ [Andrés Rivera, Chile]	see 4-752
4-754	4	18				Table 4.2: Larger overestimation arises in the austral zone (> 45ºS), where several small snow bodies were included, particularly on the surroundings of the SPI. If only the RGI glaciers outlines obtained for the NPI, SPI and surroundings are taken into account, the ice surface raises to ca 22,000 km2, which yields ~30% higher than previous estimations. Also, the overestimation appears more evidently in Cordillera Darwin Icefield (54ºS). See Bown et al in press. [Andrés Rivera, Chile]	see 4-752
4-755	4	18				Table 4.2. The uncertainty of the RGI results could be related to: 1) fractal structure of glacier outlines, revealing an automatic glacier classification procedure without manual edition and supervision. This is a key issue in the case of rock and debris-covered glaciers which are numerous in the northern half of the region; 2) wrongly inclusion of seasonal snow patches, especially in the southern half. Eventually, a better discrimination of debris-covered areas from rock areas and ice divides should be reassessed. 3) a huge number of the glacier number due to ice divide definition based on automatic procedures which not reliable. [Andrés Rivera, Chile]	see 4-753
4-756	4	19	3	19	3	Figure 4.8: color scheme with blue for tidewater glacier and green for land-terminating would be more intuitive [Regine Hock, United States of America]	Accepted: colours have been changed
4-757	4	19	3	19	3	Figure 4.8 caption: delete 'RGI' -->'into 19 regions. [Regine Hock, United States of America]	Editorial
4-758	4	19	7	19	7	The title is unfortunate: It seems that chapter 4.3.2 is about how the measurements of length, area and mass change are obtained, i.e. a focus on methodology, whereas cahpter 4.3.3 then details the actual observations. Hence the title here should analogous to the one in 4.3.3: Measurements of Glacier Length, Area and Mass [Regine Hock, United States of America]	Editorial
4-759	4	19	7	20	27	This lengthy paragraph explains things, which have been - for many years already - the basic strategy of internationally coordinated glacier monitoring (WGMS, GTN-G) such as they are described, for instance, in Haeberli et al. (2007): Haeberli, W., Hoelzle, M., Paul, F. and Zemp, M. (2007): Integrated monitoring of mountain glaciers as key indicators of global climate change: the European Alps. Annals of Glaciology 46, 150-160. It would be fair to correctly mention these imporant UN-/ICSU related programs, which indeed not only provide data but also modern concepts and help organizing funds and governmental support. [Wilfried Haeberli, Switzerland]	Taken into account: The text has been revised.
4-760	4	19	7			Structure: Chapter 4.3.2 seems to focus on measurements, however, the chapter mixes explanations about methodology and results, that better are presented in the next chapter that focuses on results. [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-761	4	19	9	19	11	These two senteces are somewhat unclear. I suggest to delet or to reformulate [Nadine Salzmann, Switzerland]	Taken into account: The text has been revised.

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4-762	4	19	9	19	15	Paragraph badly written and partially repetitive from 4.1. Delete or reformulate the first half. [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-763	4	19	9			highly > well visible? [Andreas Kääb, Norway]	Editorial
4-764	4	19	9			Physically understandable? I don't think it will be clear to most readers what is meant by that. [Andreas Kääb, Norway]	Editorial
4-765	4	19	10			The changes 'overlay' and it is non-trivial to 'separate' them. [Andreas Kääb, Norway]	Editorial
4-766	4	19	11			"them" wht does this refer to? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-767	4	19	14	19	14	The term 'normal' glaciers should be avoided here in particular since the statement is not correct. The sentence is misleading in the sense that it makes a reader believe that surging and calving glaciers are something odd. However, figure 4.8 clearly proves the opposite that marine-terminating glaciers are more 'normal' than land-terminating glaciers. Land-terminating glacier are dominant in certain regions (European Alps, Scandinavia), however, in most regions in the world the dominant mode is marine termining glaciers. In any case the termin 'normal' should be avoided. [Regine Hock, United States of America]	Taken into account: The text has been revised in order to avoid the word 'normal'.
4-768	4	19	14	19	15	In the text calving is described as a "peculiarity".This seems awkward wording to describe such an an important and widely occurring physical process. [Government of Iceland]	Taken into account: The text has been revised.
4-769	4	19	14			What is a 'normal' glacier? This sentence could be formulated more precise. [Andreas Kääb, Norway]	Taken into account: The text has been revised.
4-770	4	19	14			I would skip 'cyclic' and refer to flow instabilities in general, including, but not only, surging. [Andreas Kääb, Norway]	Taken into account: The text has been revised.
4-771	4	19	14			can delete "normal" and 'e.g.," so that "glaciers without ..." [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been revised.
4-772	4	19	15			what is meant by disintegration here? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been revised.
4-773	4	19	15			"with heavy"? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-774	4	19	17	19	35	Section 4.3.2.1 the series of “Fluctuations of Glaciers” or even some of the series should be quoted as an important data source. [Atsumu Ohmura, Switzerland]	Taken into account: We have cited WGMS, 2008, which is listing all datasets.
4-775	4	19	19			entire section: length changes are a transient signal and do not necessary reflect any specific climatic state, as the 'smooth and delayed reaction' suggest in the text. They are the (complex) output of a system to external forcing, not necessary linear. [Andreas Kääb, Norway]	Taken into account: The text has been revised.
4-776	4	19	20			Past > Historic? [Andreas Kääb, Norway]	Editorial
4-777	4	19	22	19	22	Add 'et al." in the reference Rabatel et al., 2012 [Antoine RABATEL, France]	Editorial
4-778	4	19	22	19	22	Rabatel 2012, is in fact Rabatel et al, 2012 [Patrick WAGNON, Nepal]	Editorial
4-779	4	19	22	19	22	Add "Yde and Knudsen, 2007" to the references. [Yde, J.C., and N.T. Knudsen, 2007: 20th-century glacier fluctuations on Disko Island (Qeqertarsuaq), Greenland. Ann. Glaciol., 46, 209-214]. [Jacob Clement Yde, Norway]	Rejected: Papers refering to the respective statement have been removed.
4-780	4	19	22	19	23	Length changes are not identical with front variations. [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-781	4	19	24	19	26	'But length changes ... amplification of a climate forcing... than the related temperature change": this sentence is misleading and doesn't contribute to a better understanding of the impact of climate change on glaciers. I suggest to delete it. [Martin Funk, Switzerland]	Accepted: The sentences has been removed.
4-782	4	19	24	19	26	The statement and example linking length change and temperature increase (seasonal, annual ??) is over simplistic as it ignores other factors determining glacier terminus position and mass balance. [Helmut Rott, Austria]	Accepted: The sentences has been removed.

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4-783	4	19	24		26	This sentence is at least questionable because the length changes is the less accurate indicator of warming forcing because as written elsewhere in the text surging glacier, glacier size and precipitation regime and the amount of debris cover can condition the length changes of a glacier. [Mauro Guglielmin, Italy]	Accepted: The sentences has been removed.
4-784	4	19	24			sentence strting "but ..." needs to be reworded [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted: The sentences has been removed.
4-785	4	19	25	19	26	the example should be deleted. These number are arbitrary. The response will depend on many factors determined by the glacier geometry etc [Regine Hock, United States of America]	Accepted: The sentences has been removed.
4-786	4	19	25	19	32	Discussion here is valid only for Alps; most glaciers do not have records extending back as far as stated in this paragraph. These long records are concentrated in one specific mountain range. The authors should consider a discussion of the representativeness for global change. [Government of United States of America]	Taken into account: The text has been revised.
4-787	4	19	28	19	28	Remove space before the coma after the parenthesis [Antoine RABATEL, France]	Editorial
4-788	4	19	28	19	28	Change ") ," to "),". [Jacob Clement Yde, Norway]	Editorial
4-789	4	19	30			AN independent ... reconstructionS [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-790	4	19	31			“over this period” is related to 16th or 17th Century? It is not clear. [Mauro Guglielmin, Italy]	Editorial
4-791	4	19	32	19	32	The term "independent from climate forcing" together with "calving" implies, that calving is in general unrelated to climate, which is not the case if ocean warming increases calving as later discussed, e.g. p25, l36. Clarify. [Olaf Eisen, Germany]	Taken into accout: The text has been revised
4-792	4	19	33	19	33	Change "Pasche" to Paasche". [Jacob Clement Yde, Norway]	Editorial
4-793	4	19	33	19	34	I think you should not limit yourself to the last 150 years. There exist many reliable records of glacier length that go back further in time (e.g. as used by Leclercq and Oerlemans 2012 cited above). Furthermore, this anounced limitation is not followed (e.g. page 21 line 9) [Paul Leclercq, Netherlands]	Noted: We have used this criterion only for the display in Fig. 4.9, but partly discuss length changes also for a longer time span.
4-794	4	19	33			Can we really state that they occur totally "independently"? Climate may also influence partly those behaviour that are, I agree, mainly driven by ice dynamics. Debris-covered glacier tongue could also added to the list of the glaciers for which glacier length change are not easily related to climate fluctuations. [Etienne BERTHIER, France]	Taken into accout: The text has been revised
4-795	4	19	33			Suggest the authors add Post et al., 2011 to the Yde and Pasche, 2010 reference. The authors describe the mandatory exclusion of calving glaciers for climate. Neglecting to exclude these glaciers will bias results in ways potentially stronger than neglecting debris cover. [Government of United States of America]	Noted
4-796	4	19	33			This statement illustrates the need for better background in the glacier section. This goal of this section should be stated at the onset of discussion - i.e., is the goal to describe and discuss glacier mass loss as a function of climate (remove non - climate mass loss) or to better understand the nature of glacier mass changes irrespective of forcing? Both scenarios are important, and the ratio between the scenarios is also very important. [Government of United States of America]	Taken into account: The background section has been revised
4-797	4	19	33			“calving” can be deleted, as many calvings are the result of the climate, whether from the sea water temperature or by the emergence of pro-glacial lakes (as a result of the retreat), which can accelerate retreat. [Atsumu Ohmura, Switzerland]	Taken into account: The background section has been revised
4-798	4	19	34	19	35	I am critical to the presentation of glacier length changes in this report! Why are regional studies of length changes completely neglected? In relation to area change (page 19, lines 48-50) it is said that "... it is only possible to compare studies that have analysed entire mountain ranges rather than individual glaciers and thus refer to a regional characteristic". This argument is also valid for length changes, so why are direct measurements of a few individual glaciers preferred over regional studies?? [Jacob Clement Yde, Norway]	Rejected: Whereas terminus fluctuations of individual glaciers contain their relation with climate, relative area changes do not and are only meaningful as a regional average.
4-799	4	19	39	19	51	The text should discuss the ability of area measurements to represent mass changes. Area is related to	Noted: area - volume/mass relations are used and

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						volume/mass in a non-linear fashion and this is not mentioned in the text or in Table 4.3. [Government of United States of America]	mentioned in the volume section 4.3.3.3.
4-800	4	19	39	20	27	chapter is weak in structure, style and language. For example, 4.3.2.2. is jumping around topics, it starts with point surface measurements but the (line 6) jumps to global extropolation. The chapter should clearly distinguish between how mass changes are obtained and how individual data are extrapolated to global or regional estimates. In addition the chapter 3.2.2.2 is biased towards surface mass balance and does not mention anything about the growing literature of measurements of frontal ablation, for example using a flux-gate approach. This bias should be avoided and methods to measure surface-mass balance and frontal ablation treated equally, in particular considering that more studies on the later are one of the advances from AR-4. The entire chapter should be largely rewritten. There are also many issues with language. [Regine Hock, United States of America]	Taken into account: The text has been revised. We are unable to describe all physical processes due to space limitations.
4-801	4	19	40	19	40	Suggest that the authors specify the approximate time span of the LIA here. [Government of United States of America]	Editorial
4-802	4	19	42	19	43	Sentence is unclear: what direct measurements? [Regine Hock, United States of America]	Editorial
4-803	4	19	43	19	44	Causal connection between highly variable thickness distribution and correlation of area change with climate is unclear. [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-804	4	19	44			For a citation that attempts to link area change to volume change, that points out some of the issues is, [Andrew Fountain, United States of America]	Editorial
4-805	4	19	44			Basagic, H., and Fountain A.G. 2011. Quantifying twentieth century glacier change in the Sierra Nevada, California. Arctic, Antarctic, and Alpine Research, 43, 317-330. [Andrew Fountain, United States of America]	belongs to 4-804
4-806	4	19	44			They include photos that show the glaciers at their Little Ice Age maximum moraines as well. [Andrew Fountain, United States of America]	belongs to 4-805
4-807	4	19	44			They also talk about comparison of volume change for the entire region rather than individual glaciers. [Andrew Fountain, United States of America]	belongs to 4-806
4-808	4	19	46			Rapidly emerging nunataks have been also reported in the Cook Ice Cap of the Kerguelen Islands and have been used to quantify the elevation changes in the lower reaches of glaciers (see Berthier E., Lebris R., Mabileau L., Testut L., and Rémy F. Ice wastage on the Kerguelen Islands (49S, 69E) between 1963 and 2006., Journal of Geophysical Research-Earth Surface, 114, 10.1029/2008JF001192, 2009) [Etienne BERTHIER, France]	Noted: We have declined from assessing region 19
4-809	4	19	46			Pelto citation. His work in the NW USA is considered suspect. I suggest deleting. [Andrew Fountain, United States of America]	Rejected: The fact that somebody considers somthing to be "suspect" is no scientific reason to use material or not. Either it is proven wrong or not. Yet, we will check the reference
4-810	4	19	48	19	51	Sentence does not make sense. No matter any dependence on glacier size, one can compare retreat rates of individual glaciers. What is meant that one can not compare such studies. It may be better to reformulate in a way that just mentions the size-dependency. [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-811	4	19	49			The title of the Davies and Glasser paper is “Accelerating shrinkage of Patagonian glaciers from the Little Ice Age (~AD 1870) to 2011” and does not fit with the sentence. I found also the sentence a bit misleading. It should probably be said that in general there is a tendency toward increased in the rate of area loss and that those regions are rather exception (it seems to me) [Etienne BERTHIER, France]	Taken into account: The text has been revised.
4-812	4	19	49			entire mountain ranges > complete regions (not necessary mountain ranges) [Andreas Kääb, Norway]	Editorial
4-813	4	19	50	19	51	The comment about variable analysis intervals is very important, but is not highlighted as such. An example of the potential errors that can result from comparing between mismatched intervals is important. [Government of United States of America]	Noted: We have stated it because it is important. There are no quantifiable errors associated with a comparison of different time periods. One just has to make sure that they are comparable or note when

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							they are not.
4-814	4	19	50	19	51	Delete sentece [Regine Hock, United States of America]	Noted: The comment before asks to highlight it ...
4-815	4	19	51			I would say not 'easily' compared. [Andrew Fountain, United States of America]	Editorial
4-816	4	19	51			cannot be compared for the same intervals: I don't understand. [Andreas Kääb, Norway]	Editorial
4-817	4	19	55	19	56	Sentence needs language improvement. Also replace 'net annual' by 'annual' [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-818	4	19	55	19	57	rewording/simplifying; description is based on a 'typical' winter-accumulation/summer-ablation glacier type [Dorothea Stumm, Nepal]	Taken into account: The text has been revised.
4-819	4	19	55	19	57	it does not account for e.g. summer-accumulation type glaciers that have the main ablation and accumulation [Dorothea Stumm, Nepal]	Taken into account: The text has been revised.
4-820	4	19	55	19	57	in the same season. [Dorothea Stumm, Nepal]	Taken into account: The text has been revised.
4-821	4	19	55	20	27	paragraph is confusing, dense, and poorly organized. [Christopher Little, United States of America]	Taken into account: The text has been revised.
4-822	4	19	55			I had trouble finding the four ways in the first sentence of 4.3.2.3 [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been revised.
4-823	4	19	56			Delete "net" so that it reads "…,the annual surface mass balance.." Reasoning: cf. Cogley et al. (2012, page 11) [Michael Zemp, Switzerland]	Accepted: 'net' has been removed
4-824	4	19	57	19	57	remove laborious. Other methods are also 'laborious', just in the office and not in the field [Regine Hock, United States of America]	Accepted: 'labourious' has been replaced by 'fieldwork intensive'
4-825	4	20	1	20	4	It is worth mentioning that not only are the available time series few in number, but they are in some cases not optimally located at all. (Alaska is the prime example, where the 3 long time series avaliable (McCall, Gulkana, and Wolverine) are located on the norhern and western periphery of the main body of glacier ice, in the Chugach and Wrangell/St Elias ranges. There is virtually nothing (not even mountain meteorological data) available for grond observations in the Wrangell/St. Elias. [W. Tad Pfeffer, United States of America]	Taken into account: text has been revised accordingly
4-826	4	20	2	20	2	Is this more correctly mass loss from sub-glacial ablation (melting?) and drainage? [Government of Australia]	Rejected: ablation is mass loss
4-827	4	20	2			Maybe cite Alexander et al., 2011, regarding subglacial ablation. Alexander D., Shulmeister J., and Davies T. High basal melting rates within high-precipitation temperate glaciers, Journal of Glaciology, 57, 789-795, 2011. [Etienne BERTHIER, France]	Rejected: a reference to processes is not appropriate in this section
4-828	4	20	2			Internal accumulation included in direct measurements+ [Andreas Kääb, Norway]	Noted: this is to detailed and would reduce clarity
4-829	4	20	6			See Cogley (2012) who lists that geodetic regional estimates seem to disagree with in-situ mass balances (Cogley, Nature, 488, 468-469; this is news&views but still an important point). [Andreas Kääb, Norway]	Noted: text has been revised for more clarity but does not take into account the detailed discussion as proposed
4-830	4	20	7			In some cases, however, for example … (?) [Andreas Kääb, Norway]	Noted: sentence has been removed for better clarity
4-831	4	20	8			Remove 'individual'? [Andreas Kääb, Norway]	Taken into account: text portion has been revised
4-832	4	20	8			ok now i see the 4 ways. This needs to be signposted more clearly. Perhaps use paragraph for ech method? Also list them at the start of section using same names as will be used in following paragraphs. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: paragraph has been reorganised and rephrased
4-833	4	20	10			i and ii look like the same method but with different data acquistion? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: paragraph has been reorganised and rephrased

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4-834	4	20	12			No reference is given for the geodetic method whereas references are given for other methods. Nuth & Kaab, 2011 could be cited for the differencing of DEMs. For repeat altimetry applied to glaciers, Moholdt et al. 2010 is probably a good reference. Moholdt G., Nuth C., Hagen J. O., and Kohler J. Recent elevation changes of Svalbard glaciers derived from repeat track ICESat altimetry, Remote Sensing of the Environment, 114, 2756–2767, 2010; Nuth C., and Kääb A. Co-registration and bias corrections of satellite elevation data sets for quantifying glacier thickness change, The Cryosphere, 5, 271-290, 10.5194/tcd-4-2013-2010, 2011. [Etienne BERTHIER, France]	Taken into account: reference for geodetic methods added
4-835	4	20	13	20	13	not volume but volume change [Regine Hock, United States of America]	Rejected: the statement is of general value and holds for both volume and volume change
4-836	4	20	13			Write "The conversion from volume to mass can cause a major uncertainty especially over short periods…" [Michael Zemp, Switzerland]	Accepted: 'especially' has been added
4-837	4	20	14	20	18	It would be important to note that GRACE also is limited to a short time period. [Government of United States of America]	Accepted: a note about the short time period for GRACE being available has been added
4-838	4	20	17	20	17	Larsen et al 2005 is not appropriate reference here. [Regine Hock, United States of America]	Noted: references have become unnecessary while restructuring the text
4-839	4	20	17	20	17	delete comma after "adjustment" [Matt King, Australia]	Editorial
4-840	4	20	17	20	17	Remove the coma before the parenthesis [Antoine RABATEL, France]	Editorial
4-841	4	20	18	20	18	submitted instead of Submitted. [Antoine RABATEL, France]	Editorial
4-842	4	20	18			with THE application [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-843	4	20	20	20	20	The model of Lüthi et al. (2010) uses glacier length changes records to compute corresponding ice volume changes, not ELA changes as mentionned. [Martin Funk, Switzerland]	Taken into account: text and reference have been revised
4-844	4	20	20	20	20	For the equilibrium line altitude method, Rabatel et al., 2005 and Rabatel et al., 2008 have to be cited before Luethi et al., 2010). The method to compute the annual mass balance from ELA changes has first been presented in Rabatel et al., 2005. Here are the references: [Rabatel, A., J.-P. Dedieu, C. Vincent. 2005. Using remote-sensing data to determine equilibrium-line altitude and mass-balance time series: validation on three French glaciers, 1994-2002. Journal of Glaciology, 51 (175), 539-546. doi: 10.3189/172756505781829106.] and [Rabatel, A., J.-P. Dedieu, E. Thibert, A. Letreguilly, C. Vincent. 2008. Twenty-five years of equilibrium-line altitude and mass balance reconstruction on the Glacier Blanc, French Alps (1981-2005), using remote-sensing method and meteorological data. Journal of Glaciology, 54 (185), 307-314. doi: 10.3189/002214308784886063.] [Antoine RABATEL, France]	Acepted: Rabatel et al. 2005 has been added
4-845	4	20	20			The Lüthi-model is based on auto-correlations (length/volume, length change/volume change; cf. remark on page 18, lines 4-5). IPCC should be careful about such statistical misconceptions. Furthermore, this model ignores real topography. It remains unclear what the use of such approaches could be. Better eliminate the statement and reference - there are more intelligent approaches available. [Wilfried Haeberli, Switzerland]	Noted: The text has been revised and references have been reorganised. The omission of Lüthi et al. has been rejected.
4-846	4	20	21	20	21	Scaling is the wrong term here. These models do not 'scale'. [Regine Hock, United States of America]	Noted: text has been revised accordingly
4-847	4	20	22	20	22	"…work as glacier by glacier statistically refined approaches…" is a bit opaque. Can this be clarified? [W. Tad Pfeffer, United States of America]	Taken into account: sentence has been rephrased for more clarity
4-848	4	20	22	20	23	There is no reason to ignore the contribution on estimation of mass changes of glaciers from Chinese researchers. It is recommended to include the references below which are dedicated to the estimation of mass balance of glaciers in the Yangtze River source region and a well-studied Hailuogou Glacier: 1. Liu Shiyin, Yong Zhang, Yingsong Zhang, Yongjian Ding. 2009: Estimation of glacier runoff and future trends in the Yangtze River source region, China. Journal of Glaciology. 55(190): 353-362; 2. Zhang Yong, Yukiko Hirabayashi, Shiyin Liu. 2012: Catchment-scale reconstruction of glacier mass balance using observations and global climate data: Case study of the Hailuogou catchment, south-eastern Tibetan Plateau. Journal of Hydrology 444–445, 146-160. [Jing Ming, China]	Rejected: only papers that introduce a new method or describe a method in detail are listed here.

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4-849	4	20	23			“They”? [Atsumu Ohmura, Switzerland]	Editorial
4-850	4	20	23			They improve ... [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-851	4	20	24			completeness, and add ... [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-852	4	20	26	20	27	Argument not convincing. Although the method includes consideration of a whole drainage basin, the output is the glacier mass balance of the glaciers in the catchment. [Regine Hock, United States of America]	Taken into account: text has been revised
4-853	4	20	30	20	30	Table 4.3. Although in principle a good idea, the table is confusing, subjective, incomplete and partially wrong and should best simply be deleted and the main points incorporated in the text. Given the complex nature and overlaps between the different methods in terms of advantages and disadvantages etc, it seems impossible to make this table scientifically sound. For example: why would length changes be confined to 'dozens' of glaciers? The tables of advantages and disadvantages are incomplete. Why is 'expensive' mentioned for one category but not for others. GRACE is not cheap either! All methods are laborious and not just direct glaciological field measurements. The distinction between 'precise' and 'very precise for the 2 methods can be debated. It all depends on details about the data available for each of the methods. A direct estimate can be very rough depending on the number of stakes. My point is that within each of the methods there is a range of attributes possible smearing out the differences between methods. Direct mass balance measurements do NOT measure volume but mass, and so does GRACE, i.e. it is misplaced in the table. All methods have far more advantages and disadvantages, i.e. GRACE suffers strongly from the necessity of relying on models to corret for isostatic adjustments and hydrology. Field measurements can be anything and not just 'seasonal'. In summary the table is highly problematic and scientifically not sound. [Regine Hock, United States of America]	Taken into account: The table has been revised (e.g. the advantages / disadvatages columns were removed and row separators were introduced). Integrating the information into the text is not an option as we have to shorten the other text considerably for this.
4-854	4	20	30	20	32	Table 4.3 Dividing lines between the Length, Area and Volume categories are not quite clear. [Government of Australia]	Editorial
4-855	4	20	30	20	32	Table 4.3: The matching of words in the first and second columns should be adjusted. I recommend to shift “Length”, “Area” and “Volume” to the first line of the second column matching to these glacier characteristics. [Atsumu Ohmura, Switzerland]	Editorial
4-856	4	20	30	20	32	Table 4.3: Unclear which of the methods refer to Length, Area, Volume (should actually be Volume Change); lines should be drawn to enable clear assignment. [Helmut Rott, Austria]	Editorial
4-857	4	20	30	20	32	Table 4.3: Parameter: Area. Method: Remote Sensing: "Image processing" is a very generic term, not a particular technique for mapping glacier areas [Helmut Rott, Austria]	Taken into account: The text has been revised.
4-858	4	20	30	20	32	Table 4.3: Parameter: Volume (Change ?). Method: laser and radar profiling are not very relevant: (i) however, laser scanning provides precise maps (complete coverage) of surface topography (and change); (ii) radar profiling (ice thickness measurements) is not a main tool for very precise measurement of volume change, but rather for determining the overall volume. [Helmut Rott, Austria]	Taken into account: The text has been revised.
4-859	4	20	30	20	32	improve Table 4.3; unclear what methods used for which parameter [Dorothea Stumm, Nepal]	Editorial
4-860	4	20	31	20	31	GRACE study needs "Mass" in Parameter column [Matt King, Australia]	Editorial
4-861	4	20	31	20	32	Table 4.3: Adding the parameter Mass to direct mass balance measurements as well as to gravimetry measurements would help clarifying the difference between measurement methods and related uncertainties such as the density conversion. [Michael Zemp, Switzerland]	Editorial
4-862	4	20	31			Add some horizontal lines to separate raws dedicated to Length / Area / Volume [Etienne BERTHIER, France]	Editorial
4-863	4	20		20		Table 4.3: If the authors want to provide such an overview table, I suggest to rethink clearly what to list here. Particularly the rows 'advantages' and 'disadvantages' do not look consistent to me and the choices why a certain advantage/disadvantage is listed for a certain method (but not for another one, where it also applies) is not transparent to the reader. [Nadine Salzmann, Switzerland]	Taken into account: The text has been revised.
4-864	4	20				Table 4.3 is confusing. Horizontal Dividing lines are needed between Length, area and volume. [Government	Editorial

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						of United States of America]	
4-865	4	20				Table 4.3: Organization is cumbersome and confusing. For example, most methods used to determine glacier length change can also be used to determine area change and some can be used to determine volume change. It would be better to list the methods and state the parameters for which they are suited. Also, optical and GPS surveying should be given as a method. Finally, one would expect that methods such as stereo photogrammetric analysis of aerial photography or LiDAR analysis, if properly done, yield results that are accurate, not merely precise. [Government of United States of America]	Taken into account: We apologize for the confusion that was introduced by removing the lines between the rows. We agree that other forms of organizing the table are possible, but sorting it for methods would result in a problematic assignment of the other columns. Please note that we here list only some of the most typical techniques, the list is not exhaustive. We will replace the advantages/disadvantages columns with a precision column.
4-866	4	20				Table 4:3: I recognize the usefulness and basic importance of this table. However, I wonder wether an IPCC report is the right place to provide such technical information. If it is kept, I recommend to improve it graphically, making clearer wihch methods and techniques belong to which parameter. [Christian Huggel, Switzerland]	Editorial
4-867	4	20				Tab. 4.3: * It is unclear where the separation between Length, Area and Volume is in the Methods, Techniques, etc. * Maps are usually actually from photogrammetric surveys. A bit tricky to list them as method. * Photogrammetry can be from air or space (e.g. ASTER), the latter is not necessary precise, but not expensive. * Disadvantages of area remote sensing not only debris cover; snow remains and glacier definition can be equally tricky. * field method for area: laborious > direct access necessary. * Volume parameter includes mass. Not sure if this too simple (e.g. when thinking of GRACE). * GRACE does not resolve individual glaciers, but has also problems with scattered ice (e.g. Scandinavia, Inner Tibet plateau, etc.). * In total, I am not so sure if Tab 4.3 is really necessary. The uncertainties from having it simple might overweight its benefits. [Andreas Kääb, Norway]	Taken into account: The table has been revised and clear row separators were inserted.
4-868	4	20				table 4.3 needs attntion to format so that different methods for each paramter are more clearly delineated [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-869	4	20				table 4.3 techniques need to match the discussion in the text which they do not currently do (eg 4 ways of doing volume are not shown here) [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The techniques mentioned in the table were better linked to the main text.
4-870	4	21	1	25	43	Section 4.3.3 Observed changes in glacier length, area and mass: The section should either strictly stay within the presentation of the observations, or integrate the climatic interpretation in a substantial manner. It is not fruitful to allow insufficient climatic interpretations to jump in from place to place, e.g. P 23, Line 18-21. [Atsumu Ohmura, Switzerland]	Rejected: we have made sure that no detection and attribution statements are made in this observation section but will still use modeled areal and temporal extrapolations from observed data.
4-871	4	21	3			Section 4.3.3.1: (1) Presentation of regional studies of length changes should be preferred over presentation of a few individual glacier length changes! Sentence such as (p. 21, lines 5-6) "... glacier terminus fluctuations provide a largely homogenous signal (Figure 4.9)" and (p. 21, lines 14-15) "... reveal a clear overall trend in terminus retreat, but also intermittent advances that are not globally synchronous (Figure 4.9)" worries me!! (2) How can these interpretations be derived from Figure 4.9? (3) How can one cherry-picked glacier in New Zealand or two glaciers in Svalbard be representative for the region or provide a global signal? (4) How many of the glaciers in Figure 4.9 are tidewater glaciers (which are important in many regions according to Figure 4.8)? (5) By what objective criterias were the glaciers in Figure 4.9 selected? (6) Was a proper literature study conducted before Figure 4.9 was constructed (only two sources are given)? (7) All in all, I very strongly recommend that section 4.3.3.1 is refocused towards regional studies of length changes, which not only will make it in line with the structure and arguments in sections 4.3.3.2 and 4.3.3.3, but also significantly improve the interpretation of regional trends and provide average annual terminus recession rates for given periods in time! It is likely too late to ask the researchers working with glacier length changes to write a consensus paper as the paper submission deadline for AR5 has expired, but I am sure that if the author(s) responsible for section 4.3.3.1 asks the researchers for assistance in compiling literature, constructing a new Figure 4.9 and rewriting section 4.3.3.1, we will be most helpful. (8) For example, for region 5 Greenland the following regional studies provide insigthful data on glacier length changes: Bjørk et al. 2012. Nature Geoscience, 5,	(1) Rejected: The focus is here on long-term quality controlled records. These are not available from science-driven selections of glaciers in whatever region. - (2) Noted: By looking at the curves one recognizes that periods of advance are superimposed on a long-term trend of continous retreat. - (3) Taken into account: The representativeness of the here shown sample for the conclusions made has now been better explained. - (4) Noted: None, the glaciers in GTN-G are in general land-terminating. Through retreat howver, they might become lacustrine (calving in proglacial lakes). - (5) Noted: see (1), (6) Noted: see (1) please note that WGMS is in charge of collecting standardized data on global glacier fluctuations and publishes them. Data from the literature have at least three shortcomings: (a) incomparable time periods, (b) arbitrarily selected

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						427-432; Jiskoot et al. 2012. Ann. Glaciol., 53, 35-44; Kargel et al. 2012. The Cryosphere, 6, 533-537; Leclercq et al. submitted. The Cryosphere; Mernild et al. 2012. The Cryosphere, 6, 625-639; Yde and Knudsen, 2007. Ann. Glaciol., 46, 209-214). The results from these, and similar studies from other RGI regions, should be used to deduce 'recession rates' (m yr-1) for the periods of averaging. A figure similar to Fig. 4.10 can be presented for glacier length changes (replacing the current Fig. 4.9). This will bring the quality of section 4.3.3.1 more in line with section 4.3.3.2. [Jacob Clement Yde, Norway]	regions that do not provide global coverage, (c) incomparable methodologies of determination. - (7) Rejected: Calculating average retreat rates over arbitrary time periods for a sample of glaciers with different response times does not have a glaciological meaning (in contrast to area changes). - (8) Rejected: None of these studies provide annually resolved long-term time series (starting in the 19th century). Many of them refer to tidewater glaciers or outlets from the ice sheet whih are not covered in this section
4-872	4	21	5	21	6	Not convincing to refer to Figure 4.9, as the individual glaciers presented in the figure are chosen based on unknown criterias, of unknown representativeness and too few to support the interpretation of a global "largely homogenous signal". [Jacob Clement Yde, Norway]	Noted: see comments to 4-871 (3)
4-873	4	21	6	21	6	homogenous in what sense? [Regine Hock, United States of America]	Editorial (homogenous trend of retreat)
4-874	4	21	6	21	6	up to several km is not correct. Some retreated more. Also avoid acronym LIA [Regine Hock, United States of America]	Taken into account: The statement has been revised
4-875	4	21	8			LIA was not always and everywhere the maximum extent of the glaciers during the Holocene but just the last significant advance. Several phases of glacier advances between 8000 to 1200 years BP extended more than LIA are known. (there is a huge literature on this). [Mauro Guglielmin, Italy]	Taken into account: We have clarified the time period considered in this section.
4-876	4	21	9	21	9	More references are required here, as Rabatel 2008 is a study of the tropical Andes [Paul Leclercq, Netherlands]	Rejected: the reference given includes a map (Fig. 7) with worlwide LIA maxima
4-877	4	21	10	21	10	delete from this rather extreme position [Regine Hock, United States of America]	Editorial
4-878	4	21	10			several regions': more specific [Andreas Kääb, Norway]	Editorial
4-879	4	21	11	21	11	It is not clear what 'local exceptions' refers to here, please explain. [Government of Australia]	Taken into account: This has now been explained
4-880	4	21	11	21	11	delete "but local exceptions exist". [Regine Hock, United States of America]	Rejected: The text has been changed for more clarity.
4-881	4	21	11	21	11	"but local exceptions exist". Not clear what this refers to - exception to the general retreat, or exceptions to the phases of stability or readvance?. Please clarify this sentence. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: This has now been explained
4-882	4	21	11			It is probably more correct to write that glaciers advance was around 1920s and 1980s than 1920s and 1970, considering that the “neoglacial advance” was mainly between 1978 and 1985. [Mauro Guglielmin, Italy]	Rejected: In several regions advances already started in the 1960s (e.g. Trientgl. in the Mt. Blanc region). So 1970s is about in the middle. of the entire period.
4-883	4	21	11			local exceptions': does that in engl. Means what it should. More precise? [Andreas Kääb, Norway]	Taken into account: This has now been explained.
4-884	4	21	14	21	15	Not convincing to refer to Figure 4.9, as the individual glaciers presented in the figure are chosen based on unknown criterias, of unknown representativeness and too few to support the interpretation of a global "clear overall trend in terminus retreat". [Jacob Clement Yde, Norway]	Rejected: See comments to 4-871 (3). This statement refers to WGMS (2008) that includes all measurements.
4-885	4	21	16	21	16	more or less' is vague and should be avoided [Regine Hock, United States of America]	Accepted
4-886	4	21	17	21	18	"the decadal fluctuations mentioned above" - not clear what passage of text is being referred to above here. Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text has been revised.
4-887	4	21	19			time series of individual glacier > time series of length changes of individual glaciers ? [Andreas Kääb, Norway]	Editorial
4-888	4	21	21	21	21	It might be worth being more specific about what 'regionally different climatic conditions' are here (e.g. increased precipitation in the accumulation zone). [Government of Australia]	Taken into account: The text has been revised.

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4-889	4	21	21	21	21	vagues. It has been established that these advances were due to precipitation increases [Regine Hock, United States of America]	Taken into account: The text has been revised.
4-890	4	21	21	21	22	Replace "Nesje et al., 2000" with "Winkler et al., 2009" as the latter better analyses the Scandinavian glacier length changes at the end of the 20th century. [Winkler, S., H. Elvehøj, and A. Nesje, 2009. The Holocene, 19, 395-414] [Jacob Clement Yde, Norway]	Editorial
4-891	4	21	21			need to give an example of this (presumably incrasse in snowfall?) [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-892	4	21	22	21	22	Change "Karakoram" to a reference to the appropriate RGI region. [Jacob Clement Yde, Norway]	Editorial
4-893	4	21	22	21	22	What is the reason for selecting these two regions (Karakoram and Svalbard) as examples of regions with surge activity? It is more important to inform the readers that 14 of the other 17 regions contain surge-type glaciers. Otherwise the readers might be left with the impression that surge activity is restricted to a few regions. [Jacob Clement Yde, Norway]	Taken into account: The text has been revised for clarity on the given examples.
4-894	4	21	22			The inventory by Copland et al., 2009 seems a more suitable reference for the Karakoram area. Why focusing only on Svalbard and Karakoram? Surges are also reported in the Andes (Aconcagua), Alaska, Pamir, Iceland… For example, there is much better record of historical surges in Iceland (see Björnsson H., Palsson F., Sigurdsson O., and Flowers G. E.: Surges of glaciers in Iceland, in: Annals of Glaciology, Vol 36, Annals of Glaciology, 82-90, 2003.) [Etienne BERTHIER, France]	Rejected: This is not a review of the published literature but a possible explanation for the observed changes in the mentioned regions.
4-895	4	21	22			Berthier E., Arnaud Y., Kumar R., Ahmad S., Wagnon P., & Chevallier P., Remote sensing estimates of glacier mass balances in the Himachal Pradesh (Western Himalaya, India). Remote Sensing of Environment, 108(3), 327-338, doi: 10.1016/j.rse.2006.11.017, 2007 is missing. [European Union]	Noted: This study is related to mass balance and thus cited in the respective section.
4-896	4	21	22			also debris cover? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-897	4	21	25	21	25	delete: for specific glaciers in individual years'. This is obvious from the sentence. [Regine Hock, United States of America]	Editorial
4-898	4	21	25	21	26	Delete the last part of the sentence from ", or". The loss of the lower part of the glacier tongue often provide difficulties to measurements, are related to hypsometrical conditions or debris-covered termini, and give spurious retreat rates (e.g., 300 m retreat in 1 hour), but seen in a climatic perspective this process becomes less important. [Jacob Clement Yde, Norway]	Rejected: Such events might be unfortunate for further field measurements or modelling, but it is nevertheless an extremely strong climatic signal (as illustrated in the FAQ 4.1).
4-899	4	21	26	21	27	"Highly sensitive" to what? As I understand Rivera et al., 2012, the retreat rates of more than 300 m yr-1 are due to volcanic activity. The readers are likely to have the impression that "highly sensitive" refers to climate changes, but in this case it seems to refer to volcanic activity in the sense that crater glaciers are "highly sensitive" to changes in volcanic activity. Rephrase or delete this sentence. [Jacob Clement Yde, Norway]	Rejected: Highly sensitive means strong response to a small forcing. We here refer to Glaciar Inexplorado that is a normal valley glacier and not influenced by volcanic activity.
4-900	4	21	26	21	28	It´s necessary to note that this decline is the result of volcanism, not global warming [Government of Chile]	Rejected: Monte Inexplorado is a volcano but not active. The dramatic retreat of Glaciar Inexplorado I and II is thus a result of climate change rather than volcanism.
4-901	4	21	26			the introductionm to this section says that you will only be discussing nomral glaciers but much of the text talks about the atypical (calving etc) ones. Probably need to omit this line from introduction. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The text has been revised.
4-902	4	21	27	21	27	Sensitive with respect to what? Replace calving by land-terminating [Regine Hock, United States of America]	Taken into account: The text has been revised to be more clear
4-903	4	21	28	21	29	avoid 'normal' [Regine Hock, United States of America]	Editorial
4-904	4	21	29	21	29	normal' is not an adequate expression for an IPCC report. [Nadine Salzmann, Switzerland]	Editorial
4-905	4	21	29	21	30	This part of a sentence is in contract to the paper of Kääb et al. (2012). Kääb et al. (2012) write in the abstract:	Taken into account: The text has been revised to be

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						Contrary to expectations, regionally averages thinning rates under debris-mantled ice were similar to those of clean ice despite insulation by debris covers. And further: That debris-covered ice thins at a rate similar to that of exposed ice shows that the role of debris mantles in glacier mass balance must be reassessed. (Both articles deal with himalayan glaciers) [Luzi Bernhard, Switzerland]	more clear
4-906	4	21	29			normal' glaciers. More specific [Andreas Kääb, Norway]	Editorial
4-907	4	21	29			Add a sentence about calving glaciers? [Andreas Kääb, Norway]	Taken into account: The text has been revised to be more clear
4-908	4	21	32	21	35	Figure 4.9: the number of examples is a bit meagre and could easily be expanded. Preferably with the available long records (Iceland, Scandinavia, New Zealand, Southern Andes). See for instance the data set compiled in Leclercq and Oerlemans 2012 and Leclercq et al 2011 [Paul Leclercq, Netherlands]	Taken into account: The text has been revised to be more clear on the reasons for selecting the examples shown. Further long-term datasets from Leclercq and Oerlemans 2012 were added.
4-909	4	21	33	21	33	Not all data points shown in the Figure are the result of direct measurements: e.g. Leclercq et al. 2012 include reconstructions based on historical sources for Greenland. The same is likely true for the longer timeseries in US and Iceland [Paul Leclercq, Netherlands]	Taken into account: The text has been revised to consider this deviation.
4-910	4	21	39	21	39	Tab;e replaced as "Table" [Yongjian Ding, China]	Editorial
4-911	4	21	39	21	39	Tab;e (typo) [Nadine Salzmann, Switzerland]	Editorial
4-912	4	21	39	21	49	The message of this paragraph is very difficult to follow. Please review the text and revise for clarity. [Government of United States of America]	Taken into account: The text has been revised with the main messages clearly stated.
4-913	4	21	39	21	49	paragraph needs language and structure improvement [Regine Hock, United States of America]	Editorial
4-914	4	21	39	21	49	This may be reveling my ignorance of the literature, but is an areal loss rate in percent per year really a good measure of ice loss? Doesn't this tend to decrease in time for the same applied forcing? Is there a way to use cumulative areal loss? [Christopher Little, United States of America]	Noted: We here report on what is provided in the peer-reviwed literature and this is the area loss rate in percent per year. Cumulative area loss does not provide any useful information (in terms of a climatic interpretation).
4-915	4	21	39	21	49	Please be consistent in the style you use to report rates of loss (positive or negative). Line 40 gives these rates as positive, and line 42 - 43 gives the rates as negative, then it is back to positive again later on line 43, then back to negative again on line 46. A loss of -3.4% is reporting a gain. [Thomas Stocker/ WGI TSU, Switzerland]	Editorial
4-916	4	21	39	21		Tab;e -> Table [Olaf Eisen, Germany]	Editorial
4-917	4	21	39			Table instead of Tab;e [Government of Chile]	Editorial
4-918	4	21	39			Last word should be Table [European Union]	Editorial
4-919	4	21	39			Table (not Tab;e) [Andreas Kääb, Norway]	Editorial
4-920	4	21	39			Tabl;e [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Editorial
4-921	4	21	41	21	42	It is recommended to include a reference after Sorg et al (2012): DING Yongjian, LIU Shiyin, LI Jing, SHANGGUAN Donghui. The retreat of glaciers in response to recent climate warming in western China. Annals of Glaciology, 2006, 43, 97-105. The paper is dedicated to discuss the regional pattern of glacier area changes in western China. [Jing Ming, China]	Noted: This Ch. is on observations rather than their climatic interpretation
4-922	4	21	42			It is not really clear why glaciers in Tien Shan are singled out here with a reference. No symmetric reference is given for the Arctic. There are many other glaciers in continental climatic region that could be cited here also. [Etienne BERTHIER, France]	Taken into account: The cited references have been revised.
4-923	4	21	42			in the -1 to -2% yr-1 range'. I don't think this works in english. [Andreas Kääb, Norway]	Editorial
4-924	4	21	42			what is the significance of the "-"? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The statement has been revised

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4-925	4	21	44	21	44	It would be interesting at least to cite one major paper on the tropical Andean glaciers changes. I would recommend to have in the references list: VUILLE, M., FRANCOU, B., WAGNON, P., JUEN, I., KASER, G., MARK, B. G., BRADLEY, R. S. 2008. Climate change and tropical Andean glaciers: past, present and future. Earth Science Reviews 89:79-96. [Jefferson Cardia Simões, Brazil]	Rejected: The climatic interpretation of the observed changes is discussed in the report of WGII.
4-926	4	21	44	21	44	Add 'et al." in the reference Rabatel et al., 2012 [Antoine RABATEL, France]	Editorial
4-927	4	21	44	21	44	Rabatel 2012, is in fact Rabatel et al, 2012 [Patrick WAGNON, Nepal]	Editorial
4-928	4	21	45	21	46	I suggest to delet this sentence. It is not sound to mention shorter periods [Nadine Salzmann, Switzerland]	Taken into account: The text has been revised.
4-929	4	21	45			For shorter periods … (remove even) [Andreas Kääb, Norway]	Editorial
4-930	4	21	46			I will include after the citation (Klein and Kincaid, 2006). " Even in the Alps an acceleration of the glacier shrinkage up to 4% between 2002 and 2006 was recorded (Cannone et al. 2008). [Mauro Guglielmin, Italy]	Noted
4-931	4	21	48	21	49	Mernild et al. [2012. Multi-decal marine- and land-terminating glacier recession in the Ammassalik region, southeast Greenland. The Cryosphere, 6, 625-639] found a trend towards accelerated area loss for glaciers in Southeast Greenland (see e.g. Figure 10b, page 635). Rephrase this sentence and add Mernild et al. (2012) to the references. [Jacob Clement Yde, Norway]	Noted
4-932	4	21	51	21	56	Paragraph should be shortened. The number of dissappeared glaciers is quite meaningless. In fact before 'dying' there is generally the opposite effect. Glacier split up into several individual glaciers and the number actually increases. Mentioing that 100 glaciers have dissappeared should be avoided. [Regine Hock, United States of America]	Taken into account: The text has been revised and numbers are supported by cited literature.The link to the FAQ has to be made in the main text (no references are allowed in the FAQ).
4-933	4	21	51			Pelto citation. His work in the NW USA is considered suspect. I suggest deleting. [Andrew Fountain, United States of America]	Rejected: no scintific argument is given.
4-934	4	21	51			Alternative citation could be, DeBeer and Sharp, 2009, Topographic influences on recent changes of very small glaciers in the Monashee Mountains, British Columbia, Canada. Journal of Glaciology, 55: 691-700 [Andrew Fountain, United States of America]	Noted
4-935	4	21	53	21	56	The part on disappeared glaciers is scientifically vague, despite its importance in media communication! If this statement about "more than one hundred glaciers have disappeared completely" is published in the final report, then I strong recommend that the IPCC keeps a list of the names and locations of these glaciers and double-check the references (no references are mentioned) in order to be absolutely sure about what definition of original minimum size was used (and/or what evidence of active internal deformation was reported) and how it was checked that the glaciers had disappeared completely. The word "completely" indicates that absolutely all glacier ice has disappeared, so only conventional field examinations can be trusted in this case. I strongly recommend that everything on disappeared glaciers should be deleted from WGI AR5, if the evidences are not robust enough to be thoroughly checked in the field by researchers and the media. If statements about disappeared glaciers are published without rigorous documentation and later proven to be wrong or guesswork, then the scientific value of the entire WGI AR5 could be questioned. [Jacob Clement Yde, Norway]	Taken into account: The text has been revised and numbers are supported by cited literature.The link to the FAQ has to be made in the main text (no references are allowed in the FAQ).
4-936	4	21	54			Insert “be” between “should” and “included”. [Atsumu Ohmura, Switzerland]	Editorial
4-937	4	21	55	21	56	What is the reference or collection of references that shows that >100 glaciers have disappeared? Please add references. [Dorothy Hall, United States of America]	Taken into account: The text has been revised and numbers are supported by cited literature.The link to the FAQ has to be made in the main text (no references are allowed in the FAQ).
4-938	4	21	55	21	56	"more than one hundred glaciers have disappeared" is rather a meaningless statement without some indication of what the upper range to this number might be, and/or without knowing how many glaciers we started with. Could you convert this number into a proportion of the total number of glaciers given in table 4.2? This would suggest that the number of glaciers that have disappeared is something lie at least 0.05% of all glaciers, which maybe doesn't sound so dramatic, but is important to put the "more than one hundred glaciers" into context. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text has been revised and numbers are supported by cited literature. Percentages are meaningless as the number of glaciers steadily increases (due to glacier split). See also comment 4-932.

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4-939	4	21	55			more than hundred glaciers have dissappeared'. I am sure many many more dissappeared. [Andreas Kääb, Norway]	Taken into account: The text has been revised and numbers are supported by cited literature.The link to the FAQ has to be made in the main text (no references are allowed in the FAQ).
4-940	4	21	56			Add somewhere a sentence that % area changes are for geomatric reasons larger for small glaciers and vice-versa, for the same mass-balance signal. [Andreas Kääb, Norway]	Taken into account: The text has been revised to be more clear
4-941	4	22	1			Basagic, H., and Fountain A.G. 2011. Quantifying twentieth century glacier change in the Sierra Nevada, California. Arctic, Antarctic, and Alpine Research, 43, 317-330. Provides change for the Sierra Nevada. [Andrew Fountain, United States of America]	Noted
4-942	4	22	2	12	2	In fig 4.10 caption: Mean annual mean loss rate of what?? Idem p 81 line 4 [Government of France]	Editorial
4-943	4	22	2	22	15	rephrase: Each line refers to a glacier's observed relative area loss and … (Mentioning that it is published is not necessary. If it was not published it would not be part of this report. [Regine Hock, United States of America]	Editorial
4-944	4	22	2	22	15	I would also like to recommend to use these reference "DING Yongjian, LIU Shiyin, LI Jing, SHANGGUAN Donghui. The retreat of glaciers in response to recent climate warming in western China. Annals of Glaciology, 2006, 43, 97-105" instead of "Zhang, M., S. Wang, Z. Li, and F. Wang, 2012: Glacier area shrinkage in China and its climatic background during the 20 past half century. Journal of Geographical Sciences, 22. doi:10.1007/s11442-012-0908-3" and "Li, X., et al., 2008: Cryospheric change in China. Glob. Planet. Change, 62, 210-218. 25 doi:10.1016/j.gloplacha.2008.02.001". In addition, a paper by Shangguan and others (Shangguan D, Liu S, Ding Y, et al. Glacier changes during the last forty years in the Tarim Interior River basin, northwest China. Progress in Natural Science. 2009, 19(6): 727-732.) should be cited. It gives a detailed result of area changes of glaciers over 17000km^2 and their effort should be encouraged and honored if considering and comparing the effort for changes of glaciers of some square kilometers in Tianshan by Li B. et al (doi:10.3189/172756506781828557) as cited in the section. [Jing Ming, China]	Noted
4-945	4	22	2	22	15	The authors (Zhang and other, Li and others) of above-mentioned two cited papers by the section have never measured a glacier to know its change. IPCC ARs should be a platform to demonstrate the results that have been truly derived by the firsthand authors with good experience in glacier mapping, and should not be a sign to encourage people just summarizing the results of others and never make further assessment on the quality of the data they cited. Studies on glacier changes in the western Nyainqentanglha Range could be a good example. Some at least 4 authors have given area reduction of glaciers in the same region ranging from -5.2%~-18.2%. The reason is that those not familiar with mapping of glaciers may use the earliest topographical maps for glacier outlines with errors without any validation based on aerial photos for the production of these maps. For any further examination of my comment, please refer to the 4 papers: 1. Shangguan and others, 2008: Variation of Glaciers in the Western Nyainqentanglha Range of Tibetan Plateau during 1970- 2000. J. Glaciology & Geocryology, 30(2), 204-210 (in Chinese with English abstract); 2. Wu Yanhong and Zhu Liping, 2008: The response of lake-glacier variations to climate change in Nam Co Catchment,central Tibetan Plateau,during 1970-2000. Journal of Geographical Sciences, 18: 177-189, DOI: 10.1007/s11442-008-0177-3; 3. Chen Feng et al, 2009: Glaciers and Lake Change in Response to Climate Change in the Nam Co Basin,Tibet. Journal of Mountain Science, 27(6), 641-647; 4. Bolch et al, 2010: A glacier inventory for the western Nyainqentanglha Range and the Nam Co Basin, Tibet, and glacier changes 1976–2009. The Cryosphere, 4, 419-433, doi:10.5194/tc-4-419-2010. [Jing Ming, China]	Rejected: Authors of detailed original work are cited in the papers we use. An IPCC Assessment cannot cite every paper.
4-946	4	22	2	22	15	The following references may be cited: 1. Liu Shiyin, Shangguan Donghui, Xu Junli, and others. (2013), Glaciers in China and their variations, in: Global Land Ice Measurements from Space, J.S. Kargel, G.J. Leonard, M.P. Bishop, A. Kääb, and B. Raup (Editors), Praxis-Springer, Heidelberg, ISBN: 978-3-540-79817-0; 2. Shi Yafeng, Hunag Maohuan, Yao Tandong and He Yuanqing. 2008: Glaciers and Related Environments in China, Science Press, Beijing; 3. Shi Yafeng, Liu Shiyin, Ye Baisheng, Liu Chaohai and Wang Zongtai. 2008: The Concise Glacier Inventory of China. Publishing House of Scientific Popularization Shanghai. [Jing Ming, China]	Noted
4-947	4	22	15			Region (19) has been studied as well by Cook et al., 2005 and Rückamp et al., 2011:	Noted

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						Cook, A.J., Fox, A.J., Vaughan, D.G. (2005): Retreating glacier fronts on the Antarctic Peninsula over the past half-century. Science 308, 541-544 Rückamp, M., Braun, M., Suckro, S., Blindow, N. (2011): Surface lowering, accumulation and area changes of the King George Island ice cap, Antarctica. Global and Planetary Change 79, 99-109, doi:10.1016/j.gloplacha.2011.06.009. [European Union]	
4-948	4	22	17			The organization of this chapter is confusing, neglects important advances since AR-4, and could benefit from restructuring. Much of the subchapter refers to global estimates of mass change, however, chapter 4.3.4 details the global estimates. More logical would be to describe local and regional scale mass changes in 4.3.3.3 and all global esimates in chapter 4.3.4. Overall, the chapter does not provide a balanced overview of the mass changes. Currently the entire chapter is difficult to follow, switches back and forth between topics. Also, one of the most significant advances since AR-4 are new estimates of entire regions (e.g. Patagonia, Svalbard, Alaska) for various time periods. These should be highlighted and discussed here. [Regine Hock, United States of America]	Taken into account: paragraph has been restructured
4-949	4	22	23			"Although such measurements only cover most recent". I would change to "Although such measurements generally only cover most recent" because there are some studies (Gardner et al., The Cryosphere, 2012 in mind among others) that provides geodetic mass balance for different time periods [Etienne BERTHIER, France]	Taken into account: the entire section has been modified and restructured
4-950	4	22	28	22	28	"a reasonable percentage" - please quantify. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into acount: exact number is now given
4-951	4	22	29	22	29	What is meant by "reasonable"? Rephrase. [Jacob Clement Yde, Norway]	Taken into acount: exact number is now given
4-952	4	22	30			is this the true uncertainty? Or just a measure of spread? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The discussion about statistically obatined and true uncertainty has been improved cross referencing to the respective Box 2.1
4-953	4	22	31	22	31	add "," after "two" [Yongjian Ding, China]	Editorial
4-954	4	22	32	22	36	The point of this sentence is difficult to determine. Are the authors saying that ice dynamics were included in some individual studies, but not all? (Note, the figure is not strictly illustrating mass loss from climate forcing). Some indication of the relative sizes of these processes needs to be given. [Government of United States of America]	Taken into acount: text has been revised accordingly
4-955	4	22	33	22	33	Query whether the terminology here (and following) should be about changes in "specific mass rate" rather than "specific mass change rates" since the per year units appear to be re annual specific mass balance, not the rate of change of the specific balance. [Government of Australia]	Taken into acount: terminology has been changed to mean mass balance rates
4-956	4	22	38	22	38	FIGURE 4.11: The labelling of each plot with a conversion factor between 1000 kg/m^2 (which should be kg/m^2/yr) to mm of SLE seems a very obscure coding of the area. The fact that the 1000 kg/m^2 = ... Is missing off all but the first subplot makes it even more obscure. [Government of Australia]	Taken into account: Conversions from specific mass budget in kg m–2 yr–1 to mm SLE are now given for each region
4-957	4	22	39	22	55	The sources should be on alphabetical order in the Caption: a) Marzeion, submitted, b) [...], y) Peltier (2009). The present, seemingly random, order makes it hard to find the references corresponding to the presented data in the Figure 4.11 [Paul Leclercq, Netherlands]	Taken into account: the figure has been revised.
4-958	4	22	41	22	41	The reference to Magnusson et al. 2005 for glacier mass change rates in Iceland looks odd. More appropriate and valuable for the outcome of the IPCC report would be to cite the following: a. Björnsson and Pálsson, 2008. Icelandic glaciers. Jökull, 58, 365-386. b. Jóhannesson, Tómas, Helgi Björnsson, Eyjólfur Guðmundsson, Sverrir Guðmundsson, Finnur Pálsson, Oddur Sigurðsson, Thorsteinn Thorsteinsson and Etienne Berthier.2012. Ice-volume changes, bias-estimation of mass-balance measurments and changes in subglacial lakes derived by LiDAR-mapping of the surface of Icelandic glaciers. Annals of Glaciology, 63A, in press. c. Björnsson, H., F. Pálsson, S. Guðmundsson, E. Guðmundsson, G. Adalgeirsdóttir, T. Jóhannesson, O. Sigurðsson, Th. Thorsteinsson and E. Berthier. Contribution of Icelandic ice caps to sea level rise. Trends and Variability since the Little Ice Age Submitted. This work was presented at AGU Fall meeting 20122 and the IGS 2012 meeting in Faribanks, and is now in press in GRL. This is the most up-to-date and comphrehensive information about mass changes of glaciers in	Taken into account: references c has been included

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						Iceland and it content would augment to the quality of the report [Government of Iceland]	
4-959	4	22	49			Above in the text it is stated that the 95% confidence enveloppe are given. Discrepancy. [Etienne BERTHIER, France]	Noted: 5 - 95 = 90 but statement has been clarified to avoid misunderstanding
4-960	4	22	58	23	1	Values in Table 4.4 do not seem to be quite consistent with the conversion factors between specific mass change rates and SLE in Figure 4.11. [Government of Australia]	Noted: Values in Table 4.4. have been recalculated
4-961	4	22	58	23	1	Table 4.4: Total net balances of -340kg m-2a-1 and -251 Gt a-1 are smaller (in absolute values) than the estimates based on glaciological mass balance measurements on more than 100 available glaciers for the corresponding period. [Atsumu Ohmura, Switzerland]	Noted: Table 4.4. is from one study only. Clarification is provided
4-962	4	22	58	23	1	For supporting Tables 4.4 and 4.5, I attach my own publication of 2011. [Atsumu Ohmura, Switzerland]	Rejected: Ohmura 2011 is based on old and incomplete WGI data and, thus, outdated compared to RGI based analyses
4-963	4	22	58	23	1	Table 4.4, Region 10 (North Asia), relative uncertainties in specific mass balance (-630 +-2200 kg m-2 yr-1) and mass change (2 +-1 Gt yr-1) are not consistent [Helmut Rott, Austria]	Taken into account: Table 4.4. has been revised
4-964	4	22	58	23	1	Table 4.4: Total mass depletion -251 +- 28 Gt/yr : the uncertainty +- 28 Gt/yr is inconsistent with number in executive summary (page 4.-4, line 6): -251 +- 65 Gt/yr [Helmut Rott, Austria]	Taken into account: consitency has been provided
4-965	4	22	58	23	1	Table 4.4 has a Total kg/sq m/yr that does not make any sense. Is this the spec mass change averaged over all the glaciers? [Robert Thomas, United States of America]	Noted: it is the mean area specific mass balance over the entire glacier area. Clarification was provided
4-966	4	22	58	23	1	Table 4.4, second column: Change "Greenland Periphery" to "Greenland". [Jacob Clement Yde, Norway]	Rejected: region names are according to the RGI and will not be changed here
4-967	4	22	58			Table 4.4. In the last line with totals the sum of the specific change rates is given in a different unit (Gtyr-1) than the rest of the values in the column. If this it the intention it should be included in the table text or commented below the table. [European Union]	Rejected: the numbers are not giving the sum but the overall mean over the entire global glacier area.
4-968	4	22		23		The leftmost column "Nr." in Table 4.4 should be "No." since this column means the number. [Government of Japan]	Editorial
4-969	4	22				Section 4.3.3.3: Add somewhere the thought from comment chapter4-page20-line6 [Andreas Kääb, Norway]	Taken into account: discussion about discrepancies between methods has been revised
4-970	4	22				figure 4.11 a key figure but very difficult to interpret. Would be good to have a summary of methods and legends as a separate panel. This would avoid having a legend in each panel. Also map could be moved out of current poistion to separate line (at bottom) and made bigger. legend should summarise methodology as well as reference. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: fig 4.11 has been modified for better reading
4-971	4	22				figure 4.11 wouldn't Gt/yr be a more meaninful quantity to plot rather than the rate per area? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: fig 4.11 has been modified for better reading but kg m^-2 yr^-1 are kept for consistency and conversion in to respective mm SLE was now provided for each panel separately
4-972	4	23	3	23	3	Comment text: Replace line with "This compilation does not allow assessment of the skill and uncertainty of each result." [Peter Barrett, New Zealand]	Accepted
4-973	4	23	3	23	24	Unsure what is meant by the regional panel. While reconizing the difficulty of efficiently conveying this amount of material, as written it is very difficult to follow. [Government of United States of America]	Taken into account: fig 4.11 and section text have been revised
4-974	4	23	3			First sentence is worded awkwardly. [Dorothy Hall, United States of America]	Taken into account: section text has been revised
4-975	4	23	3			this pragrph is far too dense and contains ahuge amount of information. I found it very hard to see the overall picture. Is there a better way to present this information, perhaps in a table? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: fig 4.11 and section text have been revised

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4-976	4	23	6			The averaging applied in the mentionned papers provides results which are not significantly different from a straightforward averaging of available measurement series (cf. Zemp, M., Hoelzle, M. and Haeberli, W. (2009): Six decades of glacier mass balance observations – a review of the worldwide monitoring network. Annals of Glaciology, 50 (50), 101-111.) This reference should be added in any case, because it documents the primary data source. [Wilfried Haeberli, Switzerland]	Rejected: the results in this paper are not based on the globally complete RGI inventory as are the other papers used and cited here that use directly measured mass balances. Reference to the primary data sources is given in the cited studies.
4-977	4	23	8	23	8	Related to my comment on p20/l1, I wouldn't actually put Alaska into the category of "highest density and time resolution". There are some very good remote sensing resources available there (e.g. TerraSAR-X, repeat mapping) but these are conducted in a patchy and unsystematic way. Again, in terms of broad and consistent regional coverage and especially ground-based measurements, Alaska is in some ways rather a black hole. [W. Tad Pfeffer, United States of America]	Accepted: Alaska has been removed
4-978	4	23	13			Kääb et al. 2012, Nature, 488, 495-498 in addition/instead of Yao. Kääb is more comparable to GRACE (spatially complete) [Andreas Kääb, Norway]	Accepted: reference added
4-979	4	23	13			The spelling of "Tibentean Plateau" should be settled into one standard spelling at least in one chapter. Ideally, the same spelling should be used in AR5. It reappears on P 4-43, Line 34 and 45 as "Qinghai-Tibetan Plateau", and on P 4-44, Line 12 as "Qinghai-Xizang (Tibetan)". Probably the last one is most authentic. [Atsumu Ohmura, Switzerland]	Taken into account: TSU suggest to use "Tibetan Plateau" which has been adopted throughout the section
4-980	4	23	16	23	16	replace "(Hock et al," as "(" [Yongjian Ding, China]	Editorial
4-981	4	23	17	23	17	Give latitude range for Southern Andes [Jefferson Cardia Simões, Brazil]	Noted: the regions are shown in Figure 4.8
4-982	4	23	21			Mölg (general: check umlauts in the chapter) [Andreas Kääb, Norway]	Editorial
4-983	4	23	22			The results from Marzeion … [Andreas Kääb, Norway]	Accepted
4-984	4	23	26	23	26	This statement is not supported by the observation that one or more of the 90-percent confidence intervals in each graph envelops zero mass change for the plotted timespan. [Government of United States of America]	Taken into account: text has been revised
4-985	4	23	26	23	41	The study by Glasser et al. putting the recent mass loss of the Patagonian Icefield in a longer term context probably deserve to be mentioned here Glasser N. F., Harrison S., Jansson K. N., Anderson K., and Cowley A. Global sea-level contribution from the Patagonian Icefields since the Little Ice Age maximum, Nature Geoscience, 4, 303-307, 2011 [Etienne BERTHIER, France]	Rejected: no regional long term changes are discussed in this section because of lack of sufficient infoormation.
4-986	4	23	26	23	41	Study of Kunz et al gives first multi-decadal volume change of Antarctic Peninsula glacier sub-sections (including fronts) showing only limited lowering at the front. Nield et al reconstruct ice increase due to increased accumulation since 1850s. Kunz et al suggest this increase may offset present surface lowering. Kunz, M., M.A. King, J.P. Mills, P.E. Miller, A.J. Fox, D.G. Vaughan and S.H. Marsh 2012. Multi-decadal glacier surface lowering in the Antarctic Peninsula. Geophysical Research Letters, 39: L19502 doi:10.1029/2012GL052823. Nield, G.A., P.L. Whitehouse, M.A. King, P.J. Clarke and M.J. Bentley 2012. Increased ice loading in the Antarctic Peninsula since the 1850s and its effect on Glacial Isostatic Adjustment. Geophysical Research Letters, 39: L17504 doi:10.1029/2012GL052559. [Matt King, Australia]	Rejected: the Antarctic Peninsula is considered part of the ice sheet in Ch 4 and is not discussed in the glacier section.
4-987	4	23	28	23	28	Which years are covered by the "last pentad"? [Jacob Clement Yde, Norway]	Noted: section text has been changed
4-988	4	23	30	23	34	The subject of this sentence appears to be "specific mass change rates" - suggest that it is rewritten to make this clear - e.g. "... multi-method values for specific mass change rates ... " before getting into the various types of variations observed. [Government of Australia]	Taken into account: paragraph has been revised
4-989	4	23	30			This sentence is difficult to read. Split up? [Andreas Kääb, Norway]	Taken into account: text has been revised
4-990	4	23	31			more negative what? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been revised
4-991	4	23	32	23	33	Move the opening bracket to Fischer et al. (2012). Similarly, Sharp et al. (2011). [Atsumu Ohmura,	Editorial

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						Switzerland]	
4-992	4	23	33	23	34	replace "(Fisher et al.,2012) and (Sharp et al.,2011)" with "Fisher et al.,(2012) and Sharp et al.,(2011)" [Yongjian Ding, China]	Editorial
4-993	4	23	36	23	36	Should mention the recent paper by Gardelle et al., 2012, in the parenthesis before the reference of Kaab et al., in press. Here is the reference [Gardelle, J., E. Berthier, Y. Arnaud. 2012. Slight mass gain of Karakoram glaciers in the early twenty-first century. Nature Geoscience, doi: 10.1038/NGEO1450] [Antoine RABATEL, France]	Accepted: reference added
4-994	4	23	36			Confirmation of the Karakoram Anomaly with slight mass gain of glaciers was shown first by Gardelle et al. 2012 (Gardelle J., Berthier E., and Arnaud Y. Slight mass gain of Karakorum glaciers in the early 21st century, Nature Geoscience, 5, 322-325, 10.1038/ngeo1450, 2012.). Pioneer work by Hewitt (2005) could also be cited (Hewitt K. The Karakoram anomaly? Glacier expansion and the 'elevation effect,' Karakoram Himalaya, Mountain Research and Development, 25, 332-340, 2005.) [Etienne BERTHIER, France]	Accepted: Gardelle et al. Has been added. The Hewitt paper had been assessed in AR4 but would not fit into the revised regional discussion.
4-995	4	23	36			Kääb et al. 2012, Nature, 488, 495-498 [Andreas Kääb, Norway]	Noted: references has been updated
4-996	4	23	36			Add the following reference: Gardelle, J., Berthier, E., & Arnaud, Y. (2012). Slight mass gain of Karakoram glaciers in the early twenty-first century. Nature Geoscience, 5(5), 1–4. doi:10.1038/ngeo1450 [Michael Zemp, Switzerland]	Accepted: reference added
4-997	4	23	37	23	37	I agree that the mass loss in NW Himalaya is moderate, but in the nineties, in Northern India, the glaciers even experienced mass gain, as demonstrated by Vincent et al. (The Cryosphere Discuss., 6, 3733-3755, 2012). This study, based on in-situ and geodetic measurements covering more than two decades, is unique in the Karakoram-Himalayan region and is thus worth mentionning here. Indeed, it conflicts with recent glacier compilations (Cogley, 2009; 2011), based on questionable data, and providing erroneous results, saying that the glaciers in this area are declining fast in the 90ies. This study brings clear evidence that the glaciers of this region have been advancing or were stable in this region, with is important to mention in the IPCC report. The Figure 4.11 should also be up-dated accordingly [Patrick WAGNON, Nepal]	Noted: the paper mentioned deals with one single glacier and with a small sub-region only and could not be assessed in the Ch4 regional structure. The discussion about heterogeneous mass balance patterns over the Himalaya regions has been revied.
4-998	4	23	37	23	41	There contributions to SLR should be moved to the next section (4.3.4). [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: discussion has been revised
4-999	4	23	38			The direct conversion of ice volumes to sea level equivalent is incorrect as it ignores the obvious fact that considerable parts of large glaciers can be below sea level or below the level of future lakes potentially forming in overdeepened parts of existing glacier beds. This effect is small (most likely a few centimerters sea level equivalent) but systematic and must be correctly mentionned (cf. Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion) [Wilfried Haeberli, Switzerland]	Rejected: ice mass loss can be given in m SLE without an extended discussion whether the water will finally go to the sea or not. The discussion of land storage and Sea Level Rise contributions is made in Ch 13
4-1000	4	23	38			Why parantheses around units? [Andreas Kääb, Norway]	Editorial
4-1001	4	23	40			What does ‘correlate’ mean here? Statistical meaning? Unclear. [Etienne BERTHIER, France]	Taken into account: paragraph has been revised
4-1002	4	23	42			Here at least a modest paragraph should be prepared to connect the net balance variation to recent climate change. To do this, it is necessary to evaluate at least winter Bw and summer Bs balances. I have not found any dedicated works that have tried to connect the mass balance variations to climate change, except my own work. I paste the most important figure for this analysis below: Figure: Area-weighted global mean annual mass balance, based on 35 glaciers in 11 regions. Blue, brown and red lines indicate winter, annual and summer balances, respectively. Broken lines indicate 11 year running means in each category. Further, black lines in the middle indicating the mean annual net balance are those for 137 glaciers in 17 regions, for comparison. The closeness between the brown and black lines supports the global significance of the 35 glaciers from 11 regions, on which winter and summer balances are observed.	Rejected: the link to climate is not the subject of this chapter.

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						These 11 regions are Arctic Canada, North American Cordillera, Alaska, Svalbard, Scandinavia, Alps, Pamir, Caucasus, Kamchatka, Altaishan, Tienshan/ Dzungaria. Unit in mm w.e. a-1, updated after Ohmura (2011) The figure presents the global mean area-weighted mass balance of glaciers, based on 35 glaciers with long-term winter and summer balance observations, from 11 glacierized regions. For comparison the annual net balance of 145 glaciers has been added, which includes those glaciers with only annual balance measurements. The most important features of the net balance of the last 50 years are that (1) the annual net balance was mostly negative (except for 1963/64 and 1972/73) with mean balance of -253 mm w.e. a-1, (2) the negative annual balance is accelerating at a rate of -8.3 mm w.e. a-2, (3) this trend is to a great extent determined by the summer balance, which averages at -1174 mm w.e. a-1 with acceleration of -10.3 mm w.e. a-2, (4) there was a slight increase of winter balance whose mean was 917 mm w.e. a-1 with acceleration of 3.3 mm w.e. a-2. Looking at these outcomes in more detail, the acceleration of summer balance is 10% larger than the melt acceleration of -9.2 mm w.e. a-2, expected from the temperature-melt equivalent (1K/400 mm) (Ohmura et al., 1996), calculated based on the CRUTEM3 Northern Hemisphere Land surface air temperature (0.023 Ka-1). It is possible that the global increasing trend of solar radiation during this period could fill in this gap (Ohmura, 2009). Another possibility is the stronger warming trend at high altitudes as reported by Diaz and Bradley (1997) and Ohmura (2011). The winter balance in general is increasing for the examined period of 50 years, although the rate of acceleration is modest at 3.3 mm w.e. a-2. The correlation between air temperature and winter balance has been observed in much longer mass balance series, such as Claridenfirn in Switzerland, Storglaciären in Sweden and Storbreen in Norway. This tendency, however is disrupted for the last 20 years starting in early 1990s, as a result of loss of solid precipitation in favour of rain, as a result of temperature increase (Ohmura, 2011). The mean mass balances of the 5 years for the period (2004/05 – 2009/10) after being reported in the last IPCC AR4 are 902, -1,406 and -489, for winter, summer and annual net balances, respectively. The annual net balances of this period rank as the largest (in absolute values) loss among all pentade balances since 1960/61 as presented in following Table. This largest loss is also accompanied by the largest negative (in absolute value) summer balance. Bw Bs Bn Sea-level equivalent Pentade mm/a mm/a mm/a mm/5a 1960/61-1964/65 704 -848 -104 0.8 1965/66-1969/70 864 -1068 -192 1.5 1970/71-1974/75 879 -1048 -132 1.0 1975/76-1979/80 953 -1166 -240 1.8 1980/81-1984/85 983 -1144 -168 1.3 1985/86-1989/90 927 -1260 -295 2.2 1990/91-1994/95 893 -1353 -488 3.7 1995/96-1999/00 1009 -1374 -467 3.5 2000/01-2004/05 867 -1292 -429 3.3 2005/06-2009/10 918 -1396 -478 3.6 Total 22.7 based on based on based on 35 glaciers 35 glaciers 145 glaciers Table: Area-weighted global mean winter (Bw), summer (Bs) and annual (Bn) mass balances of 35 glaciers from 11 regions, arranged for pentade means. The last column is the pentade means of the annual balance including the glaciers with only annual balance observations. Unit is mm w.e. a-1. The range of uncertainty for Bw and Bs is estimated at 24 mm a-1, and that for Bn is 34 mm a-1. References:	

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						Diaz, H.F. and Bradley, R.S., 1997: Temperature variations during the last century at high elevation sites. Clim Chang 36:253–279 Ohmura, A., Wild, M, and Bengtsson, L., 1996: A possible change in mass balance of Greenland and Antarctic ice sheets in the coming century. J.Climate, 9, 2124-2135. Ohmura, A., 2009: Observed decadal variations in surface solar radiation and their causes. J. Geophys. Res. 114 (D00D13); doi: 10.1029/2008JD011290. Ohmura, A., 2011: Observed mass balance of mountain glaciers and Greenland ice sheet in the 20th century and the present trends. Surv. Geophys., DOI 10.1007/s10712-011-9124-4. Ohmura, A., 2012: Enhanced temperature variability in high-altitude climate change. Theor. Appl. Climatol., DOI 10.1007/s00704-012-0687-x. [Atsumu Ohmura, Switzerland]	
4-1003	4	23	43	23	43	Azam et al is now published : the exact reference is : 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., 58 (208), 315-324, doi:10.3189/2012JoG11J123, 2012. [Patrick WAGNON, Nepal]	Noted: it was commented to be too local by other reviewrs and we may remove the reference
4-1004	4	23	43	23	43	Change the author name "Kaab" to "Kääb". It is misspelled here and elsewhere in the report. [Jacob Clement Yde, Norway]	Editorial
4-1005	4	23	43	23	43	Change "2011" to "2012". [Jacob Clement Yde, Norway]	Editorial
4-1006	4	23	43	24	2	"the world's glaciers are currently strongly out of balance with the present climate and ths committed to lose considerable mass in the near future, even without further incresing temperature." Are the two, location-specific results sufficient to support this conclusion? Please explain the foundation/basis/refrences for this conclusion. [Government of United States of America]	Rejected: the Bahr paper gives a global perspective and the Azam one supporst this
4-1007	4	23	43			Heid and Kääb - please note that also in the references Kaab needs to be replaced by Kääb. [European Union]	Editorial
4-1008	4	23	43			not ceratin that this paragraph adds much and seems to rely on very local information. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Rejected: the Bahr paper gives a global perspective
4-1009	4	23	46	24	1	Which high elevation site in the European Alps? It would be good to be specific here for the reader's benefit. [European Union]	Taken into account: the sites have been named
4-1010	4	23	46	24	2	Change the first part of the sentence in the following way: "Increasing ice temperatures recorded on high elevation sites in the tropical Andes (Gilbert et al., 2010) and the European Alps ....." Here the reference [Gilbert, A., P. Wagnon, C. Vincent, P. Ginot, M. Funk. 2010. Atmospheric warming at a high-elevation tropical site revealed by englacial temperatures at Illimani, Bolivia (6340 m a.s.l., 16°S, 67°W). Journal of Geophysical Research, Vol, 115, D10109, doi:10.1029/2009JD012961.] [Antoine RABATEL, France]	Accepted: reference has been added and text has been changed accordingly
4-1011	4	23				Table 4.4: Are the +-0 really correct? In case to error is estimated +-? would be a better choice. [European Union]	Rejected: the numbers shown in Table 4.4 are taken from a published paper
4-1012	4	23				Show also curves of in-situ SMBs ('WGMS' SMB figure) and shortly mention what has to be observed with in-situ measurements compared to others? I think it would be important to show also this measurement. Otherwise, others pick it up and question why IPCC doesn't show it. [Andreas Kääb, Norway]	Rejected: all available in situ measurements are a basis of the regional and global estimates shown here in order to provide regional and global information. They cannot be shown separately.
4-1013	4	23				Discuss somewhere shortly the difference/progress from AR4 to AR5 for glaciers? [Andreas Kääb, Norway]	Taken into account: Text has been revised
4-1014	4	23				i found the balance of this section strange. There are two issues. One, splitting observation into methods (4.3.2) and results (4.3.3) leads to duplication and some confusion because of differences in terminology. Two, a lot of the section is spent discussing methods for getting area and length change but then the key figure (4.11) and table (4.4) suddenly appear; it would be more efficient to focus throughout on information leading up to the derivation of these elements. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: respective paragraphs have been revised for more clarity

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4-1015	4	24	1	24	1	should this read "elevation site in the European Alps "? [Government of Australia]	Noted: sites have been specified and text has been revised accordingly
4-1016	4	24	1			Both sites are in the Alps but Vincent et al. measured temperature in the Mt Blanc area whereas Hoelzle in the Monta Rosa. So this is not a single high elevation site, but two sites in the same mountain range. The likely warming at the summit of Illimani at 6340 m asl, inferred from the temperature vertical profile, could also be cited here to extend spatially the relevance of this important observation englacial warming). See Gilbert, A., P. Wagnon, C. Vincent, P. Ginot, and M. Funk (2010), Atmospheric warming at a high - elevation tropical site revealed by englacial temperatures at Illimani, Bolivia (6340 m above sea level, 16°S, 67°W), J. Geophys. Res., 115, D10109, doi:10.1029/2009JD012961. [Etienne BERTHIER, France]	Accepted: sites have been specified
4-1017	4	24	4	24	4	Better: Glacier contribution to sea level, or 'Global scale mass changes' [Regine Hock, United States of America]	Taken into account: it now reads: Global Scale Glacier Mass Changes - Contribution to Ssea Level. For more clarity also Subtitle 4.3.3 has been changed to Regional Glacier Volume and Mass Changes
4-1018	4	24	4	24	6	Must change this wording: “The glacier’s contribution to sea level…” First of all it is in the singular, i.e., "glacier’s"; secondly, it is otherwise very confusing even though “glacier” was defined earlier. The sentence could be changed to say something like “Contribution of the Earth’s glaciers to sea level…” [Dorothy Hall, United States of America]	Taken into account: Subtitle has been changed (see previous comment). Yet, we do not see a reason for specifying glaciers here.
4-1019	4	24	6	24	11	Again there is confusion between the "four recent studies" and more recent apparently unsourced data. e.g. "Cogley (2009b) compiled 4146 (updated to 4817) annually directly measured ..." and so on. Is the intended meaning that each study has been updated (by its authors?) but that these updates are not submitted for publication? Or did Chapter Authors perform this "updating"? [Government of Australia]	Taken into account: Text has been revised
4-1020	4	24	6			no i am lost. What is the relation between this section and 4.3.3? Haven't we just derived these time series? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Titles of subsections 4.3.3 and 4.3.4 have been changed for making a clear distinction between regional and global views respectively.
4-1021	4	24	6			how does this section (4.3.4) relate to the proceeding discussion? Need to have a sense of how this relates to the other work. Is it an alternative method, an extension of the previos methods? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Titles of subsections 4.3.3 and 4.3.4 have been changed for making a clear distinction between regional and global views respectively.
4-1022	4	24	6			four recent studies but i can only see three discussed here [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Text has been revised
4-1023	4	24	8			the updated numbres in brackets are odd. Need to refer to the numbers in the publication. Either the numbers in the publication have been updated or not, if the latter then they can not be used. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Rejected: numers can be updated from published papers if the method is published. Text has been revised for more clarity
4-1024	4	24	11			by adding ... This sentence would read more easily if it was reversed so that the meat (ie calving numbers) comes first. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted: Sentence has been revised
4-1025	4	24	11			Replace "WGMS (2009)" by "WGMS (2012, and earlier issues)" since this is the latest volume published including the data of 2010 as used in AR5. Reference: WGMS (2012): Fluctuations of Glaciers 2005-2010 (Vol. X): Zemp, M., Frey, H., Gärtner-Roer, I., Nussbaumer, S.U., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 336 pp. Publication based on database version: doi:10.5904/wgms-fog-2012-11 [Michael Zemp, Switzerland]	Accepted: reference changed
4-1026	4	24	14	24	14	6 change to 6% [Olaf Eisen, Germany]	Editorial
4-1027	4	24	14	24	20	Sentence is unclear and 'possibly underestimated' is speculative. That is the proportion of calving glaciers ?	Taken into account: has been revised and clarified

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						[Regine Hock, United States of America]	
4-1028	4	24	16			earlier estimtaes ... If they are not shown here why mention them? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: Text has been revised
4-1029	4	24	17			i think this assessment is too detailed and does not give exactly what the method used was. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: paragraph has been revised
4-1030	4	24	19	24	28	Should this paragraph refer to a figure? It is written as if describing a figure ("the first is still shown back to 1800"), but no figure is shown. [Government of United States of America]	Taken into account: paragraph has been revised
4-1031	4	24	19	24	28	There is a flaw in the LeClercq paper that should be addressed: In their analysis, the make a correction to the Bahr scaling theory in an effort to extend the power law scaling to populations of glaciers as opposed to a single glacier (their equations 2-6). However, the original Bahr theory applies to populations of glaciers, not single glaciers, so this correction is producing errors. It's not immediately clear what the size of that error is, but this should be considered at some level. (It should be evident that the original theory applies to characteristic values, not specific inviduals, given that it's a dimensional analysis problem, but see Bahr entry on power law scaling in the Springer-Verlag Encyclopedia of Snow and Ice (2010) for further discussion. [W. Tad Pfeffer, United States of America]	Noted: there is no quatified estimate of the effect of the flaw which could be cited.
4-1032	4	24	19	24	39	These two paragraphs are way two long.They almost have an 'abstract' style. I suggest to shorten these two paragraphs considerably and reduce the text to the most important points. [Nadine Salzmann, Switzerland]	Taken into account: Text has been revised and shortened
4-1033	4	24	19			again i do not see how updated can be used here - what are the numbers in the paper? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Rejected: nuemrical updates are possible if it strictly follows peer reviewed and published methods. Yet, text has been revised for more clarity
4-1034	4	24	19			Write "…use length variations from 382 glaciers worldwide (updated from 349) extending from the data collection of WGMS (2012, and earlier volumes) to estimate…" Reference: WGMS (2012): Fluctuations of Glaciers 2005-2010 (Vol. X): Zemp, M., Frey, H., Gärtner-Roer, I., Nussbaumer, S.U., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 336 pp. Publication based on database version: doi:10.5904/wgms-fog-2012-11 [Michael Zemp, Switzerland]	Rejected: the reference to WGMS is already given in the cited literature
4-1035	4	24	21	24	21	What is "reasonable confidence"? Please quantify. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: confidence language has been revised
4-1036	4	24	22	24	22	it is the arithmetic mean and area-weighted mean of regional averages, not of the individual glacier records [Paul Leclercq, Netherlands]	Taken into account: has been corrected
4-1037	4	24	22	24	25	To my feeling the uncertainty in the results of Leclercq et al 2011 is overstated here. These results have a 95% confidence interval from 1800 onwards. I do not see any reason to state that the confidence is reasonable from 1900 onwards only. The area weighted result has limitations in the 19th century, but Leclercq et al. 2011 argue the arithmetic mean of regions can be used for their estimate of the glacier contribution to sea-level change. [Paul Leclercq, Netherlands]	Taken into account: has been revised and clarified
4-1038	4	24	24			Yet … . Re-phrase sentence. [Andreas Kääb, Norway]	Taken into account: paragraph has been rephrased
4-1039	4	24	25			Considering the problems of the RGI, I have serious doubts about the main conclusion of a much higher glaciers contribution to sea level rise. If you extrapolate the errors in total glacier area detected in the Southern Andes to the World, I don't know if this higher sea level rise contribution is affected by the RGI uncertainties. On the other hand, it is difficult to understand this higher contribution, because the previous IPCC report used different periods and data sets, therefore, seems to me the numbers are not comparable and we can not clearly see why this huge increase is taking place. Maybe everything is due to an overestimation of the glacier area and a consequent estimated increase in melting. [Andrés Rivera, Chile]	Noted: the RGI has been updated and effects of uncertainties on glacier mass loss estimates is now discussed. A supplementary information on the effect of uncertainties in RGI is added to Ch 13.
4-1040	4	24	27			ablation at calving fronts: do you mean calving or melting at fronts? [Andreas Kääb, Norway]	Taken into account: statement has been removed

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4-1041	4	24	28			this assessment is aloso too detailed without saying what the basic method was [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: statement has been removed
4-1042	4	24	30	24	31	Suggest you make clearer that Marzeion et al's calculations of monthly mass changes are a modelling study, as distinct from a calculation based on glacier observations. [Government of Australia]	Taken into account: 'calculate' replaced by 'model'
4-1043	4	24	30	24	39	This paragraph is a summary of a paper submitted, but not yet accepted. The paragraph lacks clear conclusions also. The method to combine atmospheric information to mass balance, especially using CRU TS is highly questionable, as this data file is not homogenous in time. Unless this problem is addressed, and until the authors present numerically evaluated results, this sort of work should not be used in IPCC report. [Atsumu Ohmura, Switzerland]	Noted: using papers 'submitted' by July 31, 2012 follows the rules of IPCC AR5 WG1. The paper was meanwhile published.
4-1044	4	24	31	24	32	It is obscure what is meant by the model being "validated by cross-validation". Please explain this. [Government of Australia]	Taken into account: "validated by cross-validation" was replaced by "independently validated" since independency is the important aspect of the cross validation, and there is no room to go into the details.
4-1045	4	24	31		32	It remains unclear which kind of model is used and what the model actually does. An explanation is required what 'validated by cross- validation' means. [European Union]	Taken into account: "validated by cross-validation" was replaced by "independently validated" since independency is the important aspect of the cross validation, and there is no room to go into the details.
4-1046	4	24	31			what model - need to say a few words about how it works [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been modified for clarification
4-1047	4	24	33	24	36	This sentence concerning uncertainty estimates is very confusing. It could be more clearly worded by (a) saying that "Uncertainty estimates are derived by propagating through the entire model system the uncertainties of: the mass balance model itself (including uncertainties of the forcing), the surface area measurement, the volume-area scaling relationships, and the representation of dynamic glacier response to volume changes". This interpretation assumes that a list of contributions to uncertainties was intended. [Government of Australia]	Accepted: the reviewers suggestion was followed.
4-1048	4	24	35			Again: IPCC should be careful with the technique applied: the fancy term "volume-area scaling" hides the fact that misleading statistical self-correlations (area in volume with itself) are aplied for numerical calculations (cf. comment on page 18, lines 4-5). [Wilfried Haeberli, Switzerland]	Rejected: the method is clearly described in the published and peer reviewed literature and unless there is no peer reviewed discussion and community agreement on why it should/can not be used, it is taken as a valid method.
4-1049	4	24	37	24	39	This sentence is unclear. If model "reproduces geodetically measured volume changes reasonably well" this suggests it does not "only calculates surface mass balances". [Government of Australia]	Taken into account: text has been modified for clarification
4-1050	4	24	42			the text about what is and what is not in each numbers is confused and needs to be simplified [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: text has been modified for clarification
4-1051	4	24	45	24	45	Reference to figure 4.12 a, bottom should be to b, bottom [Government of Australia]	Editorial
4-1052	4	24	48	24	49	This assignment to Arctic glacier mass loss applies for Marzeion et al (2012) but not Leclercq et al (2011). The latter has no data at all from the Russian Arctic in this period. [Paul Leclercq, Netherlands]	Taken into account: text has been modified
4-1053	4	24	50	24	50	Delete "Box et al., 2009" from the references as this study concerns the Greenland Ice Sheet, not the peripheral glaciers. [Jacob Clement Yde, Norway]	Accepted: Box et al refer to exeptional high temperatures but not to high mass losses
4-1054	4	24	50	24	51	Add "Yde and Knudsen, 2007" to the references [Yde, J.C., and N.T. Knudsen, 2007: 20th-century glacier fluctuations on Disko Island (Qeqertarsuaq), Greenland. Ann. Glaciol., 46, 209-214]. Yde and Knudsen (2007) was the first study to report the higher loss rates in the early half of the 20th century in the Greenland periphery (see e.g. page 213, 1st column, lines 15-18). [Jacob Clement Yde, Norway]	Taken into account: reference has been added
4-1055	4	24	51	24	52	Apart from the fact that I have difficulties to inderstand this sentence, I also think that it is not very logical. If I understand correctly, the argument is that the length changes give an earlier peak is due to the time lag	Taken into account: text has been revised

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						between mass loss and length change. But this time lag would cause the peak in the length sinal to be later than the peak in the mass balance: exactly the opposite effect. [Paul Leclercq, Netherlands]	
4-1056	4	25	2	25	12	Figure 4.12: in the lower panels the time derivative of Leclercq et al. 2012 is shown with annual resolution. However, this reconstruction doesn't have an annual resolution. Maybe it would be a good idea to apply some degree of smoothing. [Paul Leclercq, Netherlands]	Accepted: the anual rates of Leclercq have been smoothed
4-1057	4	25	10			panels for clarity (remove 'better'?) [Andreas Kääb, Norway]	Taken into account: the caption text has been revised
4-1058	4	25	15	25	19	too many acronyms. Use 'Antarctica' and 'Greenland'. The caption makes clear which ones are included. [Regine Hock, United States of America]	Taken into account: Table has been revised
4-1059	4	25	15	25	27	Table 4.5: It is unclear from the text and the Table caption how these best estimates L+M and M+C, and their uncertainties are calculated. Secondly, I think that unavailable data (regional values of L and G) should be left out. [Paul Leclercq, Netherlands]	First: Taken into account: a better explanation was provided; second: Rejected: regional values from G are available and it has been made claer how Greenland values were separated from totla values in L.
4-1060	4	25	15	25	27	For supporting Tables 4.4 and 4.5, I attach my own publication of 2011. [Atsumu Ohmura, Switzerland]	Rejected: Ohmura 2011 is based on old and incomplete WGI data and, thus, outdated compared to RGI based analyses
4-1061	4	25	15	25	27	Table 4.5: The specified numbers and uncertainties for M + C cannot be directly deduced from the literature cited (Marzeion, 2012). E.g. it is not fully clear how the large error evident in cross validation of reconstructed mass balance(Table 2) is propagated to the total error estimate. Furthermore, what is the basis for reducing the uncertainty for PG-AA from 88% (85 +-75 Gt/yr) for the period 1993 - 2009 to 16 % (64 +- 10 Gt/yr) for the period 2005 - 2009, in particular taking into account that in the cited reference (M, 2012, Table 1) the number of Antarctic and Sub-Antarctic glaciers for cross-validation of modelled mass balance is zero and (Table 2) the percentages of glacier surface area affected by data gaps or model failure are 100 % for PG AA.. Another source of uncertainty is the fact that >95% of these glaciers are tidewater glaciers. [Helmut Rott, Austria]	Taken into account: uncertainty calculations and discussion have been improved and discussion about PG AA has been removed from Ch 4. The tide water aspect is mentioned in the section text.
4-1062	4	25	15	25	27	Table 4.5: For ref G the uncertainties differ from the numbers specified in Table 4.4 (251 +-65 vs. 251 +-28; 40 +-23 vs- 40 +-10; +1 +-30 vs. 1+-13.) [Helmut Rott, Austria]	Taken into account: The Gardner et al numbers have been revised
4-1063	4	25	15			Table 4.5 - SLE should probably be defined [Sharon Smith, Canada]	Taken into account: SLE has been clarified
4-1064	4	25	30	25	30	change "sea level contribution rates" to "contribution rates to sea level change" [Olaf Eisen, Germany]	Accepted: text changed
4-1065	4	25	30	25	30	Figure 4.12 shows glacier contribution to sea-level rise from Cogley (2009, updated) decreasing from about 1980 to the mid-1990's and contributions described by Marzeion et al. (submitted) show no discernible trend until about the mid 1990's. These observations do not support the conclusion that that the contributions increased starting about 1985. [Government of United States of America]	Taken into account: discussion of Fig 4.12 has been revised
4-1066	4	25	30	25	43	I had some difficulties to understand the goal/rational for this paragraph. To clarify. [Etienne BERTHIER, France]	Taken into account: the paragraph has been removed
4-1067	4	25	30	25	43	I mention this here because Columbia Glacier's calving out put is mentioned here; this point might be addressed elsewhere, though: there is essentially no discussion of the capacity of glaciers (as opposed to ice sheets) to have rapid dynamic response. Given that (according to Gardner et al, submitted) about 35% of global glacier area is drained through marine outlets, this seems like an oversight. [W. Tad Pfeffer, United States of America]	Taken into account: the paragraph including the discussion on calving details has been removed
4-1068	4	25	31	25	33	Insert ", supported by field observation analyses," after "Model studies". It is not only model studies that "indicate strongest mass losses ... in the Arctic, particularly the periphery of Greenland" based on air temperature measurements. [Jacob Clement Yde, Norway]	Taken into account: the paragraph has been removed
4-1069	4	25	31			"slight decrease in most recent years" Please quantify "slight decrease", and specify "recent years". [Government of United States of America]	Taken into account: the paragraph has been removed

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4-1070	4	25	31			quantify 'most recent' [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1071	4	25	33	25	33	"The largest uncertainty in the estimates...". Which estimates? What are the recent estimates and their sources and what are the earlier estimates and their sources? [Government of United States of America]	Taken into account: the paragraph has been removed
4-1072	4	25	35	25	43	needs rewriting since McNabb et al, will not be published in time. A study estimating calving and that is in press is for example: Osmanoglu, B., M. Braun, R. Hock, F. J. Navarro, 2012.Surface velocity and ice discharge of the ice cap on King George Island, Antarctica. Ann. Glaciol.. accepted. [Regine Hock, United States of America]	Taken into account: the paragraph including the discussion on calving details has been removed
4-1073	4	25	35			need to justify assertion that 'time series seeem to over ...' [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1074	4	25	35			appear to be changing topic 'Large glacier ..' need to signpost this more clearly. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1075	4	25	37			this looks to be a summary paragrph but now adding new,detailed information about calving. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1076	4	25	38	25	38	presumably missing "glaciers" after "Alaskan tidewater" [Government of Australia]	Taken into account: the paragraph has been removed
4-1077	4	25	38	25	38	add "glaciers" after tidewater [Matt King, Australia]	Taken into account: the paragraph has been removed
4-1078	4	25	38	25	38	Insert "glacier" after "tidewater". [Jacob Clement Yde, Norway]	Taken into account: the paragraph has been removed
4-1079	4	25	38			"(69% of their net mass loss)". I did not understand what was quantified here? The percentage of the mass loss due to increased calving? [Etienne BERTHIER, France]	Taken into account: the paragraph has been removed
4-1080	4	25	40			not full assessed yet - not yet fully assessed? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1081	4	25	42	25	23	Interesting statement but the paper by Loriaux and Casassa appears to be unavailable in the Journal of Glaciology. Complement (or replace if necessary) with Haeberli and Linsbauer (2012), who treat the same phenomenon but also the effects of ice below sea level: Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion [Wilfried Haeberli, Switzerland]	Noted: the paper is meanwhile published in Global and Planetary Change but the topic has been shifted to Ch 13.
4-1082	4	25	42			Ocean - ocean [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Taken into account: the paragraph has been removed
4-1083	4	25	43			i think some work is need on 4.3 to ensure a smoother, more logical flow of the assessment so that it is clear how the various techniques feed into the overall assessment. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted: the discussion of global estimate section has been revised for clarity.
4-1084	4	25	45			Box 4.1: The placement of this box in the Chapter should be reconsidered. It seems to us that this box would serve the most purpose coming at the beginning of section 4.5, where it would provide the context and importance of why we are interested in changes in snow cover. [Thomas Stocker/ WGI TSU, Switzerland]	Editorial - final formatting to be decided post-Final draft
4-1085	4	25	45			Box 4.1: The box currently lacks any reference to the scientific literature. Despite the general nature of the box, it must still be firmly grounded in the underlying literature. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: Key reference supporting the statements made are now included
4-1086	4	25	47	25	47	Box 4.1 title - Suggest that title be revised to "Interaction of snow with other cryospheric components" [Sharon Smith, Canada]	Editorial
4-1087	4	25	53			This is a very strong statement -- "…have doubled meltwater runoff since the 1980s." Yet there is no reference after this statement. A strong statement like this should have more than one reference to back it up. [Dorothy Hall, United States of America]	Taken into account: references have been added
4-1088	4	25				Table 4.5: Caption does not explain what gray colour means. [European Union]	Editorial
4-1089	4	25				Table4.5: The abbreviation "PG" is not spelled out anywhere in the Chapter 4, and its meaning is unclear. [Government of Japan]	Taken into account: abbrevations have been removed

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4-1090	4	26	1	26	1	add "," before "and has strong" [Yongjian Ding, China]	Editorial
4-1091	4	26	3	26	3	Note that it would be better to say that it is the ground thermal regime (and therefore the occurrence of frozen ground) that is altered through the insulating effect of snow. [Sharon Smith, Canada]	Noted
4-1092	4	26	7	26	12	It lacks the snow storage component (as third most important physical property of snow). The box has the title Interaction of Snow with the Cryosphere [Luzi Bernhard, Switzerland]	Noted
4-1093	4	26	9	26	9	Several new papers indicate that thermal conductivity is higher than previously estimated by about a factor of 2. The effect on total heat flux is not yet known. Calonne, N., F. Flin, S. Morin, B. Lesaffre, S. R. du Roscoat, and C. Geindreau (2011), Numerical and experimental investigations of the effective thermal conductivity of snow, Geophysical Research Letters, 38, L23501, doi:10.1029/2011GL049234. AND Riche, F., and M. Schneebeli (2012), Thermal conductivity of anisotropic snow measured by three independent methods, The Cryosphere Discussions, 6(3), 1839–1869, doi:10.5194/tcd-6-1839-2012. [Martin Schneebeli, Switzerland]	Noted
4-1094	4	26	12	26	12	remove , retain full stop. [Government of Kenya]	Editorial
4-1095	4	26	12	26	12	snow,. (typo) [Nadine Salzmann, Switzerland]	Editorial
4-1096	4	26	19	26	20	The snow protects the ground from "thawing" not "melting" (only the ice changes phase not the soil) [Sharon Smith, Canada]	Editorial
4-1097	4	26	29	26	35	I think that here it would be better to specify the snow depth values which could guarantee the decoupling of the soil temperature from the air temperature. For example Bertrand et al. (1994) found that a snow pack of 30 cm provided sufficient insulation to prevent root freezing and subsequent die- back in mature sugar maples (Bertrand, A., G. Robitaille, P. Nadeau, and R. Boutin. 1994. Effects of soil freezing and drought stress on abscisic acid content of sugar maple sap and leaves. Tree Physiol. 14:413–425). Moreover also the snow density could significantly influence its thermal insulation properties, as reported for example by 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. [Michele Freppaz, Italy]	Noted: We have explicitly mentioned the importance of snow density on its isolating properties.
4-1098	4	26	29	29	35	This paragraph is rather confusing and it isn't clear that all this is necessary. The important point is that the snow insulates the ground (permafrost) from changes in air temperature. It is also noted that in this entire section there are no references. A recent paper by Smith et al. (2012) includes an investigation of the role of snow cover and the seasonal partitioning of air temperature change. In this study, warming during the winter had a greater effect on ground temperatures at tundra sites with minimal snow cover --- lacking a buffer layer and a more direct connection between air and ground temperatures. Reference: Smith, S.L., Throop, J., and Lewkowicz, A.G. 2012. Recent changes in climate and permafrost temperatures at forested and polar desert sites in northern Canada. Canadian Journal of Earth Sciences, 49: 914-924. [Sharon Smith, Canada]	Text is revised and suggested reference included.
4-1099	4	26	33	26	34	a snow cover can increase the thickness of seasonal soil freeze? Reduce or increase? [Tao Che, China]	Accepted text changed
4-1100	4	26	34	26	34	It is not clear how the insulating effect of snow cover reduces "the thickness of seasonal soil freeze" by insulating the effect of warmer air masses. It could be expected that this situation would reduce melting of frozen soil. [Government of Australia]	Same as 4-1099
4-1101	4	26	34	26	35	snow depth should be included here. [Tao Che, China]	Unclear what the comment is getting at - reject.
4-1102	4	26	34	26	35	Snow depth should be included here. [Jing Ming, China]	Same as 4-1101
4-1103	4	26	38	26	41	In the first part of the sentence the authors referred to glaciers whle in the second part to generally frozen ground. I think that an early snow cover over glaciers can prevent cooling. In particular in the Alps it could stabilize the glacier surface ice temperature to -5°C (see for example Maggioni M., Freppaz M., Piccini P., 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: The reference was evaluated.

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4-1104	4	26	39	26	41	It is suggested the sentence be revised to: "....an early snow cover on the ground surface can reduce cooling and freezing of the active layer (seasonally thawed layer) in permafrost regions and potentially contribute to long-term warming and thawing of permafrost." [Sharon Smith, Canada]	Accepted
4-1105	4	26	40			probably no need for new paragraph here [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Unclear which text this refers to - therefore no response
4-1106	4	26	43			needs some punctuation [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Unclear which text this refers to - therefore no response
4-1107	4	27	45			probably should say that need to measure velocity and thickness in order to estimate perimeter fluxes. Best to use same terminology throught - here perimeter fluxes become ice discharge [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Modified text accordingly.
4-1108	4	26	47			This section largely biased to the description of mass balance, and little is mentioned about ice/ocean interactions, possible grounding-line retreat, glacier-bed motion, and interactions between fast-flowing glaciers and subglacial (active) lakes in Antarctica. Some relevant materials are found in Chapter 13 (sea-level rise) but they are more based on modeling. Observation-based knowledge should be mentioned in this section. [Kenichi Matsuoka, Norway]	Reject. Chapter is about observations of change - physical processes only discussed when relevant to explaining observed changes. Not a textbook.
4-1109	4	26	47			Geological controls of the ice sheets (topography, bed elevations below sea level, geology (sediment or hardbed), geothermal flux) is not mentoned at all in this chapter. In other words, we don't really know which area of the Antarctic ice sheet has higher potential of rapid changes than other regions. Such general picture of the ice sheets should be given somewhere in this chapter, I think that section 4.4.4 is most appropriate to show such issues that possibly provide large uncertainties in the future prediction of the ice sheets. [Kenichi Matsuoka, Norway]	As 4-1108.
4-1110	4	26	47			Levels of sections are more than necessary, except for Section 4.4.4. In most cases, 5th level of sections should be removed, which allows authors to tie different observations togethher and present more general understanding. [Kenichi Matsuoka, Norway]	Accept for all sections.
4-1111	4	26	51	26	52	The statement "Greenland and Antarctica are losing ice mass as the polar climate becomes warmer" is strong and needs a reference here for support. Given that this is a background section, I wonder if it should be stated like this here at all. In fact, this report is about assessing the change. Having this statement as background info provides some point of attack for criticism, as it is much stronger than the statement on p31, line 1 "very likely currently losing mass". [Olaf Eisen, Germany]	Accept. First sentence deleted citations added.
4-1112	4	26	51	36	25	Suggest re-organizing this section along the lines of Section 4.3. For example, an introductory paragraph immediately under the Section 4.4 header could explain the scope of the material to be presented, and the physical characteristics/processes that will be discussed. The treatment of confidence and uncertainty is uneven. [Government of United States of America]	Para 1) As 4-1111. Para 2) Accepted.
4-1113	4	26	51			bit odd to start with a summary assessment that has no supporting references BEFORE the assessment has been made - shouldn't this come after the literature has been assessed? As an introduction it fails because it does not introduce the subsequent material (ie what is to be considered) but pre-judges it. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	As 4-111
4-1114	4	26	53			"doubled meltwater runoff since 1980s": Please provide reference(s) and uncertainties. [Government of United States of America]	Accept - citations added. As 4-111
4-1115	4	26	54	26	2	Mention should be made here of the seminal work of Thoma et al.2008 (GRL) showing that the presence of warm water near the ice sheet margin is wind-driven. Otherwise the incorrect impression is given that the glacier changes may be due to a simple warming of the oceans. This is not the case. [Eric Steig, United States of America]	As 4-1111.
4-1116	4	26	56	26	56	There seems to be an error of logic here. Glacier acceleration and increased ice discharge surely require a change in forcing - not simply the presence of warm ocean water, but the presence of a greater heat supply	This paragraph has been rewritten.

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						than previously, either warmer ocean water or more vigorous circulations (or both). [Government of Australia]	
4-1117	4	26				Section 4.4 mostly omits temperature on/near ice surface. Temperature change is important to climate change assessment and its impact on ice sheets. [Government of United States of America]	Reject. Chapter is about cryospheric change. Atmospheric temperature covered elsewhere.
4-1118	4	26				Section 4.4 does not include much about snowfall, snow accumulation. Snow accumullation is an important component of ice sheet mass balance and should be addressed to understand ice sheet change. [Government of United States of America]	Reject, accummulation covered in e.g., 4.4.2.2.
4-1119	4	26				Section 4.4. does not include observation from in-situ stations in a systematic way. Station data can provide important parameters over long term for assessment of climate and ice sheet change. [Government of United States of America]	Noted: There are only a few long-term climate observations on ice sheets and they are integrated in the proess studies assessed in this chapter. Long-term accumualtion records have been used in this assessment.
4-1120	4	26				There is very little mention of surface temperature, either satellite or in-situ in this section on the ice sheets, though there has been a great deal of work done on this subject. Beginning in 1981 or 1982, surface temperature measurements of the Arctic as a whole have been made using AVHRR data by Wang and Key and by Comiso. [Dorothy Hall, United States of America]	As 4-1117.
1121	4	27	1	27	2	Add relevant references from Annals of Glaciology 2012, numb. 60, parts 1 and 2 [Jacob Clement Yde, Norway]	Papers checked through and reference to Steig et al., 2012 added.
4-1122	4	27	1			It is a bit "un-symetrical" to have only the last statement of the introductory paragraph with references. Maybe just delete the references here and move those two references further down the text? [Etienne BERTHIER, France]	Accept - citations added.
4-1123	4	27	4	27	4	The new paper by Shepherd et al (Science 2012) is used thoughout Section 4.4.2. Their analysis is certainly the right sort of thing to be doing - taking advantage of all sources of data, and combining them - but they appear to have combined measurements by simple averaging.This is ok if the differences arise from random error, but their Figure 3 suggests that there are significant offsets between the reconciled result, laser altimetry, and the input-out result. The Chap 4 authors might want to consider the implications. [W. Tad Pfeffer, United States of America]	Noted. We have only used the IMBIE results as a comparator and have not used them in averaging.
4-1124	4	27	10			Shorten this section (techniques) or convert this section to a FAQ box. One illustrative figure and shrot text should be adequate to introduce three diferent methods, and techniques are not the main focus of AR5. [Kenichi Matsuoka, Norway]	Rejected, techniques needed to highlight advances sinces since AR4 and discuss values presented.
4-1125	4	27	12	27	34	Some discussion of the coarse resolution of GRACE (quoted earlier as 300km in the section on glaciers) could be given here. Glaciers and Ice Sheets have been separated in this assessment, but GRACE measurements of mass changes near the coasts of Greenland and Antarctica are presumably representative of some combination of glacier and ice-sheet mass changes. [Adrian Simmons, United Kingdom]	Accept, resolution added.
4-1126	4	27	12			State the three techniques here. It would be helpful to have a table here similar to Table 4.3 that directly compares the strengths and weaknesses of each of the methods. [Government of United States of America]	Partially accept - names of three methods added to sentence on page 27 line 12. Table not added as this is unnecessary.
4-1127	4	27	33	27	34	thinning should be surface lowering. Thickening surface increase [Matt King, Australia]	Partially accepted, rephrased to: Surface elevation increase / decrease.
4-1128	4	27	36	27	36	4.4.2.1.1 Mass budget method. This section should make clear that while mass budgets can be applied to any region, the standard use regarding entire ice sheets and sea level rise involves using the perimeter fluxes at the grounding line - and grounding line migration may be as relevant as changing ice velocity, since it alters the domain of integration of net surface accumulation and the perimeter flux. [Government of Australia]	Reject. This is a very small effect that does not have a significant effect on the results. If we wish to discuss specific details, they are discussed in Rignot et al GRL 2011.
4-1129	4	27	36	27	59	Section 4.4.2.1.1 Mass Budget Method does not include any discussion of uncertainties due to retention of melt by refreezing in the percolation zone, or to the extent that it is, its tacitly assumed that RACMO has this under control. However, there is very good evidence (e.g. Harper et al, Nature 491, no. 7423: 240–243. doi:10.1038/nature11566, 2012, and Humphrey et al, Journal of Geophysical Research-Earth Surface, 117:	Reject. RACMO includes meltwater retention from meltware refreezing at depth.

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						F01010. DOI:10.1029/2011JF002083, 2012) that significant quantities of melt are channeled to depths of 5-10 m in the percolation zone of the Greenland Ice Sheet; this is far beyond the capacity of RACMO to handle with its shallow-acquifer runoff formulation. The potential for large overestimates of runoff is fairly large. [W. Tad Pfeffer, United States of America]	
4-1130	4	27	38	27	45	The statements on lines 38-40 and on line 45 are not really consistent, you admit the difficulty for calculating the difference between two large numbers requiring small percentage error, but on line 45 you admit incomplete ice thickness mapping. This gives the impression of very optimistic formulation. The red circle in Fig. 4.14c with a negative value seems to confirm the difficulty! [Heinz Blatter, Switzerland]	Move sentence from line 45 to previous paragraph. Note from ER: Incomplete thickness mapping does not imply no data on ice thickness (because it can be calculated from floation as well) but higher uncertainty. We have also to factor in the fact that the largest contributors to mass loss are at present well mapped in terms of ice thickness, so the missing areas do not have a critical impact on the results as it may seem. NOTE - I have done this move, but want Eric / Koni to make sure it was still needed.
4-1131	4	27	42	27	45	Include reference to Bindschadler et al., 2011 here, too, as their work contributes to reducing flux errors. [Olaf Eisen, Germany]	Reject. ASAID grounding line is not used in these assessments.
4-1132	4	27	47	27	58	The mixing of the description of the method and the results in here is somewhat confusing. Suggest moving the results into section 4.4.2.2 and making this paragraph comparable in scope to the two other methodology sections that follow (4.4.2.1.2 and 4.4.2.1.3). The consecutive discussions of surface mass balance (in Antarctica) and surface mass accumulation (in Greenland) leads the less than careful reader to equate the two. In fact, surface mass balance for Greenland is not discussed until section 4.4.2.2.2. [Government of United States of America]	Accepted: paragraph on mass balance has been deleted
4-1133	4	27	49	27	49	here and in many other places you use the unit Gt per year for the mass balance of the ice sheets. This is not a reader fiendly unit. Why not divide this number by the area of the ice sheet and give it in m water equivalent per year. [Heinz Blatter, Switzerland]	Reject. We use Gt throughout because it is a quantity of more direct use for the sea level chapter and the ocean chapter.
4-1134	4	27	49	27	49	SMB balance of Antarctica is given here as 2418 ± 181 GT/yr. This is probably the throughput flux? [W. Tad Pfeffer, United States of America]	Accepted: paragraph on mass balance has been deleted
4-1135	4	27	49			Why citing two papers for a single number? [Etienne BERTHIER, France]	Accept. But Paragraph deleted.
4-1136	4	27	50	27	58	I would change the order: First Greenland, second Antarctica (like above and in the figure chapter) [Luzi Bernhard, Switzerland]	Accept. But Paragraph deleted.
4-1137	4	27	51	27	58	The uncertainty of current mass budget estimates for Greenland (40 Gt/yr, which is as little as 23 kg m-2 yr-1) seems to neglect comparisons with in situ data which point out large disparities (line 56 on this page). Comparisons with observations should be considered in error accounting. [Helmut Rott, Austria]	Accept. But Paragraph deleted.
4-1138	4	27	51			clarify what is meant by sub-ice melt processes - under ice sheet or ice shelf? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. But Paragraph deleted.
4-1139	4	27	52	27	52	what do you men with "surface accumulation"? The positive numbers 100 to 500 Gt per year suggest a positive mass balance for the present Greenland ice sheet? On line 49, in the corresponding statement for Antarctica, you call this quantity "mass balance"!? [Heinz Blatter, Switzerland]	Accept. But Paragraph deleted.
4-1140	4	27	52	27	52	accumulation includes runoff here? [Christopher Little, United States of America]	Accept. But Paragraph deleted.
4-1141	4	27	53			Figure 4.13 does not present the numbers of Hanna et al., 2011 but those of Ettema et al., 2009. It is quite confusing that the text refers to different literature than the figures. Both references and their numbers should be presented both in text and figures. [European Union]	Accept. But Paragraph deleted.
4-1142	4	27	57	27	57	Include a sentence stating that in Antarctica calibration against in situ data is not possible because of insufficient observations, so the estimates of surface mass balance are based on climate models alone and must be considered more uncertain. [European Union]	Paragraph deleted.

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4-1143	4	27	57	27	58	The last sentence of this paragraph needs one or more solid references. [Dorothy Hall, United States of America]	Paragraph deleted.
4-1144	4	27	57	27	58	Do mass budget studies match within these uncertainties? [Christopher Little, United States of America]	Paragraph deleted.
4-1145	4	27	57			do not depend on field data' is an odd phrase; it makes use of observations sound like a bad thing! Also it ignores that fact that RACMO2 has been tuned to field observations. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Paragraph deleted.
4-1146	4	27	58			need to explain how the uncertainties have been calculated. This is important because it will determine how useful this technique is. At present these numbers are plucked out of the air. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Paragraph deleted.
4-1147	4	27				Figure 4:13 and 4.14 - This figure presents GRACE and ICESat "temporal pattern of ice loss for Greenland", but figures d and e do not even combine to give the mass budget method. The appropriate figure would not be one of ice velocities but of ice fluxes (i.e. Involves the thickness of the ice as well - or the depth-integrated horizontal ice velocity). Even then the pictures would only be useful at the peripheral boundary. The actual analogue figure would be dh/dt = the net accumulation minus the divergence of the horizontal ice flux field, although this is likely too noisy to be compared with the GRACE and altimetry spatial maps. [Government of Australia]	Accept. Change Figure caption to explain that we are showing only two components of the mass budget method, and that the third one, ice thickness - or bed topography in that case -, is shown in Figure 4.18.
4-1148	4	27				Figures 4.13 and 4.14 should be referred in the sections about mass balance in these ice sheets, not in the method section. There are so many levels of sections; three sections under 4.4.2.1 can be merged. [Kenichi Matsuoka, Norway]	Return to this comment. *** Second part agreed.
4-1149	4	28	1	28	1	Section 4.4.2.1.2 - As this Chapter cites Helsen, M. M., et al., 2008: Elevation changes in Antarctica mainly determined by accumulation variability. Science, 14, 320, 1626-1629. doi:10.1126/science.1153894, there should be reference to the major point of that paper - that accumulation variability is a major confounding problem in relating Antarctic elevation changes from SRALT or Laser altimetry to ice sheet mass balance. [Government of Australia]	Reject. IMBIE paper has superceeded this result.
4-1150	4	28	1	28	28	Section 4.4.2.1.2 could use some dicsussion of the uncertainty in altimetric observations from desnification of firn. [W. Tad Pfeffer, United States of America]	Accept: A sentense has been added to to give the uncertainty for snow densification.
4-1151	4	28	3			might also point out that budget method also requires assumptions about density etc and does not measure mass change but volume change [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	as 1150
4-1152	4	28	6			Flament and Rémy (2012) may also be cited here as a study using repeat track radar altimetry to study elevation change for the AIS. Flament T., and Rémy F. Dynamic thinning of Antarctic glaciers from along-track repeat radar altimetry, Journal of Glaciology, 58, 830–840, 10.3189/2012JoG11J118, 2012. [Etienne BERTHIER, France]	RESPONSE TO BE ADDED.
4-1153	4	28	8			Padman et al., 2012 is missing Line 25: south-eastern. [European Union]	Para 1) Now present. Para 2) Accepted.
4-1154	4	28	9	28	9	The term 'surface footprint' is not clearly explained in this context. [Government of United Kingdom of Great Britain & Northern Ireland]	Accept. Changed to "field-of-view", "~" added to 20km
4-1155	4	28	9			What are the "early SRALT sensors", are ERS, ENVISAT also included? Unclear to me. [Etienne BERTHIER, France]	Reject.
4-1156	4	28	14	28	16	Delete this final sentence which reads like an advertisement for upcoming data. If CryoSat-2 data is not assessed in the chapter, then there is no need to mention it. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Sentence deleted.
4-1157	4	28	14			results in very small ... Might want to say they are likely to be unrealistically small [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Text revised so no longer relevant
4-1158	4	28	18	28	20	If ICESat was launched in 2003, it was an advance of capability prior to AR4 not since AR4. The point might be that many more results from ICESat are available now for inclusion in AR5 than were available for AR4. If	Accept.

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						so, then this is what the sentence should say. [Adrian Simmons, United Kingdom]	
4-1159	4	28	18	28	28	In paragraph 3, does the following statement “ICESat derived surface elevation …….. Peninsula (Shuman et al., 2011).” lead into the next sentence? It needs to be more clear if the ICESat and ASTER DEMs were used with the laser surveys mentioned in the next sentence. If not, then this should be deleted. [Dorothy Hall, United States of America]	Additonal information was given in the text to explain the accuracy assessment for Greenland and the Antarctic Peninsula
4-1160	4	28	23	28	26	Add Berthier et al., GRL, 2012 result and then delete specif reference to ASTER (as SPOT also used) and just refer to DEMs derived from stereo-optical satellite data [Matt King, Australia]	Accept. As 1159.
4-1161	4	28	23			is scarcity the right word - do you mean sparse spatial coverage? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. As 1159.
4-1162	4	28	26			Shuman et al. (2011) also used SPOT5 (Satellite pour l’Observation de la Terre) DEMs together with ASTER DEMs. [Etienne BERTHIER, France]	Accept. As 1159.
4-1163	4	28	26			is there are similar number for antarctica? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. As 1159.
4-1164	4	28	30	28	54	This paragraph needs serious rewriting following the publication of the paper by King et al., Nature. 2012 [Etienne BERTHIER, France]	Accept. Paragraph was completed by references on new GIA models used to revise ice sheet loss numbers, especially Antarctica.
4-1165	4	28	30			Section on laser altimeters. It could be stated that accuracies of elevation changes of 10 cm to 15 cm makes it difficult to determine elevation changes in the central parts of Greenland and Antarctica because the changes expected here are a magnitude less than the accuracy and the observation period short. [European Union]	Accept. Text has been added to quote the reduction of uncertainy for longer time series.
4-1166	4	28	31	28	33	See comment 177. Perhaps the discussion could be given here, if it is considered necessary. [Adrian Simmons, United Kingdom]	Accept. Text re: spatial resolution of GRACE added to paragraph at line 31 onwards. (refers to 4-1125)
4-1167	4	28	14	28	16	Wingham et al. (2006b) is a reference that predates the launch of Cryosat-2 in April 2010, so it does not relate to the release of data. I suggest the reference be moved to earlier in the sentence, so it comes after the word "tool". [Adrian Simmons, United Kingdom]	Accept. Sentence deleted.
4-1168	4	28	40	28	48	I’m somewhat confused here. In paragraph 2, last sentence, it says that the GIA rate uncertainty does not affect the estimate of change in the rate of ice mass loss. Yet in paragraph 1 it is stated that some of the difference [in GRACE-derived estimates of ice loss estimates] is caused by “…contamination by other signals within the ice sheet (e.g., GIA)…” While this may not be contradictory, I think paragraphs 1 & 2 need to be re-written for clarity. [Dorothy Hall, United States of America]	Reject. The two statements are about rates of change and acceleration of change which are not the same, however text revisited to clarify.
4-1169	4	28	43	28	44	Include more recent references on GRACE errors rather than citing studies from 2006. [Olaf Eisen, Germany]	Accept. Delete oldest reference. Also add "around".
4-1170	4	28	43			King et al. [Nature, submitted] and Whitehouse et al. [2012] (both already in references) are relevant cites here. [Laurence Padman, United States of America]	Accept.
4-1171	4	28	45	28	45	One of the new generation of GIA models should be cited. The only one published I believe is Whitehouse et al 2012 [Matt King, Australia]	Accept.
4-1172	4	28	54			good succinct summary of methods [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Noted.
4-1173	4	29	5		15	Ewert et al., 2011 is missing. That publication estimates the mass loss from GRACE and ICESat and should potentially also be mentioned 4.4.2.1.2. This also implies to compare the estimates of Pritchard et al., 2009 with Ewert et al., 2011 with respect to surface elevation changes (Fig 4.13f) Ewert, H., et al., Volume and mass changes of the Greenland ice sheet inferred from ICESat and GRACE. J. Geodyn. (2011), doi:10.1016/j.jog.2011.06.003 [European Union]	Accept. Missing reference (not published by the time we completed this iteration of the assessment) is now included in text, table and overall assessment.
4-1174	4	29	8	29	8	”20111derived” should be "2011 derived". [Government of Japan]	Accept
4-1175	4	29	8	29	8	There is a typo in the indicated time window, 20111 should read 2011 [Ernst Schrama, Netherlands]	Accept
4-1176	4	29	10			Line 10 & 32: Rignot et al., 2011c is not included in the reference list [European Union]	Relject - the reference is in the reference list.

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4-1177	4	29	24	29	24	"√n" has print error [Yongjian Ding, China]	Noted - this is for copyediting.
4-1178	4	29	33	29	35	It isn't acceptable to make the following statement unless it is referenced, preferably using more than one citation: "The total sea level contribution…..between 2005 and 2010." There is currently no reference shown for this statement. [Dorothy Hall, United States of America]	Accept that text is not clear. Clarified.
4-1179	4	29	34			It is strange to put the uncertainties in parents (not the case elsewhere) [Etienne BERTHIER, France]	Accept.
4-1180	4	29	44	29	44	This is a clear case where the IPCC consensus is degrading the result relative to state of the art. The majority of the GRACE studies (those other than Shepherd et al 2012 and King et al 2012) are now shown to be systematically biased in a way that cannot be considered in a weighted average. The old results should be set aside. The way in which you treat the small uncertainties of King et al 2012 needs some thought (because they treat the GIA model uncertainty as systematic not random). [Matt King, Australia]	Reject. However, text changed to include better discussion of new paper.
4-1181	4	29	46	29	47	Sentence is confusing; and, "cyclical" should be avoided unless it is truly cyclical (annual cycle?) [Richard B. Alley, United States of America]	Accepted. "Cyclical" deleted.
4-1182	4	29	46	29	47	This sentence does not make sense: Is there really an increasing trend, or is it smaller than the interannual (2-4 y) variability and possibly just an "alias" of this variability? (Which shows up really well in the Chen et al. [2011; JGR] already in references as a regional signal of mass loss propagating around Greenland) [Laurence Padman, United States of America]	As 1181.
4-1183	4	29	46			the number given here for 1992-2011 is exactly the same as that given above for 1993-2010. is this correct? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Check.
4-1184	4	29	49	30	5	Sections 4.4.2.2.1 (Partitioning mass loss) and 4.4.2.2.2 (surface mass balance) can be merged together. A possible section title is "Ice-sheet wide changes". Paragraphs before 4.4.2.2.1 (but under 4.4.2.2) can be included in this new section. There are short sections after many paragraphs that are not in these sectioning. [Kenichi Matsuoka, Norway]	Accepted in part - sections merged as suggested.
4-1185	4	29	50	29	51	Does this match up with the equal statement in Ch13, p. 18 [Christopher Little, United States of America]	Needs to liaise with Chapter 13 to ensure compatibility of statements. But in essence Chapter 13 should adopt the language and numbers of Chapter 4.
4-1186	4	29	51	29	51	The statement would be stronger if more references are added (e.g. Sasgen 2012b) [European Union]	Reject. Ref. to van den Broeke et al. 2009 is there. Numbers added to quantify partitionning.
4-1187	4	29	51			need to define what is meant by discharge here [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Changed to "discharge by ice flow across the grounding line"
4-1188	4	29	52			decreased - become progressively more negative - is clearer [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Surface mass balance id deleted and replaced by ice mass loss.
4-1189	4	29	53			increased glacier speed does not increase mass loss - presumably mean increased flux of calved ice bergs? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. As 1187.
4-1190	4	29	55	30	1	The statement that slight inland thickening (from altimetry, Thomas et al.) "is not confirmed by regional atmospheric models" does not follow from the references cited in this sentence, but rather the contrary. Ettema et al. (2009) state "the surface mass balance trend over the full 1958–2007 period reveals the classic pattern expected in a warming climate, with increased snowfall in the interior and enhanced runoff from the marginal ablation zone." Buchardt et al (paper in discussion, Fig. 4) show the trend in accumulation for 4 drilling sites in decadal time intervals. Apart from the fact that this sparse sample is not sufficient for full verification over the extended inland region sampled by altimetry: 3 sites (CC, NEEM, B26) show higher accumulation rates for the period 2000-2010 compared to 1990-2000, and the 4th site shows little change. The time series of laser altimeter measurements of Thomas et al. starts in the 1990s. Zwally et al (J. of Glaciology, vol. 75 (201), pp 88 - 102, 2011) also show a clear net gain in mass inland since 1992, based on radar and laser altimeter data. [Helmut Rott, Austria]	Comment Noted. Ettema et al 2009 shows a very small positive trend in accumulation in Figure 3 (less than 1 kg/m2) and note that "only total sublimation and runoff indicate significant positive trends" in that time period, i.e. precipitation does not have a significant trend. Over the period 1990-2008, Figure 2 notes that there is a DECREASE in precipitation instead of an increase. The Buchardt et al. paper Climate Past 8, 2012 reports increased temperature from ice cores in the 1990s but states that "no conclusive statement can be made about the accumulation rate from these data".

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4-1191	4	29	55	30	5	Sectiion 4.4.2.2.2 (Surface Mass Balance) states that the four highest runoof years occurred since 1995. These were years in which the Hanna et al (2011) model determined that the surface mass balance was most negative, but as mentioned for 4.4,2.1.1, the extension to runoff from the ice sheet is a much less solidconclusion than the local mass balance. Again, the model used does include retention by refeezing, but there is a solid and growing body of literature that suggests that 1D shallow acquifer retention models are not sufficient to capture what's actually happening, and this is especially true under the current conditions of greater increased surface melt. I would suggest rephrasing this say "inferred runoff" until the issue is settled. [W. Tad Pfeffer, United States of America]	Reject. RACMO does include meltwater refreezing.
4-1192	4	29	56	29	57	So the observations are not confirmed by models? I am confused. Should I not believe the altimetric measurements? [Christopher Little, United States of America]	Clarified in the text: the altimetric thickning was for the 1994-2000 time period, whereas the the model output for 1957-2009.
4-1193	4	29	56			Here, again Ewert et al., 2011 provides more evidence. There are areas where those authors show also evidence for slight thickening in some interior (even three areas at the margins) regions. One may wonder if modelling can ever confirm observations - or if the discrepancy between regional atmospheric modelling and observations rather suggest that the model underestimates the precipitation. [European Union]	Comment noted but the authors do not reach any conclusion about enhanced precipitation in the interior. A few isolated spots of thickening do not have much significance in the interior.
4-1194	4	30	1	30	1	delete "however" [Yongjian Ding, China]	Reject but change to "probable changes in accumulation would however be…"
4-1195	4	30	1	30	5	Should this event be described here, or in a separate section on the melt season of 2012? The best reference for the Greenland melt of 2012 is Nghiem (2012), but it was submitted after the deadline. How is this handled? [Dorothy Hall, United States of America]	Accept: The Nghiem 2012 reference has been added
4-1196	4	30	2	30	2	add "." after "2009)" [Yongjian Ding, China]	Accept
4-1197	4	30	2	30	5	These lines conflate runoff with the widespread "melt" observed by e.g. Tedesco et al 2011. "Melt" in the sense detected by microwave satellite measurements involves moisture in the snowpack. This may well lead to local changes in snow/firn density profiles after refreezing - complicating altimetry measurements, but not necessarily leading to significant horizontal mass transport - especially where 90% of the Greenland ice sheet is involved for "a few days". [Government of Australia]	Accept: an additional sentence has been added to clarify this point.
4-1198	4	30	3	30	4	Suggest a reference for "extreme melt event covering >90% of the ice sheet for a few days in July 2012" Nghiem et al., Geophys. Res. Lett., 2012 [Government of United States of America]	as for 4-1195
4-1199	4	30	4			I appreciate that the melt in July 2012 is mentioned here, although there is no reference yet for it. However, we should consider either to mention the sea ice minimum in 2012 as well, or leave both for a later assessment report. [European Union]	as for 4-1195
4-1200	4	30	4			The melt event covered more than 98% of the ice sheet surface [Dorothy Hall, United States of America]	as for 4-1195
4-1201	4	30	8	30	9	Unclear as written; I presume that this is contributions of mass balance and ice flow to the observed changes [Richard B. Alley, United States of America]	Accept - text modified.
4-1202	4	30	12	30	14	Add discussion of Harig and Simons, PNAS, 2012 GRACE study showing more detailed spatial patterns of change. Harig, C. and F.J. Simons 2012. Mapping Greenland's mass loss in space and time. Proceedings of the National Academy of Sciences, doi:10.1073/pnas.1206785109. [Matt King, Australia]	Accept. New reference added to the text discussing regional patterns.
4-1203	4	30	12	30	21	This paragraph is mostly about glaciers, which are discussed in a separate section. If retained here, it should be checked that this does not simply duplicate what is said in the section on glaciers. If it is duplicative, it should either be omitted, or replaced by something much shorter that cross-references the discussion in the section on glaciers. [Adrian Simmons, United Kingdom]	Reject. Text is actually about outlet glaciers from ice sheets. However, text has been clarified.
4-1204	4	30	12			Dynamic losses' is not defined at this point. Suggest that it be linked back to the concept of ice discharge in contrast to surface mass balance. [Government of United States of America]	Accept. Have moved definition of dynamic loss forward

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4-1205	4	30	12			dynamic losses' another term for the same thing - best to define terms in introduction and stick to them [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Have moved definition of dynamic loss forward
4-1206	4	30	18			The Moon et al. 2012 paper deserve to be cited here. Moon T., Joughin I., Smith B., and Howat I. 21st-Century Evolution of Greenland Outlet Glacier Velocities, Science, 336, 576-578, 2012. [Etienne BERTHIER, France]	Reference to the Moon et al. paper has been added.
4-1207	4	30	24	30	25	If the high accumulation in the 1990s in the northwest was anomalous to times both before and after, say so clearly; if not, reword. [Richard B. Alley, United States of America]	Accepted. Text modified to note "compared to earlier and later years".
4-1208	4	30	24	30	25	The remarks here about acceleration in ice loss in northwest Greenland (attributed to Sasgen et al 2012b) in the period 1996-2006 to 2006-2010 are unclear. Is it meant that there was a high accumulation anomaly in late 1990's and that the cessation of this has led to a change in ice loss? The use of accelerated ice loss to mean increasing rate of change of ice loss - rather than dynamic acceleration of ice fluxes is also potentially confusing. [Government of Australia]	As 1207
4-1209	4	30	24			Consider explaining how the high accumulation in the late 1990's led to an acceleration in ice loss over 2006 - 2010. [Government of United States of America]	As 1207
4-1210	4	30	25			After line 25, there should be another section on surface temperature changes on the ice sheet. This section could discuss the "long-term" AVHRR records that I mentioned above, and more recent work using MODIS (e.g., Hall et al., 2008). Problems related to measuring surface temperature from space (mainly clouds) should be mentioned because clouds and cloud trends introduce uncertainites to surface-temperature trends. And there should be a discussion of the value of in-situ measurements from AWS and other stations on the Greenland ice sheet. There are serious problems with lack of calibration of the stations in the AWS network that are not serviced regularly, yet the data are very valuable and shouldn't be ignored. [Dorothy Hall, United States of America]	Reject. We do not feel this issue requires extensive discussion.
4-1211	4	30	27			This discussion of Antarctica could benefit from clear statements of level of confidence and uncertainty such as those provided for Greenland (pg. 29. line 1). [Government of United States of America]	consider/discuss at LA4
4-1212	4	30	31			The word 'certainly' should be used carefully, particularly if it is not used in the context of the official IPCC likelihood scale. [Government of United States of America]	accept and modify text
4-1213	4	30	31			certainly - would this be better in IPCC uncertainty language? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	accept and modify text
4-1214	4	30	34			Section on mass balance budget for Antarctica. Repeat the statement from comment 7 that the models can not be calibrated with in situ measurements and this gives lower confidence in the statement that there is no trend in total accumulation. [European Union]	same as 4-1142. consider/discuss at LA4 . Suggest to add "It is likely".
4-1215	4	30	36			What is meant by "prior maps"? Is the sentence referring to use of regional atmospheric climate models in data assimilation, where there is a concept of prior information? I find this sentence difficult to understand. [Adrian Simmons, United Kingdom]	Accept - change to "in previous estimates"
4-1216	4	30	37	30	38	Consider adding comment that large accumulation events can effect the records as seen in Dronning Maud Land in 2009 (Boening et al., 2012; Shepherd et al., 2012). Although I note that these studies show the continent-wide mass seemed unaffected by these events suggesting concentration spatially of accum, rather than new mass. Boening, C., M. Lebsock, F. Landerer and G. Stephens 2012. Snowfall-driven mass change on the East Antarctic ice sheet. Geophysical Research Letters, 39(21): L21501 doi:10.1029/2012gl053316. [Matt King, Australia]	Accept. check relevance and need for another reference. Added reference to Boening et al 2012, van Ommen et al. 2010. Re-phrased differently but open to further edits from LA.
4-1217	4	30	37	30	39	The lack of long term trend (as per Monaghan) on a continent-wide basis may be, but the statement should acknowledge the large positive anomaly in accumulation of recent decades in Eastern Wilkes Land and Law Dome. Perhaps add "Accumulation at Law Dome from ice core records (van Ommen and Morgan, 2010) shows elevated snowfall in recent decades that lies outside the natural variability of the last 750 years. This signal also appears in other regional records (Morgan et al., 1990) and regional correlations (Monaghan,	Accept (as above) check relevance and need for another reference

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						2006) indicate this increase is representative of Eastern Wilkes Land." References next line [Tasman van Ommen, Australia]	
4-1218	4	30	37	30	39	van Ommen, T.D., Morgan, V. (2010) Snowfall increase in coastal East Antarctica linked with southwest Western Australian drought Nature Geoscience 3. 267-272, doi:10.1038/NGEO761; Morgan, V.I., Goodwin, I.D., Etheridge, D.M., Wookey, C.W. (1991) Evidence from antarctice ice cores for recent increases in snow accumulation. Nature 354. 1-3 [Tasman van Ommen, Australia]	part of comment above
4-1219	4	30	41	30	41	Figure 41314f does not exist, this is a typo [Ernst Schrama, Netherlands]	accept and modify text
4-1220	4	30	41			Figure 4.1314f should be 4.14f [David Bromwich, United States of America]	accept and modify text
4-1221	4	30	41			Figure callout is wrong (should not be "4.1314f") [Laurence Padman, United States of America]	accept and modify text
4-1222	4	30	45	30	45	replace "Dong Chen et al.,2009" as "E et al.,2009: [Yongjian Ding, China]	accept and modify text
4-1223	4	30	46	30	46	Delete Moore and King as superseded by King et al [Matt King, Australia]	accept and modify text and reference
4-1224	4	30	46	30	46	King et al submitted now published. King, M.A., R.J. Bingham, P. Moore, P.L. Whitehouse, M.J. Bentley and G.A. Milne 2012. Lower satellite-gravimetry estimates of Antarctic sea-level contribution. Nature, 491: 586–589 doi:10.1038/nature11621. [Matt King, Australia]	part of comment above
4-1225	4	30	49	30	52	The description of the contents of the Tables in the Appendix is not quite accurate. Tables as given show included and some excluded estimates, not full list and selected subsets as stated here. [Government of Australia]	Accept and modify text at lines 50-52
4-1226	4	31	1			The mass-loss statement for Antarctica in the executive summay uses the terms 'robust evidence, high agreement' for this statement, however this terminology does not appear here in the body of the text. [Government of United States of America]	Accept.
4-1227	4	31	5	31	5	replace "Shepherd and others,2012)" as "Shepherd et al.,2012" [Yongjian Ding, China]	accept and modify text
4-1228	4	31	6	31	6	"measured" does not seem the right word - "known/understood"? [Matt King, Australia]	accept and modify text
4-1229	4	31	7	31	7	delete "for details see, Shepherd and others,2012) [Yongjian Ding, China]	accept and modify text
4-1230	4	31	8	31	9	The mass change for Antarctica by Shepherd et al. (2012) in the published final version is -71 +-53 Gt/yr (small difference to -67 +-52 Gt/yr) [Helmut Rott, Austria]	check and correct as necessary
4-1231	4	31	8	31	10	The remarkably close agreement with the King et al 2012 GRACE study is noteworthy (-69+-18 (2 sigma)) [Matt King, Australia]	noted
4-1232	4	31	8			The following study could be cited here: King, M.A., Bingham, R.J., Moore, P., Whitehouse, P.L., Bentley, M.J. and Milne, G.A., 2012. Lower satellite-gravimetry estimates of Antarctic sea-level contribution. Nature, 491(7425), 586-589. [David Bromwich, United States of America]	already on our list to replace Moore and King
4-1233	4	31	9	31	10	Is it worth a comment about why the uncertainty range is larger in the Shepherd et al analysis? [Christopher Little, United States of America]	Accept? IA will relook at error budget (may look at 2 sigma?).
4-1234	4	31	12			section on ice loss from Antarctica. Include reference of Sasgen 2012a [European Union]	accepted and will add
4-1235	4	31	12			almost certainly' hints of the official liklihood scale. If this is intentional, then the appropriate, correct phrase from the likelihood scale should be used. If unintentional, suggest using a different modifier. [Government of United States of America]	accept and modify text. Changed "Almost certainly" to "very likely" because the record remains short, open to edits from other LA.
4-1236	4	31	12			again 'almost certainly' is a very strong statement and would be better made in IPCC language [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	As 1235
4-1237	4	31	15			certainly' - again care should be exercised in the use of terminology that echos official scales. [Government of United States of America]	As 1235

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4-1238	4	31	17	31	17	rewrite "indicates an increase" to "indicates an accelerating increase" to indicate that the following numbers refer to an increasing "change rate" [Olaf Eisen, Germany]	"increase in the rate of mass loss"
4-1239	4	31	17	31	18	"every year" is confusing; this is an average value and does not apply to every year individually. [Richard B. Alley, United States of America]	This will be standardised across the document during copyediting.Accepted and added "per year on average"
4-1240	4	31	19	31	19	replace "Shepherd and others,2012)" as "Shepherd et al.,2012" [Yongjian Ding, China]	accept and modify text
4-1241	4	31	19	31	19	"West Antarctica is accelerating in mass loss" is probably too general. It can likely be narrowed to just the Amundsen Sea Coast. King et al. 2012 suggest that the only basin accelerating with 2 sigma confidence is the one containing Pine Is. Flament and Remi, 2012 show the front of Thwaites to be accelerating. The Shepherd et al 2012 result show the APIS to be accelerating mainly because they include Palmer Land which experienced reduction in snowfall, giving the entire APIS an appearance of accelerating loss. Be careful on that. King et al. 2012 show now statistically significant accel in Graham Land where Larsen B is. Flament, T. and F. Remy 2012. Dynamic thinning of Antarctic glaciers from along-track repeat radar altimetry. Journal of Glaciology, 58(211): 830-840 doi:10.3189/2012JoG11J118. [Matt King, Australia]	Accept and make more precise on line 15 by adding "Southeast Pacific sector of WAIS".
4-1242	4	31	21	31	25	The wording here makes it unclear if Zwally and Gioveinetto 2011 component mass budget approach was excluded from the analysis of Antarctic mass changes. [Government of Australia]	Paper inserted in the list of papers that are not included. Text edited to indicate that the study is contrary to most other recent studies. Note that Zwally et al is not a mass budget method.
4-1243	4	31	27			Section 4.4.2.3.1 (partitioning mass loss) does not work well. Incuding paragraphs above this section, it is nice to have a section "ice-sheet wide changes". [Kenichi Matsuoka, Norway]	Section heading has been removed.
4-1244	4	31	28	31	28	This is unclear and could be misunderstood easily. [Richard B. Alley, United States of America]	Accepted. Text modified. But I think citations need to be added.
4-1245	4	31	28	31	28	Nearly steady snowfall is mentioned in a following section, not before this section 4.4.2.3.1. Please revise this sentence. [Kenichi Matsuoka, Norway]	as above
4-1246	4	31	28			define long term [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted. Changed into multi-decadal
4-1247	4	31	31	31	31	ambiguous, rewrite to "estimated long-term trend in snowfall". Would be better to write "specific surface mass balance" than just mass input. More references needed! [Olaf Eisen, Germany]	Rejected. Terminolgy used is more accessible and in this case more correct.
4-1248	4	31	31	31	31	deleted fullstop after trend [Matt King, Australia]	accept and modify text
4-1249	4	31	34			As one of the three mass balance methods is based on the estimate of thinning and thickening (volumetric method), this sentence has to be rewritten to something like 'The gravimetric and mass budget methods are in excellent agreement.... '. [European Union]	Delete "excellent". reluctantly accept and modify for clarity. Mass balance is whole ice sheet mass balance - NOT surface mass balance
4-1250	4	31	37			is likely' Is this an official use of the likelihood scale? [Government of United States of America]	accept and modify text
4-1251	4	31	38		39	Cook and Vaughan, 2010 is not the suitable reference here, as this publication presents retreat areas of ice shelves, while in this context ice discharge observations are required. Beside Wendt et al., 2010, estimates by Rott et al., 2011 or Scambos et al., 2004 (even if it is from the time of AR4) are more suitable. [European Union]	Accept. Reference deleted and replaced.
4-1252	4	31	39	31	40	The Jacobs et al study focuses on only the thinning of PIG. I agree that the reference shows a "warm" ocean, but not sure if they link it to all Amundsen Sea shelf thinning. Maybe Pritchard 2012 makes this story stronger. [Christopher Little, United States of America]	Accept. Reference inserted after "ice shelves". Check text.
4-1253	4	31	40			Add Pritchard et al. [2012] after cite to Jacobs et al. [2011] [Laurence Padman, United States of America]	As 1252
4-1254	4	31	42			Wikipedia tells me the Totten glacier is 40 miles long and 20 miles wide, and it is stated earlier in this chapter that GRACE has a resolution of about 300km. Is it one or both of the other types of measurement quoted that	Reject. Wikipedia refers only to the floating tongue. The floating tongue is 40 miles long (and registers

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						justifies the words "primarily Totten Glacier"? If so, a rewording to clarify the matter could be considered. [Adrian Simmons, United Kingdom]	nothing on GRACE if it thins and is replaced with sea water). The grounded outlet glacier (and drainage basin) are much larger.
4-1255	4	31	49			space before "(Bindschadler" [Laurence Padman, United States of America]	accept and modify text
4-1256	4	31	50	31	50	The choice of references regarding the buttressing effect of ice shelves here seems rather random and lacking in historical and theoretical context. [Government of Australia]	Accept. References revised.
4-1257	4	31	51			Here, Padman et al., 2011 is missing. [European Union]	as above
4-1258	4	31	51			Add Fricker and Padman [2012] reference here, in the list of "thickness change" references. Although it only covers the Peninsula ice shelves, it is the only one of these studies that spans 3 decades (by including Seasat) [Laurence Padman, United States of America]	Accept. Additional reference added.
4-1259	4	31	52	31	54	The statement "nearly all outlet glaciers and ice stremas are loosing mass at high rates" is incorrect. Most outlet glaciers are not monitored or too small to detect mass loss of individual glaciers. This is a misleading statemenet. [Kenichi Matsuoka, Norway]	Reject. However, text revised to clarify.
4-1260	4	31	52	31	55	The first clause of the sentence that begins and ends on these lines is confusing. It ignores the continuum of ice-shelf behavior beyond “thinning” and “disintegrated.” Indeed, the fastest ice-shelf thinning rates reported by Pritchard et al. [2012] occur in the Amundsen Sea Embayment, where ice shelves are both thinning but also presently disintegrating. This process involves both rifting and retreat that begins at the ice-shelf margins and also transverse-to-flow rifts closer to the grounding line. These observations were reported by MacGregor et al. (2012, J. Glaciol., 58(209), 458-466). I recommend that the beginning of this sentence be edited to: “Indeed, nearly all outlet glaciers and ice streams experiencing high rates of ice loss into thinning, disintegrating, or disintegrated ice shelves (Pritchard et al., 2012; MacGregor et al., 2012), …” [Joseph MacGregor, United States of America]	Reject. However, text revised to clarify.
4-1261	4	31	53	31	54	The current sentence lacks quantification (what are "high rates") and is ambiguous, even contradictory: "thinning or disintegrated ice shelves" in contrast to "stable conditions". Needs much clarification. [Olaf Eisen, Germany]	See 1260.
4-1262	4	31	54	31	55	Shepherd et al [2010; GRL] also makes the case that the large ice shelves are stable, with the advantage over Pritchard et al. [2012], that the record is for 1994-2008 instead of just 2003-2008 for the latter. [Laurence Padman, United States of America]	Consider changing reference. Added Shepherd et al. 2010 reference.
4-1263	4	31				Section 4.4.2.3.1: "… near-absence of … long-term change in total snow fall" seems to contradict " …decadal variability in snowfall which exceeds in magnitude the estimated long term trend", if one interprets "decadal" as "long term". Explain and/or clarify it. How long is long term? Are there measurements of decadal and/or long term snowfall in Antarctica? [Government of United States of America]	Accepted. Text modified to include the different time scales under consideration.
4-1264	4	31				Section 4.4.2.3.1: Please provide a reference for the conclusion: "In the near-absence of surface runoff and long-term change in total snowfall, ….". [Government of United States of America]	Related tpo 1246 - 1247. RESPONSE TO BE ADDED. Reject. Ref not needed because the topic is discussed only half a page earlier.
4-1265	4	32	1	32	2	The reference to Fricker and Padman 2012 - just one of the many sources of information about Antarctic Peninsula ice shelf retreat seems unrepresentative of the literature. [Government of Australia]	Accept. See 4-1267. Lead author of Fricker and Padman also agrees.
4-1266	4	32	1	32	2	What is ice shelf retreat? Is it ice front retreat? Does this encompass abrupt collapses (Larsen B) and thinning (Larsen C?)? [Christopher Little, United States of America]	Noted. Reduction in ice shelf extent, includes abrupt collapse but not thinning.
4-1267	4	32	1			The evidence for several decades of Ant. Peninsula ice shelf retreat (i.e., reduction in areal extent) comes from Cook and Vaughan [2010], not Fricker and Padman [2012]. What the latter paper shows is several decades of ice shelf thinning, which is relevant and probably should be cited back on 4-31 line 51. [Laurence Padman, United States of America]	As 1265. Added Cook and Vaughan, 2010; might be others.
4-1268	4	32	2			Section 4.4.3.1.2 The fact that only the outlet glaciers terminating in fjords have increased velocity in Greenland strengthens the hypothesis that the warm ocean water is the cause of the velocity changes. This	Reject. This section is entitled Antarctica so this would be inappropriate. Update: I added "glacier speedup

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						could be included in the section. [European Union]	and changes in ocean temperature on line 27 word doc. The hypothesis mentioned by the reviewer is already mentioned in the text. This addition should clarify it. This paragraphy is not only about Antarctica.
4-1269	4	32	3			Four retreated (Wilkins, Wordie, Mueller, Larsen B) and four disintegrated (Jones, Prince Gustav Channel, Larsen Inlet, Larsen A) - it is thus 8 out of 12. [European Union]	Check - is Larsen A / Larsen Inlet really counted as separate ice shelves?
4-1270	4	32	5	32	8	The comments here about relating ice shelf retreat to warming climate says nothing about the discussions in the literature about the relative importance of warming atmosphere over ice shelves versus warmer oceans beneath them. [Government of Australia]	"Warming climate" removed clarified atmosphere versus ocean.
4-1271	4	32	5	32	8	This is true, assuming by "warming climate", you mean the atmosphere. However, Fricker and Padman [2012] and Padman et al. [2012; JGR] show that western AP ice shelves have been thinning due to basal melt (i.e., ocean processes) for decades, and presumably this is a relevant "pre-conditioning" for collapse ultimately driven by surface processes. Temporal variability of basal melt was also implied by modeling by Paul Holland et al. [2010, JGR], who found an increase in basal melt of George VI ice shelf attributed to atmospheric and sea ice changes over the Bellingshausen Sea continental shelf. [Laurence Padman, United States of America]	As 1270.
4-1272	4	32	6	32	6	Need at least one reference to justify this as robust evidence. [Christopher Little, United States of America]	Accept. References added.
4-1273	4	32	6	32	8	"which in time may yield....." The assessment should not stray beyond the current state of knowledge, and avoid commenting on what future data/studies might enable. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Last phrase of sentence deleted.
4-1274	4	32	10			Section 4.4.2.5 (Total Ice Loss from Both Ice Sheets) should be a new third level section (i.e. 4.4.3) rather than a subsection under Antarctica. [Kenichi Matsuoka, Norway]	Accept, sections renumbered.
4-1275	4	32	14	32	15	In the sentence that spans these lines, either "three" should be "five" or "2007-2011" should be "2009-2011". [Adrian Simmons, United Kingdom]	accept and correct text
4-1276	4	32	14	32	23	Comment text: The text "Over the last three years (2007-201) it has been equivalent to 1.0 +-0.3mm/yr..Table 4.6" is inconsistent with Table 4.6 below where the figures are for 2005-2010 and given as 0.94 +-0.31 mm/yr. Don't mind which but they should be the same. [Peter Barrett, New Zealand]	check and correct as necessary
4-1277	4	32	22	32	24	The numbers by Shepherd et al. (2012) are apparently not considered for the numbers in Table 4.6?, as this source is not included in any of the Tables in Appendix 4. [Helmut Rott, Austria]	Accept. Also IMBIE paper added to list of papers that are not used in calculation.
4-1278	4	32	22	32	24	Table 4.6 uncertainty estimates appear to me to be too low. They may reflect the published estimates, but I suggest that IPFCC authors could comment on whether these are realistic (perhaps thay do somewhere). There is a tendency for glaciologists to under-estimate uncertainties, either by ignoring some errors, or by overly optimistic assessments. [Robert Thomas, United States of America]	As 1233.
4-1279	4	32	26			Section 4.4.3 Include a section describing that the current changes can be caused by past climate changes. This would also be useful because the process is mentioned in the introduction (page 4-6, line 38) [European Union]	Accept: added a sentence to make this point
4-1280	4	32	32	32	32	was->is [Christopher Little, United States of America]	Accept:
4-1281	4	32	32	32	33	For snowfall, "there was very little evidence for long-term change": Does this mean that a lack of measurements/observations prevents analysis, or that measurements/obserations exist and show no change? [Government of United States of America]	Accept. The measurements exist but show no change.
4-1282	4	32	32			"was" should be "is" [Adrian Simmons, United Kingdom]	accept
4-1283	4	32	36	32	37	Be careful here -- warm does not "hold" more moisture rather evaporation is accelerated in warmer air [JAMES FOSTER, U.S.A.]	Reject, incorrect.
4-1284	4	32	36	32	37	This sentence is problematical, for two reasons. Firstly, warmer air can hold more moisture, but the extent to which it actually will, and whether the increased moisture will be released as precipitation, is less clear. And even if precipiation does increase, will this mean more snowfall? If temperatures are warmer, there may be a	Accept. Recast sentence in terms of "could increase snowfall".

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						shift from snowfall to rainfall. [Adrian Simmons, United Kingdom]	
4-1285	4	32	36	32	46	One or two sentences about West Antarctica should be added here. For example: "In West Antarctica, the warming since the 1950s (Bromwich et al., 2012; Ding et al., 2011; Steig et al., 2009; Schneider et al., 2012) -- whose magnitude and seasonality are still debated -- has not manifested itself in enhanced surface melting (Kuipers Munneke et al., 2012; Tedesco and Monaghan, 2009) nor in increased snowfall (Bromwich et al., 2011; Lenaerts et al.; 2012)". References: Bromwich, D.H., Nicolas, J. P., Monaghan, A. J., Lazzara, M. A., Keller, L.M., Weidner, G.A. and Wilson, A.B., 2012: Central West Antarctica among the most rapidly warming regions on Earth. Nature Geoscience, in press. Bromwich, D.H., Nicolas, J.P. and Monaghan, A.J., 2011. An assessment of precipitation changes over Antarctica and the Southern Ocean since 1989 in contemporary global reanalyses. Journal of Climate, 24(16): 4189-4209. Ding, Q., Steig, E.J., Battisti, D.S. and Kuttel, M., 2011. Winter warming in West Antarctica caused by central tropical Pacific warming. Nature Geoscience, 4: 398- 403. Kuipers Munneke, P., Picard, G., van den Broeke, M.R., Lenaerts, J.T.M. and van Meijgaard, E., 2012. Insignificant change in Antarctic snowmelt volume since 1979. Geophysical Research Letters, 39(L01501). Lenaerts, J.T.M., van den Broeke, M.R., van de Berg, W.J., van Meijgaard, E. and Munneke, P.K., 2012. A new high-resolution surface mass balance map of Antarctica (1979-2009) based on regional atmospheric climate modeling. Geophysical Research Letters, 39(L04501). Schneider, D., Deser, C. and Okumura, Y., 2012. An assessment and interpretation of the observed warming of West Antarctica in the austral spring. Climate Dynamics, 38:323- 347. Steig, E.J., Schneider, D.P., Rutherford, S.D., Mann, M.E., Comiso, J.C. and Shindell, D.T., 2009. Warming of the Antarctic ice-sheet surface since the 1957 International Geophysical Year. Nature, 457: 459 -462. Tedesco, M. and Monaghan, A.J., 2009. An updated Antarctic melt record through 2009 and its linkages to high-latitude and tropical climate variability. Geophysical Research Letters, 36(L18502). [David Bromwich, United States of America]	Accept. Delete sentence about SAM and and ozone depletion. Add sentence on WAIS.
4-1286	4	32	36			Suggestion: "warming air will…" or "increasing air temperature will…" [David Bromwich, United States of America]	Accept
4-1287	4	32	40	32	40	Statement "Antarctica, in response to ozone depletion" needs a supporting reference right after "depletion". [Olaf Eisen, Germany]	Sentence has been modified / deleted. As 1285.
4-1288	4	32	40	32	41	The strengthening of the summertime SAM from the mid-1950s to the mid 1990s is stated to be in response to ozone depletion. However, the ozone column measurements made at the South Pole indicate that significant ozone depletion started only in the late 1970s. So what caused the strengthening of the SAM from the mid 1950s to the late 1970s? [Adrian Simmons, United Kingdom]	Sentence has been modified / deleted. As 1285.
4-1289	4	32	40	32	46	Reference should be made here to the work showing that it is not just the Antarctic Peninsula that is warming, but all of West Antarctica. Although the original work of Steig et al., 2009 was controverial, the paper which criticized it stil found significant mean annual warming in West Antarctica (O'Donnell et al., 2009). The evidence for significant warming in all seasons has been supported by new weather station data (Küttel et al., 2012), by re-calibrated analysis of old weather station data (Bromwich et al., in press), and by borehole themometry (Orsi et al., 2012). These changes are unequivocally related to tropically-forced anomalies, not to the Southern Annular mode. This is important for the cryosphere because the same atmospheric circulation anomalies that explain the rising temperatures also explain the changes in regional ocean forcing of the ice	Sentence has been modified / deleted. As 1285.

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						sheets. Refs: 1 Bromwich, D. H., Monaghan, A. J. & Guo, Z. Modeling the ENSO modulation of Antarctic climate in the late 1990s with the Polar MM5. J. Climate 17, 109-132 (2004). 2 Bromwich, D. H. et al. Central West Antarctica among most rapidly warming regions on Earth. Nat. Geosci. in press (2012). 3 Ding, Q., Steig, E. J., Battisti, D. S. & Küttel, M. Winter warming in West Antarctica caused by central tropical Pacific warming. Nat. Geosci. 4, 398-403, doi:10.1038/ngeo1129 (2011). 4 Ding, Q., Steig, E. J., Battisti, D. S. & Wallace, J. M. Influence of the tropics on the Southern Annular Mode. J. Climate 25, 6330-6348, doi:10.1175/JCLI-D-11-00523.1 (2012). 5 Orsi, A. J., Cornuelle, B. D. & Severinghaus, J. P. Little Ice Age cold interval in West Antarctica: Evidence from borehole temperature at the West Antarctic Ice Sheet (WAIS) Divide. Geophys. Res. Lett. 39, L09710, doi:10.1029/2012gl051260 (2012). 6 Schneider, D. P., Deser, C. & Okumura, Y. An assessment and interpretation of the observed warming of West Antarctica in the austral spring. Clim. Dyn. 38, 323-347, doi:10.1007/s00382-010-0985-x (2011). 7. Steig, E. J. et al. Warming of the Antarctic ice-sheet surface since the 1957 International Geophysical Year. Nature 457, 459-462, doi:doi:10.1038/nature07669 (2009). 8. Steig, E. J., Ding, Q., Battisti, D. S. & Jenkins, A. Tropical forcing of Circumpolar Deep Water Inflow and outlet glacier thinning in the Amundsen Sea Embayment, West Antarctica. Annal. Glaciol. 53, 19-28, doi:10.3189/2012AoG60A110 (2012). 8.Küttel, M., Steig, E. J., Ding, Q., Battisti, D. S. & Monaghan, A. J. Seasonal climate information preserved in West Antarctic ice core water isotopes: relationships to temperature, large-scale circulation, and sea ice. Clim. Dyn. 39, 1841–1857, doi:10.1007/s00382-012-1460-7 (2012). [Eric Steig, United States of America]	
4-1290	4	33	2	33	23	suggest combining this section with 4.4.3.2.4 [Christopher Little, United States of America]	Accept. Delete P34 lines 5-11 as they are repetative with P33 lines 3-9.
4-1291	4	33	3	33	4	"Interactions between ocean waters and the periphery of large ice sheets very likely [i.e., 90-100% probability] plays a major role in present ice sheet changes": Does this suggest that atmosperic thermodynamics (e.g., air temperature), atmospheric dynamics (e.g., wind forcing), precipitation, snowfall, radiation, black carbon, albedo, cloud feedbacks and others all add up to a minor role? [Government of United States of America]	Accept. Change "major" to "significant"
4-1292	4	33	3			very likely' plays a major role. Does this terminology refer to official IPPC likelihood? [Government of United States of America]	Yes - italicised.
4-1293	4	33	4	33	4	The choice of references regarding the likely major role of ice ocean interactions seems inappropriate in terms of the long history of this issue. One is a short commentary - the other a detailed study of ice shelf thinning with a passing correlation to large scale ocean temperatures. [Government of Australia]	Accepted. Holland et al., referemce added and Bindschadler reference deleted. I'm not sure this is sufficient - I agree that the Pritchard reference isn't really relevant and suggest that we add some that are Antarctic in scope _ Eric?
4-1294	4	33	5	33	5	remove high" [Christopher Little, United States of America]	Accept.
4-1295	4	33	10	4	17	This section refers to the Southern Annular Mode as a "likely driver" of the wind changes that have brought warm water into contact with the ice shelves in Antarctica. There is no published evidence to support this view that I am aware of. At best, it is a speculation. On the other hand, in the Amundsen Sea where the largest changes have occurred, the wind forcing is demonstrably not driven by the Southern Annular Mode. The modeling evidence suggests that it is in the austral fall and winter during which the wind-driven ocean forcing has been most important (Thoma et al.) and in those seasons there Southern Annular mode is either unchanging (fall) or declining (winter and spring). Thoma et al found no correlation between the local wind stress and the SAM. In contrast, Ding et al (2011) showed that the wind anomalies in this region are strongly connected with changes in the tropical Pacific. Steig et al. (2012) followed up on this work and showed that the local wind stress changes found in Thoma et al to be critical for the ocean changes are also directly linked to the tropics, not the SAM. The distinction here is not trivial, because attributing the wind/ocean forced ice sheet changes to the SAM implies that they will perhaps diminish as the ozone hole recovers, while attribution	Accept - delete from "associated:" to "are probable" and add "(in the north … and in the south …).

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						to the tropics completely changes the picture: the questoin then becomes "what willl happen to the tropics in the future"? The answer is: we don't know. SUGGESTED REPLACEMENT SENTENCE for lines 10-13: Ocean circulation delivers warm, salty waters to ice sheets, and variations in wind patterns associated with the North Atlantic Oscillation (Hurrell, 1995; Jacobs et al., 1992) and, in West Antartica, the tropically-forced Rossby waves (the Pacific South America Pattern; Ding et al, 2011; 2012; Steig et al., 2012) are probable drivers of increasing amounts of warm water reaching the ice sheet margins. Changes in the Southern Annular Mode (Thompson and Wallace, 2000) may be important in East Antarctica. References: 1 Ding, Q., Steig, E. J., Battisti, D. S. & Küttel, M. Winter warming in West Antarctica caused by central tropical Pacific warming. Nat. Geosci. 4, 398-403, doi:10.1038/ngeo1129 (2011). 2 Ding, Q., Steig, E. J., Battisti, D. S. & Wallace, J. M. Influence of the tropics on the Southern Annular Mode. J. Climate 25, 6330-6348, doi:10.1175/JCLI-D-11-00523.1 (2012). 3 Steig, E. J., Ding, Q., Battisti, D. S. & Jenkins, A. Tropical forcing of Circumpolar Deep Water Inflow and outlet glacier thinning in the Amundsen Sea Embayment, West Antarctica. Annal. Glaciol. 53, 19-28, doi:10.3189/2012AoG60A110 (2012). 4 Thoma, M., Jenkins, A. & Holland, D. Modelling circumpolar deep water intrusions on the Amundsen Sea continental shelf, Antarctica. Geophys. Res. Lett. 35, L18602, doi:10.1029/2008GL034939 (2008). [Eric Steig, United States of America]	
4-1296	4	33	10	33	10	Remove words before "Variations" [Christopher Little, United States of America]	Accept - words deleted as they are repetative
4-1297	4	33	10	33	17	This paragraph does not touch on the melting at deep grounding lines in Antarctica, where, due to the pressure dependence of the sea-water freezing temperature, even quite cold waters can deliver heat to melt ice at great depth. [Government of Australia]	Accept. Replace "warm water" with "relatively warm water".
4-1298	4	33	13			It is stated that "Limited observations have established …". If the observations are sufficient to establish the fact, in what sense are they limited? [Adrian Simmons, United Kingdom]	Accept. Delete "limited", and insert "some" before "marine-terminating glaciers"
4-1299	4	33	17	33	17	add "and ice shelf shape, mixing processes, and sea ice formation" (references from Little, Payne, Holland, Nicholls, Padman, others.) [Christopher Little, United States of America]	Accept. Delete sentence.
4-1300	4	33	17			add "Subglacial freshwater discharge across the grounding line [Motyka et al., 2003, 2011; Xu et al., 2012] may also play an important role, at least in Greenland (see section 4.4.3.2.4). " [Laurence Padman, United States of America]	As 1299. Sentence has been deleted.
4-1301	4	33	19	33	23	Why is there no mention of Argo floats in this paragraph? [Adrian Simmons, United Kingdom]	Accept. Delete paragraph starting at line 19 P33.
4-1302	4	33	19			Recent paper on Southern Ocean warming: Purkey and Johnson 2010: Purkey, Sarah G., Gregory C. Johnson, 2010: Warming of Global Abyssal and Deep Southern Ocean Waters between the 1990s and 2000s: Contributions to Global Heat and Sea Level Rise Budgets*. J. Climate, 23, 6336–6351. [Laurence Padman, United States of America]	Accept. Paragraph deleted.
4-1303	4	33	27	33	43	It has to be indicated that the lubrication issue has long been known, see e.g. the work of Iken from the 1980s, and is a general process, not only a direct consequence of climate change. Sentence on p34, line 2 shows an example for butressing, how a long-known fact can be stated. [Olaf Eisen, Germany]	reject. This is an assessment of recent development, not a history. Add Zwally reference at end of sentence 2.
4-1304	4	33	27	33	45	Two sections (4.4.3.2.1 and 4.4.3.2.2) describe the climate-induced ice sheet processes. Ice sheet process section (4.4.3.2) can start wtih more ice-sheet-orignated processes first and then describe the climatic effects on ice-sheet processes. [Kenichi Matsuoka, Norway]	Reject. The structure has been carefully thought through to provide a path for the reader.
4-1305	4	33	27			This section fucuses only on basal lubrication caused by surface lake outburst observed in Greenland. However, other features are widely found for basal lubrications, such as active subglacial lakes in Antarctica, and soft-bedded ice streams, fjord-type outlet glacciers which bed reaches the pressure melting point due to thick ice. More general descriptions about basal lubrication is necessary. And I propose to move Greenland surface-lake/bed relationship in a separate section, since it is not purely ice-sheet processes (see my comment about sections 4.4.3.2.1 and 4.4.3.2.2 as well). [Kenichi Matsuoka, Norway]	Accept in part. Sentence added at start of paragraph.
4-1306	4	33	28		29	quantify close and narrow [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept - delete "in narrow zones".
4-1307	4	33	28		43	An important point for this audience is that while meltwater input and sliding speed are clearly linked, the exact processes which drive basal sliding are poorly understood. This is evident from the fact that we have no	Noted.

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						working models that can predict sliding speed as a function of water input. [Joel Harper, United States of America]	
4-1308	4	33	30		32	This sentence seems to be out of context… “Such conduits” when conduits are not previously mentioned. Further, the surrounding sentences would appear to flow from one to the next without this sentence in between. [Joel Harper, United States of America]	Accept. Replace "conduits" with "drainage events".
4-1309	4	33	36	33	36	clarify the events change velocities over sub-daily timescales only? [Matt King, Australia]	Reject - observations show speed up for durations longer than sub-daily timescales.
4-1310	4	33	43	33	43	add references here? [Christopher Little, United States of America]	Reject. This summarises the discussion in the preceeding para which is referenced. Sentence edited to use uncertainly language. PLEASE CHECK YOU AGREE WITH WHAT I HAVE WRITTEN!
4-1311	4	33	45	33	49	When the bed is lubricated, internal deformation is much less important, so I can't see that this plays a role. [Olaf Eisen, Germany]	Reject - disagree internal deformation might be important where sliding occurs.
4-1312	4	33	49			spelling of Phillips should be corrected [Dorothy Hall, United States of America]	Accept.
4-1313	4	33	49			spelling of "Phillps" (also in References); should it be "Phillips" ? [Laurence Padman, United States of America]	As 1312.
4-1314	4	33	51	34	33	Three sections, 4.4.3.2.3 and 4.4.3.2.5 can be merged together under one section. [Kenichi Matsuoka, Norway]	Noted. Lowest level of subheading numbering has been removed.
4-1315	4	33	51			The sectio name has a narrower scope than it should be. How about "butressing effects in the coastal area"? For instance, ice rises also provide butressing effects. Together with the buttressing effect, grounding line instability can be mentioned here. [Kenichi Matsuoka, Norway]	Partially accept. The discussion iover this comment resulted in the subsection paragraph being rewritten.
4-1316	4	33	55	33	56	The sentence contained within these two lines is supported by MacGregor et al. (2012, 58(209), 458-466), who found the pattern of acceleration near the grounding line of Thwaites Glacier is spatially coherent with that of ice-shelf rifting and retreat immediately downstream of the grounding line. I recommend that this sentence be edited to: “Many of the largest and fastest glacier changes appear to be partly in response to ice shelf or floating ice-tongue shrinkage or loss (e.g., MacGregor et al., 2012).” [Joseph MacGregor, United States of America]	As 1315
4-1317	4	34	1			these observations are consistent with buttressing however there is insufficient evidence to say whther they are consistent with marine instability or not. Marine instability involves a lot more than increased ice flow as a consequence of ice sheet loss/thinning so that it is misleading to suggest the observed response and instability are consistent. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	As 1315
4-1318	4	34	5	34	10	Would insert after p 33, line 17 and merge sections. [Christopher Little, United States of America]	Accept. Done in response to previous comment.
4-1319	4	34	5	34	10	Would rewrite the paragraph. Here's a suggestion: "The flux of meltwater is a function of the near-ice ocean temperature and flow speed (Holland and Jenkins, 1999). Because flow speed is increased by meltwater fluxes and in regions of steep basal slope, regions of melting and mass loss from ice shelves are highly spatially concentrated (there is observational evidence for this!). The rates and locations of basal melting are thus determined by the rapid adjustment of the coupled ice-ocean system. [Christopher Little, United States of America]	Accepted. Paragraph has been deleted as it was repetative with P33 lines 3-8
4-1320	4	34	5	34	10	On the coupled response to ice shelf basal melting (CM Little,et al 2012), Journal of Glaciology, 58, 203-215 doi: 10.3189/2012JoG11J037; Simulation of ocean-land ice interactions through a strongly thermally-forced ice shelf, Part 1: Model description and behavior (Goldberg et al 2012), J. Geophys. Res. E,DOI: 10.1029/2011JF002246; Simulation of ocean-land ice interactions through a strongly thermally-forced ice shelf, Part 2: Sensitivity to external forcings (Goldberg et al 2012), J. Geophys. Res. E, DOI: 10.1029/2011JF002247 [Christopher Little, United States of America]	will check relevance and need for another reference
4-1321	4	34	5	34	10	The point of the "flow speed may increase with thermal forcing" is that this would mean that the response to	Paragraph has been deleted see response to 1319

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						changing thermal forcing would be nonlinear, perhaps quadratic? Perhaps it is worth spelling that out here, noting the importance as a spur for research to resolve whether melt rates are linear or quadratic, or somewhere in-between, w.r.t. thermal forcing. This probably depends on thermal anomaly, presence of other sources of ocean currents (tides, eddies, flow forced through the cavity from winds outside it, ...) [Laurence Padman, United States of America]	
4-1322	4	34	5	34	10	Recent studies have shown that some large ice shelves (e.g., Filchner-Ronne and Larsen) are strongly influenced by tides [Makinson et al., 2011, GRL; Mueller et al., 2012; JGR]. It is also true of Amery and Ross, i.e., the large ice shelves where ocean temperatures are fairly low so that meltwater plume speeds are also low. The importance of this is that changes in sub-ice-shelf geometry therefore affect the tidal currents and act as a feedback of further melting. This is not an "observation" and so maybe doesn't fit in this chapter. If so, ignore. However, it does point to a requirement for better sub-ice-shelf bathymetry maps, especially near grounding lines. I'm not sure where "important future observations" get flagged. [Laurence Padman, United States of America]	Paragraph has been deleted see response to 1319
4-1323	4	34	12			second paragraph of the subsubsection repeats much of the material in 4.4.3.1.2 becasue it is straying from ice-ocean interaction into ocean forcing. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Reject. The first paragraph was clearly repetative and has been deleted. The one refered to in this comment is not repetative.
4-1324	4	34	25			need to mention larsen b style collapse as a mechanism - this is different to the processes discussed in 4.4.3.2.5 and also warrants a mention in 4.4.3.1.1 [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Text added.
4-1325	4	34	27			The terminology break-up events need to be used with care. Here, the authors mean calving and hence in particular not a break-up event. Please change to calve off. Similar important: there are studies showing that calving is unlikely related to ocean swell (Bassis et al., 2008), while Brunt et al., 2011 suggests the Japan tsunami to be the cause of calving at Rupert Coast. [European Union]	RESPONSE TO BE ADDED.
4-1326	4	34	31	34	31	Regarding improve calving parameterizations - there ought to be reference to the "eigencalving" scheme of Levermann et al, which is at least as well motivated as the others cited. Levermann, A., Albrecht, T., Winkelmann, R., Martin, M. A., and Haseloff, M.: Universal Dynamic Calving Law implies Potential for Abrupt Ice-Shelf Retreat, submitted, 2011. Cited in Martin M. A., Winkelmann R., Haseloff M. , Albrecht T., Bueler E., Khroulev C, and Levermann A. The Potsdam Parallel Ice Sheet Model (PISM-PIK) – Part 2: Dynamic equilibrium simulation of the Antarctic ice sheet. The Cryosphere, 5, 727–740, 2011 www.the-cryosphere.net/5/727/2011/ doi:10.5194/tc-5-727-2011 [Government of Australia]	REPONSE TO BE ADDED
1327	4	34	35			The role of this section is unclear for me. What is the distinction between Section 4.4.3 and 4.4.4? Both refer mechanisms of changes, and some points in Section 4.4.4 (marine ice sheet hypothesis) does not really make rapid (ceturial or less) changs. [Kenichi Matsuoka, Norway]	Partially agree. Alter opening para to make clearer. Add to opening paragraph, references to sections in Ch13.Taking care not to make projections note where this section came from, i.e., the observations in the literature, and commentary on whether these might be unusual in context of the past and in some sense be irreversible? Also connect this paragraph to OBSERVATIONS
4-1328	4	34	35			A new section to summarize "remaining challenges" is quite helpful. AR4 actually generated a lot of efforts to investigate ice-sheet changes in the context of sea level rise. I wish that AR5 also shows such gudeline to the research community. [Kenichi Matsuoka, Norway]	Reject. Out of scope on advice of co-chairs
4-1329	4	34	37	36	25	Section 4.4.4 This seems like quite an important summary of potential 'tipping points'. Is it possible to draw the key mechanisms/rapid ice sheet changes into a table pointing out whether they have happened, are in the process of happening, are likely to happen soon or may happen at some point in the far future? [European Union]	Noted, and discussed, but we don't feel able to do this without overstepping into projections.
4-1330	4	34	38	34	39	I question whether "Considerable progress has been made since AR4." in understanding rapid ice-sheet changes. Much has been published, but we are still far from much quantitative understanding. This ignorance	Modified - considerable EFFORT will be used.

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						contributes much to the large uncertainties mentioned above. [Robert Thomas, United States of America]	
4-1331	4	34	39			sentence starting 'the processes ...' is odd. isn't it clear that they are underway otherwise the changes that caused AR4 to make its decision would not have been observed. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Modified. delete "now" in "processes now thought"
4-1332	4	34	40			seems odd to link a definition of rapid to sea level rise - why not say likely to affect ice sheet mass budget significantly on time scales of up to several decades.. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Agree - change made
4-1333	4	34	42			the statement about reversibility is tacked on for no apparent reason. This really is a related to projection and I do not see how observations of the current system can be used to directly say anything about this. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Disagree - the observations can certaintly indicate where types of changes are reversible, because they have been seen before, and this is important. See also 4-1327
4-1334	4	34	48	34	51	The point made in these lines simply repeats what is written earlier on the page, in the paragraph spanning lines 12-22. [Adrian Simmons, United Kingdom]	reject. This is in new section - so perhaps cross reference to more detailed discussion in 4.4.3.2.4
4-1335	4	34	48			May be "The important role that deep warm waters play in melting the priphery…" [Government of Chile]	reject
4-1336	4	34	50	34	50	"theoretical advances" -> "advances in theoretical understanding" would seem to be better [Thomas Stocker/ WGI TSU, Switzerland]	accept
4-1337	4	34	50			"rapid changes are to be expected". This seems "alarmist". It depends on a variety of things happening, including warming of the ocean that, for most places, has not been demonstrated. I don't think we know yet that present trends in ocean heat flux to the ice shelf cavities in guaranteed, given uncertainties in GCMs, and feedbacks with coastal stratification, sea ice, etc. Perhaps you mean "New observations, ... show that regions of ice sheets that are grounded below sea level are the most likely to experience rapid ice mass loss if the supply of ocean heat to the grounding zone increases." [Laurence Padman, United States of America]	accept Laurie's expanded sentence in modified, but with the addition of "especially, if…"
4-1338	4	34	50			not clear why rapid changes are to be expected in regions of the ice sheet grounded well below sea level - there are plenty such areas around antarctic where no change (rapid or not) has been observed. What is meant by this statement? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Same as 4-1337
4-1339	4	34	52	34	52	Joughin, Alley and Holland's review (Ice-Sheet Response to Oceanic Forcing) in yesterday's Science (30 Nov) would fit in well here. [Peter Barrett, New Zealand]	Accept - will check relevance and need for another reference
4-1340	4	34	54			mixing ice shelf thinning by increased bottom melt with calving here; two different porceses with likley different forcing; bunching them togther over simplifies things. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept - reword line 53 to read "can increase melting at the ice front" removing bottom melting from this sentence, ice-shelf thinning is still mentioned elsewhere.
4-1341	4	34	55			now adding further confusion by lumping in process that may affect calving but certainly do not affect basal melt and thinning of ice shelves. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Same as 4-1340
4-1342	4	34		36		Same remark as for sea-ice: the 2012 extrem record in Greenland Ice-sheet melting area (around 12 July and 29 July) should be mentioned, [Government of France]	Reject - this issue has been mentioned elsewhere in the observations sections
4-1343	4	34				Section 4.4.4: materials presented in this section seem to indicate that the bedrock and seafloor are dominating factors in rapid ice sheet changes. Does this suggest that geological factors (bedrock and seafloor) are the keys here rather than climatic factors? Please clarify. [Government of United States of America]	reject. Basal topography is important, but only one of many factors discussed in this section.
4-1344	4	35	1			not certain the link between thinning and weakening of the ice shelf has been proven - more likley is that thinning leads to less lateral and ice-rise drag and hence acceleration [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	accept? Delete "and Weakening".
4-1345	4	35	1			what is 'the' deep basin here? [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept it is unclear, delete "the deep basin" and replace "deep basins".

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4-1346	4	35	2			Is Thomas et al. [2011] an appropriate cite for this statement? [Laurence Padman, United States of America]	The I.D.Thomas 2011 paper in refs is not correct for this citation. Is the correct paper "Accelerating ice loss from the fastest Greenland and Antarctic glaciers" GRL, 38, 2011? --> (YES, ER).
4-1347	4	35	9	35	14	The choice of Pritchard et al 2012 as the sole reference here is inappropriate. This is an observational paper about ice shelf thinning, combined with general remarks about bathymetry, ocean temperature etc. that come from other research. The studies that show how ocean temperature and circulation changes bathymetry and sub-ice cavity geometry come into play ought to receive some recognition. [Government of Australia]	accept and add more relevant papers. One might be Galton-Fenzi, B. K., J. R. Hunter, R. Coleman, S. J. Marsland, and R. C. Warner (2012), Modeling the basal melting and marine ice accretion of the Amery Ice Shelf, J. Geophys. Res., 117, C09031, doi:10.1029/2012JC008214. And others .... have been added
4-1348	4	35	10	35	10	The choice of a recent paper (Jacobs et al 2011) about ocean heat delivery to the base of one ice shelf is an appropriate reference for statements on the influence of the ocean on the ice sheets. [Government of Australia]	noted, add new references, possible Pritchard, 2012, and several more e.g, Jenkins etc
4-1349	4	35	16			not certain why we need a rehash of marine instability here when there is a whole box on this in chpt 13. very hard to see how this paragraph is linked to the observations that we are supposed to be trying to interpret here. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Noted, but this section was required in Ch4 by plenary. Do need to make strong reference to Ch13, but have to go over this here. Check against what is written in the CH13 Box
4-1350	4	35	19	35	22	Cross-reference to chapter 5 required here for the paleo context. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
4-1351	4	35	19			Bamber et al., 2009 ~3.3m missing Line 32: Rott et al., 2011 missing [European Union]	Noted, will check this Add these also? (see also 4-1356)
4-1352	4	35	21	35	22	As I remember, Kopp et al shows rates of SLR but not the size of the ice sheet. [Christopher Little, United States of America]	Noted. Added "volume" in the sentence to avoid ambiguity with ice sheet size.
4-1353	4	35	30	35	30	replace ")(" as "," [Yongjian Ding, China]	accepted
4-1354	4	35	30	35	31	bracketing of Pritchard et al. and Joughin et al. references [Laurence Padman, United States of America]	same as 4-1353
4-1355	4	35	31	35	32	It is not clear why the sole reference to accelerated glacier flow following ice shelf collapse is Rignot et al 2004 and not the earlier H. De Angelis, P. Skvarca, Glacier Surge After Ice Shelf Collapse, Science 299, 1560 (2003). [Government of Australia]	Accepted - More references added. The original statement pointed to the 300-800% from one reference.
4-1356	4	35	32	35	32	The values reported by Rignot et al., 2004, are much higher than and not consistent with the ones reported by Rott et al., The Cryosphere, 5, 125–134, 2011. This fact and the reference to Rott et al. has to be included here. [Olaf Eisen, Germany]	Rejected. Rott et al. 2011 does not dispute the velocity change but presents a different estimation of ice fluxes.
4-1357	4	35	32			Scambos et al., 2004 (at least) should be cited together with Rignot et al., 2004 [Etienne BERTHIER, France]	Accepted - and Scambos
4-1358	4	35	35			the term rapid has been defined earlier as several decades - are we now really trying to suggest the loss of West Antarctic ice over a few decades? No references to support this speculation. This is arm waving. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accepted changed sentence to delete this phrase
4-1359	4	37	36		38	Scandinavian regions are underrepresented in this chapter. Dyrrdal et al have published some work additional to that mentioned. Painter et al., 2012 shows snow cover products from MODIS data in the Himalaya area. Work done by Painter and co-workers also cover other areas not mentioned here (e.g. in the US). Derksen, C. and R. Brown (2012) should also be included. [European Union]	From snow section - give to Phil
4-1360	4	35	37	35	38	See my comment on p4, line 37 [Christopher Little, United States of America]	Noted, but we can't identify this comment
4-1361	4	35	37			There is really no discussion of evidence that CDW supply of heat to Amundsen Sea ice shelves has increased, but presumably it has. Jenkins et al. [2010] really focus more on how the heat supply to the deep PIG cavity map have increased after PIG ungrounded from an ice rise some time after the mid-1970's. That's a feedback between topography (ice rises and pinning points) and increased heat flux, but doesn't address the	Accepted add the Jacobs et al, 2011 reference, (also Check Bindschadler et al., 2011, J. glaciol).

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						heat flux increase onto the coninental shelf in the first place. [Laurence Padman, United States of America]	
4-1362	4	?	?	?	?	In this section the work on freeze-up/break-up of lake ice and ice cover duration of Brown and Duguay 2010, Howell et al., 2009, Kang et al., 2012 and Duguay et al., 2012 is missing. [European Union]	Part of 4.5.5 - give to Phil
4-1363	4	35	39			the wingham obs relate to grounded NOT floating ice [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Accept. Remove "floating part"
4-1364	4	35	43			Section on role of water on the Greenland ice streams. The recent papers by Kjær in Science and Nature Geoscience (2012) on the front positions of the glaciers from aerial photos could be included in the discussion here. [European Union]	Noted, but references not relevant to the rapid ice sheet change chapter. References were however added in the discussion of regional changes in 4.4.2.2.3.
4-1365	4	35	43			seem to be jumping from greenland to antarctic and back again without much clear signposting. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Partially accepted "Similarly…" is the signpost. Reorder paragraphs, to put Ant Pen para, with Antarctic paras.
4-1366	4	35	43			not clear what the function of this paragraph is - it appears to relate to material discussed in 4.4.3.1.2 and 4.4.3.2.4. it repeats the now familiar link between warm water and glacier retreat without adding anything new. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Cross check contents with 4.4.3 and 4.4.4, but some overlap (with corss-referencing) is innevitable.
4-1367	4	35	53			again this would be better in 4.4.3.* [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Same 4-1366
4-1368	4	38	54		55	No brackets around authors name [European Union]	Phil's section again
4-1369	4	35	55	35	56	"Surface meltwater ... does not seem to be driving significant changes in basal lubrication" is somewhat contradictory to the statement on p33, line 30. [Olaf Eisen, Germany]	Reject. The same the paragraph goes on to say how important this process, not very. Add to 4-33 line 30 " ice flow speed, at least locally."
4-1370	4	35	55	35	57	The statement made does not motivate the consequences of paremeterizing lubrication in models in chapter 13, p.34, l1-3. Somehow contradictory. [Olaf Eisen, Germany]	Reject, our statement is about recent changes and observations. But the Ch13 issue is about the sensitivity and hypothetical future.
4-1371	4	38	0			There is no mention of the Great Lakes in this section on freshwater ice cover, and there have been dramatic changes in the Great Lakes over the last 20 years or more. [Dorothy Hall, United States of America]	Phil's section again
4-1372	4	36	2	36	8	Are these changes really irreversible? It is stated at the end of the paragraph that regrowth of the Larsen B ice shelf would take centuries if calving were to cease. But such regrowth does imply reversibility, even if it is a very slow process. [Adrian Simmons, United Kingdom]	Reject. Irreversible in this context defined (at 34/43-44) as "would take several decades to centuries to reverse under a different climate forcing." The statements are transparent and self-consistent
4-1373	4	36	2		3	May be "The ice shelves and glaciers on the Antarctic Peninsula have continued to experience irreversible changes,coincident with air temperatures rising at four to six times the global average rate at some stations (Vaughan et al., 2003), and with … [Government of Chile]	accept minor rewording
4-1374	4	36	2			I don't think this is the place to introduce "irreversible"; maybe just "large" here. On line 6, where the reason why a change is "irreversible" is explained, it is okay, although "centuries" is not "irreversible" unless you mean on decadal time scales and shorter only. [Laurence Padman, United States of America]	Reject. Irreversible in this context defined (at 34/43-44) as "would take several decades to centuries to reverse under a different climate forcing."
4-1375	4	36	5			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. The authors could begin 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	Noted, DGV to consider rewording. (No projections!)

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						smaller shelves have seen both decay and regrowth in the past several thousand years (Domack et al. 2005)." [Government of United States of America]	
4-1376	4	36	8			odd use of irreversible to mean that it would happen but take a few centuries [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Reject. Irreversible in this context defined (at 34/43-44) as "would take several decades to centuries to reverse under a different climate forcing."
4-1377	4	36	10	36	17	The Chen et al. [2011; JGR] GRACE paper seems relevant here. This shows a mass loss "wave" propagating clockwise around Greenland. So possibly the net loss from Greenland depends on the ability of some anomaly (ocean or atmosphere) to activate regional ice sheet mass loss. [Laurence Padman, United States of America]	Reject. We don't think this is not relevant.
4-1378	4	36	20	36	20	"likely key" - avoid informal use of 'likely', which is calibrated uncertainty terminology as layed out in the AR5 Uncertainty Guidance Note [Thomas Stocker/ WGI TSU, Switzerland]	Accept
4-1379	4	36	22	36	23	Again, please avoid statements that relate to what could/would aid our future understanding. [Thomas Stocker/ WGI TSU, Switzerland]	Accept
4-1380	4	36	25			not certain what 4.4.4 adds to the assessment. Much of the material it covers repeats themes already covered in the previous subsection. [Antony Payne, United Kingdom of Great Britain & Northern Ireland]	Reject, this specfic section was requested by Plenary and is included for that reason
4-1381	4	36	27	36	27	It isn't clear why snow and freshwater ice should be combined [Sharon Smith, Canada]	Accepted. These are now separate sections.
4-1382	4	36	27			I made extensive comments in my review of the FOD, that not mentioning snowfall or snow water equivalent (SWE) was a gap in Section 4.5. As now noted on page 36 line 31, snowfall is covered in Chapter 2. I suggest an explanation be added as to why SWE is not covered in this section (aside from some in situ measurements in Figure 4.21). If the sense is that remotely sensed and reanalysis datasets are too uncertain to allow SWE time series to be compiled, then this gap should be explicitly noted. Since AR4, significant progress has been made on SWE retrievals, including new approaches that combine microwave measurements, conventional observations, and snow emission modeling (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, 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]	Noted. As now discussed, our assessment is that in the data-rich northern hemisphere, the satellite microwave retrievals of SWE are of insufficient quality and duration to add much information to the NH record, but since there is nothing else in the SH except the 10 station records, the SWE time series has more relative value.
4-1383	4	36	27			Sec 4.5 is labelled seasonal snow cover, yet only the spring season is presented in the text and in Fig 4.19. Skeptics will accuse you of cherry-picking and point out that winter snow cover does not show a decline. So statements such as 'snow cover decreases are larget in the spring' are misleading. Please present ALL the data not just March -April and plot on an absolute scale rather than using 'anomalies' so that the relative size of the changes is clear. [Paul Matthews, United Kingdom]	Accepted. we have added a table with linear trends in each month for the satellite record (1967-2012).
4-1384	4	36	29			Section 4.5.1 - This background section only mentions snow and does not discuss freshwater ice cover. If this is to be a background section on snow and freshwater ice then freshwater ice should be included. It would seem that a separate section for snow is required. [Sharon Smith, Canada]	Accepted. Freshwater ice now has its own section
4-1385	4	36	32			Link to section 2.3.1.3 instead of 2.5.1.3 [Christoph Marty, Switzerland]	Accepted.
4-1386	4	36	33			"quantitative" could be omitted, as the word "metric" implies quantification. [Adrian Simmons, United Kingdom]	Accepted.
4-1387	4	36	35	36	35	Pls add to the explained acronyms: snow cover duration (SCD) [Government of Germany]	Accepted.
4-1388	4	36	38	36	40	To be corrected. The most comprehensive snow data set is over the former Soviet Union with several hundreds of stations having a quasi-complete daily snow depth record over more than 60 years. Many stations have also a record of SWE and density 3 times per month.These data are accessible from the NSIDC web site. After 1995, a significant part of these stations continue to make daily snowx depth observations but the recent records do not benefit from the quality-control which was performed before. This data set has been used to build the Eurasian part of the NH SCE (Fig 4,19) record over the period before the satellite-era. [Government of France]	Accepted. Text revised.

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4-1389	4	36	40	36	40	I suggest to delete 'except in certain parts of the European Alps'. Particulary when not providing more informatioin about these 'certain parts in the European Alps' I find this statement highly delicate from a 'politicla' point of view. [Nadine Salzmann, Switzerland]	Accepted. Text revised.
4-1390	4	36	44	36	44	Change 'vegetated' to 'forested' [Chris Derksen, Canada]	Accepted
4-1391	4	36	44	36	46	This sentence is a bit puzzling. Is there really a measurement challenge? If snow rarely falls in the Southern Hemisphere outside mountainous areas, is its measurement that important? Or is the challenge the measurement of snow in the mountainous regions? Is this much more of a problem in the Southern Hemisphere than the Northern Hemisphere? Is Antarctica excluded from this discussion? Some rewriting here might be helpful. [Adrian Simmons, United Kingdom]	Accepted. Text revised.
4-1392	4	36	45			Delete the word "rarely." It isn't rare to have snow accumulate in the SH outside of mountainous areas, but such snow cover is not typically extensive or long-lived. So I think the word "rare" should be changed. [Dorothy Hall, United States of America]	Accepted.
4-1393	4	36	50			Section 4.5.2 - This hemispheric view only discusses snow (see comments above) [Sharon Smith, Canada]	Accepted. Freshwater ice now has its own section
4-1394	4	36	52	37	16	Somewhere in section 4.5.3, it would sem to be essential to report the results of Pederson et al. (Pederson, G.T., S.T. Gray, C.A. Woodhouse, J.L. Betancourt, D.B. Fagre, J.S. Littell, E. Watson, B.H. Luckman, and L.J. Graumlich. 2011. The unusual nature of recent snowpack declines in the North American Cordillera. Science 333: 332-335). They reconstructed snowpack time series from 1200 to 2000 AD for western North America using tree ring chronologies calibrated by field measurements of snowpack in the contemporary period. They found that 20th century snowpack declines are the greatest in the period examined and that anthorpogenic climate change explains more of the detected snowpack decline than inter-annual or decadal climate variability. [Patrick Gonzalez, United States of America]	Rejected. Reconstructions of this sort belong in the paleoclimate chapter and this comment has been forwarded to that chapter.
4-1395	4	36	54			Significant reductions in SCE -- should add the words "in SCE" [Dorothy Hall, United States of America]	Accepted.
4-1396	4	36	56	36	57	The first clause of this sentence is provided as a fact: "Snow cover decreases are largest in the spring period...". However, the second clause reads as a possibility because of the word "potential": "...the rate of decrease increases with latitude in response to greater POTENTIAL for albedo feedbacks..." These two clauses should not be joined by the word "and", but should either be separated, or the word "potential" removed. [David Rupp, United States of America]	Accepted.
4-1397	4	36	57			Line 57 and elsewhere – please add the accent to the last name of Stephen Déry [Dorothy Hall, United States of America]	Accepted.
4-1398	4	36				Section 4.5.5: It is not clear from the description how many lakes are represented by the statistics presented, nor their distribution around the Northern Hemisphere. Some of the references describe North American lakes and "Canadian lakes" and "Canadian Archipelago" are mentioned. Are other Northern Hemisphere lakes adequately represented? The rate of change in date of freeze-up and break-up (ice cover duration) is used as a metric in the evaluation. Should other metrics such as reduction in ice cover (concentration or area), ice thickness, warming water and air temperatures, or their affect on ecosystem (increasing algal blooms) also be considered? [Government of United States of America]	Accepted. Originally we focused on very long records, which restricted us to a single metric and 39 lakes, but for geographic completeness we are adding the 1973-2010 record for the Great Lakes and other studies.
4-1399	4	36				Section 4.5.2: Suggest a discussion of uncertainty in this section. [Government of United States of America]	Accepted. Will provide uncertainty estimates wherever possible
4-1400	4	37	1	37	2	Again, need to be careful with use of the term 'accelerating'. The loss of spring SCE is statistically significant over the past decade, and this loss is greater than any other point in the satellite record, but the year to year rate of change is quite consistent during this decade of snow reductions so statistically there is no 'acceleration'. Also, change 'minimum sea ice extent' to 'September sea ice extent'. [Chris Derksen, Canada]	Accepted. Text revised.
4-1401	4	37	2	37	2	and exceeds the loss rate for minimum sea ice extent...' - not clear to us what the sentence is trying to say here. What is the connection between SCE loss and sea ice rate of loss? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text revised.
4-1402	4	37	3	37	4	To be unambigious, it would be better if it read "(7 Mkm^2 lower)". [David Rupp, United States of America]	Accepted. Text revised.

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4-1403	4	37	3	37	4	This value of 7 Mkm^2 is not supported by Fig. 4.19. In that figure, it appears to be only about 2 Mkm^2 lower. [David Rupp, United States of America]	Accepted. Text revised.
4-1404	4	37	3	37	8	line 3 (Derksen and Brown, 2012) , has been published in the meantime : Derksen,C. and R. Brown (2012), Spring snow cover extent reductions in the 2008-2012 period exceeding climate projections. Geophys. Res.Lett.,L19504,doi:10.1029/2012GL053387. With view to most recent insights into trends in the Arctic I like also to recommend to the Lead Authors of this paragraph to add a few lines more about the findings reported in that paper. [Herbert Lang, Switzerland]	Accepted in part. Derksen and Brown will be cited, but we already have two lines about the June SCE trends and cannot afford more text.
4-1405	4	37	4	37	4	Mkm2 is an inelegant unit - use scientific notation [Richard Essery, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised to use 10^6 km^2 for consistency with sea ice section
4-1406	4	37	4	37	15	Which regions of the European Alps were included in the Mary and Meister (2012) study? Even just stating whether this included the French, Italian, Austrian and Swiss Alps or a selection of those is useful to guage the scope of the results from that study. Also, what sort of changes in atmospheric circulation were found to result in declines in spring snow depth in the Pyrenees? [European Union]	Accepted. Text revised.
4-1407	4	37	6	37	16	Whilst within the WG1 report it seems legitimate to use different ways of expressing trends depending on the variable, mixing units for a particular variable, especially within a particular section would be better avoided. Here we have "days per decade" on line 6, "days a–1" on line15 and "day a-1" on line 16. If you stick with "day a-1", I would not pluralise it to "days a-1" for decimal fractions of less than one day. One speaks of "half a day" not "half a days". [Adrian Simmons, United Kingdom]	Accepted. Striving to use days/decade throughout the chapter.
4-1408	4	37	10	37	11	The Dyer and Mote (2006) study only includes observations to 2000, so this does not represent any updated information since AR4. As this study was based on re-gridded in situ observations, why is it not included in Section 4.5.3? [Chris Derksen, Canada]	Accepted. citation moved.
4-1409	4	37	10	37	11	The average southern-most extent of wintertime Northern Hemisphere snow cover has remained virtually unchanged during the period of record [JAMES FOSTER, U.S.A.]	Noted.
4-1410	4	37	10			"For North America" would be better than "In North America" [Adrian Simmons, United Kingdom]	Accepted. Text revised.
4-1411	4	37	15	37	16	It is not clear if - 1 day / a means earlier or later end to melt in the context of this sentence [Richard Essery, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
4-1412	4	37	15	37	16	Should this be consistent with the sea ice terminology, e.g., “per decade” instead of “per year”? See elsewhere also. [Dorothy Hall, United States of America]	Accepted. Striving to use days/decade throughout the chapter.
4-1413	4	37	20	37	20	"x's" can be replaced by "cross" [Tao Che, China]	Rejected. Less visual translation required if the symbol is shown rather than its text description.
4-1414	4	37	20	37	20	"x's" can be replaced by "cross". [Jing Ming, China]	See response to previous comment.
4-1415	4	37	24	37	27	"…for a well-understood reason." Which is the "well-understood reason" here? That heat melts snow, or that there is a snow cover-albedo positive feedback? One does not have to evoke the albedo feedback to get a correlation between spring temperature and SCE, yet this is what appears to be implied due to the way the second sentence follows the first in this paragraph. Some rewording is in order here. [David Rupp, United States of America]	Accepted. Text revised.
4-1416	4	37	27	37	28	“Indeed, the observed declines in land snow cover and sea ice have contributed roughly the same amount to reductions in the surface energy fluxes” is a difficult sentence to comprehend. If the decline of snow cover and sea ice reduced the albedo, this would increase the surface fluxes. Did I misunderstand the whole thing? [Atsumu Ohmura, Switzerland]	Accepted. Text revised.
4-1417	4	37	31	37	31	The statement regarding the 46% decrease in snow cover extent happening at the Arctic albedo feedback maximum may be misunderstood to imply that this decrease will have a widespread impact over the Arctic or perhaps global climate system. However, the surface area of the arctic where the actual decrease is happening is very small compared to the rest of the area of the arctic and the global system. Propagation of this decrease to the impact on the regional or global surface albedo feedback suggests it is not significant	Noted. Making statements about the climatic significance of this change is beyond the scope of this section.

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						compared to the decrease in sea ice extent (Flanner et al. 2007 cited in the document). [Government of Canada]	
4-1418	4	37	36	37	36	Further consideration is required as to whether there is sufficient evidence to suggest that "snow metamorphosis has accelerated due to higher temperatures". This statement is referred to un Flanner et al. 2007, but this study diagnosed snow metamorphism only on snow covered grid cells in April and May. Fernandes et al. 2007 (also cited elsewhere in Chapter 4) indicate that metamorphosis is substantial during partial snow cover conditions, when snow is quickly melting. Neither Fernandes et al. 2007 or Flanner et al. 2007 report trends in snow albedo but unpublished data (Fernandes et al. 2007) indicate that they are both noisy and extremely variable in space and time. Even if one were to constrain this statement only to snow covered areas, both Flanner et al. 2007 and Fernandes et al. 2007 indicate that they are unable to determine if observed changes in snow albedo are due to metamorphosis, changes in deposition, or changes in vegetation. [Government of Canada]	Accepted. Text revised. See also response to comment 4-226.
4-1419	4	37	37			In this figure caption it would be better to refer to "CRUTEM4" not "CRU" as there are lots of different CRU datasets - even if the reference indicates which one is used. [Adrian Simmons, United Kingdom]	Accepted. Text revised.
4-1420	4	37	40	37	45	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 well documented. [Chris Derksen, Canada]	Rejected. In our assessment of southern hemisphere snow data, the paucity of other records places more emphasis on the value of the passive microwave retrievals of SWE, and most of the retrievals are in the mountains which have a small footprint relative to the flat areas that contribute the most to interannual variability.
4-1421	4	37	40	37	45	“For the SH… there are no corresponding visible-wavelength satellite records…” This statement is not completely accurate. There are detailed MODIS-derived global, daily maps beginning in February of 2000 (Hall and Riggs, 2007). With respect to older records, there are papers (see Dewey and Heim, 1983; Romanov and Tarpley, 2001 and 2003) that provide longer-term measurements from NOAA satellites of the SH, though there is evidence that some of the older studies (e.g., Dewey and Heim, 1983) may have overestimated the amount of seasonal snow cover in the SH as discussed by Hall and Robinson (in press). The Foster et al. (2009) paper is the first to estimate snow extent and SWE (using passive-MW measurements). [Dorothy Hall, United States of America]	Accepted. Text revised “no correspondingly LONG”
4-1422	4	37	41	37	41	date (twice: typo) [Nadine Salzmann, Switzerland]	Rejected. Reviewer misread: It says “[the] datA datE from” not “date date”
4-1423	4	37	47			The following article may contain important information about observation trend in Japan. Tsutomu Yamanaka, Yoshifumi Wakiyama, Keisuke Suzuki 2012. Is snowmelt runoff timing in the Japanese Alps region shifting toward earlier in the year? Hydrological Research Letters. 6, p87-91. [Tosiyuki Nakaegawa, Japan]	Rejected. Runoff is covered in WG2.
4-1424	4	37	51	37	53	"temperatures" should be more clear, snow temperature, air temperature, or ground temperature? [Tao Che, China]	Accepted.
4-1425	4	37	51	37	53	"temperatures" should be more clear, snow temperature, air temperature, or ground temperature? [Jing Ming, China]	See response to previous comment.
4-1426	4	37	56			data obtained directly from the author – how can this be used without peer review? [Dorothy Hall, United States of America]	Noted. This comment refers to the digital data used to produce published figures. Text revised to clarify..
4-1427	4	38	0			Why is there nothing mentioned in this section about the Great Lakes? [Dorothy Hall, United States of America]	Accepted. See response to comment 4-1371
4-1428	4	38	4	38	5	This sentence says that some studies were not included because they did not include relative changes, yet the Bulygina (2011) study was included even though it did not include relative changes, as indicated by Fig. 4.21. [David Rupp, United States of America]	Accepted. Text revised.
4-1429	4	38	6	38	6	replace "; for annual mean snowfall" as ". For annual mean snowfall," [Yongjian Ding, China]	Accepted.

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4-1430	4	38	10	38	10	I did not find where "SCD" was defined anywhere in Section 4.5. [David Rupp, United States of America]	Accepted. Text revised - abbreviation defined.
4-1431	4	38	10			SCD is not defined earlier (or it is a typing error and it is SCE?) [Mauro Guglielmin, Italy]	Accepted. Text revised - abbreviation defined.
4-1432	4	38	10			There is use here of an undefined acronym "SCD". Is it meant to be "SD"? [Adrian Simmons, United Kingdom]	Accepted. Text revised - abbreviation defined.
4-1433	4	38	13	38	15	A more specific reference (not cited in Brown and Mote 2009) for the observed Australian trend at one of the four sites is Nicholls N 2005, Climate variability, climate change and the Australian snow season, Aust. Met. Mag., 54-177-185. [Government of Australia]	Accepted. Citation and update added (even though the paper was also cited in AR4)
4-1434	4	38	13	38	15	To say that six records "mostly show increases" is unecessarily imprecise. Please state the number out of six that show increases. [David Rupp, United States of America]	Accepted. Text revised. “Mostly” referred to a variety of periods of record.
4-1435	4	38	34	38	34	replace "changing" as "changed" [Yongjian Ding, China]	Rejected. Present tense verb form was correct as written.
4-1436	4	38	35	38	37	Instead of saying "darker snow grains that result from increased combustion…" it's more accurate to say lower snow albedo due to the deposition of black carbon from combustion of fossil fuels… Also, not clear how higher temperatures accelerate snow metamorphosis. Snow metamorphosis is either destructive (via blowing snow events), kinetic (in which case warmer temperatures slow metamorphosis) or melt induced. Any temperature induced change in albedo from a climate point of view would come from snow wetness and earlier snow melt. [Chris Derksen, Canada]	Accepted. Text revised.
4-1437	4	38	43	38	46	Studies in 1989 suggest snow albedo may have contributed to earlier spring snow disappearance at some high latitude (70 N) locations: Foster, J. L., "The significance of the date of snow disappearance on the Arctic tundra as a possible indicator of climate change," Arctic and Alpine Research, 21 (1), 60-70, 1989; [JAMES FOSTER, U.S.A.]	Noted.
4-1438	4	38	43			Make it clear that the 0.05 W/m2 refers to forcing by black carbon due to deposition on snow and ice, not just "interactions" (which could refer to black carbon absorption when suspended over snow and ice) (and that the number is global). It is not clear that the details about the Arctic wildfires in 1998 and 2001 are necessary for this sentence. [Government of United States of America]	Accepted. Text revised.
4-1439	4	38	48	39	29	As previously mentioned in ES part (Line 9-14 Page 5), lake and rive ice are very limited reported in the current researching papers. Chinese scientists have some supplementary work to this issue. River ice changes from east Asia were not presented in the SOD, we suggest cite more results from this region (e.g., Xiao et al., 2008). XIAO Cunde, LIU Shiyin, ZHAO Lin, WU Qingbai, LI Peiji, LIU Chunzhen, ZHANG Qiwen, DING Yongjian, YAO Tandong, LI Zhongqin, PU Jiancheng (2007). Observed changes of cryosphere in China over the second half of the 20th century: an overview. Annals of Glaciology, 46(1), 1-9. [Jing Ming, China]	Accepted. We will assess these and other papers for inclusion.
4-1440	4	38	50	39	3	There is another paper that reported the lake ice change in past three decades, and the result also can support the statement here. Che, T., Li, X., Jin. R. 2009. Monitoring the frozen duration of Qinghai Lake using satellite passive microwave remote sensing low frequency data. Chinese Science Bulletin, doi:10.1007/s11434-009-0044-3. [Tao Che, China]	Noted. With extremely tight space constraints we focus on papers that aggregate results for many lakes. This particular paper would cost $40 to purchase.
4-1441	4	38	50	39	3	There is another paper that reported the lake ice change in past three decades, and the result also can support the statement here. Che, T., Li, X., Jin. R. 2009. Monitoring the frozen duration of Qinghai Lake using satellite passive microwave remote sensing low frequency data. Chinese Science Bulletin, doi:10.1007/s11434-009-0044-3. [Jing Ming, China]	See response to previous comment.
4-1442	4	38	50	39	16	Somewhere in section 4.5.5, it would be good to report the results of Wang et al. (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. Journal of Climate 25: 1318–1329.) on lake ice trends on the set of the largest freshwater lakes in the world - in the Laurentian Great Lakes (North America). They found a statistically significant 71% decline from 1973 to 2010. [Patrick Gonzalez, United States of America]	Accepted. See response to comment 1371.
4-1443	4	38	50	39	16	Section 4.5.5 River and Lake Ice: The Great Lakes of North America comprise about 20% of the global fresh surface water, and merit a specific mention. In the 38 year period 1973-2010, the decline in winter lake ice for	Accepted. See response to comment 1371.

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						all the lakes was in the range 37-88%, with an average of 71%. There is a relevant reference from which information can be distilled: Wang, Jia, Xuezhi Bai, Haoguo Hu, Anne Clites, Marie Colton, Brent 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 [Henry Pollack, United States of America]	
4-1444	4	38	50	39	16	Please see comment 194. Note also that there is a "d/a" on line 11 of page 4-39. [Adrian Simmons, United Kingdom]	Not sure which his comment 194 is. Second part: Accepted. Striving to use days/decade throughout the chapter.
4-1445	4	38	54	38	55	replace "(Benson et al.,2012" as "Benson et al.,(2012)" [Yongjian Ding, China]	Endnote issue
4-1446	4	38	54			Line 54 and elsewhere Should this be consistent with the sea ice terminology, e.g., “per decade” instead of “per year”? [Dorothy Hall, United States of America]	Accepted.
4-1447	4	38		38		What about Southern Hemisphere lake freeze up and melt out data? [JAMES FOSTER, U.S.A.]	Accepted. We have located one paper on SH lake ice
4-1448	4	38				Section 4.5.5 Numerous reviewer comments questioned the representativeness of the studies used in this assessment. One suggestion was for the authors to explicitly address this issue. For example, what is the number of lakes globally relative to number of lakes in the study? Are they sufficient to be representative of the regions? In which regions are they located? [Government of United States of America]	Accepted. See response to comment 1371.
4-1449	4	38				Section 4.5.5: A valuable addition to the discussion of trends in lake ice in this section (4.5.5), especially in the Northern Hemisphere, would be the addition of trends in ice cover found on the Laurentian Great Lakes. Although airborne monitoring was carried out in the 1960’s, a 38 year (1973-2011) database has been developed, based (especially more recently) largely on satellite remotely sensed data, and analyzed for trends along with water and air temperature and other data. These trends appear to match the trends sited for smaller inland lakes, but are amplified owing to the large extent of the Great Lakes and affects on lake ecology and the socio-economics of the Great Lakes region. [Government of United States of America]	Accepted. See response to comment 1371.
4-1450	4	38				Section 4.5.5: Below are some excerpts from recent publications based on the 38 year database of ice cover concentration as well as a list of references and selected bibliography. “Lake ice cover is also a sensitive indicator of regional climate and climate change…” “Winter mean ice cover in all lakes shows a significant negative trend …., indicating that the ice extent in the Great Lakes has been decreasing since the 1970s.” “On Lake Erie, the least ice cover was found in 1983, 1991, and 1998, spaced by 7-8 years, but more frequently since 1998 with a period of about 3-4 years. This implies that interannual variability of the climate patterns tends to be greater in the Great Lakes in the past decade.” “The SAT [Surface Air Temperature] trend over the Great Lakes ranges from ~0.4 oC per decade over the lower lakes to ~0.6 oC per decade over the upper lakes, with Lake Superior being the highest (0.6 oC per decade). This is consistent with the upward trend of Lake Superior water temperature (Austin and Colman 2007). They found that summer (July– September) surface water temperatures have increased approximately 2.5°C over the period 1979– 2006, significantly higher than regional atmospheric warming.” “The regression of the first [Empirical Orthogonal Function] EOF-mode time series to sea level pressure, surface air temperature, and surface wind shows that lake ice mainly responds to the combined Arctic Oscillation and El Nino–Southern Oscillation patterns.” [Government of United States of America]	Noted.
4-1451	4	38				Section 4.5.5: References and Selected Bibliography	Noted. This comment seems to refer to the previous comment.

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						Assel, R.A., F.H. Quinn, G.A. Leshkevich, and S.J. Bolsenga. Great Lakes Ice Atlas (NOAA Atlas No. 4). DOC, NOAA, OAR, Great Lakes Environmental Research Laboratory, Ann Arbor, Michigan 48108 (1983). Wang, J., R.A. Assel, S. Walterscheid, A.H. Clites, and X. Bai. Great Lakes Ice Climatology Update: Winter 2006 – 2011 Description Of The Digital Ice Cover Dataset NOAA Technical Memorandum GLERL-155, DOC, NOAA, September 2012. Wang, J., X. Bai, H. Hu, A. Clites, M. Colton, and B. Lofgren. Temporal and Spatial Variability of Great Lakes Ice Cover, 1973-2010. Journal of Climate, 25:1318-1329 (2012). Austin, J.A., and S. Colman. Lake Superior summer water temperatures are increasing more rapidly than regional air temperatures: a positive ice-albedo feedback. Geophysical Research Letters 34, L06604, doi:10.1029/2006GL029021 (2007). [Government of United States of America]	
4-1452	4	38				Section 4.5.5: Suggest a discussion of uncertainty in this section. [Government of United States of America]	Accepted. Will provide uncertainty estimates wherever possible
4-1453	4	38				Section 4.5.5: Suggest the authors reconsider the conclusion that "very high-latitude lakes appear to be experiencing more rapid reductions in ice cover than those at lower latitude" This conclusion appears to come from one paper looking at Canadian lakes, excluding the Great Lakes. On contrary, Great Lakes have low ice in quantity or quality in consecutive years over the last decade, indicating drastic ice reduction in the Great Lakes. [Government of United States of America]	Accepted. See response to comment 4-1371
4-1454	4	39	5	39	16	“Although changes… (Prowse and Brown, 2010).” provides important information, i.e., that high-latitude lakes in Canada appear to be experiencing more rapid reductions in ice cover than those at lower latitudes. This is very sparsely referenced. There is only one reference for this assertion (Latifovic and Pouliot, 2007). More citations are needed to support this, or the wording should be changed to tie the result more closely to that one reference alone. It is an important point, and the reader needs to know if it is generally accepted or just reported for the first time in the Latifovic and Pouliot reference. [Dorothy Hall, United States of America]	Accepted. See response to comment 4-1371
4-1455	4	39	21	39	21	delete "too" [Yongjian Ding, China]	Rejected. Sentence was correct as written.
4-1456	4	39	22	39	23	Over what region has the cited 2-3 C warming occurred? [Government of Australia]	Accepted. Text revised.
4-1457	4	39	27			Please include references for the conclusions regarding acceleration of both delay in autumn freeze-up and advance in spring break-up. [Government of United States of America]	Accepted. Text revised.
4-1458	4	39	28	39	29	This statement on the seasonal assymmetry of snow cover extent trends should first be made (with a citation) in Section 4.5.2. [Chris Derksen, Canada]	Accepted. Text revised.
4-1459	4	39	35	39	37	This sentence implies permafrost to be defined as below 0°C AND frozen (containing ice). Permafrost is defined only based on temperature. [Stephan Gruber, Switzerland]	accepted
4-1460	4	39	35	39	45	Can this section also include a definition of the word 'talik' in addition to the nice explanation of permafrost? This will help non-expert readers. [European Union]	Rejected - this term is defined in the glossary - done
4-1461	4	39	35			Permafrost can also be not frozen, in fact it is a material that is at least for two consecutive years at temperature ≤0°C. [Mauro Guglielmin, Italy]	same as 1459 - accepted
4-1462	4	39	37	39	37	Avoid "over" as permafrost occurs underneath teh surface [Stephan Gruber, Switzerland]	editoral
4-1463	4	39	37	39	37	Permafrost does not occur over the land but occurs beneath the land surface. [Sharon Smith, Canada]	same as 1462 - editoral
4-1464	4	39	38	39	38	It may be better to say that permafrost can occur in the sea bed or beneath the sea floor. [Sharon Smith, Canada]	same as 1462 - editoral
4-1465	4	39	39	39	40	I suggest to delet this sentence. Of course permafrost is a sensitive climate indicator, but by far more complex than most other compornents of the cryosphere and the interpretation of change depending a lot also on soil and subsurface condtions. I would thus not explitlity stress that permafrost is a sensitive indicator. It is	disagree - text modified

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						sensitive, but not among the best inidicators. [Nadine Salzmann, Switzerland]	
4-1466	4	39	39	39	44	It is also important to mention that permafrost may contain signicant amounts of ice which is the reason that it has the various impacts mentioned (note since permafrost is defined only on the basis of temperature it may or may not have a significant amount of ice). Terms like "Dramatic" should probably be avoided when commenting on implications of changing permafrost conditions. [Sharon Smith, Canada]	accepted. Ice-rich permafrost degrades"
4-1467	4	39	44	39	45	Not only carbon stored in permafrost soils but also substantial amounts of nitrogen. N2O emissions from permafrost soils have recently been highlighted (Marushchak et al 2011, Hot spots for nitrous oxide emissions found in different types of permafrost peatlands GLOBAL CHANGE BIOLOGY DOI: 10.1111/j.1365-2486.2011.02442.x; Repo et al 2009 Large N2O emissions from cryoturbated peat soil in tundra Nature Geosciences 2, 189-192 [European Union]	accepted. Add "also nitrogen".
4-1468	4	39	45	39	48	I see no reason why the amount of carbon in permafrost soils will be increased upon permafrost thawing. Chapter 6 indicates a decrease. Delete from 'increases' up to the next comma [Ko Van Huissteden, Netherlands]	editoral, revise sentence for clarity
4-1469	4	39	46	39	46	Include N2O here too [European Union]	accepted.
4-1470	4	39	46	39	47	None of the three references here address the CH4 issue. Suggest adding a reference to either a modelling study that deals with both the CO2 and CH4 contributions and/or a field study focused on the CH4 response. [Government of United States of America]	accepted - additional references apply.
4-1471	4	39	46			Define talik [Government of France]	Rejected, term is defined in glossary
4-1472	4	39	46			Thaw exposes frozen soil, not just carbon. Microbial degradation of carbon is only one vulnerability important to climate. See for example Figure 1 of Harden et al, 2012, GRL on permafrost. The authors should note that not only decomposition but also combustion of peat layers, and hydrological shifts that dictate redox and gas species are important. Ecological shifts resulting from C and N release also contribute to the permafrost carbon feedback. If the authors wish to discuss permafrost carbon release the text should be inclusive. [Government of United States of America]	possibly add another sentence and references
4-1473	4	39	48	39	49	Suggested revision "..through impacts on the landscape, vegetation and infrastructure." [Sharon Smith, Canada]	accepted. "through its impacts on …" see WG2 for more details.
4-1474	4	39	48	39	49	In particular when permafrost degradation causes damage to oil and gas infrastructure, this could cause additional greenhouse gas release. [Ko Van Huissteden, Netherlands]	this is a WG 2 issue, will not be covered here.
4-1475	4	39	53			Section 4.6.2.1 Mention the geothermal heat flux as a cause for variations of the locations and depth of permafrost. [European Union]	agree. Additional sentences will be put in the Introduction section.
4-1476	4	39	53			Section 4.6.2.1. For most of the recent work done to assess trends, temperatures at the depth of zero annual amplitude (or measurement depth closest to it) have been utilized as these are more appropriate for examining long-term trends -- filter out shorter term variations. These are usually depths of 10-20m depending on location, site characteristics etc. For the discussion of trends, it is best to focus on these deeper temperatures which also make it easier for comparison etc. [Sharon Smith, Canada]	referring to the paper by Romanovsky et al (2010). The reviewer is also a co-author. Disagree.
4-1477	4	39	55	39	55	Please note that variations in permafrost ice contetnt also effect the physical and thermal state. [Government of United States of America]	accepted. Temperature and ice content are the key parameters that determine physical properties.
4-1478	4	39	55	39	55	This sentence is somewhat circular, stating that "temperature determines the thermal state". [Stephan Gruber, Switzerland]	same as above
4-1479	4	39	55	39	55	Why not just say "Permafrost temperature is a key indicator of its thermal state". [Sharon Smith, Canada]	same as above
4-1480	4	39	55			I will omit the first sentence because temperature is obviously the key parameter because is for definition a physical (thermal) state of the material. [Mauro Guglielmin, Italy]	same as above
4-1481	4	39	56			MAGT should be refereed only at ZAA to compare the data because, especially in the rock between 10 m to the ZAA the difference could be 0.2-0.3°C that is surely not much but in some cases can be significant. [Mauro	covered in 1476

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						Guglielmin, Italy]	
4-1482	4	39	57	39	57	Plase define "permafrost table". [Government of United States of America]	accepted. Define permafrost table.
4-1483	4	39		43		What about Southern Hemisphere permafrost and seasonally frozen ground? [JAMES FOSTER, U.S.A.]	disagree - there are limited papers available. Add more references from SH.
4-1484	4	39				Section 4.6 Frozen Ground: The draft section gives many examples of investigated changes in frozen ground, but does not give any global, hemispheric, or regional summations, such as the change in area or volume of continuous or discontinuous permafrost over time, or estimates of the amounts of water or greenhouse gasses released by changes in frozen ground. Such information is important for the comprehensive understanding of climate change effects on the cryosphere. If meaningful quantitative summaries are not yet possible, explanations of why this is so and of what additional work will be needed to provide them could be included. [Government of United States of America]	agree. Add a short paragraph as a synthesis for the frozen ground section.
4-1485	4	39				Section 4.6. I think it is good to view mountain and lowland permafrost integratively. However at some point in the section it might be good to describe how the boundary conditions of both types are similar/different and what consequences that has for response to climatic changes and impacts. [Andreas Kääb, Norway]	disagree. This is covered to some extent, in defining the climate variables that are significant, but we cannot be too specific in the text here.
4-1486	4	40	1	40	15	Is all this material really necessary? One of the important points that should be made in this introductory paragraph is the considerable effort made during IPY to enhance the observation network and to establish a baseline against which change can be measured as discussed in Romanovsky et al. (2010a). [Sharon Smith, Canada]	Agree, this section has been shortened a bit. The addition of statements wrt baseline efforts has been added, but mention of IPY is not allowed/recommended.
4-1487	4	40	1			This sentence is not correct ZAA could be deeper than 20 m especially in rock borehole where can reach also 27 meters (Guglielmin et al., 2011) [Mauro Guglielmin, Italy]	accepted. 20 m or deeper in occasionaly cases.
4-1488	4	40	2			Instead of “soil properties” I will suggest “rock/soil properties”, in fact many permafrost monitoring boreholes are within bedrock . [Mauro Guglielmin, Italy]	Sentence deleted
4-1489	4	40	3		5	Sentence is not clear because the mean annual ground ground temperature above the ZAA and at ZAA can differ also by 1°C and more (eg. Guglielmin et al. 2011). A generic above ZAA is to be avoided. [Mauro Guglielmin, Italy]	disagree. We are talking about "mean annual" temperatures within 20 m or within ZAA or at ZAA.
4-1490	4	40	5		6	The MAGT reported as -23.6°c is a MAGT close to the surface of a special case of high elevation and inland site, general temperature are higher ranging between -13.3 and -19.4 at the ZAA (Guglielmin et al.,2011; Guglielmin 2012) but in Maritime Antarctica temperature are much higher generally ranging between -3.1 and -0.5°C (Vieira et al., 2011). [Mauro Guglielmin, Italy]	noted.
4-1491	4	40	8	40	12	Note that this discussion on spatial distribution of permafrost temperature largely comes from Romanovsky et al. (2010a) and it is suggested you use it and remove the other two references. [Sharon Smith, Canada]	noted.
4-1492	4	40	12	40	12	The concept of boundaries/limits, imagined as a line that can migrate,should be avoided because: (a) it is misleading, suggesting a rather homegenous phenomenon, and (b) because this usually cannor be defined or measured in a clear way. [Stephan Gruber, Switzerland]	Sentence deleted
4-1493	4	40	12	40	13	We presume this southern limit of permafrost is relevant only for the Northern Hemisphere. Please specify. [Thomas Stocker/ WGI TSU, Switzerland]	agree. We will make a proper editing.
4-1494	4	40	12	40	14	The term "southern" for the "warm" limit of permafrost occurrence is problematic as it refers to the northern hemisphere only. A reference to the most recent state-of-the art report on mountain permafrost should be added (Haeberli, W., Noetzli, J., Arenson, L., Delaloye, R., Gärtner-Roer, I., Gruber, S., Isaksen, K., Kneisel, C., Krautblatter, M. and Phillips, M. (2010): Mountain permafrost: Development and challenges of a young research field. Journal of Glaciology 56 (200; special issue), 1043-1058.). This paper also makes clear that high-mountain permafrost ideed even occurs in tropical mountain peaks and can relate to severe slope stability problems. The somewhat oldish reference to the map by Brown et al. (1998) should be replaced or at least be	Agreed Sentence deleted

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						complemented by the new GoogleEarth-compatible worldwide simulation including mountains by Gruber (2012): Gruber, S., (2012): Derivation and analysis of a high-resolution estimate of global permafrost zonation. The Cryosphere 6, 221-233. doi:10.5194/tc-6-221-2012. [Wilfried Haeberli, Switzerland]	Sentence deleted
4-1495	4	40	14	40	14	Replace "altitude" with "elevation" [Stephan Gruber, Switzerland]	Sentence deleted
4-1496	4	40	14	40	14	Why is the number of 26° mentioned here? There is also permafrost on equatorial mountains in South America (Gruber, S. (2012): Derivation and analysis of a high-resolution estimate of global permafrost zonation, The Cryosphere, 6, 221–233). [Stephan Gruber, Switzerland]	Sentence deleted
4-1497	4	40	17	40	20	It is suggested that reference be made to Zhao et al. (2010 - already in ref. list) rather than Zhou et al (2000) for Asia/China as it is more up to date. [Sharon Smith, Canada]	noted.
4-1498	4	40	32		35	The concept of “warm and cold permafrost” is not well defined, because, as already explained earlier, the slight differences between the MAGT at the ZAA and at 10-15 m can be shift the borehole in a warm or cold permafrost. In addition the MAGT of -2°C seems quite arbitrary and it is not clear why is so important. Ice content in permafrost and areal extent of permafrost (discontinous or continuos) are concept that should be more enphatised for their implications also on the thermal properties of the rock/soil and therefore also on permafrost thermal regime and its changes. [Mauro Guglielmin, Italy]	Reject. This is intented as a general rather than specific distinction, made to drawn attention to a specific feature of the data. It's not a definition.
4-1499	4	40	33	40	40	While I agree that we need to distinguish between cold and warm permafrost, this section could probably be much shorter and simply refer to Romanovsky et al (2010a) which covers most of this. [Sharon Smith, Canada]	noted.
4-1500	4	40	35			Table 4.7 should also provide information for the European Alps and Scandinavia, where monitoring takes place at a high level of quality and intensity. The numbers given in this table are somewhat misleading: It should be made clear that they refer to individual site observations rather than to entire regions. [Wilfried Haeberli, Switzerland]	agree. Will add European Alps, but Scandinavia (Nordic countries) is already included. Pointed out that each line refers to one or more sites.
4-1501	4	40	39			The principle being referenced is that heat absorbed by partial melting of interstitial ice of warm permafrost attenuates the climate-induced, downwelling temperature wave. An explanation similar to this would be preferable to "…due to latent heat effect." [Government of United States of America]	agree. Will make changes accordingly.
4-1502	4	40	40	40	41	It is probably OK to just refer to Romanovsky et al (2010a) here (i.e. Riseborough 1990 can be removed). It is also suggested that the sentence be revised " ... due to latent heat effects as temperature approach 0°C..." [Sharon Smith, Canada]	disagree. Dan is the first person recognized the issue which now becomes more significant widely. Phrase modified
4-1503	4	40	41	40	43	There appears to be a problem with the magnitude of the temperature increase and an increase of 2°C since the 1970s is probably more appropriate. It would appear that in some cases the authors have used a trend for the last decade and extended that for 30 years which is not appropriate as the rate of change is not uniform as is clearly shown in Figure 4.22. Note in the SWIPA document an increase of up to 2°C since the 1970s was given which would be appropriate for cold permafrost (and is based on the same references that are cited here) - This is for depths 10-20 m and close to ZAA. It might also be better if the authors quoted rates over a time period rather than total change. See additional comments on Table 4.7 [Sharon Smith, Canada]	disagree but will make it consistent through text and with other reports. Here we stated clearly it is "mean annual ground temperatures within the ZAA or at ZAA", rather than a single measurement within ZAA. Check with references on magnitude of changes.
4-1504	4	40	42	40	43	Can the author explain why permafrost warming occurred in 1980s and 1990s but not in 2000s? [Government of United States of America]	Discussed, but no we cannot and this might overstep the mark on attribution, Ch10. Sentence revised
4-1505	4	40	42	40	43	This statement regarding the timing of the warming is not quite true. While there may have been less warming since the 1990s in western Canada and Alaska, this is not the case for Eastern and High Canadian Arctic or northern Scandinavia where there has been recent warming (in the case of Eastern Canadian Arctic there was	Accepted. Will make it more clear. There were high variabilites in different regions.

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						cooling until the mid 1990s). See for example Smith et al. (2010) and Romanovsky et al. (2010a). The important point here is that the timing of the warming has varied spatially. [Sharon Smith, Canada]	
4-1506	4	40	44	40	44	Could the authors clarify what is meant by "isothermal"? Does this mean that temperatures have not changed? It could be more accurate to say that no significant trend has been detected in these regions, as there is interannual variability. [Government of United States of America]	Accepted. Will explain "isothermal with depth".
4-1507	4	40	47	40	47	Can the most recent references just be used here, i.e. Zhao et al (2010), Wu et al. (2012) [Sharon Smith, Canada]	Accepted.
4-1508	4	40	49		50	It remains unclear to the reader how permafrost can thaw if the temperature does not change, assuming that it is not related to pressure. [European Union]	Accepted. Change made to text, but detailed physics is not appropriate here.
4-1509	4	40	50	40	50	Romanovsky et al (2010a) or Smith et al (2010) can be cited here instead of Riseborough (1990) as they specifically comment on the temperatures in southern disc. zone and are more up to date. [Sharon Smith, Canada]	Accepted.
4-1510	4	40	51	40	51	This statement regarding periods of cooling is not quite true. There have been longer periods of cooling related to decreasing air temperatures such as that which occurred in the eastern Canadian Arctic until the mid 1990s (see for eg. Smith et al. 2010). It is also important to note that there are areas where permafrost may be aggrading such as in coastal areas where uplift is occurring or in areas where there are shifting river shorelines and deposition is occurring. [Sharon Smith, Canada]	Accepted. Will check and be consistent. Not aware of examples
4-1511	4	40	55	41		Table 4.7. There appear to be some errors in the table. For northern Alaska, Smith et al. 2010 or Romonovsky et al. (2011 - Arctic Report Card) will have the most recent data up to 2009 or later so they should perhaps be cited rather than Osterkamp (2007). Note that nowhere in Smith et al. is 3.1°C given as an increase in temperature between 1980s and 2009 (all temperture increases for this period are <3°C as is the case for the recent Arctic Rept Card article). The authors may have confused rates at 10-20m with rates of surface temperature increase (or with changes occurring over a much longer period). Reference: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.http://www.arctic.noaa.gov/reportcard [Sharon Smith, Canada]	Accepted. will use Arctic Report Card data. Will check the magnitude of changes in pf tempertures.
4-1512	4	40	55	41		Table 4.7 For the Canadian Arctic Archipelago, the total increase at depths of 12-15 m (as shown in Figure 4.22 and figures in Smith et al. 2010) is only about 2°C so the table needs to be revised. For Interior Alaska it is suggested that you refer to Smith et al (2010) and/or Romanovsky et al (2011) as these will cover the period 1985-2009 (Osterkamp does not cover period to 2009). [Sharon Smith, Canada]	Accepted. Will check with numbers but disagree with references.
4-1513	4	40	55	41		Table 4.7 Burn and Kokelj 2009 is not relevant to Central and Southern Mackenzie Valley and should be removed from this section of the table. For Northern Quebec, Smith et al. (2010) is more up to date and covers the period indicated so Allard et al. 1995 can be removed. For European and Asian sites, the authors should consider only including those references that cover the period indicated which would be the more recent ones. [Sharon Smith, Canada]	Accepted. Will check exact locations from Burn.
4-1514	4	40		41		Table 4.7 I think that for Antarctica it could be more correct include two lines representing the two main and	Accepted. Will add new references by Bockheim et

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						contrasting climatic areas of the continent: Maritime Antarctica -0.5 to -3.1°C , with 20-25 m(Vieira et al., 2010) and Continental Antarctica (-13.9 to -19.5°C, with 20-27 m, Guglielmin et al., 2011; Vieira et al., 2010) [Mauro Guglielmin, Italy]	al., (2013).
4-1515	4	41	2	41	7	It seems that "changing HYDROLOGY INCLUDING snow" would be a better statement in this fist sentence. Warm rains can greatly impact thawing as well as snow cover. Also it is appropriate to mention smaller scale variations (heterogeneity) are affected by soil and vegetation (perhaps site "environmental protection" of permafrost. For example, surface soil temperatures varied by 7 deg MAT owing to such protection See Jorgenson et al, 2010, Can Jour For REs 40:1219 [Government of United States of America]	disagree. The main factors are air temperature and snow cover.
4-1516	4	41	2	41	7	This section is rather confusing. Changes in snow cover can play a role every where not just in the tundra. Note that Isaksen et al. (2011) doesn't really say much about forested sites. It is suggested that the authors consult Smith et al. (2012) which focusses specifically on the differences between forested and tundra sites. Throop et al. (2012) may also be relevant to this discussion. As mentioned previously Riseborough 1990 can probably be removed as Romanovsky et al (2010a) is sufficient). References: Smith, S.L., Throop, J., and Lewkowicz, A.G. 2012. Recent changes in climate and permafrost temperatures at forested and polar desert sites in northern Canada. Canadian Journal of Earth Sciences, 49: 914-924. Throop, J., Lewkowicz, A.G., and Smith, S.L. 2012. Climate and ground temperature relations at sites across the continuous and discontinuous permafrost zones, northern Canada. Canadian Journal of Earth Sciences, 49: 865-876. [Sharon Smith, Canada]	agree. Will check with these references. Done Done
4-1517	4	41	8	41	8	You need to be careful using terms such as "acceleration of degradation". Is the rate constantly increasing over time or is the current rate of degradation simply greater than the past rate. It may be more appropriate to say that the rate of degradation is higher than it was two decades ago if there is no real evidence to indicate that the rate is accelerating. [Sharon Smith, Canada]	Accepted. Refers to page 41, line 18.
4-1518	4	41	9			Section 4.6.2.2 - In this section there is a mixing of impacts of climate change with the indicators of climate change. In some cases the examples associated with land stability, thawing permafrost (or climate change) may not be the main triggering factor. It is suggested that some of the material is more appropriate in WG2 which deals more with impacts. [Sharon Smith, Canada]	disagree. The text refers changes in permafrost, not consequent changes or human activity, environmnetal, etc.
4-1519	4	41	11	41	11	area extent of …provide complete information. [Government of Kenya]	disgree. Text is clear.
4-1520	4	41	11		12	"In particular, the degradation may be manifested by the thickening of the active layer…" may be sounds not to be sure. See if it can be changed by …degradation manifestes by the thickening of the active layer…or just change may by can. [Government of Chile]	Accepted
4-1521	4	41	13	41	14	The concept of boundaries/limits, imagined as a line that can migrate,should be avoided because: (a) it is misleading, suggesting a rather homegenous phenomenon, and (b) because this usually cannor be defined or measured in a clear way. [Stephan Gruber, Switzerland]	disagree. This terminology is used widely in the community. It is well understood that the boundary does not imply a line.
4-1522	4	41	16			Permafrost degradation can be identified through … destabilized rock glaciers …'. Destabilizing rock glaciers may in part degrade due their disintegration, but in principle permafrost degradation and rock glacier destabilization have not necessary a causual connection, right? The destabilization may happen due to a rise in ground temperature, not necessary its rise above zero. [Andreas Kääb, Norway]	Accepted. Text modified

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4-1523	4	41	19			I think that it could be useful to add the following sentence " Even in an area where air temperature still almost stable or in slight decrease as in Continental Antarctica (Victoria Land) since 1996 permafrost temperature increase of 0.1°C per year and the active layer is thickening of 1cm per year (Guglielmin and Cannone, 2012). [Mauro Guglielmin, Italy]	Rejected, it's not clear what the suggested sentence means, or why it's relevant here.
4-1524	4	41	21	41	22	It should be clear that it is warm permafrost that has thawed [Sharon Smith, Canada]	Accepted. Will add "warm permafrost".
4-1525	4	41	23	41	23	The concept of boundaries/limits, imagined as a line that can migrate,should be avoided because: (a) it is misleading, suggesting a rather homegenous phenomenon, and (b) because this usually cannor be defined or measured in a clear way. [Stephan Gruber, Switzerland]	Same as 4-1521
4-1526	4	41				Section 4.6.2.2: The data for temperature of cold and warm permafrost are nicely laid out, but it is a bit deceiving to not include somewhere a reference to the interplay between temperature change and degradation. Warm permafrost temperatures changed but perhaps their active layer depths or permafrost thickness (figure) changed more than has cold permafrost. The interplay is important: latent heat of state change might absorb energy within the permafrost layers but edges might degrade faster (for example Yoshikawa and Hinzman 2003,PPP 14(2) make this point). Sugesst an expanded discussion along with next section on degradation [Government of United States of America]	Rejected. This is a rather subtle point for this assessment. Will check carefully.
4-1527	4	42	5	42	6	A recent publication (Yoshihiro I, Ohta T, Kotani A, Fedorov A, Kodama Y, Maximov TC, Sap flow changes in relation to permafrost degradation under increasing precipitation in eastern Siberian larch forest, Ecohydrology, accepted) shows permafrost degradation and forest damage related to precipitation increase in the region of Yakutsk [Ko Van Huissteden, Netherlands]	Accepted. Check with the paper first. Cannot find paper discussed
4-1528	4	42	9	42	10	Is permafrost thaw the trigger for this erosion or is the natural process of coastal erosion due to wave action that occurs along all coasts. Permafrost thaw may be a secondary cause as new ice-rich material is exposed. This is probably not a clear indicator of climate induced cryosphere change and is probably better considered in WG2 as an impact. [Sharon Smith, Canada]	Accepted. Text modified
4-1529	4	42	9	42	10	"Permafrost degradation has caused....." - this seems like a much stronger statement than the Jones et al. paper can support. Their paper suggests permafrost degradation is one factor (along with several others) that are 'potentially responsible" for changes in erosion. Wording of this sentence should be revised. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text modified
4-1530	4	42	9	42	18	This paragraph mixes a lot of different things and could be edited (or significantly reduced) [Sharon Smith, Canada]	noted. Text simplified
4-1531	4	42	10		18	The subsea permafrost degradation or thawing rates should be moved into the section below, 4.6.3. Here, terrestrial permafrost degradation rates should be given instead. [European Union]	Accepted. Move subsea permafrost stuff into subsea permafrost section.
4-1532	4	42	13	42	15	There is an entire section on subsea permafrost so this material should be removed from this section. Also - is the effect here related to climate or sea level rise that may be due to isostatic adjustments etc. so that areas that were once terrestrial permafrost are now being flooded. [Sharon Smith, Canada]	Accepted. See above.
4-1533	4	42	15	42	16	Van Huissteden et al (2011): incompletely cited. This publication suggests a decrease of lake area at continued permafrost degradation. [Ko Van Huissteden, Netherlands]	Accepted. Reference deleted, inappropriate.
4-1534	4	42	20	42	22	Better write "... mass of perennially frozen rock fragments ..." and " ... contains subsurface ice in various forms ..." rather than "intersticiual ice or an ice core" (there is no either-or alternative in reality). The reference of van Everdingen (1998) is long outdated. Replace or at least complement by the IPA/ICSI state-of-the-art report: Haeberli, W., Hallet, B., Arenson, L., Elconin, R., Humlum, O., Kääb, A., Kaufmann, V., Ladanyi, B., Matsuoka, N., Springman, S. and Vonder Mühll, D. (2006): Permafrost creep and rock glacier dynamics. Permafrost and Periglacial Processes, 17/3, 189-214. (doi: 10.1002/ppp). [Wilfried Haeberli, Switzerland]	Accepted. Will change accordingly.

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4-1535	4	42	22	42	22	Haeberli at al 2006 (a comprehenseive review) would be a much better reference than Van Everdingen 1998 (a glossary).Haeberli, W., Hallet, B., Arenson, L., Elconin, R., Humlum, O., Kääb, A., Kaufmann, V., Ladanyi, B., Matsuoka, N., Springman, S. and VonderMühll, D. (2006): Permafrost creep and rock glacier dynamics, Permafrost and Periglacial Processes, 17(3): 189–214. [Stephan Gruber, Switzerland]	Accepted.
4-1536	4	42	23	42	23	Use a better term than "speed-up" of some rock glaciers (increase in velocity?) [Sharon Smith, Canada]	Accepted.
4-1537	4	42	32	42	34	This current sentence is weak, given that the only papers cited come from one mountain range (Mt Blanc massif). Further studies could also be cited from elsewhere in the European Alps, North America, and New Zealand. Probably most appropriate to simply cross-reference here to the relevant section of Working Group II AR5, where a comprehensive assessment on this topic is given (Chapter 18 of WGII). [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. This text is removed.
4-1538	4	42	32			Add the recent and comprehensive study about the increasing frequency of large events in an extended part of the Alps by Fischer et al (2012): Fischer, L., Purves, S.R., Huggel, C., Noetzli, J., Haeberli, W. (2012): On the influence of topographic, geological and cryospheric factors on rock avalanches and rockfalls in high-mountain areas. Natural Hazards and Earth System Sciences 12, 241-254. doi: 10.5194/nhess-12-241-2012 [Wilfried Haeberli, Switzerland]	Accepted. This text is removed.
4-1539	4	42	34			permfarost IN steep slopes ? [Andreas Kääb, Norway]	agree. This text is removed.
4-1540	4	42	41	42	41	Stevens et al. (2010) is also relevant to this discussion on freezing beneath bottom-fast ice. Reference: Stevens, C.W., Moorman, B.J., and Solomon, S.M. 2010. Modelling ground thermal conditions and the limit of permafrost within the near-shore zone of the Mackenzie Delta, Canada. Journal Geophysical Research, 115: F04027. Stevens, C.W., Moorman, B.J., and Solomon, S.M. 2010. Interannual changes in seasonal ground freezing and near-surface heat flow beneath bottom-fast ice in the near-shore zone, Mackenzie Delta, Canada. Permafrost and Periglacial Processes, 21: 256-270. [Sharon Smith, Canada]	agree. Have added those references.
4-1541	4	42	47	42	56	Is this more appropriate in another chapter or in WG2 which deals with impacts of climate change? [Sharon Smith, Canada]	Rejected - we believe it is important to state why this component of the cryosphere, while being currently poorly understood is potentially important. This may arise elsewhere, but is important for the cryosphere chapter. But text rexduced
4-1542	4	43	6			Section 4.6.4.1. It is probably appropriate to cite the recent paper by Shiklomanov et al (2012), Proc. 10th Int. Permafrost Conf. pg 377-382. This gives an updated review of CALM. Also note that not all information on changes in ALT will come from CALM sites. The section is not that well organized as there is no regional order. [Sharon Smith, Canada]	Noted - updated reference will be considered
4-1543	4	43	8			Definition of the active layer appears after term has already been used (page 41, line 11) [Government of Chile]	Noted - will check and make changes.
4-1544	4	43	17	43	21	One of the things that should be mentioned here is that the reason for larger increases in ALT in some regions is the low ice-content -- reason there is higher increase in bedrock at one of the eastern Arctic sites. Somewhere in this section there needs to be some comment on the different responses in active layer to changing air temperature conditions for different ecozones due to the presence of a buffer layer etc. There will be a direct link for tundra sites with little organic layer than forested sites. Smith et al. (2009) showed for a range of locations the variability in the relationship between air temperature and ALT. [Sharon Smith, Canada]	Accepted, text added.
4-1545	4	43	18	43	18	Note that this site is in the Mackenzie Delta area. Although some sites considered (see Smith et al. 2009) are in this same region or the northern Mackenzie Valley, others are in the central or southern Mackenzie Valley Note also that the site mentioned (Burn and Kokelj 1990) is not the same one that is shown in Fig. 4.23.	Noted

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						[Sharon Smith, Canada]	
4-1546	4	43	24	43	32	Caption for Figure 4.23 - All portions of this figure should be linked to the text. In particular there is no discussion of 4.23e.. If departures are to be shown then the period that is to be used as a reference needs to be provided. This reference period should be the same for all sites. The validity of the trend that is presented might be questioned given that the number of stations has changed over time. Is there really an incease in latter years or is it simply a function of the addition of more sites that happened to have larger ALT etc (and for which the reference mean is different?). The large spread in data in latter years would seem to indicate that there is a lot of scatter and perhaps the trend is not very signifcant. [Sharon Smith, Canada]	This figure is substantially revised and the comment is no longer appropriate.
4-1547	4	43	34	43	34	This sentence surely only referes to measurements and those are not representative for the entire Plateau. Please reformulate. [Stephan Gruber, Switzerland]	Accepted - text refined to say "At measurements on the Tibetan Plateau, ALT has…"
4-1548	4	43	34	43	35	The increasing of ALT (7.8 cm/yr, Wu and Zhang, 2010) from 1995 to 2010 was much larger than recent studies from Zhao et al. (2008; 2010) and Li et al. (2012), which was due to: 1) the cited results was interpolated based on ground temperatures from metal casing boreholes, the metal case may disturb the thermal conditions of ground temperature measurement for surface layer soils which resulted in higher summer ground temperatures and thus higher ALT; 2) the boreholes are very close to highways, and the highway may take its effects on borehole temperature. The average active layer thickness changes from 1981-2010 should be 1.33 cm/per, and about 3.6 cm/yr from 1998 to 2010, which was much less than cited results. [New references: 1)Zhao Lin, S.S.Marchenko, N.Sharkhuu, Tonghua Wu. Regional Changes of Permafrost in Central Asia. Proceedings of 9th International Conference on Permafrost (Plenary paper), 2008, 2061-2069. 2) Li R, Zhao L, Ding Y J, et al. Temporal and spatial variations of the active layer along the Qinghai-Tibet Highway in a permafrost region. Chin Sci Bull, doi: 10.1007/s11434-012-5323-8]. [Jing Ming, China]	Accepted. Will make changes accordingly.
4-1549	4	43	34	43	35	This statement appears to contradict with the statement in Line 43-45. [Atsumu Ohmura, Switzerland]	Accepted, statement moved, and altered.
4-1550	4	43	34	43	35	The increasing of ALT (7.8 cm/yr, Wu and Zhang, 2010) from 1995 to 2010 was much larger than recent studies from Zhao et al. (2008; 2010) and Li et al. (2012), which was due to: 1) the cited results was interpolated based on ground temperatures from metal casing boreholes, the metal case may disturb the thermal conditions of ground temperature measurement for surface layer soils which resulted in higher summer ground temperatures and thus higher ALT; 2) the boreholes are very close to highways, and the highway may take its effects on borehole temperature. The average active layer thickness changes from 1981-2010 should be 1.33 cm/per, and about 3.6 cm/yr from 1998 to 2010, which was much less than cited reults. [New references: 1)Zhao Lin, S.S.Marchenko, N.Sharkhuu, Tonghua Wu. Regional Changes of Permafrost in Central Asia. Proceedings of 9th International Conference on Permafrost (Plenary paper), 2008, 2061-2069. 2) Li R, Zhao L, Ding Y J, et al. Temporal and spatial variations of the active layer along the Qinghai-Tibet Highway in a permafrost region. Chin Sci Bull, doi: 10.1007/s11434-012-5323-8] [Tonghua Wu, China]	Accepted. Same as 1548.
4-1551	4	43	34	43	39	Please see comment 194. Here we have "yr-1" as opposed to "a-1" [Adrian Simmons, United Kingdom]	Editorial - final formatting to be decided post-Final draft
4-1552	4	43	34			replace “of” in the idle of the line with “by”. [Atsumu Ohmura, Switzerland]	editoral.
4-1553	4	43	41	43	41	Change to "Measured changes in ALT..." [Stephan Gruber, Switzerland]	editoral.
4-1554	4	43	45			I think that it could be useful to include the following sentence “On the other hand in a very cold permafrost, rich in ice and above all where air temperature is still almost stable or in slight decrease as in Continental Antarctica (Victoria Land) since 1996 an active layer thickening of 1 cm per year caused mainly by the increase of the solar radiation is recorded (Guglielmin and Cannone, 2012). [Mauro Guglielmin, Italy]	Accepted.
4-1555	4	43	55	43	58	It is not obvious how the subsidence could become the cause for the negligible ALT increase. Some explanation of processes may help. [Atsumu Ohmura, Switzerland]	Accepted. Will add more details.

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4-1556	4	43	55			Please see comment 194. Here we are back to "per decade" [Adrian Simmons, United Kingdom]	Editorial
4-1557	4	44	1	44	13	Seasonal frozen ground is tranditionally considered not underlaid by permafrost. This new definition can lead to confusion between Active Layer and SFG. [Jing Ming, China]	Checked, the definition we use apppears to be widely accepted.
4-1558	4	44	5			A word is missing between ”identified” and “late-1960s”. [Atsumu Ohmura, Switzerland]	Editoral.
4-1559	4	44	5			It appears that the words "before the" are missing before the words "late-1960s". [Adrian Simmons, United Kingdom]	editoral.
4-1560	4	44	10	44	11	As for the Tibetan Plateau, the maximum seasonally frozen depth has decreased by 33 cm since the middle of 1980s (Li et al., 2009). [New reference: Li Ren, Zhao Lin, Ding Yongjian. The Climatic Characteristics of the Maximum Seasonal Frozen Depth in the Tibetan Plateau. Journal of Glaciology and Geocryology, 2009,31(6): 1050-1056.] [Tonghua Wu, China]	Addition accepted.
4-1561	4	44	11	44	13	During the period 1977-2006, the frost day number has decreased at a rate of 7.4 day per decade in the source of Yangtze River and 2.1 day per decade in the source of Yellow River on the Qinghai-Tibet Plateau (Zhao et al., 2011). [New reference:Zhao Lin, Li Ren, Ding Yongjian, et al. Soil Thermal regime in Qinghai-Tibet Plateau and its adjacent regions during 1977-2006. Advances in Climate Change Research, 2011, 7(5): 307-316.] [Tonghua Wu, China]	Rejected, this conclusion is not relevant
4-1562	4	44	16	44	17	Caption for Figure 4.24 - Similar comment to above - If departures are shown, reference value (period) is required. The 3 lines need to be defined. Do all stations have the same record length? [Sharon Smith, Canada]	Accepted, change made
4-1563	4	44	19	45	50	FAQ 4.1 (Mountain Glaciers). This FAQ makes many references to the "equilibrium line altiutude" (ELA), a term that will not be familiar to the general reader, without explaining what it is. [David Wratt, New Zealand]	Noted: ELA is in the Glossary (link added)
4-1564	4	44	19			FAQ 4.1: The FAQ does a nice job of explaining the basic principles of glacial retreat and why some glaciers disappear and others do not. However, what is currently missing is some quantitative evidence that glaciers ARE disappearing. The chapeau suggests that glaciers have disappeared in several regions, but then the subsequent text does not expand upon this by providing more details on the number of glaciers, or ice covered area that has disappeared. From the chapter, we know such information is available and could be added here to strengthen the quantitative content of the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text has been revised and the related references were added and discussed in the main text.
4-1565	4	44	21	44	21	FAQ 4.1: The word "disappearing" is a loaded term that appeals to the emotions of the readers rather than being based on a rigorous scientific definition. "Disappearing" and "disappeared" are not well-defined scientific terms and must be avoided in a scientific report. A better and much more neutral heading for FAQ 4.1 would be "Are Glaciers in Mountain Regions Diminishing?" [Jacob Clement Yde, Norway]	Rejected: The selection of a limited number of FAQs was decided in a joint AR5 WG1 effort lead by a respective commission.
4-1566	4	44	21	45	41	FAQ 4.1, Why is there any mention to the Andes and Patagonian Ice caps? And the ELA studies in the Andes? [Government of Chile]	Taken into account: The text has been modified
4-1567	4	44	21	45	41	Some glaciers are disappering, some new glaciers appear due to glacier shingking which separates glacier branches from main one. This should be mentioned here? [Shichang Kang, China]	Rejected: Two (or more) glaciers resulting from a split of a former one are not really new glaciers.
4-1568	4	44	21	45	41	Some glaciers are disappearing; some new glaciers appear due to glacier shingking which separates glacier branches from main one. This should be mentioned here? [Jing Ming, China]	Rejected: Two (or more) glaciers resulting from a split of a former one are not really new glaciers.
4-1569	4	44	21	45	50	FAQ 4.1: Quantitative data for the statements about disappeared and disappearing glaciers must be included to give them scientific support. Otherwise, they should be deleted. [Jacob Clement Yde, Norway]	Noted: Detailed numbers are given in the main text.
4-1570	4	44	21	45	50	FAQ 4.1: In this context the reader deserves to know whether these "many small glaciers" only have existed for a short period (50-100 yr) during the Little Ice Age or they have existed for thousands of years. [Jacob	Rejected: This information is not available and also not relevant, as each steady-state extent resets the

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						Clement Yde, Norway]	memory about the climatic past of a glacier.
4-1571	4	44	21	45	50	FAQ 4.1: Due to the problematic (emotional rather than scientific) terminology and lack of quantitative data, I recommend that FAQ 4.1 focuses on glacier volume change rather than whether glaciers are "disappearing" or have "disappeared". [Jacob Clement Yde, Norway]	Rejected: The selection of a limited number of FAQs was decided in a joint AR5 WG1 effort lead by a respective commission.
4-1572	4	44	21	45	50	FAQ 4.1: There is a problematic use of ELA! In FAQ 4.1 the ELA refers a climatic ELA based a decadal-scale mean with large interannual variations. Theoretically, a single year with a very high positive mass balance may compensate for many years with an ELA "above the highest glacier elevation" (page 4-45, line 40). This is especially relevant for small glaciers, which are the primary focus of FAQ 4.1 as they are the ones that are relevant when it comes to "disappearing". Small glaciers may have an elevation span of 50-100 m, so they are likely to have an annual ELA either below or above their elevation span in most years. Thus, ELA is not a useful term when it comes to very small glaciers! It is much more relevant to talk about glacier mass balance and volume change, not ELA. Therefore, I recommend that everything on ELA is deleted from FAQ 4.1 and replaced by discussion on glacier mass balance and volume change. [Jacob Clement Yde, Norway]	Rejected: We refer her to the ELA0 (indicative of a balanced budget) rather than to a mean value of annual values. Glacier mass balance and volume change is discussed extensively in the main text. The topic of the FAQ cannot be changed.
4-1573	4	44	21			FAQ 4.1: Related to the lack of quantitative information, we would recommend that a second figure is added which provides quantitative evidence that glaciers are disappearing. [Thomas Stocker/ WGI TSU, Switzerland]	Noted: References were added in the main text.
4-1574	4	44	21			FAQ 4.1: It seems an opportunity has been lost here to really highlight some of the advancement in glacier monitoring that has occurred since the AR4, and these new datasets/earth observation techniques should be mentioned here in the context of providing the evidence that glaciers are disappearing. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected: These new datasets do not provide any evidence about glaciers that have disappeared.
4-1575	4	44	23	44	24	It would be good to recognize, either here or in the main body of the response, that there are other mechanisms besides a direct temperature response that could be responsible for mass balance changes in some instances. For example, the ice cap on Kilamanjaro? [Francis Zwiers, Canada]	Noted: This is why we have written 'in may regions' (i.e. not in all).
4-1576	4	44	25	44	27	Is the list needed? Can we be confident that it is complete? [Francis Zwiers, Canada]	Noted:The list is likely not complete but required to make the poin. Citations are provided in the mean text.
4-1577	4	44	27	44	27	Change "Irian Jaya" to "West Papua" or "West Papua (ex-Irian Jaya)". Indonesian goverment change the name in 2007! [Jefferson Cardia Simões, Brazil]	Editorial
4-1578	4	44	27	44	28	The sentence "If atmospheric warming continues through the 21st century …" seems to imply there is a possibility it will not do so. However my reading of the "Projections" section of the SPM is that continued warming is highly likely if not virtually certain. So how about changing this sentence to something like: "Given the expectation of continuing warming through the 21st Century, many more glaciers will inevitably disappear" ? [David Wratt, New Zealand]	Editorial
4-1579	4	44	28	44	29	The last sentence "It is also likely that some mountain ranges will lose most, if not all, of their glaciers." introduces a new question as to why some mountain ranges will lose "most, if not all, of their glaciers." Consider adding some clarification as to why this may be the case. [Government of Canada]	Noted: Answering this question is actually the main goal of the text following.
4-1580	4	44	29	44	29	include the tropical mountains, Mt. Kenya and Kilimanjaro and give indication of time span [Government of Kenya]	Accepted: Glaciers in 'East Africa' were added.
4-1581	4	44	33	44	34	This is a very nice explanation of the importance of this particular feature, which will be beneficiel for the more "general reader". I would like to see more of that throughout this chapter (i.e. a bit more "why", and "important issues") [Hans Linderholm, Sweden]	Noted
4-1582	4	44	33			Reconsider use of the word shrinkage (line 33). It is not clear what aspect of change the authors are describing. Does this refer to length, area or mass/volume? [Government of United States of America]	Noted: It does refer to all of the three (explanation added).
4-1583	4	44	37	44	39	More accurate to talk about a 'few glaciers' and not entire regions that have had glaciers advancing AND/OR [Dorothea Stumm, Nepal]	Taken into account: The text has been modified accordingly.
4-1584	4	44	37	44	39	gaining mass. At the westcoast of New Zealand, there are only very few temporarily advancing glaciers (mass	see 4-1583

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						gain [Dorothea Stumm, Nepal]	
4-1585	4	44	37	44	39	questionable), to my knowledge mainly Franz Josef and Fox Glacier, which have exceptional local [Dorothea Stumm, Nepal]	see 4-1584
4-1586	4	44	37	44	39	conditions, such as very big and relative high accumulation area with extreme precipitation, and narrow valleys for [Dorothea Stumm, Nepal]	see 4-1585
4-1587	4	44	37	44	39	the ablation area. The lower extend of these two glaciers is exceptionally low. In the past there have been a few [Dorothea Stumm, Nepal]	see 4-1586
4-1588	4	44	37	44	39	more glaciers advancing, as it happended also in other parts of the world. Regarding mass gains of glaciers in [Dorothea Stumm, Nepal]	see 4-1587
4-1589	4	44	37	44	39	NZ, it has to be considered that the later cited study uses proxy values and no mass balance [Dorothea Stumm, Nepal]	see 4-1588
4-1590	4	44	37	44	39	measurements. I think these proxy values have to be treated with great care. [Dorothea Stumm, Nepal]	see 4-1589
4-1591	4	44	37	44	39	Similarly, it is not the case that the glaciers in general have been growing in the Karakoram [Dorothea Stumm, Nepal]	see 4-1590
4-1592	4	44	37	44	39	(good paper is e.g. Gardelle J, Berthier E Arnaud Y, 2012, Nature Geoscience, doi:10.1038/NGEO1450) [Dorothea Stumm, Nepal]	see 4-1591
4-1593	4	44	38	44	38	FAQ 4.1: " ... special local conditions ... " is a vague expression. Rephrase to be more specific. [Jacob Clement Yde, Norway]	Taken into account: An explanation has been added.
4-1594	4	44	38	44	39	Numbers should be avoided here. They are arbitrary and depend on the glacier (climate, geometry etc). [Regine Hock, United States of America]	Noted: Assuming this comment refers to L33/34, we decided to keep the numbers as they are requested by others (4-1581).
4-1595	4	44	39	44	39	this is also the case for Northern India, where glaciers were in Equilibrium or slightly gaining mass in the nineties (see above, and Azam et al, 2012, Vincent et al, 2012) [Patrick WAGNON, Nepal]	Noted
4-1596	4	44	39	44	39	FAQ 4.1: Use RGI region name instead of "Karakoram". [Jacob Clement Yde, Norway]	Rejected: The RGI name is too unspecific to be used here
4-1597	4	44	39			mention also Southern Patagonia? [Andreas Kääb, Norway]	Noted
4-1598	4	44	41	44	41	It can take more than several decades. Better: It CAN take several decades … [Regine Hock, United States of America]	Editorial
4-1599	4	44	42	44	42	Possibly replace "time lag for the adjustment" with "time required for the adjustment"? That might be a bit more intuitive for non-expert readers. [Francis Zwiers, Canada]	Editorial
4-1600	4	44	47	44	52	Further in FAQ 4.1 there is discussion of accumulation and ablation processes but these are not explained anywhere in the FAQ. Suggest such information could be brought into this paragraph describing factors that influence the development of a glacier. [Government of Canada]	Rejected: These are detailed in the introduction of section 4.3
4-1601	4	44	49			dito comment No No 8 [Luzi Bernhard, Switzerland]	Noted
4-1602	4	44	51	44	51	Please specify what is meant by "shorter time scales" - e.g. "In detail, and over shorter time scales (LESS THAN ABOUT 50 YEARS ??)…" [David Wratt, New Zealand]	Editorial
4-1603	4	44	52			glaciers respond individually to climate change' ? [Andreas Kääb, Norway]	Noted
4-1604	4	44	54	44	54	delete 'robust' [Regine Hock, United States of America]	Rejected: see comment 4-1606 (the statement is required)
4-1605	4	44	54	44	55	I think I understand what this is saying, but lay readers might be a bit baffled by "robust modelling	Taken into account: The text has been modified

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						approaches". Here is a suggestion for how to rephrase this sentence: "Over periods longer than 50 years, the response is more coherent and less dependent on local environmental details, which means that long-term trends in glacier development can be well modelled." [Francis Zwiers, Canada]	
4-1606	4	44	54	45	14	This two paragraphs show too specific only one method (Paul, F.; Maisch, M.; Rothenbühler, C.; Hoelzle, M. & Haeberli, W. 2007. Calculation and visualisation of future glacier extent in the Swiss Alps by means of hypsographic modelling. Global and Planetary Change, 55, 343-357) to the FAQ 4.1. From my point of view it would be important to present an other, more physically based approach (e.g. Farinotti, D.; Huss, M.; Bauder, A., F. M. & Truffer, M. A. 2009. method to estimate the ice volume and ice-thickness distribution of alpine glaciers. Journal of Glaciology, 2009, 55, 422-430 or Huss, M. & D. Farinotti 2012. Distributed ice thickness and volume of all glaciers around the globe. Journal of Geophysical Research - Earth Science. 117(F04010), 10pp) [Luzi Bernhard, Switzerland]	Rejected: This section is on the determination of future glacier change rather than ice thickness distribution.
4-1607	4	44	56	45	1	The concept of Equilibrium Line Altutude" is introduced here without explanation. Would it be possible to add a brief explanation in parentheses for the general reader? E.g. "…upward shift of the equilibrium line altitude (ELA) by about 50m for each degree Celsius of atmospheric warming. THE ELA IS THE HEIGHT BELOW WHICH THE ANNUAL LOSS OF SNOW AND ICE BY ABLATION EXCEEDS THE GAIN BY ACCUMULATION (PREDOMINANTLY PRECIPITATION)." [Or some improved definition of ELA - this suggestion is a cobbled-together one] [David Wratt, New Zealand]	Noted (the term is explained in the glossary)
4-1608	4	44	57	44	57	The ELA needs to be defined clearly for the FAQ reader. You cannot jump straight in to talking about an upward shift of the ELA without first defining what this is. [Thomas Stocker/ WGI TSU, Switzerland]	Noted (the term is explained in the glossary)
4-1609	4	44	57	45	41	Please avoid using the abbreviation ELA and use equilibrium line altitude instead. [Government of NORWAY]	Noted (the term is explained in the glossary)
4-1610	4	44	57			150 m/ 1 K is a strong simplification, and this value is more representative for dry regions. The lifting rate of ELA by 1 K temperature increase is the reciprocal of the ambient temperature lapse rate, and ranges from 140 m/ 1 K to 200 m/1 K, the median value being close to 170 m/1 K. [Atsumu Ohmura, Switzerland]	Noted: As we talk about ELA rather than the zero degree altitude, effects of precipitation are included. Reported values in the literature for ELA shifts are between 120 and 170 m / 1K.
4-1611	4	44				FAQ 4.1: The answer to the FAQ skips through space and time scales with no apparent pattern. Reorganizing this section from short to long time scales would make the reponse to the question more tractable and relevant. [Government of United States of America]	Noted: We agree that other possibilities to organize the FAQ exist. But sorting for time scales would create confusion elsewhere as the described processes do not follow a clear temporal pattern either.
4-1612	4	44				FAQ 4.1 Figure 1 The slope instability would be better demonstrated if the original ELA perturbation moved up the steep section of the glacier. This would give very little dA from a step change. A second, equally-sized change would yield a larger dA, illustrating well the sensitivity to climate. [Government of United States of America]	Noted: We appreciate this suggestion but have decided not to implement it as the figure is only a two dimensional cartoon that does not go into this level of detail.
4-1613	4	44				FAQ 4.1: Please consider including a discussion of the role of drought (no or little snowfall) on glacier disappearance. [Government of United States of America]	Rejected: There is no evidence in the literature that glaciers have disappeared due to drought (strong decrease in precipitation).
4-1614	4	45	2	45	3	It will not be clear to most readers as to why the "more rapid decrease in the extent of sea ice at the summer minimum is a consequence of these trends." The decrease in summer sea ice extent is an important and much quoted measurement that readers will want to understand the reasons for. It is worth elaborating on this reason here. [Government of Canada]	XXX belongs to sea ice
4-1615	4	45	4	45	4	Initial state for (FAQ 4.1., Figure 1a) should also be described, then in line 4, should be (FAQ 4.1, Figure 1b) [Dorothea Stumm, Nepal]	Editorial
4-1616	4	45	4	45	4	not (…, Figure 1a) [Dorothea Stumm, Nepal]	Editorial
4-1617	4	45	4	45	5	Maybe explain "accumulation area" and "ablation area". [Francis Zwiers, Canada]	Noted: This is extensively discussed in the main text.
4-1618	4	45	4			Section on ELA. It could be added that some of the glaciers in the Himalaya are at so high altitudes that a very significant warming must occur before the ELA reaches these altitudes so the glaciers will remain for a long time span. [European Union]	Taken into account: The text has been modified accordingly.

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4-1619	4	45	6			mention somewhere also the decrease of speed (ice flux) as consequence of negative SMB [Andreas Kääb, Norway]	Taken into account: The text has been modified accordingly.
4-1620	4	45	12			More detail is needed on the process of when a retreating glacier creates a proglacial lake and this complication changes the control on glacier length in a way that is usually regarded as decoupling it from climate forcing. [Government of Australia]	Noted: Length changes of glaciers calving into (proglacial) lakes are often excluded from further measurements.
4-1621	4	45	13			Add “solar irradiance” between “of “ and precipitation”, as solar irradiance possesses the weight of 25% on an average as the melting energy source. [Atsumu Ohmura, Switzerland]	Noted: For the here described long-term changes the variability of solar irridiance has only a minor effect.
4-1622	4	45	18	45	18	Suggest deleting the elaboration "largely independent of aspect, shading or debris cover". [Francis Zwiers, Canada]	Noted: We prefer to keep it as the description will be too vague otherwise.
4-1623	4	45	26			requires detailed knowledge of other individual glacier characteristics …' ? [Andreas Kääb, Norway]	Editorial
4-1624	4	45	27	45	29	"...and the response of climate change is thus difficult to model" In the short-term??. Should specify that this statement is being made in relation to short-term changes, to avoid any contradiction with page 44, lines 54/55. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text has been modified accordingly.
4-1625	4	45	27			mass balance sensitivity' needs to be explained [Andreas Kääb, Norway]	Editorial
4-1626	4	45	28			Approximations for estimating reasonable response times for large numbers of unmeasured glaciers exist for many years already and clearly document the strong influence of slope inclination (Haeberli, W. and Hoelzle, M. (1995): Application of inventory data for estimating characteristics of and regional climate-change effects on mountain glaciers: a pilot study with the European Alps. Annals of Glaciology, 21, 206-212. Russian Translation in: Data of Glaciological Studies, Moscow, 82, 116-124.) [Wilfried Haeberli, Switzerland]	Editorial
4-1627	4	45	31	45	31	FAQ 4.1: Use RGI region name instead of "Karakoram-Himalaya". [Jacob Clement Yde, Norway]	Rejected: The FAQ readers might not be aware of the RGI.
4-1628	4	45	34	45	34	Can you list the current/planned satellites that will contribute to reducing the gaps in knowledge in terms of changes to glacier mass and extent? This may help to manage/ensure research funding. [European Union]	Rejected: This is not the purpose og the report.
4-1629	4	45	37	45	37	For a summary, the statement "the fate of glaciers in the mountain regions of the world will be highly variable" is too weak. Mass loss is certain. It is the amount of loss, which will vary. The statement leaves too much room for the interpretation that some glaciers could also gain a considerable amount of mass and increase. [Olaf Eisen, Germany]	Taken into account: The text has been modified accordingly.
4-1630	4	45	38	45	39	Are there any that could grow? [Francis Zwiers, Canada]	Noted: Not in the long-term when considering the future climate development. But in the short-term glaciers with short response times can still advance, e.g. by an increase of winter precipitation.
4-1631	4	45	54	45	54	The question posed here seems rather innocuous and not a question that would peak interest. Suggest rephrasing to say something like: "Do we understand why sea ice changes in the Arctic and Antarctic are different?" There is a widely held notion that warming is amplified at the poles; this WGI report should clarify if that is true or not. The issue of sea ice changes is related to that in the mind of non-experts. Has warming been large at both poles and if so, why have sea ice changes been different? That's the story this FAQ should tell. The FAQ does a good job of describing how different the conditions are for sea ice at the two poles but fails to bring in the regional warming angle. [Government of Canada]	Noted.
4-1632	4	45	55			FAQ 4.2: Very nice FAQ. We suppose the details in here will be updated based on the latest record breaking observations from 2012. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised.
4-1633	4	45	56	46	7	I think it would be good to try to avoid the concept of statistical significance in these FAQs (which introduces an additional layer of technical concepts that the lay reader has to absorb). [Francis Zwiers, Canada]	Accepted - text revised.
4-1634	4	45	57	45	57	Which last 32 years? Please specifically state the years. [Government of Australia]	Accepted - text revised.
4-1635	4	45	57	45	58	Data from 2011 and 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Accepted - text revised.

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4-1636	4	45	57	45	58	For simplicity and directness suggest replacing "Over the last 32 years, there has been a significant trend of -3.9% per decade in the annual average extent of sea ice in the Arctic" with " ... there has been a significant downward trend of 3,9% per decade …" [David Wratt, New Zealand]	Noted.
4-1637	4	45	57	45	58	Suggest re-writing this sentence as "Over the past 32 years, the annual extent of sea ice in the Arctic has decreased about 4% per decade." [Francis Zwiers, Canada]	Noted.
4-1638	4	45	58	46	3	These lines propose a speculative mechanism for the year-to-year loss of Arctic sea ice extent being largest in summer. I should mention that it has been discussed in the literature that this and similar mechanisms are inconsistent with Antarctic sea ice extent changes in GCM simulations (Eisenman et al., 2011, doi:10.1175/2011JCLI4051.1), and an alternative mechanism for this involving the geometry of landmasses in the Arctic has been proposed (Eisenman, 2010, doi:10.1029/2010GL043741). [Ian Eisenman, United States of America]	Noted.
4-1639	4	45	58			This is FAQ 4.2. A trend of −3.9% per decade is given – relative to what base period? [Harry Stern, United States of America]	Accepted - text revised.
4-1640	4	45				FAQ 4.1 deals only with glacier shrinkage. However there are also growing/stable ones. This should be mentioned. Along with the explanation that even such growth/stability is in line with our understanding of climatic changes, that bring in some regions more precipitation, perhaps even (over-)compensating temperature increase [Andreas Kääb, Norway]	Rejected: The FAQ4.1 is on disappearing glaciers. Extending the contents to the question 'why glaciers are growing when temperatures are increasing' is beyond the scope of the question.
4-1641	4	46	1	46	1	The value of average winter sea ice thickness in 1978 should be given, to allow an interpretation of the strength of thickness decrease. [Urs Neu, Switzerland]	Taken into account - covered in main text.
4-1642	4	46	1			FAQ 4.2: It does not seem appropriate to provide a quantitative answer regarding the changes in the thickness of the Arctic sea ice cover (i.e. 1.8 m between 1978 and 2008), given the level of uncertainty in submarine and satellite-derived estimates. Suggest instead: “The average winter thickness of the Arctic Ocean sea ice has thinned and the total volume (mass) of Arctic sea ice has decreased significantly at all times of year.” [Government of United States of America]	Accepted - text revised.
4-1643	4	46	2			Add after "(mass) of Arctic sea ice has decreased significantly at all times of year." Add: "The mean annual cycle of arctic sea ice volume over the 1979 -2011 period ranges from 28,700 km3 in April to 12,300 km3 in September. Monthly averaged ice volume for September 2012 was 3,400 km3. This value is 72% lower than the mean over this period, 80% lower than the maximum in 1979. " Reference :PIOMAS (Pan-Arctic Ice-Ocean Modeling and Assimilation System) from Polar Science Center . Applied Physics Laboratory . University of Washington. USA.These texts were obtained from its page http://psc.apl.washington.edu/wordpress/research/projects/arctic-sea-ice-volume-anomaly/ and the main bibliographic reference about how the data were obtained is Schweiger, A., R. Lindsay, J. L. Zhang, M. Steele, H. Stern, and R. Kwok, 2011: Uncertainty in modeled Arctic sea ice volume. Journal of Geophysical Research-Oceans, 116. doi:10.1029/2011jc007084, already cited in this chapter, page 12. [CELSO COPSTEIN WALDEMAR, BRAZIL]	Rejected - outside the scope of this FAQ in an observations chatper
4-1644	4	46	3	46	5	Suggest re-writing this sentence as "Over the same 32-year period, the annual extent of sea ice in the Antarctic inrreased about 1.4% per decade." The follow-on statement about regional differences would presumably also be true in the Arctic, and maybe doesn't have to be part of the chapeau statement. [Francis Zwiers, Canada]	Accepted - text revised.
4-1645	4	46	4	46	4	Replace "show" by "shows" [Urs Neu, Switzerland]	Accepted - text revised.
4-1646	4	46	10	46	10	Replace "the Barents" with "Barents". [Francis Zwiers, Canada]	Accepted - text revised.
4-1647	4	46	11	46	11	Replace "A fraction" with "Some". [Francis Zwiers, Canada]	Accepted - text revised.
4-1648	4	46	13			6 of 8. Further ambiguity is added in the FAQ 4.2 section, where multiyear ice is defined as “sea ice that is more than one year old”. Obviously this is inconsistent with the definition on page 10. [Harry Stern, United States of America]	Accepted - changed to perennial ice (defined in the glossary)

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4-1649	4	46	16	46	16	Replace "transports" with "moves". [Francis Zwiers, Canada]	Rejected.
4-1650	4	46	18	46	19	Replace "changes of the sea ice cover" with "sea ice cover changes". [Francis Zwiers, Canada]	Accepted - text revised.
4-1651	4	46	18	46	21	FAQ 4.2: This statement will need to be revised to reflect new historic lows for sea ice. [Government of United States of America]	Accepted - text revised.
4-1652	4	46	18	46	27	FAQ 4.2: It would make sense to include total percentage of Arctic Sea Ice decline, say from 1979-2011 (like on p47 l26). This would help putting the changes into context. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised.
4-1653	4	46	19	46	19	Explain extent, e.g. "(i.e. extent where at least 20% of the area is covered by sea ice)". [Urs Neu, Switzerland]	Taken into account - covered in main text
4-1654	4	46	20	46	21	The record minimum in 2012 should be mentioned. [Thierry Fichefet, Belgium]	Accepted - text revised.
4-1655	4	46	20	46	21	The record minimum extent observed in 2007 was beaten by a large margin in 2012. This statement requires updating. [Government of Canada]	Accepted - text revised.
4-1656	4	46	20	46	21	should read: ….reaching a record minimum in 2012. [Government of NORWAY]	Accepted - text revised.
4-1657	4	46	21	46	21	Replace 2007 by 2012. Reason: be up-to-date, if possible. [Urs Neu, Switzerland]	Accepted - text revised.
4-1658	4	46	21	46	21	"… reaching a record minimum in 2007" . I think there was a new record minimum in 2012. Please update. [David Wratt, New Zealand]	Accepted - text revised.
4-1659	4	46	21	46	21	After 2007, add "that was exceeded in 2012". [Francis Zwiers, Canada]	Accepted - text revised.
4-1660	4	46	21			Again, mention should be made of the all-time sea-ice minimum in September 2012 [David Bromwich, United States of America]	Accepted - text revised.
4-1661	4	46	21			Replace “2007” with “2012”. [Atsumu Ohmura, Switzerland]	Accepted - text revised.
4-1662	4	46	21			The record minimum is now 2012. [Harry Stern, United States of America]	Accepted - text revised.
4-1663	4	46	23	46	25	FAQ 4.2: “Changes in the relative amounts of multiyear and seasonal ice are also contributing to the reduction in ice volume: approximately 17% of multiyear sea ice per decade has been lost to melt and export out of the basin since 1979 and 40% since 1999.” [Government of United States of America]	Accepted - text revised.
4-1664	4	46	24	46	25	I’m not sure I understand the units here. The loss of multiyear ice is 17% per decade from 1979 to when (2011?), relative to what base period? And the loss of multiyear ice is also 40% (of what?) from 1999 to when? Please be clear. [Harry Stern, United States of America]	Accepted - text revised to clarified based period.
4-1665	4	46	36	46	36	Explain why the snow-to-ice conversion is more important in the Antarctic than in the Arctic (is this because in the Antarctic, sea ice formation occurs at lower latitudes with higher precipitation?) [Francis Zwiers, Canada]	Accepted. Text revised
4-1666	4	46	37	46	39	Snow ice formation and its importance for the Antarctic sea ice cover is not discussed in the main text. [Thierry Fichefet, Belgium]	Noted. The main text focuses on observed changes, and there are no observations of changes to snow ice. This FAQ however describes the background processes to sea ice change.
4-1667	4	46	43	46	45	I think the notion of divergent drift needs to be explained to lay readers. [Francis Zwiers, Canada]	Rejected.
4-1668	4	46	50			The increase in Antarctic sea ice extent of 1.4 +/- 0.2 % per decade is referred to as "small compared to natural variability". But isn't naturally variability precisely what the +/- 0.2 is meant to account for (albeit assuming it takes the form of white noise)? And if so, how is 1.4 considered "small"? Similar instances occur elsewhere in the chapter, where the Antarctic sea ice extent trend is referred to as "small" (e.g., p. 8, line 35; p. 46, line 4) or "slightly positive" (p. 14, line 56), whereas the Arctic sea ice extent trend is referred to as "strong" (e.g., p. 7, line 18; p. 16, line 39). I don't see the basis for this. Comparing with the standard estimate of natural variability, the Antarctic trend is (1.4/0.2=) 7x larger than the threshold for statistical significance, whereas the Arctic trend is (3.9/0.2=) 19.5x larger than the threshold for statistical significance. The Arctic value is of course larger and more significant, but calling 7 "small" and 19.5 "strong" seems like a rather subjective evaluation. [Ian Eisenman, United States of America]	Accepted - text revised (reference to natural variability deleted).

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4-1669	4	46	52	46	52	Significant in the colloquial sense, or in the statistical sense? In any case, I suggest avoiding the word (both because of the possibility of confounding meanings, and to avoid introducing statistical concepts into the FAQs if possible). In this case, the message is about the magnitude of the increase in the Ross Sea - so you could just say that a large increase in the Ross Sea dominates the overall trend. [Francis Zwiers, Canada]	Accepted - text revised to read 'larger'.
4-1670	4	48	10	48	10	Figure 4.25: It is unfortunate that Glaciers and Ice Sheets are spaced about with maximum distance. They are the same 'category' when contrasted to the other components (snow, lake, frozen ground). Why are mass loss rates mentioned for glaciers but not for the ice sheets? Total numbers are given at the bottom, but if breakups are given they should be given for both, glaciers and ice sheets and for both periods. What is meant by 'recent' in the glacier captions. Aconyms are not explained and should best be avoided altogether. [Regine Hock, United States of America]	Ordering of the chapter was carefully considered and we consider the optimum has been achieved.
4-1671	4	47	15			The whole text which follows is heavily repetitive and similar to the Executive Summary - is it really necessary here? [Wilfried Haeberli, Switzerland]	JC: Noted. Repetitions will be minimized but can't be helped.
4-1672	4	47	15			It may be worth looking at conclusions of other cryospheric syntheses to provide a more integrated picture. One of the main findings in Derksen et al (2012) for example is that there has been a consistent pan-cryospheric response to increasing air temperatures and the evidence points to a reduction in spatial extent and mass of the cryosphere and temporal persistence of melt related parameters. More can probably be said about linkages between cryospheric components, e.g. sea ice reduction and glacier melt or snow cover extent etc. Reference: Derksen, C., Smith, S.L., Sharp, M., Brown, L., Howell, S., Copland, L., Mueller, D.R., Gauthier, Y., Fletcher, C., Tivy, A., Bernier, M., Bourgeois, J., Brown, R., Burn, C.R., Duguay, C., Kushner, P., Langlois, A., Lewkowicz, A.G., Royer, A., and Walker, A. 2012. Variability and change in the Canadian cryosphere. Climatic Change, 115: 59-88. [Sharon Smith, Canada]	Noted. Paper looks good but is aregional study. Will try to incorporate relevant information
4-1673	4	47	17	47	17	specify for 'few' [Government of Kenya]	Accepted - text revised.
4-1674	4	47	17	47	26	Are the authors able to characterize the uncertainty associated with the percentrages presented here? [Government of United States of America]	Taken into account
4-1675	4	47	18	47	18	I suggest to use 'correlate' instead of 'consistent' [Nadine Salzmann, Switzerland]	Accepted
4-1676	4	47	18	47	19	" … consistent with surface warming" is inaccurate because melt can be caused by melting temperature on the surface and/or subsurface melting due to warm water underneath the ice (sea ice, floating ice shelves, intrusion of warm water, etc.) or indirectly by albedo decrease or indirectly by wind-forcing transport of sea ice to warm waters areas. Suggest revising to say "... consistent with direct and indirect warming" [Government of United States of America]	Accepted. Will use correlate and modify text
4-1677	4	47	21	47	22	Data from 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Accepted. Done
4-1678	4	47	23	47	23	Should be noted that the thinning hs been observed only in the central Arctic basin over this period. [Seymour Laxon, United Kingdom]	Accepted
4-1679	4	47	24	47	24	Update to include 2012 data [Jeffrey Obbard, Singapore]	Accepted. Done
4-1680	4	47	25	47	26	Observed increases in Antarctic sea ice extent are likely due to ice interactions with wind as opposed to surface temperature as suggested here. See: Holland and Kwok. 2012. Wind-driven trends in Antarctic sea-ice drift. Nature Geoscience. 10.1038/ngeo1627 [Chris Derksen, Canada]	Accepted. Holland and Kwok and also Comiso et al., 2011 cited.
4-1681	4	47	28			Not sure this statement will be well understood. Somehow (through extrapolation) the rest of the ice-covered area was also included in previous volume change assessments. [Etienne BERTHIER, France]	Taken into account: The text has been modified
4-1682	4	47	28			section on changes of volume of glaciers. Could the assessment of the change of volume in this assessment also be given a percent to be compared with the AR4 estimate of 42%? [European Union]	No, such numbers are not avaliable
4-1683	4	47	29	47	29	delete 'vector' (see above) [Nadine Salzmann, Switzerland]	Editorial
4-1684	4	47	33	47	34	"Most glaciers worldwide" would be better than "Most of the worldwide glaciers" [Adrian Simmons, United Kingdom]	Editorial

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4-1685	4	47	33			“worldwide glaciers” is not a good expression. Further, global mass balance of glaciers and their sea level contribution must be quantitatively presented in this paragraph. [Atsumu Ohmura, Switzerland]	Noted: expression was changed
4-1686	4	47	36	47	43	It is not certain if these ice losses refer to those of the continents (Greenland and Antarctica) or those of only the ice sheets. By reading the preceding sections, it is not certain of the satellite-gravimetry can distinguish between the ice sheet and the peripheral glaciers. The paragraph has a mixed wording of “Greenland and Antarctic ice sheet” and later simply “Greenland and Antarctica”, making the interpretation difficult. [Atsumu Ohmura, Switzerland]	accept and amend text
4-1687	4	47	36			On page 29, line 44 and page 31 line 9&10 these numbers are given: Greenland 1992-2011: 0.34+-0.06mm/a; Antarctic (0.18+-0.14mm/a)0.16+- 0.09mm/a. On page 32 line 13, we find 10.0+-2.8mm from 1992-2011. I think it is not useful to change the time span in the synthesis chapter compared to the individual chapters, as it is very confusing. [European Union]	consider/discuss at LA4
4-1688	4	47	37	47	37	About 6 mm and 3 mm. If the values table 4.1 is used 6.1 mm and 3.2 mm could be used to be consistent with the number of digitals. For reference it could be helpful if these number also where mentioned in the figure text to figures 4.15 (page 4-19, line 35) and 4.16 (page 4-31, line 3) [European Union]	accept
4-1689	4	47	45	47	49	While snow extent reductions are clear, I don't think compelling evidence is presented within this chapter that global snow depth has significantly decreased over the 1922 to 2010 time period as suggested here. I think a point of emphasis should be that spring (May and June) SCE, when snow is largely confined to the Arctic has declined since 1979 at a greater rate than September sea ice extent (Derksen, C., and R. Brown. 2012. Spring snow cover extent reductions in the 2008-2012 period exceeding climate model projections. Geophysical Research Letters. 39: L19504 doi:10.1029/2012GL053387) [Chris Derksen, Canada]	Accepted. Revisions made
4-1690	4	47	45			Snow cover does not have "global extent". Perhaps this sentence could begin with wording such as "The extent of snow cover over different regions of the world" or something similar. [Adrian Simmons, United Kingdom]	Accepted. Done
4-1691	4-4	4-47	4-48			Are the authors able to characterize the uncertainty associated with the percentrages presented here? [Government of United States of America]	Accepted.
4-1692	4-4	4-47	4-49			This cooling at high elevations is not shown in this chapter. Please reference! [Christoph Marty, Switzerland]	Noted. Reference cited.
4-1693	4-4	4-47	4-51	4-47	4-57	Are the authors able to characterize the uncertainty associated with the data presented here? [Government of United States of America]	Accepted
4-1694	4-4	4-47	4-52	4-47	4-52	See earlier comments with respect to increases in permafrost temperature - Probably more appropriate to say increases of up to 2°C since the 1970s to be consistent with other recent publications. Also, there are limited data sets longer than 20-25 years to make statements about change over 40 years. We also shouldn't focus only on the extremes and it should be mentioned here that these larger increases are generally in colder tundra conditions etc. [Sharon Smith, Canada]	Noted. Checked the actual temperature increase as reported in literature and revised.
4-1695	4-4	4-47	4-55	4-47	4-56	How representative is the 90 cm active layer thickness change? The statement of "up to 90 cm..." could include extreme cases and typical changes may be much smaller. Elsewhere in the text where 90cm is used, it is made clear subsequently that there are also places with little or no change. [Government of United States of America]	Noted. Checked how typical the increase is. Text modified
4-1696	4-4	4-47	4-55	4-47	4-56	It is a bit misleading to give only the extreme maximum value here for changes in ALT without some explanation etc. The evidence for example shows that there has been limited increase in ALT in forested areas (or no significant trend). These larger changes are usually in tundra areas and where there is low ice content, bedrock etc. A better representation of the evidence needs to be given in the conclusion. [Sharon Smith, Canada]	Noted. Text modified
4-1697	4-4	4-47				4.7b Synthesis: this section could perhaps be positioned before, rather than after the FAQs, for consistency in chapter format. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Will position this section before the FAQ
4-1698	4-4	4-48	4-7	4-48	4-7	eferences to changes in ecology seem unjustified, given lack of reference to ecologial literature in the chapter [Jeffrey Obbard, Singapore]	The text is highly modified to remove issue

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1699	4	48	9	48	10	Delete this final sub-sentence "but it is very likely....." - this vague, generalising sentence is not within the scope of the assessment provided by WGI. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
4-1700	4	49	5	49	5	Probably "Ieee" must be written in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1701	4	49	35	49	35	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1702	4	49	38	49	38	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1703	4	49	41	49	42	Title of Barry et al. (1979) paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1704	4	49	49	49	49	Probably something is wrong with the DOI number [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1705	4	49	62	49	62	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1706	4	49	67			New references quoted in my review: Bown, F., Rivera, A., Zenteno, P., Bravo, C., and Cawkwell, F. (in press): First glacier inventory and recent glacier variations of Isla Grande de Tierra del Fuego and adjacent islands in Southern Chile, In: Global Land Ice Measurements from Space, edited by: Kargel, J. S., Leonard, G. J., Bishop, M. P., Kaab, A., and Raup, B., Praxis-Springer (Publishers), Heidelberg, ISBN: 978- 3-540-79817-0. Casassa, G., Rodrıguez, J. and Loriaux.(in press). A new glacier inventory for the Southern Patagonia Icefield and areal changes 1986–2000, in: Global Land Ice Measurements from 25 Space, edited by: Kargel, J. S., Leonard, G. J.,Bishop, M. P., Kaab, A., and Raup, B., Praxis- Springer (Publishers), Heidelberg, ISBN: 978-3-540-79817-0. DGA, (2009). Estrategia Nacional de Glaciares. Dirección General de Aguas, Ministerio de Obras Públicas, Santiago de Chile, SIT Nº 205. Falaschi, D., C. Bravo, M. Masiokas, R. Villalba, and A. Rivera. (in press). First Glacier Inventory and recent changes in glacier area in the Monte San Lorenzo region (47ºS), southern Patagonian Andes, South America. Arctic, Antarctic and Alpine Research. Rivera, A., M. Koppes, C. Bravo, and J. Aravena (2012). Little Ice Age advance and retreat of Glaciar Jorge Montt, Chilean Patagonia. Climate of the Past, 8, 403-414. Rivera, A., F. Bown, C. Acuña, and F. Ordenes (2008). Chilean glaciers as indicators of climate change. Terra Glacialis, 11, 193-207. Skvarca, P., H. de Angelis, R. Naruse, C.R.Warren and M. Aniya. (2002). Calving rates in fresh water: new data from southern Patagonia. Ann. Glaciol., 34: 379-384. [Andrés Rivera, Chile]	Unclear what being requested. No change made.
4-1707	4	49				Regarding previous comments I propose to include the following references:Cannone N, Diolaiuti G, M. Guglielmin, Smiraglia C (2008). Accelerating Climate Change Impacts On Alpine Glacier Forefield Ecosystems In The European Alps. Ecological Applications, Vol. 18, P. 637-648.Guglielmin M, Balks MR, Adlam LS, Baio F. (2011). Permafrost Thermal Regime from Two 30-m Deep Boreholes in Southern Victoria Land, Antarctica . PERMAFROST AND PERIGLACIAL PROCESSES, vol.22, p. 129-139.Guglielmin M., Cannone N. (2012). A permafrost warming in a cooling Antarctica?. CLIMATIC CHANGE, vol. 111, p. 177-195. [Mauro Guglielmin, Italy]	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1708	4	49				Refs: Azam et al and Bhambri et al are incomplete [Umesh Kulshrestha, India]	Noted - Formatting of references revised and checked for completeness
4-1709	4	50	7	50	7	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1710	4	50	11	50	11	There is no enough information on the paper (journal, volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1711	4	50	28	50	28	There is no enough information on the publication [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1712	4	50	39	50	40	Title of Brown & Cote (1992) paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1713	4	50	44	50	44	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1714	4	50	46	50	46	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1715	4	51	15	51	15	It should be "Recent" instead of "Srecent" [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1716	4	51	25	51	25	The words "Maladeta" and "Pyrenees" should start with capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1717	4	51	38	51	38	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1718	4	51	38	51	41	two references are same [Yongjian Ding, China]	Noted - Formatting of references revised and checked for completeness
4-1719	4	51	38	51	41	These 2 papers by Comiso are in fact the same paper, listed twice [Andrey Shmakin, Russian Federation]	Noted - one removed
4-1720	4	51	45	51	46	Unclear which DOI should be taken; probably the first one should be deleted [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1721	4	51	50	51	50	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1722	4	52	4	52	4	The info on volume and year is repeated twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1723	4	52	7	52	8	The reference doesn't look well: the name of journal and other information must be written [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1724	4	52	21	52	22	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1725	4	52	27	52	27	replace "Dong Chen E." as "E D. C." [Yongjian Ding, China]	Noted - Formatting of references revised and checked for completeness
4-1726	4	53	5	53	5	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1727	4	53	10	53	10	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1728	4	53	18	53	18	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1729	4	53	26	53	26	repeated references [Matt King, Australia]	Noted - one removed
4-1730	4	53	26	53	26	All authors should be listed, not just "et al." [Andrey Shmakin, Russian Federation]	Noted - we comply with house style rules
4-1731	4	53	26	53	27	two references are same [Yongjian Ding, China]	Noted - one removed
4-1732	4	53	26	53	27	These 2 papers by Gardner et al. are in fact the same paper, listed twice [Andrey Shmakin, Russian Federation]	Noted - one removed
4-1733	4	53	27	53	28	Repeated reference. [Andrés Rivera, Chile]	Noted - one removed
4-1734	4	53	31	53	31	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1735	4	53	42	53	42	The page numbers are missing [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1736	4	53	44	53	44	The paper by Glazovsky and Macheret is located on pages 97-114 in the cited book [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1737	4	53	46	53	46	Not enough information: if it is a book, one needs publishing company; or a journal info [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1738	4	53	53	53	54	Not enough information on the publication [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1739	4	54	10	54	10	Not enough information on the publication [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1740	4	54	25	54	25	Probably something is wrong with the DOI number [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1741	4	54	45	54	45	Incomplete reference. [Jing Ming, China]	Noted - Formatting of references revised and checked for completeness
4-1742	4	54	45	54	45	There is no enough information on the paper (volume, pages, etc.) [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1743	4	54	45			Incomplete reference. [Shichang Kang, China]	Noted - Formatting of references revised and checked for completeness
4-1744	4	54	49	54	49	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1745	4	54	50	54	51	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1746	4	54	54			Huss & Farinotti (2012) is published now: Huss, M. & D. Farinotti 2012: Distributed ice thickness and volume of all glaciers around the globe. Journal of Geophysical Research - Earth Science. 117(F04010), 10pp [Luzi Bernhard, Switzerland]	Noted - Formatting of references revised and checked for completeness
4-1747	4	54	54			Incomplete reference. [Andrés Rivera, Chile]	Noted - Formatting of references revised and checked for completeness
4-1748	4	54	55	54	55	The name of the second author is spelled incorrectly: it should be "Stockli" [Heinz Blatter, Switzerland]	Noted - Formatting of references revised and checked for completeness
4-1749	4	54				FAQ 4.2: Suggest a discussion of caveats with regard to trend deductions (e.g., from different method in different parts of the Arctic at different time over different periods) [Government of United States of America]	Misplaced comment - noted
4-1750	4	55	2	55	2	It should read "Yamal-Nenets" [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							for completeness
4-1751	4	55	7	55	7	Not enough information on the book [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1752	4	55	15	55	16	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1753	4	55	55	55	56	Kääb et al. (2012) is published now: Kääb, A.; Berthier, E.; Nuth, C.; Gardelle, J. & Arnaud, Y. 2012. Contrasting patterns of early twenty-first-century glacier mass change in the Himalayas. Nautre, 488, 495–-498 [Luzi Bernhard, Switzerland]	Noted - Formatting of references revised and checked for completeness
4-1754	4	56	50	56	50	Volume and year are printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1755	4	56	60	56	60	Page numbers are missing [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1756	4	57	8	57	13	Duplicate references [David Bromwich, United States of America]	Noted -one removed
4-1757	4	57	10	57	10	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1758	4	57	31	57	31	Not enough information on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1759	4	57	44	57	44	Not enough information on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1760	4	58	8	58	8	Probably "Ieee" must be written in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1761	4	58	18	58	18	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1762	4	58	28	58	28	Not enough information on the journal [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1763	4	58	45	58	46	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1764	4	58	63	58	63	The volume number is repeated twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1765	4	59	20	59	20	Probably something is wrong with the DOI number [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1766	4	59	27	59	27	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1767	4	59	45	59	45	It should read "Yamal-Nenets" [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1768	4	59	57	59	57	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1769	4	60	1	60	1	In Figure 4.2, in Type: Land-terminating glaciers in mountainous areas, please include "Antarctic Peninsula" [Jefferson Cardia Simões, Brazil]	Noted
4-1770	4	60	7	60	7	Year and volume are printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							for completeness
4-1771	4	60	13	60	13	Year and volume are printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1772	4	60	22	60	22	Page numbers are missing [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1773	4	60	52	60	52	Probably something is wrong with the DOI number [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1774	4	60	62	60	62	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1775	4	61	8	61	8	the exact reference is : Rabatel, A. B. Francou, A. Soruco, J. Gomez, B. Cáceres, J. L. Ceballos, R. Basantes, M. Vuille, J.-E. Sicart, C. Huggel, M. Scheel, Y. Lejeune, Y. Arnaud, M. Collet, T. Condom, G. Consoli, V. Favier, V. Jomelli, R. Galarraga, P. Ginot, L. Maisincho, J. Mendoza, M. Ménégoz, E. Ramirez, P. Ribstein, W. Suarez, M. Villacis, and P. Wagnon, Review article of the current state of glaciers in the tropical Andes: a multi-century perspective on glacier evolution and climate change, The Cryosphere Discuss., 6, 2477-2536, 2012 [Patrick WAGNON, Nepal]	Noted.
4-1776	4	61	20			Incomplete reference. [Andrés Rivera, Chile]	Noted - Formatting of references revised and checked for completeness
4-1777	4	61	28	61	28	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1778	4	62	16	62	16	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1779	4	62	47	62	47	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1780	4	62	60	62	60	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1781	4	63	32	63	32	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1782	4	63	55	63	55	Probably "Ieee" must be written in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1783	4	64	21	64	21	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1784	4	65	14	65	14	Probably something is wrong with the DOI number [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1785	4	65	20	65	20	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1786	4	66	7	66	8	Title of the paper is printed in capital letters [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1787	4	66	19	66	22	It would be fair to provide the names of those who edited these publications - this was a lot of work! [Wilfried Haeberli, Switzerland]	Noted. We comply with house style
4-1788	4	66	22			Replace "WGMS (2009)" by "WGMS (2012)": Reference:	Noted. Reference updated

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						WGMS (2012): Fluctuations of Glaciers 2005-2010 (Vol. X): Zemp, M., Frey, H., Gärtner-Roer, I., Nussbaumer, S.U., Hoelzle, M., Paul, F. and Haeberli, W. (eds.), ICSU (WDS) / IUGG (IACS) / UNEP / UNESCO / WMO, World Glacier Monitoring Service, Zurich, Switzerland: 336 pp. Publication based on database version: doi:10.5904/wgms-fog-2012-11 [Michael Zemp, Switzerland]	
4-1789	4	66	30	66	30	Not enough info on the paper [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1790	4	66	59	66	59	The word "doi" is printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1791	4	66	60	66	60	Change "Yde, Y. C." to "Yde, J. C.". [Jacob Clement Yde, Norway]	Noted - Formatting of references revised and checked for completeness
4-1792	4	66	60	66	60	Change "Pasche" to Paasche". [Jacob Clement Yde, Norway]	Noted - Formatting of references revised and checked for completeness
4-1793	4	67	2	67	2	Year and volume are printed twice [Andrey Shmakin, Russian Federation]	Noted - Formatting of references revised and checked for completeness
4-1794	4	67	17			Journal name is missing. [Andrés Rivera, Chile]	Noted - Formatting of references revised and checked for completeness
4-1795	4	68	1	68	1	Change colour lines to horizontal colour bars. It is very difficult and sometimes impossible to distinguish the lines in Figure 4.10. [Jefferson Cardia Simões, Brazil]	Unclear which page/figure this comment applies to.
4-1796	4	68	1	68	1	Give latitude range for Southern Andes [Jefferson Cardia Simões, Brazil]	Unclear which page this comment applies to.
4-1797	4	68	12	68	12	The start date and end date have some puzzles,such as 2002.375, 375 need to explain [Yongjian Ding, China]	Noted. Numbers are actually decimal years. Allnow cited to the same number of significant figures to make it clearer
4-1798	4	68	12	68	12	The remark "CNES fields are truncated to lower harmonics than other fields" is not very informative or accurate. What should be said instead in my opinion is that the CNES fields are interconstrained at 10 daily time steps which is a unique solution strategy because the rest of the GRACE community usually delivers unconstrained monthly solutions. [Ernst Schrama, Netherlands]	Accepted. Text revised along the lines suggested
4-1799	4	69	0	69	0	Table 4.A.3. It is not clear why Zwally and Giovenetto 2011 does not figure in the consideration of Antarctic mass balance (or in the excluded list in Table 4.A.4) [Government of Australia]	Noted. Zwally and Giovinetto has been excluded, but is now included in the Table with a an explanation for exclusion
4-1800	4	69	0	70	0	Table 4.A.3 and Table 4.A.4. Heading of second-last column seems incorrect - should this be includes/excludes peripheral glaciers, as in the Greenland Tables 4.A.1 and 4.A.2? [Government of Australia]	Accepted. Correction made.
4-1801	4	69	3	69	3	update King et al number to -69+-9 (1 sigma) [+-18 at 2 sigma given in paper]. It is important to note the bounds in the comment column as this study treats systematic uncertainty as bounds not random error unlike all other GRACE studies cited. [Matt King, Australia]	Accepted. SOD number was from an earlier draft of paper. Correction made
4-1802	4	69	3	70	3	move Moore and King to Table 4.A.4 [Matt King, Australia]	Accepted
4-1803	4	69	3	70	3	all GRACE studies used should have the GIA model used included in the comments column [Matt King, Australia]	Rejected. This information is available in the original papers
4-1804	4	72	0	71	0	Figure 4.1: it would seem appropriate to replace the 2011 image with a 2012 one, if allowed by IPCC rules. [Government of Australia]	Accepted. The figure has been revised accordingly
4-1805	4	72	5	72	5	I found Figure 4.1 confusing. NH is minimum, SH is maximum sea ice cover. Best would be to show both maximum and minimum sea ice cover on both hemispheres. [Martin Schneebeli, Switzerland]	Rejected. There are many possibilities and in this case we are highlighting the highlights in both hemisphere. The Antractic ice is increasing in extent mainly because of its more extensive winter ice cover.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1806	4	72	5			Figure 4.1: For consistency with the text, this figure might better show the glacier dataset by Arendt et al. (2012). [Michael Zemp, Switzerland]	Accepted. We will use the best glacier data set available.
4-1807	4	72	5			Figure 4.1: The GLIMS database is to be cited as GLIMS (YEAR) and not as Zheltyhina (2005). The latest version is: GLIMS (2012): GLIMS glacier database. Armstrong, R., B. Raup, S.J.S. Khalsa, R. Barry, J. Kargel, C. Helm, and H. Kieffer (eds.), U.S. National Snow and Ice Data Center, Boulder, Colorado, USA: Digital media. Online available from: http://www.glims.org [Michael Zemp, Switzerland]	Accepted. Will revise accordingly.
4-1808	4	72	8	72	10	Clarify whether the greatest extent Northern Hemisphere snow cover during the July 2009 - March 2010 period is represented by the red line or the black line (as the caption states). [Government of Canada]	Accepted. lines for snow are now better represented and cited.
4-1809	4	72	10	72	11	Figure 4.1, caption. Clarify whether the Greenland ice sheet in the figure is blue/grey in colour. [Government of Canada]	Noted. color is now clarified
4-1810	4	72		72		Figure 4.1: "The shaded area over land and permafrost shows snow cover…" This shading is not clear in the image. The black line does not correspond to the maximum snow cover extent between July 2009 and March 2010 as noted in the caption. [Chris Derksen, Canada]	Accepted. Figure has been altered and shading is now better represented.
4-1811	4	72		72		In Fig. 4.1, the meaning of "50% snow extent line" is unclear. It must be commented in the figure caption. Is it for a certain time or averaged over some months? Generally it looks like the line must be placed farther to the south: most of the East European plain is entirely covered by snow every year. [Andrey Shmakin, Russian Federation]	Accepted. Snows lines are now better represented in the figure and discussed accordingly.
4-1812	4	72				Figure 4.1: The color differences between the following items should be much clearer. "Sea Ice" and "Ice Sheet" "Continuous Permafrost" and "Discontinuous Permafrost" [Government of Japan]	Accepted. Figure has been revised
4-1813	4	72				In Figure 4.1, regarding "Sea Ice 30 Yr Ave Extent", "50% Snow Extent Line" and "Max Snow Extent Line", their border lines are drawn. Therefore, their shape in the legend should be changed from square to (thick) line. [Government of Japan]	Accepted. Figure has been altered accordingly.
4-1814	4	72				Figure 4.1. The low resolution of the South America portion of the figure preclude its analysis [Andrés Rivera, Chile]	Noted. Not easy to depict everything in the figure at high resolution
4-1815	4	72				Figure 4.1: The colours of continuous and discontinuous permafrost should be easier to separate in the figure. [Jacob Clement Yde, Norway]	Accepted. The figure has been altered to eliminate the problem.
4-1816	4	73	1	73	1	Figure 4.2b-e. The trend color scale could be improved to better contrast positive and negative trends, e.g., the dark blue of large positive looks similar to the purple of moderate negative trends [Walter Meier, United States of America]	Rejected. The lack of contrast is not obvious. The important results are well represented by distinct colors.
4-1817	4	73	5	73	5	Figure 4.2 caption states that "…five-year average daily ice extent from 2009 to 2011" Should this state three-year average? [Chris Derksen, Canada]	Accepted. The new version is for 4 years (2009-2012).
4-1818	4	73	5	73	5	replace "five-year" as "three-year" [Yongjian Ding, China]	Accepted. See above
4-1819	4	73	5	73	5	"a five-year average" should be "a three-year average" because the period from 2009 to 2011 is 3 years. [Government of Japan]	Accepted. See above
4-1820	4	73	5	73	5	Figure 4.2: Suggest revision to: "…and a three-year average daily ice extent from 2009 to 2011 (BLACK) …" [Government of United States of America]	Accepted. See above
4-1821	4	73	5			Should read 'three-year average' (or is it the start date which is wrong, with the figure covering 2007-2011?).	Accepted. See above

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						[Government of Australia]	
4-1822	4	73	5			"and a five year average daily ice extent from 2009 to 2011." Should this be 3 year? [Donald Perovich, United States of America]	Accepted. See above
4-1823	4	73	5			Change "five-year average" into "three-year average" in text below Figure 4.2 [Terje Wahl, Norway]	Accepted. See above
4-1824	4	73				Figure 4.2: The size of Figure (b), (c), (d) and (e) should be larger if possible. [Government of Japan]	Accepted. Figure was revised
4-1825	4	73				Figure 4.2a: Suggest the authors provide error estimates associated with the measurements. [Government of United States of America]	Figure redrafted
4-1826	4	73				identify black line in caption [Christopher Little, United States of America]	Accepted.
4-1827	4	73				This is Figure 4.2. In the caption (line 5), “five-year” should be “three-year” because it refers to 2009-2011. Also, following “from 2009 to 2011”, add “in black”. About the figure itself: in my opinion, panels b, c, d, and e are too small, and the latitude/longitude grids that are overlaid on the panels are a distraction that make the panels more difficult to process visually and that mask the underlying information. [Harry Stern, United States of America]	Accepted. See above
4-1828	4	74	1	74	1	Figure 4.3. Is the satellite part of the timeseries plotted in (a) the same as in (b) and are they the same as used elsewhere in the chapter? (I think Walsh and Chapman originally used NASA Team) [Walter Meier, United States of America]	Noted. The plots now make use of Bootstrap results. The Walsh and Chapman values are not consistent with the NT1 from GSFC.
4-1829	4	74	1			The line that connects the satellite PM data and the Walsh data should be removed…it masks the fact that they are different data sets. [Ron Lindsay, United States of America]	Noted. A normalization technique with the satellite data used as the baseline where there is coincident data is now being developed
4-1830	4	74	4	74	6	It is doubtful that the in situ measurements from1870 measured the same quantities as the satellites in 2919 [Vincent Gray, New Zealand]	Noted.
4-1831	4	74				Figure 4.3, Caption. Suggest revising to: “Figure 4.3. (a) Yearly and (b) seasonal ice extent in the Arctic using averages of mid-month values derived from in situ (1870 to 1978) and satellite (1979 to 2011) data (updated from Walsh and Chapman, 2001). The apparent drastic reduction of sea ice extent from 1978-1979 is in part due to the change from one data set to another.” [Government of United States of America]	Accepted
4-1832	4	74				Figure 4.3: Suggest including error bars. Why there is a jump between Wash and Chapman and passive microwave in all plots except for JFM ice extent for 1980? [Government of United States of America]	Noted
4-1833	4	74				Figure 4.3. The graph does not plot measurement uncertainties which is quite limited. [Andrés Rivera, Chile]	Accepted. Figure being revised
4-1834	4	74				This is Figure 4.3. In the caption (line 6), change “triangle fonts” to either “triangles” or “triangle symbols”. About the figure itself: I really don’t trust this data set before 1979!! But never mind, it’s published so go ahead and use it. [Harry Stern, United States of America]	Accepted
4-1835	4	74				Is Walsh and Chapman 2001 really the most recent publication regarding historical sea ice extent? [Terje Wahl, Norway]	Noted. We don't know of any other that covers the entire Arctic region
4-1836	4	75	1	75	1	Figure 4.4. Typo in legend: "Perennial" [Walter Meier, United States of America]	Accepted. Typo corrected
4-1837	4	75	1			There are too many significant figures in the absolute trends. [Ron Lindsay, United States of America]	Accepted. Values now rounded
4-1838	4	75	4	75	7	expalanation in page 10, line 36 may be include here for easily understanding. [Hiroyuki Enomoto, Japan]	Noted
4-1839	4	75	4			The trend appears to become increasingly non-linear - is this important fact correctly mentionned or should a non-linear fit even be given in Figure 4? [Wilfried Haeberli, Switzerland]	Noted. The data is indeed non-linear and shows an acceleration in the decline but the trend line is just a

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							first order estimate.
4-1840	4	75		75		Figure 4.4: Why are AMSR-E retrievals shown with a separate gray line? Why not just show the SMMR + SSM/I time series? [Chris Derksen, Canada]	Noted. Just to indicated that the best data set available shows consistent results.
4-1841	4	75				Figure 4.4. Per the comments on section 4.2.2.3, suggest this figure be revised to feature the difference in trend between the winter ice extent and multiyear ice, the difference showing the increase in the relative amount of seasonal ice. [Government of United States of America]	Accepted. The trend of winter maximum will be added in the texgt.
4-1842	4	75				Figure 4.4: Suggest the authors provide error bars for data points on the plots. [Government of United States of America]	Accepted. Error bars will be added
4-1843	4	75				Figure 4.4 (showing trends in multiyear and perennial ice (note that 'perennial' is misspelled in the graph's legend)) does little to get the main point across. You have to look really hard to see a difference in the trends, even when it is pointed out in the text. An alternate approach would be to plot the trends in the winter extent of the ice cover and the multiyear ice. The difference between the two indicates the increase in the relative amount of seasonal ice. If the policy of the report is to only include figures and results that have been published in the peer-review literature, than I suggest that the authors draw from the work of Maslanik et al. (2007, 2011) or Stroeve et al. (2012, see Figure 4), versus adapting the figure from Comiso, 2012 (or Comiso, 2011a, according to this report). Comiso, Josefino C., 2012: Large Decadal Decline of the Arctic Multiyear Ice Cover. J. Climate, 25, 1176–1193. doi: http://dx.doi.org/10.1175/JCLI-D-11-00113. Maslanik J. A., C. Fowler, J. Stroeve, S. Drobot, J. Zwally, D. Yi, W. Emery, 2007. A younger, thinner Arctic ice cover: Increased potential for rapid, extensive sea-ice loss, Geophys. Res. Lett., 34, L24501, doi:10.1029/2007GL032043. Maslanik, J., J. Stroeve, C. Fowler, and W. Emery. 2011. Distribution and trends in Arctic sea ice age through spring 2011. Geophysical Research Letters 38, L13502, doi:10.1029/2011GL047735. Stroeve, J.C., M.C. Serreze, M.M. Holland, J.E. Kay, J. Malanik and A.P. Barrett (2012) The Arctic's rapidly shrinking sea ice cover:A research synthesis, Climatic Change, 110:1005–1027 DOI 10.1007/s10584-011-0101-1. [Government of United States of America]	Noted. It is not known that the "Ice Age Retrieval" is more dependable than the multiyear ice retrieval. Keeping track of the age of ice floes can be problematic, especially during the summer period when the signature of the ice changes drastically.
4-1844	4	75				I think a better plot to replace the current Fig. 4.4 would be Fig. 3 of Nghiem et al. 2007. This provides more long-term context for the changes in Arctic sea ice extent than Comiso, 2011b. [Ignatius Rigor, United States of America]	Rejected. The Nghiem et al 2007 do not provide a longer record length for ice extent.
4-1845	4	75				This is Figure 4.4. Several comments: 1. In light of my comments above about the definitions of multiyear ice and perennial ice, the caption and labeling of this figure would have to be changed. 2. The caption says the data represent the years 1979 to 2011, but the actual data points appear to be plotted from 1980 to 2012. Also, the caption refers to the gray line as starting in 2002, but the first gray data point is plotted at 2003. So there appears to be a one-year offset between the description in the caption and the plotting (or axis labeling). 3. The word "Perennial" is misspelled in the figure panels. 4. The trend values given in the figure panels (in square kilometers per year) have way too many significant figures. I'm certain that the trend in sea ice loss is not known to the nearest one-tenth of a square kilometer per year. [Harry Stern, United States of America]	1. Noted; The plot for perennial ice is for September while that for multiyear ice is for January. The is the reason why the perennial data point appears to start in 1980 instead of 1979. The spelling and significant digit problems have been fixed.
4-1846	4	76				Figure 4.5. Given the uncertainty in satellite-derived ice thickness estimates and the shortness of the record, suggest removing this figure. Further, much of the data is also included in Figure 4.7. If the figure is retained, suggest the authors provide error bars for the data points and explain the white areas where data are missing. [Government of United States of America]	Rejected. This shows important spatial structure of the Arctic sea ice cover.

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4-1847	4	77	1	77	1	Figure 4.6b-e. As in previous comment, color scale is a bit ambiguous [Walter Meier, United States of America]	Noted. Current figure does a good job in representing the important information.
4-1848	4	77	5	77	5	"average value of" should be "a three-year average" because the period from 2009 to 2011 is 3 years. [Government of Japan]	Accepted. Figure has also been revised and the black line is now a 4-year average
4-1849	4	77	6	77	6	There is no citation for this Figure. It should perhaps be the same citation as per Figure 4.2? [Government of Australia]	Accepted.
4-1850	4	77				Figure 4.6: The size of Figure (b), (c), (d) and (e) should be larger if possible. [Government of Japan]	Accepted. Figure has been revised
4-1851	4	77				Figure 4.6a: Suggest the authors find a way to illustrate uncertainty in the graphs. Are the error bars different in the austral winter and austral summer? [Government of United States of America]	Accepted. Uncertainties added.
4-1852	4	77				This is Figure 4.6. Same comments as for Figure 4.2 (page 73) above. [Harry Stern, United States of America]	Accepted
4-1853	4	78	1	78	1	Figure 4.7, The trend images here use a better color scale and I think use the same color scheme for all of the trend images would be better. [Walter Meier, United States of America]	Accepted.
4-1854	4	78	1	78	1	Figure 4.7. Typo "Length" in header to right side of (d) [Walter Meier, United States of America]	Editorial.
4-1855	4	78	1	78	2	Figure 4.7a should be updated with data through September 2012. [Seymour Laxon, United Kingdom]	Accepted.
4-1856	4	78	1		2	After discussing both the Arctic and Antarctica, the figure shows data only for the Arctic. To be fair you should show both the Arctic and Antarctica in the same or add a separate figure. Otherwise you will be accused of creating one-sided view of polar regions. [Petr Chylek, United States of America]	Rejected. These are summary plots of the Arctic sea ice cover. The Antarctic is summarized in a following section.
4-1857	4	78	4	78	4	in a), due to the vertical axis is 106 km2, it is better to give the trend as 106 km2/decade (-4.6 ±0.2% /decade)"; in c), what's means of "Jan-1" [Yongjian Ding, China]	Noted.
4-1858	4	78		78		Clarify whether there is a reference error. Kwok 2009 in the list of references on page 56 is a Fram Strait export paper, not a Quikscat MYI trend paper. There does not appear to be a Kwok in his list of publications which analyses a 1990-2010 time series of Quikscat data. The correct reference for the data shown in this panel needs to be provided. Figure 4.7c is not referenced in the text but it could be referred to in the MYI section, along with Figure 4.4b. [Government of Canada]	Noted. This has been corrected.
4-1859	4	78		78		Figure 4.7d. The data presented in the left hand panel of Figure 4.7d appear to have been updated/extended from that presented in Markus et al. (2009). The reference in the caption should therefore read (updated from Markus et al., 2009). Furthermore, the right hand panel is not found in Markus et al. (2009), as such a reference for this panel is needed. Figure 4.7d is never referenced in the text, but it could have been referred to in the last paragraph of section 4.2.2.6 on page 13. [Government of Canada]	Accepted.
4-1860	4	78		78		Figure 4.7e contains a possible reference error. While the right-hand panel is contained in Spreen et al., 2011, the lefthand panel is not. Furthermore, there is an error in the trend value shown in the left-hand panel: "0.94±0.3 km/day/decade" should be corrected to "0.94±0.8 cm/s/decade". And even further, the buoy drift in this panel is not found in Spreen. Although mention of buoy drift is made in the introduction and discussion sections of Spreen (again in cm/s/decade, NOT km/day/decade), the data appear to have come from Rampal et al., 2009 and the value in that paper is 0.056±0.011 km/day/year (or -0.56±0.11 km/day/decade) NOT 0.55±0.04 km/day/decade. This needs to be checked. [Government of Canada]	Accepted.
4-1861	4	78		78		Fig 4.7b. I am certainly biased here but wouldn't a time series of volume from a validated sea ice reanalysis that covers the entire Arctic (not just that DRA) be a better representation of the ice loss? The uncertainty of the reanalysis isn't any worse than that of the observations and the "regression model" used here. Why is winter used here instead of summer? Figure 4.7a shows summer, 4c I don't know. For consistency and to minmize confusion the multi-year time series should probably be the same as the one shown in 4.4. See above statement regarding the redundancy of figures 4.3 and 4.4. If 4.4 multi-year time series was shown here, then 4.4 could be eliminated. [Axel Schweiger, United States of America]	Noted. The discussion has been revised.

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4-1862	4	78				Figure 4.7a indicates an extent trend of -4.6%, with a reference to Comiso and Nishio (2008). However, this reference does not contain such a number; Comiso and Nishio (2008) give -3.4% (extent) and -4.0% (area). Furthermore, the concentration trend panel to the right of the time series plot in Figure 4.7a is not found in Comiso and Nishio (2008). A reference for this plot (and for the 4 others immediately beneath it) should be given in the caption as well. [Government of Canada]	Accepted.
4-1863	4	78				Figure 4.7: Suggest that this figure be used in place of Figure 4.5 and that it be addressed more directly in the text. For instance, there is no reference to this figure in Section 4.2.2.5, even though the last panel presents related results. [Government of United States of America]	Rejected. But, we have included additionally discussion of the figures in the text.
4-1864	4	78				Fig. 4.7: Suggest revising the top panel set so that both show ice extent, versus one showing extent and the other concentration. [Government of United States of America]	Rejected. It is more intuitive to show the ice extent changes as a line plot.
4-1865	4	78				Figure 4.7: This figure is confusing in showing different parameters with different coverage of the Arctic, where there are missing data zones in different regions, and at different times of the year. For the first panel, extent in what season? Summer? [Government of United States of America]	Noted. This has been clarified in the caption. The ice extent time series represents average annual ice extent.
4-1866	4	78				Figure 4.7: In order to follow the discussion in the text of Section 4.2.2, suggest that panel (e) and panel (d) be swapped, so that ice drift comes before length of melt season. [Government of United States of America]	Accepted.
4-1867	4	78				Figure 4.7: Would it be possible to include error bars on the plots? [Government of United States of America]	Accepted.
4-1868	4	78				This is Figure 4.7. In the caption (line 9), add the word “ice” before “drift speed”. In the panel called “Length of melt season” that shows a map of the Arctic, the word “Length” is misspelled (it is written as “Lenght”). [Harry Stern, United States of America]	Accepted.
4-1869	4	78				Figure 4.7 (e). The legend is incomplete. It should be: (e) Buoy-derived (Rampal et al., 2009) and satellite-derived (Spreen et al., 2011) drift speed. [Jérôme Weiss, France]	Accepted.
4-1870	4	78				Figure 4.7: Caption (a) "Arctic ice extent" - Not clear if this is annual, September minimum, JAS minimum. Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
4-1871	4	79	4	79	4	write out RGI in caption, reference to Figure in text (p 6) is before abbreviation is mentioned first time in text (p 17) [Dorothea Stumm, Nepal]	Accepted
4-1872	4	79	5	79	5	Page 79 line 5 Figure 4.8 suggest .....tide water (TW).... [Nathaniel Lee Bindoff, Australia]	Accepted
4-1873	4	79	5	79	5	"Table 4.3" should be "Table 4.2". [Government of Japan]	Accepted
4-1874	4	79	5	79	5	Table 4.3 doesn't show any RGI information. 19 RGI Regions need to be explained. [Jing Ming, China]	Noted: has been corrected to Table 4.2
4-1875	4	79	5			Add (TW) after tidewater [Etienne BERTHIER, France]	Accepted
4-1876	4	79	5			Table 4.3 doesn't show any RGI information. 19 RGI Regions need to be explained. [Shichang Kang, China]	Noted: has been corrected to Table 4.2
4-1877	4	80	1	80	1	Figure 4.9: in the graph for the region 16 Low latitude, there is a mistake that I have already mentioned in the FOD. The glacier Tyndall (blue curve) is in the southern patagonian icefield and do not belong the the low latitudes but to the southern Andes. Furthermore, more data about glacier cumulative changes can be found in Rabatel et al. 2012, a review paper on tropical glaciers changes which is in press in The Cryosphere [Antoine RABATEL, France]	Mistake: Noted: Here we refer to the glacier Tyndall in Kenia. - Paper Noted: For inclusion of data we would have needed the data rather than the paper.
4-1878	4	80	1	80	6	Fig. 4.9: Which criteria were applied to select this sub-sample of glaciers. In some cases is not representative for the majority of glaciers in the given region. E.g. in the Southern Andes (dominated by the Patagonian Ice Fields) only very few glaciers showed advancement in the 1990s. On the other hand the rapid retreat of the only glacier of the Patagonain Icefield shown here (Glaciar Upsala, a calving glacier) can to the main part be attributed to retreat of the ice front to deeper water and associated dynamic thinning. On page 4-19, line 32-22, it is stated that "some glacier types show front variations that occur independently from the climatic forcing (e.g. calving or surging glaciers) and should be excluded in climate-related analysis". This was apparently not	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked.

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						taken into account when drawing this figure. [Helmut Rott, Austria]	
4-1879	4	80				Figure 4.9: The size of each figure should be larger if possible. [Government of Japan]	Editorial
4-1880	4	80				fig. 4.9: Condsider including Alaska as one of the fastest changing regions. [Government of United States of America]	Noted: There are no long-term records of length changes of glaciers in Alaska, so we have added two reconstructed shorter-term time-series.
4-1881	4	80				Figure 4.9: Would it be possible to include error bars on the plots? [Government of United States of America]	Noted: Error bars are generally not reported for field measurements of terminus fluctuations. They are in the range of afew metres and thus below the thickness of the line.
4-1882	4	80				How are glaciers selected in each region? [Christopher Little, United States of America]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked.
4-1883	4	80				Figure 4.9. The inclusion of the Upsala calving glacier in this figure is missleading, because this is a freshwater calving glacier, known to have experienced non-climatic retreats. [Andrés Rivera, Chile]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked.
4-1884	4	80				Figure 4.9: Seems very problematic to show only a 'selection' of length changes from around the world, without having some explanation of how this 'selection' was done. Perhaps these are the longest available time series from each region, in which case this should be clearly stated. Otherwise this figure becomes an easy target for readers who will criticise why some length changes are shown when others are not. For example, why would you include only a single glacier from New Zealand, when this glacier is known to have unique characteristics which make it a poor representative of New Zealand glaciers in general? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked. In some regions we indeed only have long-term records of one glacier.
4-1885	4	80				Figure 4.9: This figure should be replaced by a figure showing regional terminus retreat rates (km yr-1) as a function of the period used for averaging (comparable to Figure 4.10). [Jacob Clement Yde, Norway]	Rejected: As glaciers have different response times retreat rates should never be averaged. Terminus changes are reported in m per year (as the km scale might only apply to some surging glaciers).
4-1886	4	80				Figure 4.9: Legend to region 5 Greenland: Used the modern names of these glaciers (see Leclercq et al., submitted). [Jacob Clement Yde, Norway]	Editorial
4-1887	4	80				Figure 4.9: Data used in region 5 Greenland: Parts of the data for Sermikavsak are from Gribbon, 1970 [Gribbon, P. W. F., 1970. J. Glaciol., 9, 279-282]; for Tunorsuaq are from Yde and Knudsen, 2007 [Yde, J.C., and N.T. Knudsen, 2007. A. Glaciol., 46, 209-214]; and for Motzfeld and Sermitsiaq are from Weidick, 1988 [Weidick, A. 1988. Grønlands Geologiske Undersøgelse]. The sources of these data should be traced to, and therefore reference, the original datasets according to good academic practice (see Leclercq et al., submitted, Figure 2). [Jacob Clement Yde, Norway]	Rejected: These references are cited in the cited publication so they will not be repeated here.
4-1888	4	80				Figure 4.9: Legend to region 6 Iceland: Inform the reader what "E3" after Skeidararjökull refers to? [Jacob Clement Yde, Norway]	Editorial: This is the name of the measured glacier part
4-1889	4	80				Figure 4.9, second line in captions: Breidamerkurjökull and Skeidarárjökull are also surging glaciers!! I have not check the remaining three Icelandic glaciers but this should be done before publication. [Jacob Clement Yde, Norway]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked.
4-1890	4	80				Figure 4.9: In the captions, the reader must be informed on what objective criterias were used to select these specific glaciers and neglect other glaciers were similar records are available. [Jacob Clement Yde, Norway]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked.
4-1891	4	80				Figure 4.9: E.g. 12 Caucasus: The presentations of interpolations between two points of observation are misleading when the points are separated by several decades, e.g. for glaciers in Caucasus, Central Asia and Southern Andes. [Jacob Clement Yde, Norway]	Noted: The presented glaciers havebeen re-selected.

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4-1892	4	80				Figure 4.9: second line in captions: Change "show surge-type behaviour" to "experience active glacier surge events". Some glaciers show signs of surge-type behaviour but do not experience active glacier surge events anymore. [Jacob Clement Yde, Norway]	Editorial
4-1893	4	80				Figure 4.9: What is the purpose of including surge-type glaciers (6 Iceland), if the aims of this section and WGI AR5 are to deduce a climatic signal from glacier length changes? Delete surge-type glaciers from the figure. [Jacob Clement Yde, Norway]	Taken into account: The text has been modified to better describe the selection criteria. Some calving and surging glaciers are marked and it is made clear why these are shown.
4-1894	4	81	1	81	1	Figure 4.10: Should include data from Rabatel et al., The Cryosphere, in press, or at least a reference to this paper. [Antoine RABATEL, France]	Rejected: We only report here results from large samples of glaciers when averaged over entire mountain ranges.
4-1895	4	81	1	81	17	Fig 4.10: It is very difficult to discriminate some of the colours in this figure, and to relate these to the various glacier regions, in particular as many of the lines are very closely spaced. Using a representative subset of these data with clear focus would better serve the case. [Helmut Rott, Austria]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1896	4	81	4	81	4	The colors of lines is difficult to identify,suggest use different line style or thickness [Yongjian Ding, China]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1897	4	81	4	81	4	"for 14 out of the 19" should be "for 15 out of the 19". Four regions ("5", "6", "9" and "12") are not described in Figure 4.10 and its caption. [Government of Japan]	Editorial
4-1898	4	81		82		Figure 4.10 and 4.11. Resolution in printing form is insufficient to distinguish trends. With this resolution and the lack of precise information of eachdata set, it is impossible to check if the numbers are OK or not [Andrés Rivera, Chile]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1899	4	81				Figure 4.10: The number of lines per region should be reduced if possible, for the ease of understanding the figure. [Government of Japan]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1900	4	81				Figure 4.10. This figure is almost unreadable. The color values among the different regions are so similar that it is not possible to identify the attribution on the chart in all instances. Suggest trying another approach to presenting this data. [Government of United States of America]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1901	4	81				Fig 4.10. Impossible to read figure. Similar colour for different regions. I think you will have e.g. to split up the figure in sub-figures of only a few regions (which to compare is interesting/meaningful). [Andreas Kääb, Norway]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table.
4-1902	4	81				This figure is very confusing, I suggest revisiting. If only 14 out of 19 regions are included, why is there a key for 19? [Christopher Little, United States of America]	Taken into account: Figure 4.10 has been revised for better readability and all data used are listed in a separate table. The key for 19 regions is provided for consistency with the other figures.
4-1903	4	81				Figure 4.10: Include data for the region 5 Greenland! Data can be found in Bjørk et al. 2012. Nature Geoscience, 5, 427-432; Mernild et al. 2012. The Cryosphere, 6, 625-639; Jiskoot et al. 2012. Ann. Glaciol., 53, 35-44; Kargel et al. 2012. The Cryosphere, 6, 533-537. [Jacob Clement Yde, Norway]	Rejected: These studies only use arbitrarily selected glaciers rather than providing results over entire mountain ranges.
4-1904	4	82	1	82	1	Figure 4.11: in the graph for the region 16 Low latitude, more data can be found from Rabatel et al., The Cryosphere, in press. In this review paper, there is a synthetic figure presenting all the available data for mass changes over the last 50 years in the tropical Andes, with an average curve for this region. [Antoine RABATEL, France]	Rejected: the information given in Rabatel et al does not fit into the rules applied for inclusion in Fig 4.11, i.e. they have too small areal coverage
4-1905	4	82	1	82	20	Fig 4.11: The great efforts in compiling this figure have to be acknowledged. However, so much information has been squeezed in that it is very difficult for the reader to catch the essential information. Also on this case using a representative subset of these data with clear explanations should be better serve the case of this report. [Helmut Rott, Austria]	Taken into account: Figure 4.11 has been revised for better readability

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4-1906	4	82	9	82	9	I could not trace legend index K in sub-figure 5 where I would expect it. [Ernst Schrama, Netherlands]	Taken into acount: figure 4.11, references, and legends have been revised
4-1907	4	82				Figure 4.11: The size of each figure should be larger if possible. [Government of Japan]	Noted: figure has been revised for more clarity
4-1908	4	82				fig. 4.11 Are SLE meant to be mm or mm/yr? Specific balances are given as rates, but SLE as total - the time period that total corresponds to is unclear. This SLE info is also missing for AK, where a conversion is given instead for 1000 kg /m^2 and this should be a rate anyway, correct? [Government of United States of America]	Noted: figure has been revised
4-1909	4	82				Figure 4.11: Line 7: Delete the first "Schiefer". [Jacob Clement Yde, Norway]	Editorial
4-1910	4	82				Figure 4.11: Line 13: Are t and e from the same dataset (Gardner et al., submitted)? [Jacob Clement Yde, Norway]	Noted: they are from different papers. Figure has been revised.
4-1911	4	82				Figure 4.11: Data from a (Marzeion et al., submitted) are presented in a way, so that it appears more clearly to the reader compared to data from other references. Consider to change how the data sets are presented in the figure. [Jacob Clement Yde, Norway]	Noted: figure has been revised and redisigned
4-1912	4	83	1	83	13	Fig. 4.12: Large differences between individual estimates of mass depletion clearly exceeding the specified error bars (in particular before 1970 but also during some of the more recent epochs), point towards some major bias(es) not taken into account in the error bars. The error estimates need to be revisited. [Helmut Rott, Austria]	Taken into account: error estimates and respective discussions have been revised
4-1913	4	83				Fig 4.12 too complex and difficult to understand [Andreas Kääb, Norway]	Noted: figure has been revised. In addition, a simplified plot is shown in the Synthesis Figure
4-1914	4	84	1	84	6	Figs 4.13 (a), (b), (c) show significant mass depletion also over the interior elevated parts of Greenland, contradicting other investigations (see comment number 15). How reliable are the patterns of isolines for ice loss taking into account the limited spatial resolution of the signal. In the reference cited (Velicogna, 2009) figures of this type (spatial pattern of ice loss) are not shown . [Helmut Rott, Austria]	consider/discuss at LA4
4-1915	4	84	6	84	6	the paper Velincogna (2009) does not contain information which could be updated to the maps in Fig. 4.13 [Heinz Blatter, Switzerland]	? Noted. Paper by Velicogna and Wahr containing these figures has been submitted and will hopefully make it before March 15 deadline.
4-1916	4	84	7	84	7	Reference Sasgen et al., 2012a (in discussion) deals with Antarctica [Helmut Rott, Austria]	Reject, the Sasgen et al. 2012 paper deals with Greenlan d
4-1917	4	84	7			Figure 4.13: ICESat (gray) [Luzi Bernhard, Switzerland]	accept
4-1918	4	84	10	84	11	Caption to Fig. 4.13(f): Pritchard et al (Fig. 2, page 973) show rate of change in surface elevation (not "ice-thinning rates") over the period 2003 - 2007 with a mean time span of 746 days (only 2 years), (not "ice thinning rates for years 2003 - 2008"). Was this altimetric analysis used to compile Greenland mass balance numbers? n Table 4.A.1 the paper Pritchard et al. (2010) is mentioned, and GRACE as data source, not altimetry. . [Helmut Rott, Austria]	Noted - the individual measurements that made up the Pritchard map were made over intervals with an an average length 746 days, but the entire period of obs was 03-08 and the figure is scaled as such.
4-1919	4	84		85		Do we need the velocity (panel e in both of these figures) [Christopher Little, United States of America]	yes, they may appear on different pages
4-1920	4	84		85		Do these mass change plots use only the Velicogna analysis? Or are various analyses merged? [Christopher Little, United States of America]	Noted - only V analyses are used as an example.
4-1921	4	84				figure 4.13 Mention in the figure text that the scales are non-linear [European Union]	Disagree - it is clear in the figure that the scales are non-linear. Repeating in the text would not increase the emphasis signficantly.
4-1922	4	84				Figure 4.13: Title and period should be added in all figures, because each figure deals with different elements and periods. [Government of Japan]	Disagree - this is made clear in the caption

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4-1923	4	84				Figure 4.13: Please explain the conflicting results between GRACE and ICESat especially in southern Greenland. [Government of United States of America]	Disagree, it is not appropriate to discuss this in the figure caption
4-1924	4	84				Fig 4.13c: The colors of the circles around 4.13c and the description of colors in the caption don't correspond. [Government of United States of America]	Accepted Circles have been removed.
4-1925	4	85	1	85	8	Fig. 4.14 (a), (b), (c): The reference cited as source of these figures (Velicogna, 2009) does not show any spatially detailed analysis of ice loss, only time series of total ice loss for Antarctica. [Helmut Rott, Austria]	Noted -
4-1926	4	85	6	85	6	the paper Velincogna (2009) does not contain information which could be updated to the maps in Fig. 4.13 [Heinz Blatter, Switzerland]	See 4- 1925
4-1927	4	85				figure 4.14 Mention in the figure text that the scales are non-linear [European Union]	See-1921
4-1928	4	85				Figure 4.14: Title and period should be added in all figures, because each figure deals with different elements and periods. [Government of Japan]	See 4-1922
4-1929	4	85				Figure 4.14c: Opposing results from different methods: gain 22 or 35 Gt/yr versus loss 30 Gt/yr. This brings to question whether such differences are caused by fundamental basis of the different approaches rather than just numerical error bars. Would encourage a discussion of the basis for variability and the level of confidence for these measurements. [Government of United States of America]	Accepted - Circles have been removed.
4-1930	4	85				Reference suggests results are based on Velicogna 2009. If shown as published, these are now effectively discredited by the development of the new GIA models. Results should be updated (if not already) to those used in the Shepherd et al 2012 analysis or replaced with something else (like the King et al. 2012 result) [Matt King, Australia]	Noted -
4-1931	4	86	1	87	5	The actual ranges for the minimum and maximum uncertainty bounds are not specified; are they e.g. 5 to 95%? [Government of United Kingdom of Great Britain & Northern Ireland]	neither. Derivation is explained in text
4-1932	4	86		88		Merge these three figures (15-17) into 1 figure? [Christopher Little, United States of America]	reject
4-1933	4	86				Figure 4.15: In the legend, "Maximum uncertainty" and "Minimum uncertainty" could be integrated as "Uncertainty". [Government of Japan]	accept
4-1934	4	86				Figure 4.15: The area between "Maximum uncertainty" and "Minimum uncertainty" should be colored or shaded. [Government of Japan]	accept
4-1935	4	87	4	97	4	Why is the starting blue line below zero? Was ice in the Antarctic increasing in those years. Only by looking at the graph I shard to get the logic [European Union]	Curves are cumulative, starting at 1 Jan 1992 as explained in text (IA to check). There was mass gain over 1992. I am glad that the reviwer decided to look at the graph in order to understand it!
4-1936	4	87				Figure 4.16: In the legend, "Maximum uncertainty" and "Minimum uncertainty" could be integrated as "Uncertainty". [Government of Japan]	accept
4-1937	4	87				Figure 4.16: The area between "Maximum uncertainty" and "Minimum uncertainty" should be colored or shaded. [Government of Japan]	accept
4-1938	4	88				Figure 4.17 compares the loss rates from the Greenland and Antarctic ice sheets for 4 time intervals. I suggest that this figure (or anorther figure) show these loss rates along with loss rates from glaciers (i.e. the data presented in Table 4.5). Except for Figure 4.25 (which I believe will go into the SPM), the reuslts for ice sheets and for glaciers are completely segregated. This makes a comparison of their relative contributions much harder to appreciate, and reinforces the already significant sense of disconnection between glaciers and ice sheets. [W. Tad Pfeffer, United States of America]	consider/discuss at LA4 with CLAs and 4.3 Las
4-1939	4	89	1	89	1	letters used in the legend hardly show up in the plates, this should be improved. [Ernst Schrama, Netherlands]	Figure is revised for clarity
4-1940	4	89	4	89	6	Arrows showing access routes for ice discharge are not visible in the figure. [Helmut Rott, Austria]	Figure is revised for clarity

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4-1941	4	89	5	89	5	The arrows in Fig 4.18 are not recognizable (too small) [Martin Schneebeli, Switzerland]	see 4-1940
4-1942	4	89		89		Comment Fig 4.18 The caption says "..marine-based parts of the ice-sheet highlighted.." but that's not clear to me.Nor can I see the "arrows showing access routes for rapid discharge…". Nice maps though. [Peter Barrett, New Zealand]	Figure is revised for clarity
4-1943	4	89				Fig. 4.18: There are no 'arrows' visible in Figure 4.18 [Government of Germany]	see 4-1940
4-1944	4	89				Figure 4.18: As to characters such as "J", "H" and so on in the Figure, their color should be changed and/or their size should be larger, to make them more clearly distinguished. [Government of Japan]	Figure is revised for clarity
4-1945	4	89				fig. 4-18 Check caption for reference to arrows that do not appear in figure. [Government of United States of America]	see 4-1940
4-1946	4	89				Figure 4.18 is largely repeated as Figure FAQ 13.2, Figure 1 [Matt King, Australia]	Yes - we gave it to them as their earlier was too "conceptual"
4-1947	4	89				Figure 4.18 There are no arrows on this figure as stated in the caption. [Kate Willett, United Kingdom]	see 4-1940
4-1948	4	90				Figure 4.19: The legend for "circle" and "x" should be added in the Figure. [Government of Japan]	Accepted.
4-1949	4	90				Figure 4.19: A running mean through the June values would be helpful. The pattern in the red "x"s is somewhat difficult to see. [David Rupp, United States of America]	Accepted. Will add smooth curve for June
4-1950	4	91	1	91	2	air temperature anomaly (C), the unit should be oC [Tao Che, China]	Accepted.
4-1951	4	91	1	91	2	air temperature anomaly (C), the unit should be ℃. [Jing Ming, China]	See response to previous comment.
4-1952	4	91				Figure 4.21 is too complicated to be understood straightly. [Jing Ming, China]	Accepted. Figure has been simplified
4-1953	4	92	1	91	2	This figure is too informative and difficult to understand for public, 1) marks are too small and too much, 2) marks overlaped for the studies that station number is large, 3) some results include the relationship of air temperature or elevation, some not. 4) what is the meaning of several numbers in the right part, 5) it is not visible to find the key point. Maybe, using several tables to describe these results, and some important results can be described in the context. [Tao Che, China]	Accepted. Figure has been simplified
4-1954	4	92	1	91	2	This figure is too informative and difficult to understand for public, 1) marks are too small and too much, 2) marks overlapped for the studies that station number is large, 3) some results include the relationship of air temperature or elevation, some not. 4) what is the meaning of several numbers in the right part, 5) it is not visible to find the key point. Maybe, using several tables to describe these results, and some important results can be described in the context. [Jing Ming, China]	See response to previous comment.
4-1955	4	92	1			Fig. 4.21: Most of the cited studies are either referenced in Brown and Mote (2009) nor in bibliography of this chapter. [Christoph Marty, Switzerland]	Accepted. Refs will be added.
4-1956	4	92	1			Fig. 4.21: The cited winter study of Marty, where max SD was analyzed is from 2011 and not from 2008 as an other study from the same author in the spring section. [Christoph Marty, Switzerland]	Noted. Winter study deleted in simplifying the figure.
4-1957	4	92	8			Fig. 4.21: "for the Ishizaka winter data", because there are also Ishizaka spring data in the same table. [Christoph Marty, Switzerland]	Noted. In revised version, winter data have been removed to simplify the figure.
4-1958	4	92		92		I stand by my comment in my FOD review that Figure 4.21 is too complicated and virtually unreadable. I don't think it effectively synthesizes 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 has been further simplified
4-1959	4	92				Figure 4.21: The meaning of the column labeled as "%<0" and of the numbers painted yellow should be added in caption of Figure 4.21. [Government of Japan]	Accepted.
4-1960	4	92				Figure 4.21: The horizontal axis scaled "-2 -1 0 1 2" on the right side of the Figure should be labeled. [Government of Japan]	Accepted.

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4-1961	4	92				Figure 4.21: More explanation is needed for the black circles plotted on the right side of the Figure, including the followings: meaning of black circles difference between black, red and blue circles [Government of Japan]	Accepted.
4-1962	4	92				Figure 4.21: The meaning of "+" should be added. [Government of Japan]	Accepted.
4-1963	4	92				Figure 4.21: Can a figure try to accomplish too much? One has to dig through the caption to find units, which could be one of two types. Maybe label the top half "Winter" and bottom half "Spring" on the left-hand side? Should read "…none of the trends were statistically significant". [David Rupp, United States of America]	Accepted. Figure has been simplified
4-1964	4	93				Figure 4.22: Can the authors provide some representation of error on this figure? [Government of United States of America]	Noted - Figure redrafted completely and text rewritten
4-1965	4	94				fig. 4.23e: Is the trend of composite ALT significant, given the magnitude of the standard deviation? Is the increased composite ALT variability after about 1990 due to inherently increased variability in these systems or to the addition of more sites to the composite? [Government of United States of America]	Noted - Figure redrafted completely and text rewritten
4-1966	4	94				Fig. 4.23 - It is probably more useful to show the location of the CALM sites for which data are presented in the figures rather than show the location of all CALM sites. The intent is not to describe the monitoring network but rather to document trends in ALT. Readers can find detailed information on CALM elsewhere (Note that details on the permafrost temperature monitoring network are not provided, but location information is provided for sites for which data are presented in fig. 4.22). If only the CALM sites highlighted on the graphs (b,c,d) are shown on the map they could be labelled so the reader knows where they are. Alternatively the lat/long could be provided for each site in the legend for each graph. Location is important as conditions within a region can vary. For example it is important to know where in western Canada C5 is, as conditions range from disc to cont permafrost, boreal to tundra etc. (see earlier comments re caption for fig.) [Sharon Smith, Canada]	Noted - Figure redrafted completely and text rewritten
4-1967	4	95				Figure 4.24: The meaning of top line and bottom line in the bottom figure should be added in caption of Figure 4.24. [Government of Japan]	Noted - Figure redrafted completely and text rewritten
4-1968	4	95				fig. 4.24: please explain the 3 lines on the lower plot in either a caption or legend (do the lines represent min, mean, max??) [Government of United States of America]	Figure redrafted completely and text rewritten
4-1969	4	95				Figure 4.24: Suggest removing top panel - provides very little crucial information for the space required. [Thomas Stocker/ WGI TSU, Switzerland]	Figure redrafted completely and text rewritten
4-1970	4	96				FAQ4.1 Figure 1: This figure is difficult to understand, possibly because the white and blue shaded areas are not explained. Are they accumulation and ablation zones? [Government of Canada]	Taken into account: The different colours are explained now.
4-1971	4	97	1	97	1	FAQ4.2 Figure 1: I recommend to present the trend of minimum sea ice extent (or September in the Arctic and April in the Antarctic) instead of annual average, since most public discussions and trend numbers presented therein are about the minimum (or monthly) extent and not about annual average (e.g. NSIDC discussions). It is better to focus on the same thing in all (important) public communications. [Urs Neu, Switzerland]	Rejected.
4-1972	4	97				FAQ 4.2, Figure 1: The Antarctic panel incorrectly shows sea ice max/min shading over the ice shelves, instead of masking these areas out. The figure seems to indicate that there is perennial ice over the Ronne-Filchner ice shelf but only seasonal ice over the Ross ice shelf. Why would this be since both areas preclude the formation of sea ice because floating ice shelves occupy the seas? [Government of Canada]	Accepted - text revised.
4-1973	4	97				FAQ 4.2, Figure 1: What is the accuracy of of the -2%/decade trend in annual average ice extent at the central Arctic? Is it real? [Government of United States of America]	Rejected - outside the scope of FAQ (see main text for significance).
4-1974	4	97				This is FAQ 4.2 Figure 1. The meaning of the arrows (ice drift speed) should be explained in the caption. [Harry Stern, United States of America]	Accepted - text revised.
4-1975	4	97				FAQ 4.2, Figure 1: Y-axis label - shouldn't this be extent anomaly?, and if so, what is the reference period? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4-1976	4	98	42	98	53	For the sentence beginning “Arctic sea ice cover is seasonal, with the average ice extent varying between…”. Indicate over what period (i.e. years) these averages are calculated. [Government of United States of America]	Accepted.
4-1977	4	98				Figure 4.25: The bottom line could be itemized as follows. 1.0 to 1.4 mm/yr for 1993-2009 1.2 to 2.2 mm/yr for 2005-2009 [Government of Japan]	accept
4-1978	4	98				Figure 4.25: Would like to see uncertainty estimates for the parameter given in this figure. [Government of United States of America]	Figure redrafted completely and text rewritten
4-1979	4	98				Figure 4.25: "Lake and River Ice … evidence of recent acceleration in both" is unsubstantiated, please provide references/evidence for this statement. [Government of United States of America]	Figure redrafted completely and text rewritten
4-1980	4	98				Fig. 4.25 See earlier comments on text regarding magnitude of temperature increase as the figure will need to be modified to reflect increase in permafrost of 2°C since the 1970s. Note that the time period given here is since the 1980s but this should only refer to active layer not permafrost temperature. The movement of the southern boundary of permafrost is only based on evidence from Russia and it is not appropriate to present this as representative of the entire northern hemisphere. The increasing thickness of seasonally frozen ground is only based on Eurasian data and may not be representative of the northern hemisphere. (note that there is some discussion in the text regarding spatial variation in trends in ALT and lack of trends in some regions and we might expect similar variability for seasonal freezing). [Sharon Smith, Canada]	Figure redrafted completely and text rewritten
4-1981	4	98				Figure 4.25 refers to decline of ice shelves on the Antactic Peninsula and "some areas in the rest of Antarctica". This is very weak. Pritchard et al. (2012) showed that everywhere that the Antarctici glaciers meet areas of relatively warm water, they are retreating. Should this piont not be amplified? [Eric Steig, United States of America]	Text on figure is revised substantially - point is noted
4-1982	4	98				This is Figure 4.25. Several comments: 1. At the bottom of the figure, the bold text says “Total contribution to sea level rise from ice melt is …”. This is very deceptive – it implies that all of the above forms of ice contribute to sea level rise, which is not true. Only glaciers and ice sheets contribute. The bold text should read: “Total contribution to sea level rise from melting glaciers and ice sheets is …”. Also, the word “respectively” at the end of the bold text should be deleted – it is not necessary. 2. In the “Snow Cover” section of the figure, delete the word “the” just before “1972/1973”. 3. In the “Ice Shelves and ice tongues” section of the figure, the title words “ice tongues” should be capitalized – Ice Shelves and Ice Tongues 4. In the caption (line 4), the word “insert” should be “inset” 5. In the caption (line 6), add the word “with” after “together” 6. In the caption, in reference to the inset figure, say that Gt is gigatons and SLE is sea level equivalent [Harry Stern, United States of America]	Text on figure is revised substantially - point is noted
4-1983	4	98				Figure 4.25: In the text under 'Glaciers' it states that shrinkage rates have increased with time, "except perhaps in recent years". Perhaps we missed it, but we did not notice any further discussion of this notion that the rate of glacier retreat has slowed down in recent years. If true, and supported by multiple lines of evidence, this should be clearly discussed in the text, and not slipped in as a "perhaps" statement in a synthesis figure. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account: text in synthesis figure and in section has been revised
4-1984	4	98				Figure 4.25: Final statement of figure should read "Total contribution to sea level rise from "ICE LOSS"...given that ice melting is not the dominant process in Greenland/Antarctica. [Thomas Stocker/ WGI TSU, Switzerland]	accept