IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 4

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

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4-1	A	0:0	0:0	I think it would be worthwhile to place more emphasis upfront (in the executive summary for example) about the global importance of cryospheric changes. There is the right amount of emphasis on how this relates to sea level but more emphasis should be placed on the significance of changes in sea ice and alpine glaciers, which have little to no sea level impact. [Michelle Koutnik]	Accepted. Text modified.
4-2	A	0:0		Excellent chapter-no comments. [Richard Anthes]	Noted.
4-3	A	0:0		Chapter 4 is comprehensive and coherent with the other two observational chapters. [Roxana Bojariu]	Noted.
4-4	A	0:0		Nice to see a dedicated chapter on the cryoshere but the treatments in the various cryopshere components are somewhat uneven. The introductory material on the cryosphere seems a little weak - this was handled better in the Cryosphere chapter in the EOS Science plan which includes a nice figure summarzing the linkages and feedbacks between the various components of the cryosphere and the climate system. Some sections appear to provide literature reviews rather than focus on new findings since TAR. This contributes to excessive length of some parts of the Chapter. The break-out question 4.1 "Is snow and ice changing" needs to be rethought as it does not work the way it is now. [Ross Brown]	Accepted. Text modified in the various sections.
4-5	A	0:0		Congratulations! Overall the chapter is well written and appears comprehensive. However, there are some sections where some rewriting is appropriate. Also, there is quite a bit of background material which although interesting could be deleted as it is not the main purpose of the assessment. [John Church]	Accepted. Text modified in the various sections.
4-6	A	0:0		Through the chapter there are a number of places where error estimates should be but are not given. [John Church]	Accepted. Text modified in the various sections.
4-7	A	0:0		Some sections of the chapter are more a compilation of results from the literature rather than an "assessment" of the science. Where there are a number of of different results in the literature there needs to be an attempt to use the expertise of the author team to assess the relative merits of the different results/conclusions [John Church]	Accepted. Text modified in the various sections.
4-8	A	0:0		I would have expected some more focussed attention on a number of the "big" questions - the Stability of the West Antarctic Ice Sheet in the longer term (there is related discussion	Accepted. Text modified.

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				but not focussed on this particular issue); will the Arctic Ocean be ice free by 2100; what is the future of the Greenland Ice sheet. While these latter two topics will presumably get discussed in Chapter 10, some discussion here also seems appropriate. [John Church]	
4-9	A	0:0		On the whole, I feel this chapter has adequately dealt with the relevant issues in a manner that is consistent and deals with the issues and associated uncertainities. [Rowan Fealy]	Noted.
4-10	A	0:0		There is a degree of repetition that may need to be sorted [Rowan Fealy]	Accepted. Text modified in the various sections.
4-11	A	0:0		Chapter indicates that mountain glaciers retreat began in about 1850 (p4-7 line 15) and began a retreat after the 17th to 19th centuries (p4-23, ln 43). Which level of specificity should be used? [Howard Feldman]	Accepted. Text modified.
4-12	A	0:0		Somewhere the report should discuss the retreat of the glaciers before the change in temperatures as described in Question 3.1 Figure 1 page 3-165. The change in glaciation is preceeding the change in global temperature and the input of anthropogenic ghg. [Howard Feldman]	Rejected. Chapter structure is fixed. In addition, changing glacier volume is a complex response to precipitation and heat and radiative fluxes.
4-13	A	0:0		This chapter covers new material not in the TAR and as such some of the sections are (and need to be) more comprehensive. However, some of the sections (4.4, 4.5, 4.6, 4.7) are extremely coherent and well-written, while others (particularly 4.2) come across as either heavily-edited or disparate in their ideas. [Melanie Fitzpatrick]	Accepted. Text modified.
4-14	A	0:0		Box and Questions are a good idea – but do they form a stand-alone section? Ie. Is the idea that they can be extracted from these chapters as in the Synthesis Report? [Melanie Fitzpatrick]	Boxes are just for the text. Questions will serve also other purposes.
4-15	A	0:0		- [Savitri GARIVAIT]	
4-16	A	0:0		The state of the art appears fairly accurate, some gaps are mentioned, but there is hardly any clear statement as to where the major uncertainties lie and where there is an urgent need for progress in the future. Aren't such recommendations in the scope of IPCC reports? Shouldn't a section be dedicated? With respect to the role of ice in the climate system, feedbacks, related ice and climate changes, I am not sure where the urgent matters are, but concerning contribution of ice to sea-level change, it seems that there are still major uncertainties. See comment below. [Christophe GENTHON]	Accepted. Text modified.
4-17	A	0:0		Many issues are raised concerning uncertainties related to the dynamics of the (Antarctic)	Accepted. Text modified.

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				ice sheet (section 4.6.4) while issues related to the surface mass balance (SMB) of the Antarctic ice sheet are not properly considered.Stakes networks and firn/ice cores that make it possible to figure out not only the mean, but also the variability of accumulation are very few, and their spatial significance can be very poor considering the high spatial variability of SMB in Antarctica (see e.g. Frezzotti et al., 2004, Spatial and temporal variability of snow accumulation in East Antarctica from traverse data, J. of Glaciology; and Genthon et al, 2005, Interannual variability of the surface mass balance of West Antarctica from ITASE cores and ERA40 reanalyses, Climate Dyn. 24, 759-770). The very coastal regions, where accumulation is largest and where changes in accumulation are expected largest are little sampled (Genthon and Krinner 2001, cited in the chapter). The chapter states that models and meteorological analyses provide valuable information, but this information cannot be accurately verified by lack of observation. Precipitation is not observed, neither ablation (not mentioning wind effects). Seasonal cycles are generally stated from model results, without observational control (meteorological analyses are (somewhat) controled by observed atmospheric moisture, not by observed precipitation (there are no such observation!)). A 10% uncertainty on the SMB of Antarctica is 0.5 mm/yr of sea-level.Neither models nor observations reach this level of accuracy. It is an important issue that more field observations are needed, more appropriately fitted for model (as well as satellite data) verification (spatial significance, temporal information) and validation, in order to better assess the current state and evolution of the SMB of Antarctica. From a general point of view, the ISMASS statement (Jacka et al. 2004, cited in the chapter) lists mjor issues related to the Antarctic ice sheet mass balance. [Christophe GENTHON]	
4-18	A	0:0		The inclusion of a cryosphere obs chapter means there's space for a much more detailed discussion of aspects of the cryosphere than could be done in the TAR processes and sea-level chapters. This is good, and the material in this draft is useful and generally well written. However we have to be careful that what is said in chapter 4 fits well with areas covered by other chapters, especially 5, 6 and 10. Chapter 5 is responsible for synthesising the explanation of 20th-century sea level. To do this, it needs quantitative ranges for the cryospheric components to appear in chapter 4. Chapter 10 is responsible for projections of sea level change, including caveats about the models used; I think these caveats belong with the projections (rather that in chapter 4), so that the user of the projections is made aware of them. Overlap and duplication among chapters should be minimised. [Jonathan Gregory]	Accepted, text modified in accord with results of cross-chapter meeting.
4-19	A	0:0		This chapter is very important to the report at a whole as we expect changes to most dramatic at high latitudes which are characterised by snow ice and frozen ground. It is	Accepted. Text modified.

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				therefore essential that data are presented in a fair way. The previous reports have been unsatisfactory in their coupling between paleoclimate and present climate. I believe this is one of the most important issues for this report to bridge over this gap. In its present shape this chapter is too focussed on the last two decades. In order to make this report understandable and reliable all time series should be discussed in a longer timescale like the entire 20th century. This problem can perhaps be solved by adding a climate curve with relevant parameters presented covering at least a hundred years. It now feels somewhat history-less, a history of existing measurements. [Per Holmlund]	
4-20	A	0:0		One of the best indicators of the severity of winter in northern Europe is the maximum extent of the Baltic Sea ice. I suppose that it would be useful to involve an overview of changes in sea ice in the Baltic Sea into the subchapter 4.4.2. For example, have a look on Omstedt, Chen, 2001 (J. Geophys. Res.). [Jaak Jaagus]	Accepted. Baltic Sea included in the discussion of 4.4.2.
4-21	A	0:0		Ch 4 - Lead Authors - none whose expertise lies in sea ice observations [Ola M. Johannessen]	Rejected. The reviewer is incorrect.
4-22	A	0:0		This chapter well reviews the observed primary changes in elements of the cryosphere and the balance of the chapter between snow, sea ice and land ice appears appropriate and thorough. Certainly it provides an update to the ACIA report and the thorough reviews given by Serreze and colleagues in 2000 and Dickson et al., 2002. My main concern is that these primary changes are not followed through to secondary changes especially for the carbon cycle and the hydrological cycle. I've perused Chapters 5&7 but still do not find this sort of connectivity. I don't think it matters structurally whether this material is included in the present chapter, or included in Chapters 5 or 7 but it would be helpful to include such material somewhere and cross reference it. [Robie Macdonald]	Text adjusted to include variations of the cryosphere and general statements on their importance.
4-23	A	0:0		The literature during the past decade is replete with expressions of 'the ice is thinning', 'the snow is melting', 'Arctic Ocean water masses are changing', 'Greenland ice is melting' but we appear to move little beyond these dire warnings/observations. It seems clear that these primary changes impact biogeochemical cycles and, therefore, are important factors that cannot be neglected in a variety of time series (Macdonald et al., 2005). The IPCC's scope includes the cycling of greenhouse gases among which carbon dioxide is of course central. Nevertheless, there is almost no indication in Chapter 4 of any connections to such cycles nor does this Chapter direct the reader to where such a discussion might occur elsewhere in the 2005 review. There are at least three reasons for looking at these connections: 1) there is a potential feedback to the carbon cycle which ought to be addressed by future models, 2) Proxy records for ice conditions are likely to	Text adjusted to include variations of the cryosphere and general statements on their importance. More is beyond the scope of the chapter. Impacts are dealt with in WG II

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				be found in recorded products of the carbon cycle (e.g., for Arctic Lakes see Smol et al., 2005 and references therein), and 3) If we are not presently addressing these kinds of changes either in process studies or time series, it is crucial to point that out. Else, we will still be listing shrinking cryosphere ten years from now with greater authority but no better understanding of what it means. Macdonald RW, Harner T, Fyfe J. Recent climate change in the Canadian Arctic and its impact on contaminant pathways. Science of the Total Environment 2005;342:5-86. Smol JP, Wolfe AP, Birks HJB, Douglas MV, Jones VJ, Korhola A, Pienitz R, Rüland K, Sorvari S, et al. Climate-driven regime shifts in the biological communities of Arctic lakes. Proceedings of the National Academy of Sciences 2005;102:4397-4402. [Robie Macdonald]	
4-24	A	0:0		First, this is a very well considered, and thoughtful Chapter 4. At the outset, I fully recognize that my scientific expertise base is less qualified than for this Chapter than any of the other 11 Chapter I am reviewing. Because of this, I would like to begin by acknowledging and thanking Peter Lemke, Bill Hibbler, Richard Alley, Doug MacAyeal, Lonnie Thompson, and Ellen Mosley Thompson for their episodic, but patient discussions with me over the years on many aspects of this daunting "frozen world" set of scientific and societal challenges. Any errors I make in this review, I willingly accept full responsibility for my incomplete understanding of some of the most crucial aspects of this fascinating set of scientific challenges and opportunities. Obviously, the challenge to perform unambiguous, long-term measurements of snow and ice with accuracy and continuity over many decades is quite formidable. I was pleased to see the level of care and caution applied by the authors, and their colleagues, to the snow, ice, and frozen-ground measurments challenge. I find these patient efforts to be inspiring and heroic. Without their patient efforts, this Chapter 4 Draft would have been vastly less valuable than what has actually been achieved. I extend my thanks, and congratulations to them all. I found myself frustrated at times with the frequently cryptic use of poorly-defined units of measure and other undefined acronyms. Often, they were "jargonesque", thus leaving this reviewer to work backwards to guess the underlying meaning of the jargon being used. Actually, sticking with standard MKS units would be a very helpful start. Fortunately, the Chapter 4 figure captions tended to be less prone to this problem. Other phrases, or made-up acronyms, are simply never defined, ISMASS for example, but a number of others lurk in the text, as well, such as InSAR. In this, and in the three earlier drafts that I have already reviewed, there is a tendency to "put in what we know and/or what we do as "frozen scientists" (Sorry.), thusdrifting away from the IPCC business at hand: assessing the observational credibility of human-caused climate warming science, and its implications for life on earth.	Noted. Accepted. Text modified for clarification. (discussion of units, acronyms, etc.).

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				Given the impressive complexity of the glaciers and the ice sheets, there is a tendency at a number of times to "duck the bottom-line questions" concerning global-warming-induced secular trends. Some impresive progress is now quite evident in the Greenland Ice Sheet and the Arctic sea ice. However, the struggle to understand "Antarctica", its myriad complexities, and its possible but not quite convincing secular(?) trends is diligent, but arguably non-convergent for some very good physical reasons. Less lengthy discussions and more focus on what still needs to be accomplished scientificall in the Antarctic region would be a laudable quest. [Jerry Mahlman]	
4-25	A	0:0		I throughly enjoyed reading this Chapter. My level of understanding of this very important field of research has been improved greatly through my reading of this draft. [Jerry Mahlman]	Noted.
4-26	A	0:0		I estimate that the chapter is currently about 44% over its target length. Sections/ subsections that I thought might be overly long at present were: the background subsections starting each major section, most of section 4.4.3, and section 4.6.3.4. [Martin Manning]	Taken into account. Reductions made in 4.4.3
4-27	A	0:0		"Data set" should be two words. [Melinda Marquis]	Corrected
4-28	A	0:0		This chapter is written in an excellent style. [Steven Massie]	Noted.
4-29	A	0:0		Write "Little Ice Age" throughout the chapter [Atle Nesje]	Accepted.
4-30	A	0:0		Excellent overview - I learned a great deal from reading the chapter. [Neville Nicholls]	Noted.
4-31	A	0:0		Perhaps 4.6 and (especially) 4.7 are overlong and detailed, relative to the rest of the chapter? [Neville Nicholls]	Accepted. Text modified and shortened.
4-32	A	0:0		This is a good chapter and generally presents the state of the cryosphere clearly: most of my specific comments are fairly minor. [David Parker]	Noted.
4-33	A	0:0		A readable and nicely organised chapter - very logical and structured.Nice figures too. [Stefan Rahmstorf]	Noted.
4-34	A	0:0		Spelling is a little variable throughout the chapter. One thing that caught my eye was the alternate use of British and American spelling for "modelling" (or "modeling"). [James Renwick]	Editorial decision.

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4-35	A	0:0		Chapter 4 General comments. This chapter reads fairly well, although several sections seem a bit long and unduly wordy. It is not quite as up to date as I had expected and several variables have not been updated (the worst perhaps being Figs 4.3.1, 4.3.2 and 4.3.3 which go only into the early 1990s). There are substantial shortcomings in describing the ice-albedo effect (Box 4.1) because clouds are not taken into account, and sea level changes are not as thorough as they might be, with no assessment of error bars in the 1 mm/yr change. Some parts of the chapter are not an assessment but a recitation of several studies without comments on their value and shortcomings or putting them together as a synthesis. In some places year is abbreviated yr, in some a. It is confusing. [Kevin Trenberth]	Accepted. Text modified and shortened. Time series updated. Error bars included.
4-36	A	0:0		Section 4.8.2 needs a more thorough assessment of the contributions and error bars to sea level. [Kevin Trenberth]	Accepted, assessment improved, text modified
4-37	A	0:0		Informative and clear chapter [Philippe Tulkens]	Noted.
4-38	A	0:0		Having seen the first author draft, I am pleased to note that this is a substantial improvement and well on the way to being an excellent chapter. [David Vaughan]	Noted.
4-39	A	0:0		Overall comments on this chapter are that it is, at present, somewhat thick and indigestible. I can see that it is allways going to be a catalogue but anything that can be done to make it an easier read would be good. The editors may want to consider the use of text boxes for of portions of text that are background for the inexpert reader, and case studies etc. For example, the section on ice shelves might make a good case study, set in a box separate from the ice-sheet section. Good luck. [David Vaughan]	Accepted. Text modified.
4-40	A	1:1		General point - would be good to connect the nice chap 4 perspective on ice sheet dynamic instabilities and out inability to model them to chap 10. This latter chapters convey's a confidence in future ice sheet change that might underestimate the true rates of future change given what we are now starting to see in ice sheet behaviour. Chaps 4 and 10 should be coordinated more with chap 6 also. [Jonathan Overpeck]	Accepted, issues discussed at cross-chapter meeting to improve coordination.
4-41	A	1:9	1:14	The list of authors should be given with the full name of the people, as in Chapter 1 [Philippe Tulkens]	Editorial decision.
4-42	A	1:47		Should TOC use abbreviation G&IC? Wouldn't it be better to spell out "Glaciers and Ice Caps" in the TOC?	Accepted. Abbreviation not used.

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				[Melinda Marquis]	
4-43	A	2:6		'70N' should be '70oN' (Similar comment at 4-15-12.) [Ian Simmonds]	Accepted. Text modified.
4-44	A	3:0		Perhaps the summary could go by order of importance, and start with warming trends, effects on ice sheets and glaciers, sea ice cover, permafrost, and snow. [Eric Rignot]	Not accepted. Executive summary is organized as the chapter is structured.
4-45	A	3:0		I would welcome a statement for context in the Executive Summary that makes it clear why the cryosphere is so importance and is seen to warrant a chapter of its own in the 4th Assessment. [David Vaughan]	Accepted. Text modified.
4-46	A	3:0		The bulleted statements are too long an involved to be punchy and give an overall impression of lots of opposing changes or uncertainty. At the least, the statements should be ordered in a way as to show the most compelling evidence for secular change first, with the more conflicted statements coming later. [David Vaughan]	Accepted. Text modified to be punchy.
4-47	A	3:1	3:7	It seems clear to me that we are now very close to a global-warming theory-based attribution to the large Arctic warming that may be highly credible. I think that a much less defensive opening statement in the Executive Summary is clearly warrented. [Jerry Mahlman]	Accepted. Text modified.
4-48	A	3:1	4:	Exec Summary - Excellent summary – same order as sections. Bullet points easy to read, main points picked out. [Melanie Fitzpatrick]	Noted.
4-49	A	3:1	4:46	Summary is too long and detailed. Probably this chapter would serve better if an effort were made to choose the key points and summarize (not explain!) them on 1 page. At the risk of perhaps oversimplifying, the two main points that I've taken away from this chapter are that the snow and ice covered surface areas of the Earth are decreasing, thus providing a positive-feedback impetus to the fundamental CO2 induced climate change, and that the contribution of ice mass change to sea-level change has accelerated recently and now provides about 1 mm/yr of equivalent sea-level rise compared to 0.2-0.4 mm/yr in the Third Assessment Report. To me, an executive summary would emphasize these main points. [Thomas James]	Accepted. Text modified.
4-50	A	3:1		Exec. Summary - not balanced with respect to importance; e.g., too many bullets for glaciers (see next comment) [Ola M. Johannessen]	Accepted. Text modified.

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4-51	A	3:3	3:5	Opening sentences seem clumsy - suggest wording "In the past, the cryosphere has undergone significant variations on many time scales, associated with the ice ages and with shorter-term variations like the Younger Dryas or the Little Ice Age. Ice volume generally rises or falls in step with falling or rising temperatures. Recent changes are in accordance with the rise of global surface air temperatures over the past century." [James Renwick]	Accepted. Text modified.
4-52	A	3:4	3:7	An attribution of large-scale cryospheric change to large-scale temperature change might be consistent with observational evidence, but in detail it's more complicated and I'd say that the quantitative link would be a modelling result, which belongs better in chapter 9 than in an observational chapter like 4. [Jonathan Gregory]	Accepted. Text does not include a quantitative link.
4-53	A	3:5	3:7	Is the statement that polar regions warmed by 3 times the global average consistent with Fig 3.2.7 which appears to show that temperatures north of 65N warmed by about 2 times the global average? [Martin Manning]	Accepted. Text corrected.
4-54	A	3:6	3:7	Better to present here the century-long trend estimates (e.g., based on Lugina et al. 2005 or Polyakov et al. 2000; or Jones and Moberg 2003) data to avoid accusations in "cherry-picking" the last 50 years. Due to warm Arctic in the 1930s, the Arctic temperature trend for the 1936-2000 is statistically insignificant (cf., Groisman et al. 2005c, J. Climate, in review, cited as Groisman et al. 2005 in Chapter 4). Also, a selection of miniscule size of the Arctic (north of 70N) makes this statement of a relatively minor importance. Please, use region north of 60N (7% of the globe) as it was done by Lugina et al. 2005 or north of 65N as it was done by CRU in "Climate Monitor" . By the way using Lugina et al. data, the mean rate of change for the last 50 years in the region north of 60N (1.0K) is close to that for the 1881-2004 (1.3K). This may mitiIgate the controversy (and possible critique). The Lugina et al. time series is presented in Lugina, K.M. P.Ya. Groisman, K.Ya. Vinnikov, V.V. Koknaeva, and N.A. Speranskaya, 2005: Monthly surface air temperature time series area-averaged over the 30-degree latitudinal belts of the globe, 1881-2004. In: Trends: A Compendium of Data on Global Change. Carbon Dioxide Information Analysis Center, Oak Ridge National Laboratory, U.S. Department of Energy, Oak Ridge, Tenn., USA. [Available at http://cdiac.esd.ornl.gov/trends/temp/lugina/lugina.html]. The basic refereed reference on this data set is: Vinnikov et al. 1990 (J.Climate). [Pavel Groisman]	Accepted. Statement modified.
4-55	A	3:6	3:7	There are few surface air temperatures north of 70N. 65N would be better, as used in Question 4.1 Figure 1. [David Parker]	Accepted. Text modified.
4-56	A	3:6	3:7	50-year period begins just after the mid-century warm peak so this statement may be an	Accepted. Statement modified.

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				over-interpretation of the facts which also (Question 4.1 Figure 1) only suggest about twice the global warming, not three times, for this period north of 65N [David Parker]	
4-57	A	3:9	3:10	This conclusion is based on the data presented in Figure 4.2.1. These data are inconsistent with the NH average temperature data presented in Figure 3.2.6. Specifically, NH average temperature decreased between 1940 and 1974, then increased from 1975 to the present. The snow cover data show a large year-to-year variability, but the smoothed trend line in Figure 4.2.1 shows a significant increase from 1950-1955, then a general decline to about 1975, then another increase 1975-1980, and still another one in the late 1990s. An explanation for why snow cover trends do not follow average temperature trends needs to be provided, both in this Executive Summary and in Section 4.2.4 [Lenny Bernstein]	Accepted in part. As was noted in the text, the relevant area for comparing snow cover and temperature is 40-60°N, which has a strong correlation with NH SCA of –0.68. We have now smoothed the time series of SCA in exactly the same way as the graph of temperature north of 65°N, which brings out some important similarities like the fact that SCA was generally lower in the 1930s and 1940s than in the 1950s and 1960s, and SCA since the mid-1980s was substantially lower than average except for two years.
4-58	A	3:9	3:16	How large was the decline? [John Church]	Accepted: text modified
4-59	A	3:9	3:10	As written, this conclusion implies a consistent trend in decreasing snow cover. However, Figure 4.2.1 shows a highly variable history for snow cover, with a significant increase from 1950-1955, then a general decline to about 1975, then another increase 1975-1980, and still another one in the late 1990s. Also, these trends do not track the average temperataure history of the Northern Hemisphere polar region shown in Figure 3.2.7. The Executive Summary needs to acknowledge the significant variability in snow cover, and also provide an explanation for why snow cover trends do not follow average temperature trends for the NH polar region. This assessment also needs to be presented in Section 4.2.4. [Jeffrey Kueter]	See response to 4-57.
4-60	A	3:9	3:16	This is a powerful statement, honestly and forcefully stated. [Jerry Mahlman]	Noted. Will keep, except modifications to deal with 4-57 through 4-59.
4-61	A	3:9	3:18	The N.H. changes in snow cover timing are very intriguing and provide important observational evidence for increasing momentum of global warming [Jerry Mahlman]	Noted
4-62	A	3:10	3:10	Why "also"? Isn't the remark about mountain snowpack in W N America an instance of the preceding remark about NH snow cover? [Jonathan Gregory]	Accepted: text modified.

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4-63	A	3:18	3:21	This statement implies that freeze-up and break-up dates have changed consistently for the past 150 years, which is not what is shown in Figure 4.3.1. As would be expected, many of the records show considerable decadal variability. This variability should be acknowledged in the Executive Summary. [Lenny Bernstein]	Noted. Text revised.
4-64	A	3:18	3:21	This conclusion implies a consistent trend in freeze-up and break-up dates for the past 150 years. This is not what is shown in Figure 4.3.1, where many of the locations show considerable variability. This variability should be acknowledged in the Executive Summary and in Section 4.3.2. [Jeffrey Kueter]	Noted. Text revised.
4-65	A	3:19	:21	If they are so different, then why not present stats for eastern and western separately? [Melanie Fitzpatrick]	Noted. Text revised.
4-66	A	3:20	3:20	become later" change to "occurred later [Thomas James]	Accepted, text modified.
4-67	A	3:21	3:21	Suggest "become earlier" rather than "advanced", which is ambiguous. [Jonathan Gregory]	Accepted, text modified.
4-68	A	3:23	3:30	Exec. Summary - trends (%) in winter multi-year area can be reported here, as can the record summer lows of the last three or four years (since 2002) [Ola M. Johannessen]	Taken into account. Trend in multiyear ice is not as clear as summer trend because of inaccuracies in microwave ice classification (see 13:54-14:2 of FOD). Note about record minima included in 4.4.2.1.
4-69	A	3:23	3:30	This Arctic sea-ice thinning and contracting phenomenon is now almost the "global warming poster child". What helps give it even more high credibility is that the leading climate models are simulating a very similar phenomenon by simply adding greenhouse gases to the calculations with current and future assumed data. [Jerry Mahlman]	Noted
4-70	A	3:23	3:30	Difference "extent" and "actual area covered" is not clear here (only later in chapter) [Stefan Rahmstorf]	Taken into account. Only extent is now discussed in the Exec. Summary.
4-71	A	3:23	3:23	Does Arctic ice refer to land or sea ice or both -- clarity required [Sharon Smith]	Accepted
4-72	A	3:23	:26	What are the confidence limits on these estimates? [John Church]	Accepted. Text modified to include error estimate
4-73	A	3:23		Add “sea” in between “Arctic” and “ice” [Melanie Fitzpatrick]	Accepted, text modified.
4-74	A	3:23		Arctic ice" insert: "Arcitic sea ice	Accepted, text modified.

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				[Hartmut Grassl]	
4-75	A	3:23		Everywhere in this chapter it is important to distinguish between "ice" and "sea ice" - abbreviating "sea ice" to "ice" is confusing. Here I could read the first sentence and assume that it is about glacier ice, sea ice and river ice. I suggest a global edit to make sure that wherever the authors mean "sea ice", it is written in full. [David Vaughan]	Accepted, text modified.
4-76	A	3:24		It would be valuable to explain the difference between extent and area somewhere in the chapter - apologies if I missed it. I guess there is a formal definition but there is presumably an operational one as well and both would be helpful for the non-expert. [Martin Manning]	Taken into account. Only extent is now discussed in the Exec. Summary. Fuller definition of extent/area is in 4.4.1.
4-77	A	3:25	3:27	I don't understand the sentence commencing "The actual area…",especially how the second half of the sentence relates to the first half. This is probably explained better in the main text, but I think other readers will struggle with this sentence. [Neville Nicholls]	see response to 4-76
4-78	A	3:25	4:30	Please clarify? [Kevin Trenberth]	Should obviously be 3:25 – 3:30. Accepted as above
4-79	A	3:25		by ice" insert: "by sea ice [Hartmut Grassl]	Accepted, text modified.
4-80	A	3:27	3:28	You seem to dismiss the proxy records of Antarctic sea ice extent that do imply a decline over a longer period. I presume the reasons for ignoring this evidence are explained in the chapter. [Neville Nicholls]	Noted. Text explains more fully.
4-81	A	3:27		I do not agree with the statement regarding Antarctic sea ice thickness trends, and I believe that as it is written it will be in opposition to WGII chapter 15. In that chapter we're saying that there are trends in sea ice duration, which are significant by the normal tests, but these are spatially limited, i.e. a Ross Sea increase, vs. a Bellingshausen/Amundsen Sea retreat. If you take the average for all Antarctica, then yes it would be a "no-trend" result, but we can do much better than that now. In summary, there are spatially-limited but opposing trends, and we should be highlighting that, rather than simply saying, "on average, no change". [David Vaughan]	Taken into account. The regional changes in extent are discussed in the text (and in the ZOD were discussed in a lot more detail – that had to go because of length restrictions). We believe the Arctic/Antarctic asymmetry is a much more important result. We can say NOTHING about Antarctic sea ice thickness trends – duration does not relate to thickness
4-82	A	3:32	3:38	Exec. Summary - submarine sonar are not the only data to consder (ULS, altimer, etc.) [Ola M. Johannessen]	Taken into account. For consideration within space limitations
4-83	A	3:35	3:36	Is the statement in this sentence worth making in the ES? [Martin Manning]	Accepted. Sentence deleted.

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4-84	A	3:40	3:42	This bullet would be more informative if it were possible to quantify the effect of wind on the variability of some parameter for sea ice. [Martin Manning]	Accepted. Bullet to be deleted
4-85	A	3:44	3:51	The melting of glaciers and ice caps is a phenomenon that is intrinsically global, thus deserving of less cautious language than used here. It is more comortable to use cautious language, but your case is quite compelling, and the world has already recognized this. I understand the caution concerning Antarctica and its surroundings, but for a nunber of reasons, theoretically and in modeling, we already expect it to be a considerably more complex subsystem to deal with than the Arctic, particularly so, because of the deep Circumantarctic Ocean surrounding it. [Jerry Mahlman]	Noted. Text modified.
4-86	A	3:44		I would like to see a statement like, "the ability of glaciers to integrate climate makes them natural sensors of climate change, and provide a unique potent and visible expression of climate change" [David Vaughan]	Accepted. Text modified
4-87	A	3:45	3:45	better replace the term "retreat" (which refers to length change only) by a more general term such as "shrinking" or "ice loss". [Wilfried Haeberli]	Accepted. Text modified accordingly.
4-88	A	3:46	3:47	If by the two ice sheets the authors mean Greenland and Antarctica then I think it would help most readers to say Greenland and Antarctica. [Martin Manning]	Accepted. Text modified accordingly.
4-89	A	3:46		Suggest explaining that Antarctica and Greenland are the only two ice sheets (many do not understand the terminology). [Kevin Trenberth]	Accepted. Text modified accordingly.
4-90	A	3:47	3:48	The 2 periods (1967/68-1996/97 and 1992-2003) overlap only partly. Why not give a sea level equivalent rate for 1967-2003? What about uncertainties? [Anny CAZENAVE]	Taken into account, sea-level rise intervals based on data availability and on revised requirements of other chapters, text modified
4-91	A	3:47	3:47	0.36 mm yr-1 should have an uncertainty stated. [Jonathan Gregory]	Taken into account, sea-level rise values and uncertainties modified to reflect latest results and time intervals.
4-92	A	3:47	3:47	"…sheets, is estimated at 0.36mm in sea level…" ("at" missing in original) [James Renwick]	Accepted. Text modified accordingly.
4-93	A	3:48		with rates about twice as high from [John Church]	Accepted. Text modified accordingly.
4-94	A	3:49	3:51	After reading the relevant part of the chapter and some of the references I feel that the key	Noted – entire bullet reorganized

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				message here should be that tropical glacier changes are especially sensitive to humidity and solar radiation changes which can in turn be driven by regional climate change. The case of Kilimanjaro almost seems too specialized to be worth mentioning unless one were to bring out the point made by Moelg et al (2003) that its continual retreat during the 20th century appears to coincide with a shift to a drier regional climate. [Martin Manning]	
4-95	A	3:49		Patagonia is missing. [Eric Rignot]	Noted – entire bullet reorganized
4-96	A	3:49	:50	This sentence seems to imply that while Tropical glaciers are sychronous with global changes, the cause might be different or, at least, in doubt. [David Vaughan]	Noted – entire bullet reorganized
4-97	A	3:50	3:51	Suggest removing sentence on Kilimanjaro - interesting as this is! [Thomas James]	Noted – entire bullet reorganized
4-98	A	3:50	3:51	Last sentence doesn't convey meaning well. Can you reword, or delete? [James Renwick]	Noted – entire bullet reorganized
4-99	A	3:50	:51	Given that Kilimanjaro is a special case - the Exec Summary should outline more generally the changes to all tropical glaciers. [Melanie Fitzpatrick]	Noted – entire bullet reorganized
4-100	A	3:53	3:54	This bullet does not present any findings and it is not clear in what way precipitation is important (suggest it be deleted) [Ross Brown]	Noted – entire bullet reorganized
4-101	A	3:53	3:54	Section needs to be expanded or removed. Don't understand the 2nd sentence - what are implications of pre-1950's precipitation anomalies? [Thomas James]	Noted – entire bullet reorganized
4-102	A	3:53	3:54	This bullet looked like it was going to cite the Dyuregerov and Meier results and then backed off? Unless the authors feel they can make a quantitative statement on reconstructed temperatures it seems hardly worth including in the ES. [Martin Manning]	Noted – entire bullet reorganized
4-103	A	3:53	3:53	When accumulation-season precipitation is accounted for, glacier length variations.... [Atle Nesje]	Noted – entire bullet reorganized
4-104	A	3:53	3:54	Last bullet seems too brief. Suggest "…variations on global and regional scales, provided associated precipitation variations are properly taken account of." Is the "before 1900" comment relevant? [James Renwick]	Noted – entire bullet reorganized
4-105	A	3:53	3:53	reconstruction of temperature variations would be a better phrase to use [Sharon Smith]	Noted – entire bullet reorganized

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4-106	A	3:53		Not convinced this statement is correct, namely than glacier length is an indicator of temperature. [Eric Rignot]	Noted – entire bullet reorganized
4-107	A	3:54	3:54	sentence unclear [Anny CAZENAVE]	Noted – entire bullet reorganized
4-108	A	3:54	3:54	Dyurgerov (2003) says precipitation anomalies are important in recent decades too. [Jonathan Gregory]	Noted – entire bullet reorganized
4-109	A	3:54	3:54	Precipitation anomalies are important to consider when reconstructing climate (temperature) from glacier length variations at any time [Atle Nesje]	Noted – entire bullet reorganized
4-110	A	3:54		I am not sure I agree with this statement. Vincent et al. (2005, cited in the chapter) guess a 25% precipitation increase in the 1760-1830 period compared to the 20th century average and this is based on a simple model (degree-days) to explain glaciers extent. However, Casty et al. (Temperature and precipitation variabiltiy in the european Alps since 1500, Int. J. Climatology, in press) find no such precipitation increase in the observations.The 2 results are thus in conflict. [Christophe GENTHON]	Noted – entire bullet reorganized
4-111	A	4:0	5:	Excellent summary - covers the important topics succinctly and (in my opinion) accurately. [Neville Nicholls]	Noted – entire bullet reorganized
4-112	A	4:1	4:2	Same comment as on page 4-23 line 6. Please be more specific, what extraordinary and a long period means [Christof Appenzeller]	Noted – entire bullet reorganized
4-113	A	4:1	4:3	Commenting on a single extreme event in one region does not seem to fit the tone of this section which is summarizing larger scale variability and trends in the cryopshere. [Ross Brown]	Noted – entire bullet reorganized
4-114	A	4:1	4:3	Exec. Summary - this bullet reporting one particular year's (2003) glacier conditions in one very small region (European Alps) is not merited [Ola M. Johannessen]	Noted – entire bullet reorganized
4-115	A	4:1	4:1	....glaciers in Scandinavia in 2002 and in the European Alps..... [Atle Nesje]	Noted – entire bullet reorganized
4-116	A	4:1	4:3	This is more of a comment on variability and extremes (summer 2003 was extreme warm year in Europe) and perhaps the statement should indicate this. What is meant by a long period - some clarification is required. [Sharon Smith]	Noted – entire bullet reorganized
4-117	A	4:2	4:3	Suggested rewording: "..extremely low precipitation, as well as albedo feedback from the	Noted – entire bullet reorganized

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				negative mass balance of previous years". [Thomas James]	
4-118	A	4:2	4:3	It is unclear what is meant by "albedo feedback from a previous series of negative mass balance years" [Anne Nolin]	Noted – entire bullet reorganized
4-119	A	4:2		What does "long period" mean? [John Church]	Noted – entire bullet reorganized
4-120	A	4:5	4:7	Suggest some numbers be included to give some indication of the magnitude of the changes to be consistent with other bullets in this section. [Ross Brown]	Noted – entire bullet reorganized
4-121	A	4:5	4:5	Should "intermediate" be "immediate"? [Thomas James]	Noted – entire bullet reorganized
4-122	A	4:7	4:7	"Hazardous lakes" is non-specific. change to "glacial lakes with the potential for outburst floods". [Anne Nolin]	Noted – entire bullet reorganized
4-123	A	4:7		The term “hazardous lakes” is not explained here (as it is much later in the text). [Kevin Trenberth]	Noted – entire bullet reorganized
4-124	A	4:9	4:18	Suggest splitting the summary of ice sheets into two parts Greenland and Antarctica and emphasizing the changes since the TAR. Ch-.11 in the TAR found the mass balance of Greenland was "not significantly different from zero", and this has changed in the AR4. It would be good to put the main conclusions first they way this is presently drafted it does not convey the main messages in the same way that the other paras do. [William Hare]	Accepted, text modified.
4-125	A	4:9	4:18	Exec. Summary - do not agree that we can say that Greenland has contributed to sea-level rise and that losses have been increasing - melt runoff, increases, but there is an open question of whether these are offset by increased accumualtion [Ola M. Johannessen]	Noted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified.
4-126	A	4:9	4:18	It seems to me that the case for anthropogenic warming of the Greenland ice sheet margins is now compelling, but Antarctica remains a wild card. In my opinion, the real differences between these quite different systems needs to be more carefully highlighted.in the Executive Summary. [Jerry Mahlman]	Accepted, text modified.
4-127	A	4:9	4:18	Is this bullet consistent with chapter 5 - especially their Exec Summary page 3 lines 6 - 9? [Martin Manning]	Noted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified, results

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					communicated with chapter 5.
4-128	A	4:10	4:11	"broadly consistent with expectations for a warming world" is a model result for predictions, not an observation - I think this belongs in chapter 10. [Jonathan Gregory]	Accepted, text modified.
4-129	A	4:12	4:18	We need a quantitative range for Antarctica. The TAR gave such a range (separately for recent and long-term). It doesn't seem to make sense to give a range for Greenland, and for Greenland+Antarctica, but not for Antarctica. [Jonathan Gregory]	Accepted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified.
4-130	A	4:13	:15	This is correct but observations are also lacking to check models (see general comments above and specific comment below) [Christophe GENTHON]	Noted. Text modified.
4-131	A	4:13		The statement that the contribution from Antarctica remains uncertain, is in conflict with the statement at the end of the paragraph which seems to imply that it can be summed with Greenland in a sensible way. I think that you're actually implying that it is now known that , to within quite a narrow range, we know that Antarctica in is balance. Do you mean to say this? [David Vaughan]	Accepted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified.
4-132	A	4:15	4:18	While technically consistent with the conclusion in Chapter 5, there are very different implications to the two chapters' conclusions. Chapter 5 (Pg.3, lines 8-9) states: The contribution from the Greenland and Antarctic ice sheets during 1993-2003 is assessed as 0.0 +/- 0.2 mm/yr, where this chapter concludes that the Greenland and Antarctic ice sheets made a positive contribution to sea level rise of up during that period, which rose to 0.2 mm/yr over the last five years. The two conclusions should be harmonized. [Lenny Bernstein]	Accepted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified, results communicated with chapter 5.
4-133	A	4:15	4:18	This conclusion is inconsistent with the conclusion in Chapter 5 (Pg.3, lines 8-9) which states: The contribution from the Greenland and Antarctic ice sheets during 1993-2003 is assessed as 0.0 +/- 0.2 mm/yr. The two chapters should be sending the same message. [Jeffrey Kueter]	Accepted, sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified, results communicated with chapter 5.
4-134	A	4:17	4:17	Define "past 5 years" - give time frame [Thomas James]	Accepted. Text modified.
4-135	A	4:17	4:18	The uncertainty on the estimates of mass balance of Greenland and Antarctica is greater than 0.1 mm/yr and more like 0.5 mm/yr. [Eric Rignot]	Noted, all numbers on sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified
4-136	A	4:20	4:20	As people may read these chapters separtely the abbreviation TAR should be explained	Accepted, text modified.

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				[Per Holmlund]	
4-137	A	4:20	4:20	First time that TAR is used (in the chapter), give it in full "Third Assessment Report". [Thomas James]	Accepted, text modified.
4-138	A	4:20	4:20	Define TAR before using the acronym (perhaps this was defined in a previous chapter) [Anne Nolin]	Accepted, text modified.
4-139	A	4:20		TAR: spell-out acronym when first use. I assume Third Assessment Report [Konrad Steffan]	Accepted, text modified.
4-140	A	4:21	4:22	It is not justified to suggest that the melting margins of Greenland Ice Sheet lead to sea level rise and neglect the thickenning of the Greenland Ice Sheet interior at the rate of 6 cm/year (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356). A selective use of data (picking some and neglecting others) is not a proper way to represent the state of ice sheet. [Petr Chylek]	Noted, all numbers on sea-level contributions from ice sheets updated to reflect additional papers published since preparation of FOD, text modified
4-141	A	4:22	4:26	I'm not sure that the statement "Prognostic models …" really belongs in the Exec Summ of an obs chapter, since its main importance is for projections. I don't think the statement "projections from such models …" belongs in this chapter; that is definitely an issue for chapter 10, where it is discussed and estimates are made by other means as well. [Jonathan Gregory]	Accepted, text modified.
4-142	A	4:22	:23	This is uncorrect, at least for Antarctica, as models cannot be properly verified due to insufficient observations (see general comments above) [Christophe GENTHON]	Accepted, text modified to avoid ambiguity in wording
4-143	A	4:24	4:25	Include iceberg calving with this list. [Anne Nolin]	Entire bullet rewritten.
4-144	A	4:25	4:25	replace "underestimate" with "incorrectly estimate". Why assume that all modelling approximations would lead to an underestimate of sea-level rise? [Thomas James]	Rejected. Fast flowing glaciers are a source that is neglected so far in modeling.
4-145	A	4:28	4:42	I'm not an expert in permafrost, so won't attempt to shorten, but surely this summary section could be shortened by half and still get across the ideas that permafrost is melting, the temperature of permafrost is increasing, the southern extent of permafrost (in the northern hemisphere) is moving northwards, and the thickness of the active layer is increasing? [Thomas James]	Accepted, text modified.
4-146	A	4:28	4:42	This is a very compelling discussion that builds an increasingly strong case for anthropogenic climate warming. [Jerry Mahlman]	Noted
4-147	A	4:28	4:28	Specify which decades. Reference to "in the past decades" is too vague.	Accepted, text modified.

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				[Frederick Nelson]	
4-148	A	4:28	4:42	The material in this bullet item employs what seem to be specific geographical examples, and the reader is left to wonder why they were chosen--are they somehow representative of larger (e.g., hemispheric) areas? It may well be better to write about hemispheric trends and perhaps point out local or regional departures. [Frederick Nelson]	Accepted; boundaries defined.
4-149	A	4:28	4:28	suggest ending sentence with "…in the past several decades." [James Renwick]	Bullet rewritten.
4-150	A	4:29	4:42	Poor grammar in parts of this bullet. [James Renwick]	Bullet rewritten
4-151	A	4:29		delete: "Permafrost" and insert: "Surface air temperature over permafrost" [Hartmut Grassl]	Bullet rewritten
4-152	A	4:30	4:30	replace "is accelerating" with "has accelerated". [Thomas James]	Bullet rewritten
4-153	A	4:30	:31	Increase of 2-3 degrees – over what time period - is this from 1980-2005? Perhaps re-express as per decade. [Melanie Fitzpatrick]	Bullet rewritten
4-154	A	4:31	4:31	replace "in Canadian" with "in the Canadian" [Thomas James]	Accepted. Bullet rewritten
4-155	A	4:32	4:32	high mountains in Europe" would be more appropriate than just "Europe [Wilfried Haeberli]	Bullet rewritten
4-156	A	4:32	4:32	Material here and at several other locations in Section 4.7 mentions "boundaries" but does not define what these represent or how they are defined. Such definition is absolutely necessary if this material is to be meaningful. It may be preferable to delete reference to "boundaries" and instead use more general language. [Frederick Nelson]	Accepted with additional definition of boundaries
4-157	A	4:33	4:34	thawing … 0.4" change to: "thawing rate of permafrost lies between 0.02 and 0.4 [Hartmut Grassl]	Bullet rewritten
4-158	A	4:34	4:34	0.4 should be 0.04 [David Parker]	Rejected. 0.4 is correct.
4-159	A	4:36		but with large" delete: "with [Hartmut Grassl]	Accepted. Bullet rewritten.
4-160	A	4:37	4:37	Sentence starting with "Maximum" should start with "The maximum…" [Thomas James]	Accepted. Bullet rewritten.
4-161	A	4:38		delete: "China" [Hartmut Grassl]	Accepted. Bullet rewritten.

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4-162	A	4:41	4:41	Suggest "become earlier" rather than "advanced", which is ambiguous. [Jonathan Gregory]	Accepted. Bullet rewritten.
4-163	A	4:41		? problems with understanding the meaning [Hartmut Grassl]	Accepted. Bullet rewritten.
4-164	A	4:42	4:42	Period 1988-2002 is very short and conclusions based on it are not very helpful. For Northern Eurasia, this finding directly contradicts results about a significant increase in growing season duration for the 1951-2004 period 8% to 14%, depending upon the definition of the growing season and particular region (in Siberia these changes are more spectacular). References: Groisman et al. 2005b from Chapter 3 or ACIA Report (Chapter 2) . [Pavel Groisman]	Accepted with modification
4-165	A	4:44	4:46	I would suggest omitting this statement. It does not add anything to the separate assessments for glaciers, Greenland and Antarctica, and it is less quantitative than those, since it doesn't specify a period or state the uncertainty. [Jonathan Gregory]	Noted, text updated to reflect revised estimates of sea-level rise from the cryosphere from papers published since FOD
4-166	A	4:44	4:46	Consideration of the source, accuracy and resolution of data should be added to this statement and to the very brief draft section 4.8.2. [Haroon Kheshgi]	Accepted, text updated to reflect revised estimates of sea-level rise from the cryosphere, and the source, accuracy and of the data.
4-167	A	4:44	:46	State the dates which the TAR estimates apply to and give updates with error bars. Also give time periods and uncertainty limits for the recent estimates. [John Church]	Accepted, text updated to reflect revised estimates of sea-level rise from the cryosphere,
4-168	A	4:45	4:45	Explicit each contribution leading to the 1 mm/yr number [Anny CAZENAVE]	Noted, contributions updated previously and further in chapter.
4-169	A	4:45	4:45	Explicit the exact period which the 1 mm/yr refers to (1998-2003?). As the 4th Assessment report will be published in 2007, readers may understand 'last 5 years' as 2002-2007. [Anny CAZENAVE]	Accepted, text updated to reflect revised estimates of sea-level rise from the cryosphere,
4-170	A	4:45		What are error bars on sea level change from cryosphere? [Kevin Trenberth]	Accepted, text updated to reflect revised estimates of sea-level rise from the cryosphere,
4-171	A	4:46	4:46	Add phrase ", primarily due to an increase in the melting of glaciers and ice caps", to the end of the sentence? [Thomas James]	Accepted. Text modified.
4-172	A	5:0	5:	Table 4.1.1 - Are these not estimated volumes,particularly for frozen ground and permafrost. Is the volume supposed to represent the volume of water/ice? In the case of	We use ice volume rather than total permafrost volume,

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				permafrost and frozen ground,is it the volume of ice (rather than ground) that is given? [Sharon Smith]	
4-173	A	5:0		Table 4.1.1. Line 4. The association between the numbers 0.51 (0.54) and 0.05 (0.13) and the sources (a=Ohmura, b=Dyurgerov & Meier) is not 100% unambiguous and could be made so with minor changes. [Garry CLARKE]	Accepted. Text modified
4-174	A	5:3	5:3	"its" refers cryiosphere? If so, mass and heat capacity of cryosphere may not make sense. "Concerning snow and ice mass and its heat capacity…" does make more sense. [Kenichi Matsuoka]	Accepted. Text modified.
4-175	A	5:3	5:8	Define cryosphere and its components in the first paragraph of the introduction. [Anne Nolin]	Accepted. Text modified.
4-176	A	5:3	5:8	Add some text describing the role of the cryosphere in the global water and energy cycle. [Anne Nolin]	Accepted. Text modified.
4-177	A	5:3	5:8	It would also be useful to add a sentence or two describing the differences between snow and ice on Earth compared with other planets. Life on our planet exists not only because water is present but because it exists in its various forms. [Anne Nolin]	Rejected. Beyond the scope of the chapter. Page limitations do not allow this.
4-178	A	5:3	5:8	The storage of water in the cryosphere should be mentioned as well as its role in the hydrological cycle. [Sharon Smith]	Accepted. Text modified.
4-179	A	5:3		In terms of instead of "Concerning"? [Neville Nicholls]	Accepted. Text modified.
4-180	A	5:3		Concerning" = "In respect of [David Vaughan]	Accepted. Text modified.
4-181	A	5:5	5:6	I am not convinced about "low heat conductivity" being a correctly attributed property. Snow has a low thermal conductivity but ice is more conductive than air, water and many earth surface materials. In terms of thermal conductivity, ice is not very special. [Garry CLARKE]	Accepted. Text modified.
4-182	A	5:5	5:5	Indicate that these comments on albedo refer to the portion of the cryosphere that is on the earth's surface (snow, glaciers etc.) rather than that portion that is below the surface such as permafrost. [Sharon Smith]	Accepted. Text modified.
4-183	A	5:14	5:15	In winter this would be the case. [Sharon Smith]	Accepted. Text modified.
4-184	A	5:15	5:18	Statement not very clear. Do you mean that changes in the components of the cryosphere can occur at different time scales.	Accepted. Text modified.

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				[Sharon Smith]	
4-185	A	5:20	5:20	Figure 4.1.1. What do the dashed lines mean going along the top of the surfaces? [Jonathan Gregory]	Snow cover. Figure improved.
4-186	A	5:22	5:29	Table 4.1.1. Please add sea level equivalent to this table. [Kevin Trenberth]	Accepted. Table modified.
4-187	A	5:24	5:24	Table 4.1.1. Please give sea level equivalents for snow on land, glaciers and ice caps, and ice sheets. The ice sheet SLEs are needed in Section 4.6.1. Please give Greenland and Antarctica separately and explain why their numbers differ from the TAR. Their sum in TAR Table 11.3 is 28.56e6 km3, a lot less than your 32.33. [Jonathan Gregory]	Accepted. Table modified.
4-188	A	5:24	5:25	Table 4.1.1 would be more valuable, if it includes equvalent sea level rise. Ice shelves and Ice sheets data should be updated. See comments about BEDMAP below. [Kenichi Matsuoka]	Accepted. Table modified.
4-189	A	5:24		What do the two sets of numbers for Glaciers and ice caps in the table represent? [John Church]	Accepted. Table modified.
4-190	A	5:24		would it make sense to add "sea level equivalent" or something similar to this table - to remind readers which quantities are relevant to sea level, and also to quantify how much [Jonathan Overpeck]	Accepted. Table modified.
4-191	A	5:26	5:26	Ohmura (2005) is not included in the reference [Kenichi Matsuoka]	Accepted. Table modified.(2004)
4-192	A	5:26		Ohmura (2005) should be Ohmura (2004) [Konrad Steffan]	Accepted. Table modified.
4-193	A	5:28	5:28	Drewry (1983) is out of date. Refer "Lythe, M. B., D. G. Vaughan, et al. (2001). "BEDMAP: A new ice thickness and subglacial topographic model of Antarctica." Journal of Geophysical Research 106(B6): 11335-11351." and update numbers. [Kenichi Matsuoka]	Accepted. Referencing generally updated, and shortened to reflect limits on chapter length.
4-194	A	5:28		New and better estimates of the volume of Antarctica and ice shelves etc are available in Lythe, M., D.G. Vaughan, and BEDMAP Consortium, BEDMAP: a new ice thickness and subglacial topographic model of Antarctica, Journal of Geophysical Research, 106 (B6), 11335-11352, 2001. [David Vaughan]	Accepted. Referencing generally updated, and shortened to reflect limits on chapter length.
4-195	A	5:32	5:33	Statements regarding albedo only apply to ice that is on the earth's surface and not subsurface ice associated with permafrost and seasonally frozen ground. [Sharon Smith]	Text refers to surface albedo.
4-196	A	5:34		hand, 90%" add: "hand, about 90% [Hartmut Grassl]	Accepted. Text modified.

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4-197	A	5:36		The sentence starting "Therefore…" is not very clear. [Neville Nicholls]	Accepted. Text modified.
4-198	A	5:38	5:38	Change "spherical" to "spheroidal" [Anne Nolin]	Accepted. Text modified. See 4-200.
4-199	A	5:38	5:48	The meridional thermal gradient is amplified by the presence of high albedo snow and ice at high latitudes. [Anne Nolin]	Accepted. Text modified.
4-200	A	5:38		Approximately spherical [Kevin Trenberth]	Accepted. Text modified.
4-201	A	5:40	5:44	This seems to be a bit of a stretch to me, and appears to creep into the territory of atmospheric dynamicists, but without a truly robust argument. But the punchline appears to be OK. [Jerry Mahlman]	Noted.
4-202	A	5:40	5:40	"which is influenced by" does not so much sense. Here, the role of cryosphere must be more emphasized. For example, which further drive many interactions …. [Kenichi Matsuoka]	Accepted. Text modified.
4-203	A	5:41		“tracks of low pressure systems are influenced”? Temperature gradients affect baroclinicity and development, as well as winds. This could be written better. [Kevin Trenberth]	Accepted. Text modified.
4-204	A	5:45	5:46	Sentence 'Because of the positive…' unclear [Anny CAZENAVE]	Accepted. Text modified.
4-205	A	5:45	5:48	To clarify meaning of sentence, insert a comma after "feedback" on line 46. [Melinda Marquis]	Accepted. Text modified.
4-206	A	5:45	5:48	not clear for me. [Kenichi Matsuoka]	Accepted. Text modified.
4-207	A	5:45	5:48	Suggest wording: "Because of the positive destabalizing temperature-ice albedo feedback, cyrospheric components (especially those with short response times) represent very effective indicators of climate variations (Box 4.1). Elements of the cryosphere are found at all latitudes, enabling a near-global assessment of cryosphere-related climate changes." [James Renwick]	Accepted. Text modified.
4-208	A	5:47	5:48	Sentence "An advantage of this…" does not make sense as written. I think the point trying to be made is that the cryosphere is a useful indicator of change and that elements of the cryosphere exist globally. [Ross Brown]	Accepted. Text modified.
4-209	A	5:47	5:47	Why is it an "advantage"? [Jonathan Gregory]	Accepted. Text modified.

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4-210	A	6:3		55 m for the Antarctic ice sheet is substantially less than in the TAR. Suggest comment. [John Church]	Accepted. Text modified
4-211	A	6:3		Does the sea level equivalent account for changes in pressure etc? Somewhere a more complete explanation is warranted. [Kevin Trenberth]	Accepted. Text modified
4-212	A	6:9	6:34	The tone of the material is very uneven. The initial explanation of the albedo feedback seems to have been written for people with no background in climate science, while the latter part abruptly switches to the level of a scientific review. I think we have seen enough layman explanations of the ice-albedo feedback to not repeat this in the FAR. [Ross Brown]	Accepted. Text Box deleted.
4-213	A	6:9	6:34	The Boxes work well throughout the report, and this is an excelletn example. [Neville Nicholls]	Noted. Box and figure deleted
4-214	A	6:11	6:22	Is this much explanation necessary? This section could be shorter. [Sharon Smith]	Noted. Box and figure deleted
4-215	A	6:11	6:15	Please make the numbers here consistent with Chapter 1. [Kevin Trenberth]	Noted. Box and figure deleted
4-216	A	6:12	6:13	Suggest wording change to: "Since the atmosphere is nearly transparent to incoming energy, 50% of solar energy reaches the surface." [James Renwick]	Noted. Box and figure deleted
4-217	A	6:12		....surface. Since the variably cloudy atmosphere is nearly ... [Jerry Mahlman]	Noted. Box and figure deleted
4-218	A	6:14	:15	These values are for pure snow/ice. Snow is actually often 'dirty' (dust, vegetation parts, etc). On alpine glaciers, the albedo can be as low as 0.2 (Jonsell et al. 2003, Spatial and temporal variabtions of albedo on Storglaciaren, Sweden, J. Glaciol. 49, 59-68; and Gerbaux et al., Surface mas balance of glaciers in the French Alps: Distributed modeling and sensitivity to climate change, J. Glaciol. In press) [Christophe GENTHON]	Noted. Box and figure deleted
4-219	A	6:17	6:29	This is a very nice tutorial. [Jerry Mahlman]	Noted. Box and figure deleted
4-220	A	6:17	6:22	This is simplistic. It applies only if there is no change in clouds. After “system on L 19 suggest adding “if no change in clouds”. Suggest adding at end of para: “However, retreat of sea ice exposes the ocean and potentially increases evaporation and leading to low cloud and fog, which can offset albedo changes to a large degree.” In fact this is known to occur as part of the annual cycle in spring (and it is poorly done by models). [Kevin Trenberth]	Noted. Box and figure deleted
4-221	A	6:24	6:29	The statement that "negative feedbacks must be predominant or the system would be	Rejected. Following the usual

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				unstable" is incorrect - or, if somehow it were to be correct, then the definitions of what are "forcings" and what are "feedbacks" requires a lot of explaining. Perhaps the first quantitatve analysis of climate feedbacks and climate response is the study by Hansen et al. (1984). Manabe and Wetherald (1975) had obtained a 2C global warming for doubled CO2, while the new GISS results obtained 4C. Both models had comparable physics and radiation treatments, so it was puzzling why the results were so different. The key differences were the use of fixed clouds in the GFDL model versus computed clouds in the GISS model, and little sea ice in the GFDL model versus a more complete sea ice field in the GISS model. The feedback magnification in the GISS GCM due to a decrease in low clouds, an increase in cirrus, and the melting of sea ice, was sufficient to account for the factor of 2 difference in climate sensitivity between the two GCMs. [Andrew Lacis]	definitions of climate sensitivity and feedback, as in Ch. 8 and as explained in detail, e.g. in Boer and Yu (Climate Dynamics (2003) 20: 415–429), the global climate feedback parameter must be negative. Box and figure deleted.
4-222	A	6:25	6:26	This statement is incorrect! It confuses positive feedback with runaway feedback - see basic textbook literature on feedback loops, gain etc. A positive feedback means that an initial warming of 1.0 degree due to forcing, can be amplified to say 1.5 degree; it does NOT imply an unstable system. [Stefan Rahmstorf]	Rejected. Following the usual definitions of climate sensitivity and feedback, as in Ch. 8 and as explained in detail, e.g. in Boer and Yu (Climate Dynamics (2003) 20: 415–429), the global climate feedback parameter must be negative. Noted. Box and figure deleted
4-223	A	6:27	6:27	The statement that ice-albedo feedback does not operate in the tropics seemingly conflicts with the statement (p. 4-5, L47-48) that "cryospheric components are found at all latitudes" [Garry CLARKE]	Accepted. But box and figure deleted.
4-224	A	6:29	6:29	subscript "2" [Kenichi Matsuoka]	Noted. Box and figure deleted
4-225	A	6:29		forcing instead of "effect" [Neville Nicholls]	Noted. Box and figure deleted
4-226	A	6:30	6:30	Hansen et al. (1984) also showed that the relative strengths of water vapor, cloud, and snow-ice albedo feedbacks could be quantified by means of 1D model calculations, and that the different feedbacks do not combine linearly and must instead be combined in a multiplicative fashion. For the globally uniform forcing of doubled CO2, Lacis and Mishchenko (1995) showed how the resultant equilibrium temperature change is distributed with latitude, and also how it can be attributed to different feedback mechanisms (water vapor, clouds, snow-ice albedo, advection) as a function of latitude.	Noted. Box and figure deleted

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				For doubled CO2, if no climate feedback effects were allowed to operate, the global surface temperature would have to warm by about 1.25 C to re-establish global energy balance. When feedback effects (water vapor, clouds, sno-ice albedo) are allowed to operate as in typical GCM simulations, the equilibrium warming is about 3 C - a clear indication of positive (and stable) feedback magnification. [Andrew Lacis]	
4-227	A	6:31	6:34	As shown in Hansen et al. (1984), the temperature response due to feedback effects is manifested in multiplicative fashsion. The "roughly 1/3" temperature response magnification attributed to ice feedback arises only in the environment of high positive water vapor and cloud feedbacks. If in the same GCM experiment the cloud feedback were set to zero, the same ice albedo feedback magnification would then be "roughly 1/4" . [Andrew Lacis]	Noted. Box and figure deleted
4-228	A	6:31		There are other studies that cover ice albedo feedback – are these two papers indicative of other research? If so, mention that. [Melanie Fitzpatrick]	Noted. Box and figure deleted
4-229	A	6:42	6:54	I like the clarity and straightforwardness with which this Question 4.1 is presented. [Jerry Mahlman]	Noted.
4-230	A	6:42	7:44	This question box is very wordy for a succinct summary and does not make the point that observed change depends on the period of available observations. Suggest the first para be followed by bullets highlighting the key change numbers. [Ross Brown]	Accepted. Text modified.
4-231	A	6:42	7:44	Question 4.1 - Some references should be added. [Sharon Smith]	Rejected. Questions will also be used separate from the chapter.
4-232	A	6:42		Question 4.1. The appearance of this question here is premature. It needs to be moved to later in the document, perhaps at the end. [Kevin Trenberth]	Accepted. Text modified
4-233	A	6:44	6:45	This may be only a linguistic problem, but it is a trivial information that snow and Ice have been melting over years. I am missing a word like increased melting , enhanced melting! [Per Holmlund]	Accepted. Text modified
4-234	A	6:44	6:49	Question 4.1: (Format): This paragraph should be in italics since it is the "headline answer" to the question. [David & David Wratt & Fahey]	Accepted. Text modified
4-235	A	6:48	6:49	It is not proper to state that melting in the coastal region of the Greenland Ice Sheet contributes to sea level rise and do not consider interior thickenning that acts in an	Noted. Ful discussion included.

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				opposite direction (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356). [Petr Chylek]	
4-236	A	6:51	6:57	The adjective "consistent" is used repeatedly but there is no explanation of what is intended. Consistent instruments? Consistent methologies? Results that are consistent with other measurements? [Garry CLARKE]	Accepted. Text modified
4-237	A	6:52	6:52	To clarify meaning of sentence, insert "of such measurements" after "limitation" on line 52. [Melinda Marquis]	Accepted. Text modified
4-238	A	6:56	6:57	How about a rewording that goes .... Unfortunately, satellite data do not allow ... permanently frozen ground, thus they do not yet contribute meaningfully to obtaining these needed global observations. [Jerry Mahlman]	Accepted. Text modified
4-239	A	6:57		I don't understand the use of "contributing" in this sentence. Do you mean "perhaps due to…"? [Neville Nicholls]	Accepted. Text modified
4-240	A	7:0		4.2. General comments on a section [Roxana Bojariu]	-
4-241	A	7:5	7:5	"consistent coverage" (same comment as above). The intended meaning of "consistent" is not conveyed. [Garry CLARKE]	Accepted. Text modified
4-242	A	7:5		Consider omitting 'passive microwave' for simplicity. [David & David Wratt & Fahey]	Accepted. Text modified
4-243	A	7:6		In Antarctica, significant positive and negative trends in sea ice are observed, and I don't think it's helpful just to give the overall/mean change. [David Vaughan]	Rejected. Regional changes may be appropriate to discuss in main text. Not in a FAQ about "ice on earth"
4-244	A	7:7		I do not understnd why this trend is insignificant - it is more than three times the quoted error estimates [John Church]	Taken into account. Error estimates are being re-computed.
4-245	A	7:7		What do the error bars mean? [Kevin Trenberth]	Taken into account as above.
4-246	A	7:8	7:19	The "Snow - Albedo Feedback" is actually not a feedback, but rather, is "radiative forcing of black carbon in snow-ice" effect as described in Section 2.5.4 of Chapter 2. Current climate models do not have the appropriate model physics to describe the deposition of soot in snow, nor the ability to calculate the effect of the soot on snow albedo. Instead,	Page and line numbers seem wrong. Should obviously be page 8. Noted. The section heading was confusing but the section has now been

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				the snow albedo change is inferred from relevant measurements and off-line radiative transfer modeling, and the results prescribed as an "external radiative forcing" - at least as far as the climate model is concerned. [Andrew Lacis]	folded into the introduction.
4-247	A	7:15	:18	Give error estimates for both time periods. [John Church]	Accepted
4-248	A	7:15		I think that this Line needs more careful qualification than given here [Jerry Mahlman]	Accepted. Text modified
4-249	A	7:16	4:16	write "Many northern hemisphere glaciers" (no information is available on southern hemisphere glaciers) [Wilfried Haeberli]	Accepted. Text modified
4-250	A	7:16		Replace a-1 with 'per year' [David & David Wratt & Fahey]	Accepted. Text modified
4-251	A	7:17	4:17	better replace the term "retreat" (which refers to length change only) by a more general term such as "shrinking" or "ice loss". [Wilfried Haeberli]	Accepted. Text modified
4-252	A	7:17	7:18	Suggest wording change to: "…with sea level contributions through the past decade about twice as high as between 1968 and 1997." [James Renwick]	Accepted. Text modified
4-253	A	7:18		What about after 1997? This is a major shortcoming not to deal with the more recent period here. [Kevin Trenberth]	Accepted. Text modified
4-254	A	7:20	7:20	The statement that coastal thinning offset interior thickenning is unjustified and probably not correct (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356). I did not find any presented evidence that would support the statement. [Petr Chylek]	Noted. Full discussion of relevant papers including those published since FOD is included.
4-255	A	7:20	7:39	Perhaps it might be better to label the "other indirect feedbacks" as dynamic feedbacks. There are clearly substancial regional shifts in atmospheric circulations patterns of sensible heat, latent heat, geopotential energy, Hadley circulation, etc., in respone to a globally uniform radiative forcing such as doubled CO2. And, there are bound to be connections, correlations, and interactions between changes in snow and ice cover and other meteorological manifestations. [Andrew Lacis]	Noted. Text modified.
4-256	A	7:20	7:31	It somehow doesn't seem appropriate yet to discuss Antarctic "climate trends" (or imply them), considering their bewildering complexity that makes possible the odd phenomenon	Accepted. Text updated.

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				of "Regional decadal to-century-scale natural variability In Lines 20-44, this concern is covered carefully and perceptively. [Jerry Mahlman]	
4-257	A	7:23		"coastal thinning likely offsets this". I cannot find this backed up in the rest of the technical text with references? [Melanie Fitzpatrick]	Accepted. Text updated.
4-258	A	7:33	7:44	It is stated that the effect of warming is more importatnt than anything else. However, only a small fraction of Greenland Ice Sheet margins is directly affected by global warming (Chylek and Lohmann, Ratio of Greenland to global temperature change: Comparison of observations and climate model results, Geophysical Research Letters, 32, doi:10.1029/2005GL023552, 2005), while temperature and current melting at most of margins is dominated by North Atlantic Oscillation. Also the observed temperature changes at the Summit show a significant cooling - especially during the summer - since 1986 (Chylek, Box and Lesins, Global warming and the Greenland Ice Sheet, Climatic Change, 63, 201-221, 2004). [Petr Chylek]	Noted. Additional discussion of surface mass balance of Greenland including papers published since FOD added.
4-259	A	7:33	7:44	The last paragraph of Q 4.1 seems a little laboured. Suggest shortening to convey the essential points. [James Renwick]	Accepted. Text modified
4-260	A	7:33	7:37	Warming is implicated in the changes in snow cover but in this discussion it is the change in climate that should be considered which includes changes in both temperature and precipitation. While the extent and duration of snow cover may depend on changes in air temperature, the actual amounts of winter precipitation will reflect overall changes in climate. [Sharon Smith]	Accepted. Text modified
4-261	A	7:35	7:35	Does snow cover only refer to areal extent or is it the depth that has also decreased? [Sharon Smith]	Text clarified.
4-262	A	7:37	7:37	Both the extent and thickness of snow cover affect frozen ground (actually more correct to say the frozen ground distribution and thermal regime), lake ice etc. [Sharon Smith]	Accepted. Text modified
4-263	A	7:37	7:39	Loss of ice volume is linked to rising surface air temperature and to the atmospheric circulation pattern. However, the latent heat of phase-change in the melting of ice stores some of the available energy, so suppressing the observed increase in temperature. Without this factor, the increased temperatures in the arctic region due to the positive feedback of the albedo effect (driven by reduced ice/snow reflective areas) would be even higher. Net energy uptake of the system is significantly greater than is indicated by observation of the temperature increase taken in isolation.	Accepted. Text modified

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				[David Wasdell]	
4-264	A	7:39		This wording is not helpful "sensitive to many things, but most are more sensitive to temperature…" In fact this is not shown. Later it claims this but it is not shown and it is more likely that it is the surface heat budget that matters and temperature is correlated with that. Temperature changes do not cause the cryosphere changes. [Kevin Trenberth]	Accepted. Wording clarified.
4-265	A	7:41		The glaciers are "benchmarks", they are not "calibrated" [David Vaughan]	Accepted. Text modified
4-266	A	7:44	7:44	I think stronger language is justified here: Overall, the Earth is losing ice because of warming. Remove "probably". [Michelle Koutnik]	Text modified. See 4-267.
4-267	A	7:44		The last sentence 'Overall, the Earth appears to be losing ice because of warming' is an important and direct attribution statement. Suggest putting this in the opening paragraph. [David & David Wratt & Fahey]	Accepted. Text modified
4-268	A	7:48	7:51	The same basic considerations described for ice albedo feedback are also applicable to show albedo. Except that, though higher in albedo, snow is actually less effective as a feedback in effecting a larger feedback temperature response than sea ice because snow melts more readily as the spring time temperature increases while sea ice stays around much longer and is thus able to reflect more solar radiation than snow. [Andrew Lacis]	Noted.
4-269	A	7:48		General comment: With the exception of section 4.3, the snow cover section (4.2) is thin in comparison to the other cryospheric elements. While the sea ice section includes discussion of multiple variables such as ice extent, concentration, flux/motion, thickness, landfast ice, and snow on ice, the snow cover section focuses mainly on extent with a brief discussion of snow water equivalent. Variables such as depth, density, snowfall, and snow cover duration are not included. Details on issues such as measurement uncertainty and modelling approaches are also lacking in the snow section compared to the others. The majority of comments below are aimed at beefing up the snow section. [Chris Derksen]	Noted, although most probably the length of the text will remain and also its general content. The chapter must be cut substantially. Snowfall belongs in Chapter 3.
4-270	A	7:48		Literature on GCM simulations of snow cover and links to the climate system are not cited/discussed in this section. Recent relevant papers include: Frei, A., and G. Gong. 2005. Decadel to century scale trends in North American snow extent in coupled atmosphere-ocean general circulation models. Geophysical Research Letters. 32. doi:10.1029/2005GL023394. Frei, A., J. Miller, and D. Robinson. 2003. Improved simulations of snow extent in the second phase of the Atmospheric Model Intercomparison Project (AMIP-2). Journal of Geophysical Research. 108(D12): 4369-4384. doi:10.1029/2002JD003030. Frei and Gong (2005) is particularly relevant as it	Rejected. This is covered in Chapter 8.

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				utilizes the suite of AR4 AOGCM's. Perhaps modelling studies such as these are included in another chapter? [Chris Derksen]	
4-271	A	7:48		Given the extremely limited discussion of albedo, should the title of Section 4.2 be reduced simply to Snow Cover? A complete discussion of albedo in the snow cover section should include information on snow cover/vegetation interactions, and snow albedo feedbacks. The discussion of albedo (section 4.2.2) is very incomplete in it's present form. [Chris Derksen]	Accepted. See response to 4-283. Albedo is also covered in Chapter 2.
4-272	A	7:48		This section has a completely different structure from other sections – almost like it has been chopped up and edited. Needs re-working to make the section cohesive. Other sections are covering new material and hence are almost as comprehensive as some chapters of the TAR. [Melanie Fitzpatrick]	Taken into account. The structure is actually quite similar, and other sections are too long.
4-273	A	7:50	8:7	"indirect feedbacks"? There is nothing indirect about water vapor and cloud feedbacks. Water vapor and clouds are fast feedback processes that respond quickly to local temperature changes (via Clausius-Clapeyron equation). Whether snow and ice changes are forced by seasonal changes in solar insolation or by increasing GHGs, there will additional water vapor feedback interactions (since the feedback processes are interactive and multiplicative). Any necessary discussion of radiative forcings and feedbacks relevant to this Chapter should be adapted from material presented in Chapter 2. [Andrew Lacis]	Accepted. It needed a comma: "other, indirect feedbacks". The effects on soil moisture and temperature *are* indirect, unlike those of water vapor and clouds.
4-274	A	7:53	7:53	Insulation of underlying surface -- does this refer to the ground and the ground ice it contains? [Sharon Smith]	Yes. Noted.
4-275	A	7:54	7:55	What is the climate significance of these chemical reactions in the snow? I would think that the role of soot (e.g Robock's work) in albedo reduction is more important. [Ross Brown]	Accepted. This sentence was a fossil and has been removed.
4-276	A	7:54	7:54	"Snow cover also helps determine the ice growth rate." Statement should be clarified to note: Snow depth over frozen lakes, rivers, and sea ice helps determine the ice growth rate. [Chris Derksen]	Accepted. Will be clarified as suggested.
4-277	A	7:54	7:55	This surface halogen chemistry is interesting, and even exotic, but it has little or no place in this Chapter since it has no known quantitatively relevant role in the climate system. [Jerry Mahlman]	Accepted. See response to 4.275.
4-278	A	7:54	7:54	Since this section is not concerned with snow on sea ice or land ice, 'Snow cover also helps determine the ice growth rate', should be removed, as it is not mentioned again.	Rejected. The introduction explains the significance of snow. Snow on ice is

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				[Jeff Ridley]	discussed over 100 times beginning on page 12.
4-279	A	7:54	7:55	It is true that this sentence tellsabout; however, it is related to slightly different topics, and afterwards, no comments are made related to this fact. The sentence does not seem to fit in this paragraph. [Takashi Yamanouchi]	Accepted. See 4-275.
4-280	A	8:2	8:2	Should be relationships of snow to "air" temperature [Sharon Smith]	Accepted.
4-281	A	8:5	:6	"snow on various forms of ice … covered in subsequent section". Cannot find where this is covered? [Melanie Fitzpatrick]	Rejected. See response to 4-278.
4-282	A	8:8		This section is VERY short – given the importance of feedbacks I suggest combining sections 4.2.2 and 4.2.3 [Melanie Fitzpatrick]	Taken into account.
4-283	A	8:8		Subsection 4.2.2: This seems to say very little. Papers that "speculate" about snow-albedo changes, that may "possibly" be related to Arctic warming, are not satisfactory for this publication. While the effect discussed may well be valid, it might be a topic for AR5. [James Renwick]	Accepted. See response to 4-282
4-284	A	8:10	8:13	It is good to see use of the original reference. IPCC's request to use emphasize mainly post-TAR references has produced some obvious inequities. [Jerry Mahlman]	Noted.
4-285	A	8:15	8:18	This highlights the importance of partitioning of effects. [Sharon Smith]	Noted.
4-286	A	8:15		"speculated"? maybe "suggested"? [Kevin Trenberth]	Accepted.
4-287	A	8:18		… probably contributing to recent Arctic melting. (However, we have long known(25years) that carbon aerosols emitted in Northern mid-latitudes act to collect inside the Arctic Circle, thus acting as a heat source when clouds are not present. [Jerry Mahlman]	Noted.
4-288	A	8:20		4.2.3 The local hydrological feedback of snow-cover during summer is not mentioned here, and, in my opinion, it should be (e.g. Yasunari, T. A., A. Kitoh, and T. Tokioka, 1991: Local and remote responses to excessive snow mass over Eurasia appearing in the northern spring and summer climate-A study with MRI GCM, J. Meteor. Soc. Japan, 69,473– 487.) [Roxana Bojariu]	Accepted. The soil moisture effect is implicit in the monsoon suggestion, and wording to that effect has been added.
4-289	A	8:20		Section 4.2.3: This section starts very abruptly. The first sentence ("The indirect	Accepted. See response to 4-282.

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				feedbacks may involve two types of circulation, monsoonal and annular, though these connections are statistically tenuous and controversial.") does not provide a clear introduction to this section. Section 4.2.2 is also very brief, so collectively the background information on terrestrial snow cover lacks depth compared to the other elements (sea ice, glaciers etc.). [Chris Derksen]	
4-290	A	8:20		Section 4.2.3: This section provides a very brief overview on interactions between snow cover and the climate system, with a focus on (1) monsoonal circulation and (2) the AO. While a complete overview is not realistic for this document, a brief description of low frequency atmospheric controls on North American snow cover, and ENSO interactions with snow cover would provide a more complete perspective. Possibe references: Clark, M., and M. Serreze. 1999. Snowfall responses over the USA to phase and amplitude variations in the tropospheric wavetrain. In Interactions Between the Cryosphere, Climate, and Greenhouse Gases, IAHS Publication 256, 45-54. Leathers, D., T. Mote, A. Grundstein, D. Robinson, K. Felter, K. Conrad, and L. Sedywitz. 2002. Associations between continental-scale snow cover anomalies and air mass frequencies across Eastern North America. International Journal of Climatology. 22: 1473-1494. Clark, M., M. Serreze, and G. McCabe. 2001. Historical effects of El Nino and La Nina events on the seasonal evolution of the montane snowpack in the Columbia and Colorado river basins. Water Resources Research. 37(3): 741-757. [Chris Derksen]	Taken into account. Most of the examples noted by the reviewer (e.g., Clark et al.) are not feedbacks but merely atmospheric patterns that have an identifiable influence on snow, not the other way around.
4-291	A	8:22	8:22	It is not clear what an indirect feedback could be. And a direct one? [Roxana Bojariu]	Accepted. See response to 4-273.
4-292	A	8:22	8:22	The concept of an indirect feedback is introduced without defining it. Suggest that a comment be made in the previous para that the albedo feedback is a direct one, and line 22 start "Snow also generates a number of indirect feedback to the climate system through soil moisture which affects land surface albedo, latent heat and cloud cover". [Ross Brown]	Accepted. See response to 4-273.
4-293	A	8:22	8:29	There are numerous other studies that indicate the effects of snow cover on climate: Snow cover depresses local air temperatures (see Cohen, 1994 and references therin), Extensive regional snow cover can alter atmospheric c irculation patterns through effects on diabatic heating (e.g. Dickson and Namias, 1976), and the effects of snow cover anomalies on subsequent snow cover and temperatures (Marshall et al., 2003). [Anne Nolin]	Noted. However, the purpose of this section is primarily to assess changes in snow, with minimal attention to the roles of snow in climate.
4-294	A	8:22	8:29	Note the following two studies on observational evidence of a short memory of anomalies from central Eurasian snow, soil moisture to air temperature, suggesting a weak dynamical link through land-surface/atmosphere interaction between the Eurasian snow	Accepted. Rather than adding to the studies emphasizing how weak the link is, we have substantially trimmed the

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				and Asian monsoon; Shinoda, M., H. Utsugi, and W. Morishima: Spring snow-disappearance timing and its possible influence on temperature fields over central Eurasia. Journal of the Meteorological Society of Japan, 79(1), 37-59, 2001. Shinoda, M.: Climate memory of snow mass as soil moisture over central Eurasia. Journal of Geophysical Research, 106(D24), 33393-33403, 2001. [Masato Shinoda]	text saying there are links and eliminated the section. See also comment 4-301.
4-295	A	8:22	:29	Section 4.2.3 would be improved if quantified in some way [Melanie Fitzpatrick]	Taken into account. See response to 4-294.
4-296	A	8:31	8:38	In my opinion, the discussion about lead-lag correlation analysis of snow cover and the North Atlantic/Arctic Oscillation (NAO/AO) is incomplete: in the literature, Eurasian snow cover during the warm seasons has also been suggested to at least partially modulate the winter AO/NAO (see Bojariu, R., L.Gimeno, 2003: The influence of snow cover fluctuations on multiannual NAO persistence. Geophysical Research Letters, 30(4), 1156, doi:10.1029/2002GL015651). The paper of Saito and Cohen (2003) is wrongly cited showing only the lag relationship between Eurasian snow cover in autumn and the strength of Arctic Oscillation (AO). In fact, their paper adds extra credence to the hypothesis that snow is partially forcing the atmosphere on interannual to decadal time scales. [Roxana Bojariu]	Taken into account. See response to 4-294.
4-297	A	8:31	8:38	comment to the whole paragraph: "?" [Hartmut Grassl]	Taken into account. See response to 4-294.
4-298	A	8:31	8:31	Second' - list statement not needed [Jeff Ridley]	Taken into account. See response to 4-294.
4-299	A	8:31		“Second” – where is “First”? [Melanie Fitzpatrick]	Taken into account. See response to 4-294.
4-300	A	8:32	8:38	Please use the term Northern Annular Mode (NAM) not AO, see chapter 3. [Kevin Trenberth]	Taken into account. See response to 4-294.
4-301	A	8:32	8:38	This material needs to be assessed. How credible is Gong et al? I do not believe it, see chapter 3. [Kevin Trenberth]	Accepted. See response to 4-294.
4-302	A	8:37	8:39	This is a far more plausible explanation. [Jerry Mahlman]	Taken into account. See response to 4-294.
4-303	A	8:40		The heading states that snow amount is included. There seems to be little on this subject although it would be a valuable contribution, particularly for the sea level implications. [John Church]	Rejected. Snow water equivalent and snow depth are discussed wherever available (e.g., 4.2.4.2). Snow is irrelevant to sea level (see Table 4.1).

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4-304	A	8:40		The regional breakdown of 4.2.4.1, 4.2.4.2 and 4.2.4.3 may be necessary but does not make for good reading or high impact. At the very least a strong synthesis paragraph is required at the head of this section [Garry CLARKE]	Accepted, though only room for a short paragraph.
4-305	A	8:43	8:43	You need to include SNOTEL measurements of SWE and snow depth. [Anne Nolin]	Rejected. SNOTEL measurements became common only in the 1980s and although they supplemented longer time series of manual observations in the study by Mote et al. (2005), there is insufficient space to discuss this minor data source here.
4-306	A	8:45	8:47	In the … 1950. Please complete this sentence by information on other parts of the world or suppress ! Switzerland has old, good quality data. [ERIC MARTIN]	Rejected. There is insufficient space to provide the details about each country's measurements; the reader is referred to the foundational literature cited.
4-307	A	8:48	8:48	Also need to make the point that surface observations are often biased to lower elevations and populated regions; observing networks in mountainous and high latitude regions tend be sparse. [Ross Brown]	Accepted. Text modified.
4-308	A	8:51	8:51	How is snow cover measured (what is measured)? How are snow cover observations validated? [Steven Massie]	Rejected. The reader is referred to the literature referenced for details about the measurements.
4-309	A	8:51	8:52	It would be useful here and at all other locations where government agencies are mentioned, to specify what country they represent. In this example, the text should read "the U.S. National Oceanic and Atmospheric Administration." [Frederick Nelson]	Accepted.
4-310	A	9:0		Figure 4.2.1: Indicate the derivation of the smoothed line in the figure (5-yr running mean/binomial filter?) [James Renwick]	Accepted. Smoothing has been redone to be consistent with Chapter 3.
4-311	A	9:1	9:3	It may be useful to add here that an evaluation of both new NOAA and MODIS daily high resolution snow cover maps over Canada now indicate good agreement with in-situ snow courses suggesting the snow cover record will improve in performance SIMIC, A., R. FERNANDES, R. BROWN, P. ROMANOV, AND W. PARK, (2004) Validation of VEGETATION,MODIS, and GOES+SSM/I snow-cover products over Canada based on surface snow depthobservations. Hydrological Processes. 18, pp: 1089-1104. This would remove doubt that the snow cover trends observed in northern latitudes are not being well	Noted. Rather than adding the reference, we have deleted the one that raised the doubts.

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				mapped for climate analysis purposes that the sentence currently present implies. [Richard Fernandes]	
4-313	A	9:1	9:2	Explain why these problems are UNLIKELY to affect the snow-cover estimates? When did the NOAA satellites solve this problem? [Jerry Mahlman]	Accepted. Text modified: The areas involved are very small.
4-314	A	9:5	9:13	A nice and honest explanation. [Jerry Mahlman]	Noted
4-315	A	9:6	9:9	Suggest adding a sentence and modifying to: "Volume scatter of emitted microwave energy due to the depth and density of the snowpack allows estimates of depth and water equivalent to be derived - variables that cannot be monitored with optical satellite data. Microwave brightness temperature data are available from 1978, although differences in sensor calibration between SMMR and SSM/I in 1987 must be resolved in order to generate homogeneous depth or SWE data series (Derksen et al., 2003)." [Chris Derksen]	Accepted. Text modified.
4-316	A	9:11	9:13	A relevant reference for new snow depth data derived from passive microwave measurements: Grippaa, M. , N. Mognard, T. Le Toan, E.G. Josberger, 2004: Siberia snow depth climatology derived from SSM/I data using a combined dynamic and static algorithm.Remote Sensing of Environment, 93, 30–41. [Roxana Bojariu]	Noted. Armstrong and Brodzik reference is comprehensive for the microwave data and results. We do not have space to include more references.
4-317	A	9:15	10:2	Variability in snow cover is described but causes are not discussed [Anny CAZENAVE]	Rejected. The correlation with temperature is already noted in the text. Beyond that, the subject of causality belongs to chapter 9.
4-318	A	9:16	9:16	Don't use "snow cover extent" (SCE). The correct and commonly used term is "snow covered area" (SCA). The word "extent" means the boundary of the snow cover (e.g. "the southernmost extent of the snow cover) whereas you actually mean "area" here. [Anne Nolin]	Depends on what you mean by "correct." In fact, the gridded NOAA data indicate grid cells that are at least 50% covered by snow – hence not truly an estimate of snow covered area. Nonetheless, we have changed the term to area (accepted reluctantly).
4-319	A	9:16	9:16	Are you sure that these figures 'include' snow over the Greenland ice sheet? If they do then they should be changed to remove the icesheet area. Presenting these figures in the following three paragraphs, only makes sense if these are for perennial snow. [Jeff Ridley]	Accepted. Figures now represent NH minus Greenland.
4-320	A	9:21	9:22	Explain : 'in absolute/relative terms' [Anny CAZENAVE]	Rejected. See 4-321.

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4-321	A	9:21	9:36	Nicely explained! [Jerry Mahlman]	Noted.
4-322	A	9:22	9:23	Please also do as %. [Kevin Trenberth]	Accepted. Percentage change included.
4-323	A	9:24	9:26	"There remains some uncertainty as to whether the microwave satellite data show similar interannual variability and trends except in autumn (see 4.2.4.1)." Reference can be added here: Armstrong, R., and M. Brodzik. 2001. Recent Northern Hemipshere snow extent: a comparison of data derived from visible and microwave satellite sensors. Geophysical Research Letters. 28 (19): 3673-3676. [Chris Derksen]	Reference already included and discussed.
4-324	A	9:28	9:29	This conclusion is based on the data presented in Figure 4.2.1. These data are inconsistent with the NH average temperature data presented in Figure 3.2.6. Specifically, NH average temperature decreased between 1940 and 1974, then increased from 1975 to the present. The snow cover data show a large year-to-year variability, but the smoothed trend line in Figure 4.2.1 shows a significant increase from 1950-1955, then a general decline to about 1975, then another increase 1975-1980, and still another one in the late 1990s. An explanation for why snow cover trends do not follow average temperature trends needs to be provided, both in this section and in the Executive Summary, where this conclusion is repeated. [Lenny Bernstein]	See response to 4-57.
4-325	A	9:28	9:29	This conclusion implies a consistent trend in decreasing snow cover. However, Figure 4.2.1 shows a highly variable history for snow cover, with a significant increase from 1950-1955, then a general decline to about 1975, then another increase 1975-1980, and still another one in the late 1990s. Also, these trends do not track the average temperataure history of the Northern Hemisphere polar region shown in Figure 3.2.7. The Executive Summary needs to acknowledge the significant variability in snow cover, and also provide an explanation for why snow cover trends do not follow average temperature trends for the NH polar region. This information also needs to be presented in Section 4.2.4. [Jeffrey Kueter]	See response to 4-57.
4-326	A	9:40	9:43	Please say what n/a means. [Kevin Trenberth]	Accepted.
4-327	A	10:0		Figure 4.2.2: Caption seems incorrect. Don't positive values indicate where snow extent is greater in the later part of the record, not the earlier? (since it is later minus earlier) [James Renwick]	Accepted, fixed.
4-328	A	10:0		Figure 4.2.3a: What does the solid line in panel (a) indicate? [James Renwick]	Accepted. Modified.

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4-329	A	10:0		Figure 4.2.3b: The plotting convention is not well explained. Presumably the diagonal lines indicate values of the trend in days of snow cover? [James Renwick]	Accepted. Figure has been replaced.
4-330	A	10:2	10:2	Marshall et al., 2003 also show that the albedo feedback is the key component determining subsequent air temperature and snow cover persistence for weeks to months. Albedo feedback only plays a role in the spring when insolation is sufficient. [Anne Nolin]	Rejected. One reference is sufficient. See also new material in introduction
4-331	A	10:4		Caption to Figure 4.2.2 is inconsistent with the header text on the figure. Caption reads "Positive values indicate greater extent in the earlier portion of the record." but the header reads (1988 through 2004) - (1967 through 1987) which indicates positive values mean greater extent in the later portion of the record. [Chris Derksen]	Accepted, fixed (see 4-327).
4-332	A	10:6		Section 4.2.4.2.1: consider adding a brief mention of SWE trend/variability analysis in non-mountainous areas from satellite passive microwave data. For example: "Trends from the passive microwave data record are spatially constrained by land cover controls on retrieval algorithm performance, but these data have great potential for identifying trends and variability in SWE. By merging conventional and passive microwave data, Derksen et al. (2004) showed SWE anomalies during the satellite era fall within the range of observed anomalies calculated back to 1915 across the U.S. Great Plains and Canadian prairies." (Derksen, C., R. Brown, and A. Walker. 2004. Merging conventional (1915-92) and passive microwave (1978-2002) estimates of snow extent and water equivalent over central North America. Journal of Hydrometeorology. 5(5): 850-861.) [Chris Derksen]	Noted – given our space constraints and that the study area was a modest-sized part of North America, we will stick with existing references.
4-333	A	10:7	10:22	Again, well written. [Jerry Mahlman]	Noted.
4-334	A	10:8	10:9	Modify to: "Over the same period of record, trends in other months are not significant; they become significant only when trend analysis is initiated from mid-century." [Chris Derksen]	Accepted; changed roughly as indicated.
4-335	A	10:8	10:9	Sentence should read - "Trends in other months only become significant after mid-century" [Rowan Fealy]	Accepted. See response to 4-335
4-336	A	10:13	10:13	See also Hodgkins et al Climatic Change Vol 71 pp. 319-340 (2005). [David Parker]	Rejected. The cited reference concerns number of ice-affected days on rivers and hence is more suitable for section 4.3.
4-337	A	10:16		How large was the decline? [John Church]	Rejected. The text in question refers to a study of hundreds of sites (point

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					locations) which, owing to the large variation in terrain, cannot easily be aggregated into an area-averaged value. See Figure 4.2.3a.
4-338	A	10:22	10:22	Should text refer to Fig. 4.2.3 a (as it does) or to Fig. 4.2.3 b? [Melinda Marquis]	Rejected. The reference is correct.
4-339	A	10:26		Discussion of glacier retreat post Little Ice Age should be included in this section [Rowan Fealy]	Rejected. Glaciers are discussed in the glacier section.
4-340	A	10:26		Advance of Scandinavian maritime glaciers should also be mentioned in this section [Rowan Fealy]	See response to 4-340.
4-341	A	10:27	10:44	I found this piece to be dense and obscure, without a discernable punchline. [Jerry Mahlman]	Rejected. The first sentence in the paragraph provides an accurate assessment of the literature, and there is no punch line. Not every result in IPCC is amenable to punch lines.
4-342	A	10:32		How large was the decline? [John Church]	Noted. Data in the original paper were not spatially aggregated, so it is not possible to report a single number representing "the decline".
4-343	A	10:35		possibly instead of "possible"? [Neville Nicholls]	Comment seems to apply to a different line of the text. No “possible” on 10-35.
4-344	A	10:37	10:44	How does this reconcile with the results from Ye, 2001 in which she showed increases in snow season length in North Central and Northwest Asia over the period 1937-1994? [Anne Nolin]	Noted. A dated study, and partly dealt with already in the citation of other Ye papers and others.
4-345	A	10:37	10:44	There is scope for blending this paragraph with the last paragraph of 3.3.2.3 in Chapter 3. [David Parker]	Noted.
4-346	A	10:44		Japan Island is one of the most snowy place on earth. Drastic changes there should be cited. Sentence: The heavy-snowfall areas of plain in Japan facing the Sea of Japan, snow depth showed periodic changes up to mid-1980’s, but it drastically decreased afterwards (Nakamura and Shimizu, 1996; Ishizaka, 2004). References: T. Nakamura and Shimizu, M. 1996: Variation of snow, winter precipitation and winter air temperature during the last century at Nagaoka, Japan. Journal of Glaciology, 42(140), 136-140.	Accepted with deep regret for this important omission.

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				M. Ishizaka. 2004:Climate responce of snow depth to recent warmer winter seasons in heavy-snowfall areas in Japan. Annals of Glaciology 38. 299-304. [Tetsuo Ohato]	
4-347	A	10:44		There is no expression refered to the trends in snow cover in Japan. Please refer to several litelatures telling about snow cover in Japan. [Takashi Yamanouchi]	See 4-346.
4-348	A	11:0	11:0	The section on changes in river and lake ice is a nice and relevant inclusion. [Michelle Koutnik]	Noted.
4-349	A	11:1	:7	ZIA – take out this section and remove Figure 4.2.4 This study uses a single derived quantity ZIA in one location – and hence does not give useful information regarding trends in snow melt in South America. [Melanie Fitzpatrick]	Accepted. Figure will be removed.
4-350	A	11:9		The value of Figure 4.2.4 is limited as it doesn't address a major theme of the section, and presents a very limited spatial and temporal perspective. [Chris Derksen]	Accepted. Figure will be removed
4-351	A	11:11	11:24	I am not finding an IPCC-level punchline here. [Jerry Mahlman]	See response to 4-341.
4-352	A	11:25	11:25	There needs to be a section specifically addressing issues on snow and ice albedo. How snow and ice are prescribed and modeled in climate GCMs is an important aspect of correctly modleing climate change, particularly in the polar regions. Radiatively, a scheme for modeling the spectral and solar zenith angle dependence of snow and sea ice is described by Warren and Wiscombe (1980). Wet and dry snow have different albedo, and there is also a snow age dependence (Loth and Graf (1998). Sea ice albedo is spectrally dependent and also depends on ice thickness and melt pond extent (Ebert et al., 1995; Schramm et al., 1997). There are also important aspects of salinity and freezing point of ice (Holland and Junkins, 1999; Schmidt et al., 2004) that should be addressed [Andrew Lacis]	Rejected. Section 4.2.2, which was panned by reviewers (see comments 4-283 and 4-284), has been folded into the introduction
4-353	A	11:26	12:35	section 4.3 : too few references. I suggest to add a reference to Kouraev et al., , 2005 for the change in Caspian and Aral lakes ice cover [Anny CAZENAVE]	Noted. Caspian and Aral Seas are not relevant to Lake section. References to be reviewed for sea-ice section.
4-354	A	11:30	11:53	This is an intriguing, but somewhat tangential, excursion into climate impacts analysis, the work of WG2, I believe. See also, Lines 30-53. [Jerry Mahlman]	Noted. Included here for context.
4-355	A	11:35	11:35	Change phrase "and possible flooding" to "and, in some cases, flooding" [Thomas James]	Accepted. Text modified.

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4-356	A	11:35		Change "...possible flooding..." to "sometimes flooding..." [Chris Derksen]	Accepted. Text modified.
4-357	A	11:43	11:43	typo, last word in line "give" should be "given" [Michelle Koutnik]	Accepted. Text modified.
4-358	A	11:43		Change "...at a give..." to "...at a given..." [Chris Derksen]	Accepted. Text modified.
4-359	A	11:43		at a give location" correct to: "at a given [Hartmut Grassl]	Accepted. Text modified.
4-360	A	12:0	19:	The section on sea ice is really long and I found it hard to get through. [David Vaughan]	Noted. SOD will be shorter
4-361	A	12:0		Section 4.4: Appears to have 5 US and 1 UK author; accordingly biased, e.g. 33 references, 25 of which are American [Ola M. Johannessen]	Rejected. The reviewer is incorrect.
4-362	A	12:1	12:35	This is messy and idiosyncratic data, but seems to, in aggregation, reveal some very interesting insights on freeze-up and breakup statistics that are unique and valuable. [Jerry Mahlman]	Noted.
4-363	A	12:5		Delete "the specific data and". I think this phrase confuses the sense of the sentence. [Neville Nicholls]	Noted. Text modified.
4-364	A	12:8	12:35	Figs 4.3.1, 4.3.2, 4.3.2, this is all quite dated material and an effort needs to be made to update it. [Kevin Trenberth]	Noted. This is what is available from published sources.
4-365	A	12:17	:23	If there are papers on this subject from Russian sources – why are none of them quoted? This would give better geographical coverage. [Melanie Fitzpatrick]	Noted. Review of Russian data is as cited in Smith (2001)
4-366	A	12:21	12:31	Why is New Zealand missing? The data exist surely: see message from David Wratt. [Kevin Trenberth]	Noted. Nothing available in published literature.
4-367	A	12:25	12:36	These figures are not effective. Perhaps the freeze-up and break-up dates could be described in words and the figures left out. [Andrew Lacis]	Rejected.
4-368	A	12:27		correct: "analysis" [Hartmut Grassl]	Accepted.
4-369	A	12:28		indicate a fairly general trend" – loose wording - the question is "is it SIGNIFICANT? [Melanie Fitzpatrick]	Rejected. Statistical significance is explicit in Figure and caption.
4-370	A	12:37	15:14	section 4.4 : the discussion mostly concerns trends. What about interannual variability(quite clear on figure 4.4.2)? Is there any correlation with AO and/or NAO? [Anny CAZENAVE]	Taken into account in revised text to clarify decadal variability. Some discussion of AO (NAM) links already

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					includede.
4-371	A	12:37		Section 4.4: What drives the changes reported here - air/sea fluxes, changed inflow and ocean structure, terrestrial input etc? [John Church]	Taken into account where appropriate. But chapter is about observed changes. In many cases cause can only be explored by modelling (later chapters)
4-372	A	12:37		Section 4.4 is excellent - suggest that it be kept in its entirety. Fills a gap that was not in the TAR. [Melanie Fitzpatrick]	Noted
4-373	A	12:37		Section 4.4 The document has the shortcoming without enough description of mechanisms. Only one example has been stated: i.e., ice-albedo feedback in the general section 4.1. First of all, ice-ocean interactions are missing completely in this section. I wonder if the reason is to separate the cryosphere chapter 4 and the ocean chapter 5. Whatever the reason is, this section should include the importance of oceanographic aspects. Many references are well known among the polar oceanography community. The other mechanism is the cloud effect, which emits longwave radation downward and has warming on sea ice. We can refer to some of the papers such as Makshtas et al. (1999) and Ikeda et al. (2003), who used human-observed cloud data collected from North Pole Stations in 1950 to 90. The trend of the cloudiness was positive all in fall, winter and spring. Its warming effect is comparable with the ice-albedo feedback in the last 30 years. References1/Makshtas, A.P., E.L. Andreas, P.N. Svyashchennikov and V.F. Timachev, 1999: Accounting for clouds in sea ice models. Atmos. Res., 52, 77-113. Reference2/ Ikeda, M., J. Wang and A. Makshtas, 2003: Importance of clouds to the decaying trend and decadal variability in the Arctic ice cover. J. Meteorol. Soc. Japan, 81, 179-189 [Motoyoshi Ikeda]	Accepted. Links with Chapters 3 and 5 strengthened
4-374	A	12:40		Fig 4.5.3: No explanation of top panel in Figure. [Kevin Trenberth]	Incorrect page number referenced. Refers to p71 : Figure and caption redesigned.
4-375	A	12:41	13:31	In the introductory material it would be worthwhile to discuss explicitly 1) the difference between thermodynamic and dynamic forcing for ice production; later discussion alludes to this with little forewarning and the non-specialist reader may misunderstand or not understand at all the difference between these two processes, and 2) the large-scale difference between Arctic and Antarctic sea-ice formation in which the latter depends far more on snow loading followed by flooding from beneath and freezing. These differences are important to understanding sea-ice response to projected climate change. [Robie Macdonald]	Taken into account in revised text
4-376	A	12:41	13:	Section 4.4.1. This was a well-written and helpful introduction. [Nick Rayner]	Noted

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4-377	A	12:44		Why hemispheric temperature? Shouldn't it be local temperature? [Kevin Trenberth]	Incorrect page number referenced.
4-378	A	12:54		"transport fresh water" ? [Hartmut Grassl]	Accepted. Text revised
4-379	A	13:1	13:10	This is a nice tutorial, but seems to lack IPCC-significant assessment punchlines. [Jerry Mahlman]	Taken into account. Text shortened.
4-380	A	13:19	13:22	There should be more emphasis on negative effects to native peoples. Their lifestyle is not only suffering due to changes in landfast ice -- changes in the entire ecosystem (particularly the Arctic) are slowly destroying their resources and ability to live in these Northern locations. We should somehow be able to incorporate their observations of change, even though not taken in a scientific manner. They are some of the first people being affected by global warming and it is important that they are considered and serious efforts made to allow them to uphold their traditional lifestyle. [Michelle Koutnik]	Rejected. This is a topicfor WGII
4-381	A	13:19	13:22	Ice scour is also an important consideration for offshore pipeline design. [Sharon Smith]	Rejected. Less relevant than other issues discussed
4-382	A	13:22		As for land fast ice only examples in the Arctic are shown; however, those in the Antractic also should be refered to, where much larger area of fast ice exists. [Takashi Yamanouchi]	Rejected. This is a general introduction to fast ice which applies equally to the Arctic and Antarctic. Antarctic fast ice is specifically discussed later in this chapter
4-383	A	13:24	13:29	This sentence could be improved in terms of content and order, as follows: after "its extent (the area enclosed by the ice edge - operationally defined as the 15% concentration contour);" add "the total area of ice within its extent (i.e., extent weighted by concentration)", followed by the phrase about multi-year ice, which can be moved as written from line 27-28. [Ola M. Johannessen]	Accepted. Text revised
4-384	A	13:30		largely be transport" correct to: "by [Hartmut Grassl]	Accepted. Text revised
4-385	A	13:30		which is in turn determined largely be transport .." should be "by transport .. [Nick Rayner]	Accepted. Text revised
4-386	A	13:33	14:20	Section 4.4.2. This is one of the most important sections in Ch 4, as there have been very well documented and dramatic arctic sea ice changes, upon which consensus has been reached. However, this section is rather brief and limited to NASA work almost exclusively. [Ola M. Johannessen]	Taken into account, but the reviewer is incorrect in stating that the section is limited mostly to NASA work.

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4-387	A	13:35	15:14	Please, update the references and discussions to include the most recent publications on sea ice extent (by Stroeve et al. 2005; GRL, doi: 10.1029/2004GL021810 and Meier et al. 2005 EOS doi: 10.1029/2005EO360003) [Pavel Groisman]	Taken into account. Absolute minimum discussed with reference to Fig 4.4.2
4-388	A	13:36	13:40	Operational chart data provide quite good coverage for many areas back to the 1950s, at least. The WMO GDSIDB (see http://nsidc.org/noaa/gdsidb/ for example) is a collection of such and provides useful information to supplement those sources you use here. [Nick Rayner]	Taken into account. GDSIDB data source to be considered
4-389	A	13:38	13:40	This is also possibly only reprasing problem. As I read it now we learn that we have fair documentation of pack ice in the north back to the late 19th century, and a similarily good documentation in the south to the mid 19th century. There are very good resaons to believe that the documentation from the north is significantly better and longer records compared to the south. [Per Holmlund]	Accepted. Text revised
4-390	A	13:39	13:40	"Similar but not as comprehensive data exist in the Southern Hemisphere since the middle of the 19th Century" rather overstates the data availability, in my opinion. The data sources you list later on pages 14-15, lines 54-5 certainly don't come anywhere close to those available for the Northern Hemisphere, even with the additions I suggest below. [Nick Rayner]	Accepted. Text revised
4-391	A	13:42	13:52	The discussion of algorithms in this section is out of balance with the other sections. The other sections either should have a similar discussion of algorithms and associated errors (if space permits) or this section on sea ice algorithms should be shortened. [Anne Nolin]	Rejected. Following comments 4-392 to 4-400 indicate requirement for a revised algorithm/accuracy discussion
4-392	A	13:44	13:45	suggest changing this partial sentence to "their accuracy has been evaluated in a limited way using .." the validation of these algorithms that one finds in the literature does not amount to a systematic assessment of their accuracy. Often by necessity, validation data are used at limited times or in limited regions and it is misleading to imply that the retrievals have been systematically validated or that their accuracy has been systematically evaluated, as this text does. [Nick Rayner]	Taken into account in revised text
4-393	A	13:46	13:46	Should text refer to 5% or +/- 5%? Similarly, should text refer to 10% or +/- 10%? [Melinda Marquis]	Taken into account in revised text
4-394	A	13:46	13:47	The accuracy figures presented here for passive microwave retrievals are misleadingly high. That is, their error bars should be larger. It could be that certain algorithms are this accurate at certain times in certain regions, but the text implies that the figures are valid for all passive microwave retrievals. Some algorithms, which may have larger errors than others, are in general use and are easily available, so it might make more sense to present	Taken into account in revised text

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				a range of uncertainties or present best and worst case figures, rather than just these best case ones. My reading of the literature and discussions with experts have indicated that melt season errors are often more like 20-25%, or more in some places, which does not accord with the impression given by "errors of 10% or more". General accuracies, outside of the melt season, might be better represented by 5-10%. If the accuracy of retrievals is in doubt, this Chapter should present it openly, not hide behind best-case figures. [Nick Rayner]	
4-395	A	13:46	13:46	Claiming that passive microwave derived summer ice concentration accuracy is 10% is very generous. It may be true under some circumstances but is misleading. Are we talking about mean bias or std dev? Comiso et al., 1997 shows just two algorithms std dev in summer is ~20% with biases of up to -50%. [Jeff Ridley]	Taken into account in revised text
4-396	A	13:48	13:49	My understanding is that passive microwave retrievals tend to compress a diffuse ice edge and smear out a compact ice edge. [Nick Rayner]	Taken into account in revised text
4-397	A	13:49	13:51	"Summertime concentration errors do lead to a bias in estimated ice extent in the warm seasons of both northern and southern hemisphere (Agnew and Howell, 2003; Worby and Comiso, 2004)." Bias in what direction? [Chris Derksen]	Taken into account in revised text
4-398	A	13:49	13:52	Which way is the bias? It is unclear. I assume that it is a resolution bias that "expands the ice edge". [Jerry Mahlman]	Taken into account in revised text
4-399	A	13:49	13:49	Should word "bias" be changed to something like "apparent increase" or "spurious increase"? [Melinda Marquis]	Taken into account in revised text
4-400	A	13:49		What are "Summertime concentration errors"? [Neville Nicholls]	Taken into account in revised text
4-401	A	13:54	14:2	Section 4.4.2.1: The presentation of multi-year ice studies is incomplete, misleading, and biased to NASA work (e.g., Comiso). The contention that passive microwave multi-year ice estimates are "probably not a reliable climate indicator" is misleading. Multi-year ice area in winter has been successfully derived using algorithms other than NASA\s. Comiso's (2002) roundabout way to get at multi-year ice area actually confimed the climatic trend in multi-year ice area published by Johannessen et al. in Science in 1999. Subsequently, Belchansky and coworkers (2004 and 2005) have developed and applied multi-year algorithms and found the resulting negative trend to be comparable to Johannessen et al. (1999) as well as Comiso's (2002) trend in summer minima. [Ola M. Johannessen]	Taken into account in revised text

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4-402	A	14:1	14:2	Given the low-biases in summertime retrievals of concentration, the summer minimum ice area is likely to be a parameter which is highly prone to algorithm errors. [Nick Rayner]	Taken into account in revised text
4-403	A	14:4	14:2	Section 4.4.2.2: The presentation of hemispheric, regional and seasonal trends is also incomplete, misleading, and biased to NASA work (Comiso). This is moreover rather ironic, given the fact that it is these NASA researchers who consistently resisted the notion of a decline in arctic sea ice, at the same time that non-NASA analyses (Johannessen et al. (1995); Bjørgo et al. (1997); Johannessen et al. (1999) and others subsequently) presented compelling evidence, which was then later corroborated by NASA (See next few comments) [Ola M. Johannessen]	Taken into account in revised text
4-404	A	14:4	14:20	Is it certain that sea ice area/extent time series from the passive microwave record are not adversely affected by satellite drifts, instabilities or inappropriate splicing of partial records? These are issues which have been hotly debated in other areas (e.g. free atmospheric temperature from MSU). Given the extremely short overlap periods between the various SMMR and SSM/I instruments, it is not inconceivable that there may be hidden problems in the amalgamated record. There is some literature on this subject. It would be helpful to summarise the findings to head-off potential criticisms / expose any deficiencies. [Nick Rayner]	Taken into account in revised text
4-405	A	14:7	14:7	The sentence ending in "… and Antarctic changes." should have the reference (Cavalieri et al., 1997). [Ola M. Johannessen]	Rejected. Relevance of publication (which was cited in TAR) is accepted, but more recent publication (Comiso, 2003) also show this for a longer period.
4-406	A	14:7	14:7	There should be a sentence added before "An updated version of the analysis done by Comiso.." (which, by the way, appears to be an update that is not a published or accepted paper). The sentence could be such as follows: "The first trend analysis based on SMMR data found a slight negative trend in arctic sea-ice extent from 1978-87 (Gloersen and Campbell, 1991), a 3.2 × 104 km2 yr-1 decrease (2.4% per decade). Data from the subsequent SSM/I has provided the basis to follow up the SMMR trends. The Johannessen et al. (1995) analysis of SMMR and SSM/I records taken separately revealed a greater reduction in arctic sea-ice area and extent during the SSM/I period – decreases from 1987-94 were ~4% per decade compared to ~2.5% per decade from 1978-87. Merged SMMR–SSM/I time series have since been produced and analyzed, establishing the trends more robustly (Bjørgo et al., 1997; Cavalieri et al., 1997). Two independent analyses of merged SMMR–SSM/I data established the trend in arctic ice area and extent	Taken into account with inclusion of Johannessen et al. (2004) work. But AR4 is meant to be the most recent assessment, not a history of prior assessments. Updates of data sets using previously published methodology is acceptable for IPCC.

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				(1978-95) to be about –3.0 × 105 km2 per decade, corresponding to ~3% per decade (Bjørgo et al., 1997; Cavalieri et al., 1997). Since then, several studies have come to this consensus, e.g., Johannessen et al., (2004)." Alternatively, recommend using text from Section 3 in Johannessen et al., (2004) for a current and objective state-the-art. [Ola M. Johannessen]	
4-407	A	14:9		I do not understand why this trend is insignificant - it is more than three times the quoted error estimates [John Church]	Taken into account in revised text
4-408	A	14:10	14:10	How can a trend of 0.7 +- 0.2 be "insignificant"? Is not 0.2 the confidence interval, so it clearly is significantly positive? [Stefan Rahmstorf]	Taken into account in revised text
4-409	A	14:11	14:12	After the end of the single sentence about region-to-region differences, there is clearly the need to expand on this point and to illustrate it with a figure mapping the spatial variability in trends in summer and winter, e.g., Johannessen et al. (2004), Fig. 4c,d. [Ola M. Johannessen]	Rejected. Chapter needs to be cut, not expanded
4-410	A	14:14	14:14	Recommend strongly to show the powerful satellite images of sea ice here, e.g. Sept 1979 and 2005 - I recommend this as a cover figure for the whole report! [Stefan Rahmstorf]	Noted
4-411	A	14:14	14:22	Figure 4.4.1 and 4.4.2. Related to my concerns above, it would be instructive to the reader to include error bars on the values presented in these time series. At present, uncertainties are presented on the linear trends, which relate only to the variability about the trend and not to any uncertainty in the data themselves. If errors in summertime retrievals are of the order 20% or more, as I have suggested, then the error bars on the ice area curve in Figure 4.4.2 will be substantial. Adding error bars will draw the reader's attention to the true uncertainty in this value, which should be at least twice that quoted. [Nick Rayner]	Taken into account. The assessment of significance includes consideration of the accuracy of the retrievals
4-412	A	14:16	14:50	This section is not convinsing. The time series are very short and their significance can be questioned. The long record by Omstedt and Chen which is mentioned should be shown. [Per Holmlund]	Rejected. Presumably by “short time series” the reviewer refers to the summer minimum trend, which other reviewers do find convincing.
4-413	A	14:16	14:20	Wow! It is not often that nature gives us such remarkably strong signals. [Jerry Mahlman]	Noted
4-414	A	14:16	14:20	Would it be possible to include a mention of the record minimum sea ice extent for 2005? [Anne Nolin]	Noted. We are unaware though of any yet published paper that cites this.
4-415	A	14:18	14:19	The sentence ending with "… (updated from Comiso et al, 2002)" concerns another unpublished update. Serreze et al. (2003) and Stroeve et al. (2005) have reported and	Noted. Updates of data sets using previously published methodology is

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				diagnosed the record low summer ice cover from 2002-2004. [Ola M. Johannessen]	acceptable for IPCC. Absolute minimum discussed with reference to Fig 4.4..2]
4-416	A	14:19	14:20	The last sentence in the paragraph does not go far enough -- at least mention the NAO here! -- nor does it include more pertinent references than Belchansky et al., such as Deser et al. (2001). [Ola M. Johannessen]	Taken into account.
4-417	A	14:19	14:20	More specific expression is appropriate for the "variability in atmospheric circulation". Moreover, this is one of the most high-lighted area of discussion in this section (summer Arctic sea ice might disappear within a while), much more expression should be added. For example reference to Stroeve et al. (2005; GRL 32, L04501) might be included. [Takashi Yamanouchi]	Taken into account.
4-418	A	14:24	14:50	Section 4.4.2.3: No mention of sea ice decreases in the early 20th century warming, see, e.g., Johannessen et al. (2004). The decreases are shown to be pronounced, though not as large as in the most recent, ongoing warm period in the Arctic. [Ola M. Johannessen]	Taken into account.
4-419	A	14:26	14:27	Rayner et al (2003) does not compare "several compilations of available data spanning the 20th century". Rather it attempts to compile a data set of sea ice concentration for the 20th century from available sources and account for the inhomogeneity between them. Also, the sentence as it stands doesn't make sense: "the most recent" what is shown in Figure 4.4.3? These time series from Rayner et al (2003) can be updated to 2005 and I will supply Peter Lemke with the values. [Nick Rayner]	Accepted and text revised
4-420	A	14:27	14:27	…are blended in Rayner et al. (2003) giving the series shown in Figure 4.4.3. [David Parker]	Accepted and text revised
4-421	A	14:27	14:28	The summer decline in HadISST1 starts in the early 1970s, not the 1960s. [Nick Rayner]	Accepted and text revised
4-422	A	14:30	14:33	It seems more accurate to describe the trend in the Vinje data set as a generally continuous decline from the start of the record to the end. [Nick Rayner]	Accepted and text revised
4-423	A	14:33	14:38	The decline in the Russian data is reversed in the late 1990s. This is not mentioned in the text, which makes the text seem rather selective. [Nick Rayner]	Accepted and text revised
4-424	A	14:38		What is the relevance of the unexplained Koch index – this should be simply left out or else adequately explained in the text? [Melanie Fitzpatrick]	Accepted. Koch index has been removed from the figure.

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4-425	A	14:45		sea ice occured" add: "sea ice that occured [Hartmut Grassl]	Accepted and text revised
4-426	A	14:45		Is "significant" used here in a formal statistical sense, or as meaning "substantial"? Should only use this word in the formal sense. [Neville Nicholls]	Accepted and text revised
4-427	A	14:48	14:49	As indicated above, these general statements aren't entirely consistent with what the time series actually show. It seems that the text is selectively reporting what the authors want to see rather than what is apparent. Although this probably isn't the case it is, nevertheless, how it appears. You could additionally look at the GDSIDB to see what that shows (Vasily Smolyanitsky (AARI) has created a blended data set from the available ice charts). [Nick Rayner]	Taken into account. Text revised
4-428	A	14:52	14:52	Figure 4.4.3. This figure is strange and may need some discussion. The recent trends are not at all clear except for the modelled scenario which shows a clear recent decrease in sea ice extent. To me it rather disqualify the model than adding any new knowledge. [Per Holmlund]	Rejected. HadISST1 is a data set – not a model run
4-429	A	14:52		Figure 4.4.3. This is a very useful figure, as the Norwegian and Russian data sets shown are not yet included in HadISST, so there is little duplicated information. It takes a little understanding though, as it is quite complicated! [Nick Rayner]	Noted
4-430	A	14:54	15:5	HadISST1 additionally incorporates information from atlases of ice extent from Tolstikov, for the 1940s to 60s, and Deutsches Hydrographisches Inst for rthe 1920s and 30s (see Rayner et al, 2003). These support the conclusion of increased ice extent prior to the satellite era. [Nick Rayner]	Taken account of. HadISST1 compilation referenced as additional Antarctic data source
4-431	A	15:1	15:14	It seems that we are consistently finding statistically insignificant Antarctic trends! Seems like we need to pay attention to the "non-trends" of Greater Antarctica. [Jerry Mahlman]	Noted
4-432	A	15:1	15:5	I understand that Torgny Vinje has digitized historical ice maps in Nth Atlantic from 1553 to 2002 as an ACSYS project. It shows major retreats in sea ice. Is this compatible with what is stated here? [Kevin Trenberth]	Noted. Vinje compilation covering same time period as other sources is Barents compilation is grey literaure only: Vinje, T., Barents Sea ice edge variation over the past 400 years. Proceedings of the Workshop on Sea-Ice Charts of the Arctic. Seattle, WA, USA, 5-7 August 1998, World Meteorological Organization,

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					WMO/TD No. 949, 4-6, 1999.
4-433	A	15:2	15:4	Please check wording of this sentence, as it appears to something is missing or otherwise wrong with it. [Melinda Marquis]	Taken into account. Long sentence broken in to 2 to clarify.
4-434	A	15:7	15:7	This section needs an addition about how the observed ice conditions were prior to the 1950s. This is a way of hiding very important information and open up doors for severe critisism. [Per Holmlund]	Taken into account in revised text
4-435	A	15:10		"However" here suggests that Curran et al., contracted the Ackley criticism, it does not, since Curran's work, while importance, has only local significance. [David Vaughan]	Accepted
4-436	A	15:15		So what is the summary of this section? [Kevin Trenberth]	Taken into account. Summary added
4-437	A	15:19	:57	Is all of this detail necessary? [John Church]	Taken into account. Section shortened
4-438	A	15:21		of ice" add: "of sea ice"; delete: "thickens [Hartmut Grassl]	Accepted
4-439	A	15:47	15:53	Eliminate this paragraph since this does not discuss any substantive results. [Anne Nolin]	Taken into account in shortened text
4-440	A	15:55	16:3	No mention of basin-wide ice thickess estimates from Russian North Polar drifting stations (see Johannessen et al., 2004). [Ola M. Johannessen]	Taken into account. Johannessen now referenced in Chapter
4-441	A	16:1	16:56	This is not an assessment but a recitation of several studies without comments on their value and shortcomings or putting them together as a synthesis. [Kevin Trenberth]	Taken into account. Summary added
4-442	A	16:5	16:8	Or we can use improved time-dependent radiatively forced climate models, assuming availabilty of higher-resolution, radiatively forced models. [Jerry Mahlman]	Noted
4-443	A	16:10	16:38	This section (and the sea ice section in general) seems to provide a general review rather than focus on new findings since TAR. The sea ice section could be trimmed considerably if it focussed on recent findings. The level of the material aslo seems to be targetted toward the general public. I thought this was the task of the synthesis reports. [Ross Brown]	Taken into account in shortened section.
4-444	A	16:11		Give the uncertainty estimates. [John Church]	Taken into account. Uncertainty added when possible
4-445	A	16:11	:38	Suggest the authors should given an assessment/synthesis of these different results rather	Taken into account. Summary added

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				that just compile the results. [John Church]	
4-446	A	16:21	16:21	42% compared to what? Compare to mean value from 1958-1977? [Melinda Marquis]	Accepted. Text revised
4-447	A	16:21		Give the uncertainty estimates. [John Church]	Taken into account. Uncertainty added when possible
4-448	A	16:23	16:23	"…43% reduction in average ice draft…" compared to what? Compared to mean value from 1976-1996? [Melinda Marquis]	Accepted. Text revised
4-449	A	16:23		Give the uncertainty estimates. [John Church]	Taken into account. Uncertainty added when possible
4-450	A	16:26	16:26	"It has been suggested…" this will not do. By whom? Ref? [Stefan Rahmstorf]	Accepted. Text revised
4-451	A	16:32	16:32	Delete "distribution"? [Melinda Marquis]	Accepted. Text revised
4-452	A	16:36	16:36	Delete "It is apparent that" [Melinda Marquis]	Accepted. Text revised
4-453	A	16:40	17:2	Section 4.4.3.3.: The other sources of sea-ice thickness data are undervalued here, in terms of somewaht discrepant variability trends vis-a-vis those leported for the submarine data. For example, the 20-year continuous record pack ice thickness derived from drifting stations (1971-90) indicatesabout a 4% decrease (Johannessen et al., 2004). The large variability inherent in the arctic sea-ice-climate system renders the evaluation of ice thickness trends from the available observational data (sonar and other) an open question. [Ola M. Johannessen]	Taken into account. Value of other sources vis-a-vis submarine and models assessed in revised text
4-454	A	16:40	17:27	The difficulties of estimating sea-ice thickness distribution – and thus ice volume – are well reviewed. Direct measures such as satellite imagery and soundings from submarines both have problems as is discussed. Another way of viewing changes in ice formation and melting is to use the record of d18O stored in the upper ocean (e.g., see Ostlund and Hut, 1984). Although this geochemical tool has its own problems, it does offer unique insight especially in the complete record of water-column freshwater manipulation by runoff and by the ice, in the distribution and location of important processes, and in providing useful time series of change in freshwater or salt storage in the ocean – again, reflecting changes in the ice. Although we are clearly interested in changes in ice volume and area as 'physical features' of freshwater storage and change, the linkage to the greater question of ocean stratification and thermohaline circulation cannot be pursued without considering the ocean response to such change and I find this component practically	Taken into account but not included as this technique is less direct than many other sea ice thickness techniques criticized by some reviewers. Of more relevance to ocean chapter?

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				omitted from the discussion. Östlund HG, Hut G. Arctic Ocean water mass balance from isotope data. J Geophys Res 1984;89:6373-6381. [Robie Macdonald]	
4-455	A	16:40	17:27	As alluded to in the Sea Ice component of Chapter 4, change in ice thickness can be the result of in-situ melting or ice export – the ice itself may not give any clues as to the relative importance of these two mechanisms, but clearly if one wants to understand the significance in changing ice volume to ocean processes, one needs the answer to this question. It has long been known that the isotopic composition of the water beneath the ice provides a record of the balance between ice production and melting and, with an appropriate time series, yields changes in that balance. A number of authors have discussed the importance of assigning source of recent change in freshwater composition using isotopic tools (see, for example; Aagaard and Woodgate, 2001; Ekwurzel et al., 2001; Macdonald et al., 1999;2000; Schlosser et al., 2000; 2002) and their relevance to time series (e.g., Muller-Lupp and Bauch, 2005). Finally, water-column measurements appropriately link freshwater sources (runoff and ice) and interactions between them. Aagaard K, Woodgate RA. Some thoughts on the freezing and melting of sea ice and their effects on the ocean. Ocean Modelling 2001;3:127-135. Ekwurzel B, Schlosser P, Mortlock RA, Fairbanks RG, Swift JH. River runoff, sea ice meltwater, and Pacific water distribution and mean residence times in the Arctic Ocean. J Geophys Res 2001;106:9075-9092. Macdonald RW, Carmack EC, McLaughlin FA, Falkner KK, Swift JH. Connections among ice, runoff and atmospheric forcing in the Beaufort Gyre. Geophys Res Lett 1999;26:2223-2226. Macdonald RW. Arctic estuaries and ice: a positive-negative estuarine couple. In: Lewis EL, Jones EP, Lemke P, Prouse T, Wadhams P, editors. The Freshwater Budget of the Arctic Ocean. Kluwer, Netherlands, 2000, pp. 383-407. Müller-Lupp T, Bauch H. Linkage of Arctic atmospheric circulation and Siberian shelf hydrography: A proxy validation using d18O records of bivalve shells. Global and Planetary Change 2005;48:175-189. Schlosser P, Ekwurzel B, Khatiwala S, Newton B, Maslowski W, Pfirman S. Tracer studies of the arctic freshwater budget. In: Lewis EL, Jones EP, Lemke P, Prowse TD, Wadhams P, editors. The Freshwater Budget of the Arctic Ocean. Kluwer Academic Publishers, Boston, 2000, pp. 453-478. Schlosser P, Newton R, Ekwurzel B, Khatiwala S, Mortlock R, Fairbanks R. Decrease of river runoff in the upper waters of the Eurasian Basin, Arctic Ocean, between 1991 and 1996: evidence from d18O data. Geophys Res Lett 2002;29(9) 10.1029/2001GL013135:3-1 - 3-4.	Taken into account but considered of more relevance to total ocean fresh water budget (Chapter 5) than to sea ice component only

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				[Robie Macdonald]	
4-456	A	16:46	16:46	What is 'modal' thickness? [Anny CAZENAVE]	Accepted and defined
4-457	A	17:8	17:27	This is a nice analysis that is well written. [Jerry Mahlman]	Noted
4-458	A	17:9	17:9	"constrained" instead of "contrained". [Roxana Bojariu]	Accepted
4-459	A	17:14		I suggest that "suggest" should be used rather than "indicate" when models are used to estimate trends [Neville Nicholls]	Accepted
4-460	A	17:17	17:17	Replace "change" with "decrease" for clarity. [Melinda Marquis]	Accepted
4-461	A	17:18	17:18	Replace "change" with "decrease" for clarity. [Melinda Marquis]	Accepted
4-462	A	17:18	17:27	The last sentence of this paragraph (line 25-27) seems to state what the three sentences (starting with "Although …" on line 18) say. Consider re-wording this paragraph for clarity. [Melinda Marquis]	Taken into account in revised text
4-463	A	17:20	17:20	"..attribute the abrupt change" Which abrupt change? I don't see what is meant. [Stefan Rahmstorf]	Rejected. Abrupt change in thickness has been previously discussed in observational data in 4.4.3.2
4-464	A	17:30	17:56	This strikes me as being unnecessarily vague, with not much discernible connection to this IPCC assessment chapter. [Jerry Mahlman]	Taken into account in shortened text
4-465	A	17:31	17:34	Sewall and Sloan (2004) showed in their modeling study that putting a future Arctic sea ice distribution into the model produced markedly reduced precipitation for western North America, particularly the Pacific Northwest. [Anne Nolin]	Noted, but not considered relevant here
4-466	A	17:31	17:34	Not an assessment. [Kevin Trenberth]	Taken into account and revised
4-467	A	17:31	:34	why not add this data to Figure 4.4.4? [John Church]	Rejected. Fichefet modelling is for Antarctic. Fig 4.4.4 is Arctic only.
4-468	A	17:31		The more recent study of Goosse, H., and H. Renssen, 2005: A simulated reduction in Antarctic sea-ice area since 1750: Implications of the long memory of the ocean. International Journal of Climatology, 25, 569-579 should be cited here [Ian Simmonds]	Taken into account but Goosse and Renssen paper is essentially a modelling sensitivity study and does not fit in an observational chapter.

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4-469	A	17:36		Section 4.4.3.5. Landfast ice changes. A definition of landfast ice would be welcome for non-specialist readers [Philippe Tulkens]	Rejected. Definition is in the glossary
4-470	A	18:1	18:13	This discussion still doesn't seem to connect with its neighboring sections. Lines17-57. This all looks like natural variability to me. [Jerry Mahlman]	Taken into account in revised text
4-471	A	18:5	18:5	"… all available ship observations" for what time period? [Melinda Marquis]	Accepted. Text modified
4-472	A	18:15	19:25	Section 4.4.4: The climate-change justification for including sea ice motion is not specified here. [Ola M. Johannessen]	Taken into account. Text modified and shortened
4-473	A	18:27	18:27	No need to spell out "International Programme for Antarctic Buoys" here, as it's already spelled out in paragraph above. [Melinda Marquis]	Rejected. IABP is previously defined, but not IPAB
4-474	A	18:38	18:39	I cant telll if this accuracy is important or not. I would either delete this sentence or indicate the relevance of the errors. [Neville Nicholls]	Taken into account in revised text
4-475	A	18:43	:57	What is the implications of these results? [John Church]	Taken into account in revised text
4-476	A	18:47	18:47	Using a coupled atmosphere-ocean-ice model… [David Parker]	Accepted
4-477	A	18:50	18:50	Change "is" to "are." [Melinda Marquis]	Accepted
4-478	A	18:51	18:55	Please use NAM not AO: it is neither Arctic nor an Oscillation! Also p 4-19. [Kevin Trenberth]	Accepted and changed throughout. The title of many cited publications refers to AO, but the glossary defines “Annular Mode” and explains NAM-AO link.
4-479	A	18:54		effect" instead of "affect [Hartmut Grassl]	Accepted
4-480	A	19:0		General comment on the section "Glaciers and Ice Caps": The basic knowledge and concepts used by internationale observing programmes would have enabled the structure and formulations of the text to be more logic and precise: especially the correct consideration of englacial temperature conditions (cold firn areas in polar regions and in continental type mountains!) and better - more integrated - analysis of mass balance, length change and inventory data would have helped. Next time, international monitoring programs should be included from the very beginning. Contributing Author Martin	Noted. The authors believe that these issues are adequately presented for the AR4..

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				Hoelzle would have been a representative of the World Glacier Monitoring Service but was not asked for comments on glaciers. The permafrost section relates better to GTOS programmes. [Wilfried Haeberli]	
4-481	A	19:9	19:9	This is about ice export – freshwater fluxes are not equivalent to same and either should be discussed in the section or the subtitle changed. [Robie Macdonald]	Taken into account – title changed
4-482	A	19:9	19:25	This discussion is very interesting, but seems to not connect well with the overall theme of this Chapter 4 Draft Report. [Jerry Mahlman]	Taken into account as above
4-483	A	19:12	19:12	1950-2000" instead of "19502000 [Roxana Bojariu]	Accepted. Text corrected.
4-484	A	19:12	19:12	Hypenate 1950-2000 [Jeff Ridley]	Accepted. Text corrected.
4-485	A	19:12		19502000" add "1950 to 2000 [Hartmut Grassl]	Accepted. Text corrected.
4-486	A	19:12		Huh? [Kevin Trenberth]	Accepted. Text corrected.
4-487	A	19:15	19:25	comment to the whole paragraph: where are freshwater fluxes? [Hartmut Grassl]	Taken into account as above.
4-488	A	19:15	19:25	Please assess this study. [Kevin Trenberth]	Taken into account in revised text.
4-489	A	19:24		What does minor variations mean - there seems to be a trend in the data in figure 4.4.6. [John Church]	Rejected. Time series show decadal variability but not a significant trend.
4-490	A	19:29		Section 4.5 is good. [Jonathan Gregory]	Noted
4-491	A	19:33	19:33	write "… (G&IC) are close to melting conditions and have a high …" [Wilfried Haeberli]	Rejected – not all and not all entire G&ICs are close to melting conditions. This is not different from ice sheets.
4-492	A	19:33	19:33	how about "relatively high ratio" - to emphasis we are comparing with big ice-sheet? [Richard Hindmarsh]	Accepted. Text modified
4-493	A	19:33	19:56	This paragraph seems to make a straightforward explanation surprisingly opaque. A rewrite might help. [Jerry Mahlman]	Noted, background paragraph reorganized
4-494	A	19:34	19:34	write "…conditions much more rapidly …" [Wilfried Haeberli]	Accepted

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4-495	A	19:35	19:36	"and have direct input to oceans" -- sea level rise? This is unclear [Michelle Koutnik]	Accepted. Text modified
4-496	A	19:36	19:36	add: They have been systematically monitored for more than a century and are now observed using integrated multilevel strategies as part of global climate-related observing systems (Global Terrestrial Network for Glaciers GTN-G within GTOS/GCOS; Haeberli et al. 2002, Haeberli, 2004). References: Haeberli, W., Maisch, M. and Paul, F. (2002): Mountain glaciers in global climate-related observation networks. WMO Bulletin, 51/1, 18-25. Haeberli, W. (2004): Glaciers and ice caps: historical background and strategies of world-wide monitoring. In: Bamber, J.L. and Payne A.J. (eds): Mass Balance of the Cryosphere. Cambridge University Press, Cambridge, 559-578. [Wilfried Haeberli]	Rejected – beyond scope of WG1 science assessment report
4-497	A	19:37	19:38	I suspect that 540H103 means 540x103 (idem for 133H103) [Anny CAZENAVE]	Editorial (conversion problem)
4-498	A	19:37	19:38	citations for the figures missing? [Hartmut Grassl]	Accepted. Text revised.
4-499	A	19:37	19:38	Not sure what the "H" is in 540H10 and 133H10 - is this a conversion problem from Word to PDF? Should it be the "times" symbol? [Thomas James]	Editorial (conversion problem)
4-500	A	19:37	19:38	notation doesn't make sense: "540H10^3", mean 540 * 10^3? [Michelle Koutnik]	Editorial (conversion problem)
4-501	A	19:37	19:38	Define the meaning of the H1000 km3 unit [Steven Massie]	Editorial (conversion problem)
4-502	A	19:37	19:38	Delete the "H" from the area and volume numbers. [David Parker]	Editorial (conversion problem)
4-503	A	19:37	19:38	exponent symbol [Jeff Ridley]	Editorial (conversion problem)
4-504	A	19:37	19:38	What is '540?103 km3' etc? [Ian Simmonds]	Editorial (conversion problem)
4-505	A	19:37	19:38	What are the numbers here and the H? [Kevin Trenberth]	Editorial (conversion problem)
4-506	A	19:37	19:37	540H10exp3, isn't there a typo error ? (the H) [Philippe Tulkens]	Editorial (conversion problem)
4-507	A	19:37		Change units from H 10^3 to 10^6 [Eric Rignot]	Editorial (conversion problem)
4-508	A	19:38	19:38	133H10exp3, isn't there a typo error ? (the H) [Philippe Tulkens]	Editorial (conversion problem)

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4-509	A	19:38	:39	What is the authors assessments of these different estimates? [John Church]	Noted – measured data are by far not sufficient to calibrate estimation methods.
4-510	A	19:40	19:40	Maybe give the DM-05 reference at the end of the sentence "….SLE (Dyergerov and Meier, 2005)." [Thomas James]	Accepted
4-511	A	19:42		the gain or loss of "snow and ice on the surface" Mass of what?? (I know it, but I prefer to define it more clearly) [Kenichi Matsuoka]	Accepted
4-512	A	19:46	19:49	Is it worth to mention this? I think that it is too much detail, and does not help reader's better understanding, and does not give a set of understandings. [Kenichi Matsuoka]	Rejected – But text is clarified
4-513	A	19:48	19:48	missing comma: "Atmospheric temperature lapse rates, precipitation gradients, and the balance… [Michelle Koutnik]	Accepted
4-514	A	19:51	19:51	write: "…(maritime - temperate firn/ice, large mass tornover) [Wilfried Haeberli]	Rejected – in combination with 515
4-515	A	19:52	19:52	write: "…(continental - polythermal to cold firn/ice, small mass turnover). The latter often contain cold firn areas which react by firn warming rather than mass loss to atmospheric temperature increase and are most sensitive … [Wilfried Haeberli]	Rejected – not entirely correct and to detailed for WG1 assessment report
4-516	A	19:54	19:54	"the mass balance to become zero" is correct, if the mass balance is that averaged over the glacier. My suggestion is to remove too much details about mass balance gradient lines46-49 and introduce "accumulation area" and "ablation area" instead and then mention glacier-wide averaged mass balance. [Kenichi Matsuoka]	Rejected – this is not necessarily the same. Mass balance gradient is a strong climate indicator.
4-517	A	19:55	19:55	Whether mass balance "always tends toward zero" is open to challenge. Surging glaciers, for example, manifest dramatic overshoots in mass balance and certainly don't tend toward zero. [Garry CLARKE]	Taken into account – text modified
4-518	A	19:55	19:56	Mass-balance does not always tend to zero - this is the whole point about the G'land ice-sheet disappearing - there are tipping points. Is it clear that the discussion is about valley glaciers? [Richard Hindmarsh]	Taken into account – text modified
4-519	A	19:56	19:56	better write "Changes in length ..." (not "geometry") [Wilfried Haeberli]	Accepted– text modified

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4-520	A	19:56	19:56	Insert "behind" after "lag." [Melinda Marquis]	Accepted, text revised.
4-521	A	20:0		Table 4.5.2, first line: write -204 (not 204) [Wilfried Haeberli]	Accepted - corrected
4-522	A	20:0		Why is "b" for Dyurgerov and Meier positive in Table 4.5.2? [Neville Nicholls]	Accepted – corrected
4-523	A	20:0		Dyurgerov and Meier (2005)'s sources should be referenced in the table. Dyrugerov and Meier (2005) is not a peer-reviewed paper and should not superseed references that have been published. That report has lots of problems. [Eric Rignot]	Rejected – the paper is an update of previous papers; ref. list of compilation paper cannot be copied into Chapter ref. list
4-524	A	20:0		Raper and Braithwaite 2005 GRL paper has a lot of problems as well, as criticized by Meier and others in GRL 2005. That critic should be mentioned here. [Eric Rignot]	Noted. Text on general problems with area and volume estimates modified.
4-525	A	20:0		MB-96 has little value since it is based on statistical fitting of data that are not representative of the contribution of glaciers to sea level change. [Eric Rignot]	Noted . Measured data are by far to few in order to assess which estimate is right. Text on general problems with area and volume estimates modified
4-526	A	20:0		I do not understand the rationale for averaging all published values regardless of the quality of each estimate. With this approach, the end results is likely to be close to zero plus or minus an enormous error bar. [Eric Rignot]	Comment unclear. There is no averaging done.
4-527	A	20:2	20:12	There is an inconsistency between "5-7m" on line 2 and "4-7m" on lines 9 and 12. [John Hunter]	Cannot find the commented numbers. Page and/or line numbers wrong.
4-528	A	20:4	20:20	Table 4.5.1 is unreadable. What numbers refer to? volume or SLE? [Anny CAZENAVE]	Taken into account – Table design modified
4-529	A	20:4	20:8	Table 4.5.1: please explain in caption all the items in the table. [Kevin Trenberth]	Taken into account – Table design modified
4-530	A	20:6		What is the authors assessments of thes non overlapping estimates G&IC volumes and how do these estimates differ from the TAR? [John Church]	Noted. Text on general problems with area and volume estimates modified
4-531	A	20:6		Table 4.5.1 show absolute volue/area and sea level equivalents more clearly. E.g. put SLE into brackets. [Kenichi Matsuoka]	Taken into account – Table design modified
4-532	A	20:8	20:12	the text should clearly point to the problems with volume/area-scaling (which is not a generally accepted scientific concept): volume/area scaling is indeed problematic for several reasons: (a) from a statistical point of view, it makes no sense to correlate a	Noted. The problem is apparent but not how it should be discussed within the limited space of the WG1 report. Text

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				variable with itself (area is contained in volume) – volume/area-scaling just suppresses the large scatter (roughly 30% standard deviation for mountain glaciers) in area/thickness relations, which are statistically more reasonable. The large scatter in ice thickness data as related to area is due to (i) the small number and often incomplete coverage of accurately measured glaciers and (ii) the fact that not area is directly/physically related to ice thickness (an indefinitely wide but very short glacier would be very thin and not very thick!) but shear stress as governed by mass turnover or the mass balance gradient times the altitudinal extent of glaciers (inverse flow law). Volume/area relations – besides being physically and statistically questionable – are neither constant in time nor in space if climatic conditions change from maritime to continental or vice-versa. [Wilfried Haeberli]	on general problems with area and volume estimates modified.
4-533	A	20:17	20:17	This is not really correct as the ice volume needs to be first converted to water equivalent volumes [William Hare]	Accepted. Corrected
4-534	A	20:21	22:33	secti 4.5.2 : recall briefly (as in section 4.6.2.1) what techniques are used to measure mass balances. [Anny CAZENAVE]	Noted – has been removed from ZOD for space reasons
4-535	A	20:23	20:36	give some minimum information about the '300 glaciers'. Location, representativeness? Do they include Alaska and Patagonia glaciers? [Anny CAZENAVE]	Rejected – would fill a two page table. Information is given in cited papers.
4-536	A	20:23	20:36	Does the mean value (0.36 mm/yr) account for the Patagonia and Alaska glaciers contribution? [Anny CAZENAVE]	Noted – numbers and text modified
4-537	A	20:24	20:24	Insert something like "necessary to observe and record such measurements," after "fieldwork." [Melinda Marquis]	Noted – see 539
4-538	A	20:24		... Because of the required very intensive field work, ... [Jerry Mahlman]	Noted – see 539
4-539	A	20:24		Insert "required" after "fieldwork" [Neville Nicholls]	Accepted. Inserted.
4-540	A	20:25	:26	Gerbaux et al. (in press, details above) lists and estimates the various sources of field measurement uncertainties of glaciers mass balance. [Christophe GENTHON]	Taken into account. Text modified
4-541	A	20:34	20:34	"measure" should be "measured". [Jonathan Gregory]	Accepted. Corrected
4-542	A	20:34	20:34	change "directly measure mass" to "directly measured mass".	Accepted. Corrected

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				[Thomas James]	
4-543	A	20:34		correct: "measure mass" to "measured mass" [Hartmut Grassl]	Accepted. Corrected
4-544	A	20:36	20:36	remove one period from end of sentence [Thomas James]	Accepted. Corrected
4-545	A	20:36	20:36	delete . at the end of the line [Atle Nesje]	Accepted. Corrected
4-546	A	20:38	20:43	Table 4.5.2 : is it possible to provide uncertainties with the SLE numbers? [Anny CAZENAVE]	Taken into account, but Table 4.5.2 is removed
4-547	A	20:38	20:43	Table 4.5.2 and associated text : what is the reason for the large difference between Cogley and Duygerov&Meier estimates? [Anny CAZENAVE]	Taken into account, but Table 4.5.2 is removed
4-548	A	20:38	20:43	We used without explanation. I have to say when it is explained many pages over as water equivalent surely it could be made W.E. or something more intuitive? [Kevin Trenberth]	Taken into account – text modified
4-549	A	20:41		in the first line it must be "-204" [Hartmut Grassl]	Accepted –corrected
4-550	A	20:41		Table 4.5.2 First row, right column. 204 should be - (negative) 204. [Kenichi Matsuoka]	Accepted –corrected
4-551	A	21:0		What is PIF in Table 4.5.3? [Neville Nicholls]	Taken into account – modified
4-552	A	21:3	21:17	These are impressively large numbers that are real attention getters. Hard work does pay off! [Jerry Mahlman]	Noted
4-553	A	21:3	21:3	Table 4.5.3 and Fig 4.5.1 appear to include Antarctica. Should the text say "outside of interior Greenland and Antarctica"? [David Parker]	Taken into account – modified for clarification
4-554	A	21:4	21:4	Insert "mean" after "specific." [Melinda Marquis]	Accepted - modified
4-555	A	21:4	21:4	Hyphen missing, in "1960/1961-1997/1998" [James Renwick]	Noted – corrected
4-556	A	21:4	21:4	Hypenate dates [Jeff Ridley]	Noted – corrected
4-557	A	21:4		a space is missing between "1960/1961" and "1997/1998" [Hartmut Grassl]	Noted – corrected

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4-558	A	21:7	:8	As a consequence of this compensation, the overall numbers for European glaciers are misleading. Should Northern and Southern Europen glaciers be separated (e.g. in table 4.5.3)? [Christophe GENTHON]	Noted – also different regions in the Himalaya and the Andes would merit to be shown separately. Beyond space limit.
4-559	A	21:11	21:13	Fig. 4.5.2 seems to show that Patagonia has lost the most mass, but here text says Alaska + High Mountains have contributed the most. Seems confusing. [Melinda Marquis]	Noted – Figures and Tables rearranged
4-560	A	21:13	21:17	I would suggest to add the following sentence at the end of Line 17. "Also, uncertainity remains due to no information on the mass balance of accummulation area of Asian large glaciers, which accounted for 80 % in area of the total glaciarized basin (Fujii and Higuchi, 1977). They have large ablation area which are mostly covered with debris, where ice thickness is decreasing at a rate of about one meter per year (Kadota et al., 2000; Nakawo et al., 1999)." Information of the literatures: Fujii, Y. and Higuchi, K.(1977): Statistical analyses of the forms of the glaciers in Khumbu Himal. Seppyo, 39, Special Issue, 7-14; Kadota, T., Seko, K., Aoki, T., Iwata, S., and Yamaguchi, S.(2000): Shrinkage of the Khumbu Glacier, east Nepal from 1978 to 1995. IAHS Publication, 264, 235-243; Nakawo, M., Yabuki, H. and Sakai, A.(1999): Characteristic of Khumbu Glacier, Nepal Himalaya: recent change in the debris-covered area. Ann. Glaciol., 28, 118-122. [Masayoshi NAKAWO]	Taken into account. Text modified
4-561	A	21:13	:17	What are the implications of not considering iceberg calving? [John Church]	Taken into account – text modified
4-562	A	21:14		Why is iceberg calving important? This is unclear. In Patagonia it is of NO importance. Same in Alaska. What matters is volume change. Loss of ice front area is typically less than 10% of the total loss in Alaska and Patagonia. [Eric Rignot]	Taken into account – text modified
4-563	A	21:14		What does Antarctica mean? Does it include the Antarctic Peninsula? If yes, then this estimate is too low and ought to be justified based on published results. [Eric Rignot]	Taken into account. Text modified
4-564	A	21:14		This part of the chapter misses an important point, which is that the largest contributors to sea level rise contribute far more melt than predicted from melt/accumulation models, especially Patagonia because ice dynamics is an important factor, possibly as large or larger than the effect of surface melt. Most thinning glaciers have a high rate of ice discharge or flow too rapidly to maintain ice balance. This is the case of Columbia glacier and other tidewater glaciers in Alaska, and even more so in Patagonia where tidewater glaciers represent 90% of the glaciers in terms of ice volume. This fact is reported in both Science papers and is very important. It shows that predictions of the contribution to sea	Taken into account – text modified

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				level of these glaciers based on surface mass balance model that ignore ice dynamics are underestimates. This discussion should be added in Special features before 4.5.4. [Eric Rignot]	
4-565	A	21:21		"we" should be explicitly defined as "water equivalent" to avoid confusion. [Jerry Mahlman]	Accepted. Text changed.
4-566	A	21:23	21:31	Table 4.5.3: It is not clear why the table provide separate values for 1960-2003 and 1960-1998. I understand that 1960-1998 corresponds to the period common to the 3 studies (Ohmura, cogley and Dyugerov&Meier) -as discussed earlier in the text.But this is -patly- redundant with table 4.5.2. Why not combine Tables 4.5.2 and 4.5.3? Are the values given in Table 4.5.3 for the Alaska/Patagonia glaciers those of Arendt et al./Rignot et al.? Some clarification would be helpful. [Anny CAZENAVE]	Noted – periods defined in accordance with chapter 5. Table rearranged
4-567	A	21:23	21:23	Define the meaning of the unit (km3 we) [Steven Massie]	Accepted
4-568	A	21:26	21:26	Table 4.5.3. I think it would be useful to give the marginal glaciers of Greenland separately from the rest of the Arctic. [Jonathan Gregory]	Taken into account – table rearranged
4-569	A	21:26		Table 4.5.3 PIF is not defined elsewhere (perharps) [Kenichi Matsuoka]	Taken into account – caption modified
4-570	A	21:37	21:37	suggests confidence" change to "gives confidence [Thomas James]	Accepted. Text Changed
4-571	A	21:37	21:38	This seems to be a reasonable and important result, even though the prose is quite cautious. [Jerry Mahlman]	Taken into account – text modified
4-572	A	21:38		Green, in review is not listed in the reference. May be Greene?? [Kenichi Matsuoka]	Noted – corrected
4-573	A	21:40	21:40	Figure 4.5.3. The top panel is not mentioned in the caption. This panel probably duplicates something in chapter 3, to which it would be better to refer. In the second panel, do both estimates come from Cogley? If so, could you show Dyurgerov and Meier as well for comparison? [Jonathan Gregory]	Taken into account – figure rearranged
4-574	A	21:45	:46	I don't think that correlations with other meteorological variables that affect the mass balance of glaciers through the surface energy budget (e.g. components of radiation) have been evaluated. The fact that net balance correlates with temperature does not necessary implies that one primarily reponds to the other. It might also be that the 2 respond to a third forcing. It is actually likely that temperature has a primary role, but studies like	Taken into account – Text modified .

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				Gerbaux et al (in press, details above), that separately evaluate the sensitivity of glacier net balance to all surface meteorological variables provide a quantitative (although model based) assessment. [Christophe GENTHON]	
4-575	A	22:0		This section is important to find out spatial valiabilities. However, it does not include Alaska, Patagonia, European Alps, Antarctic Peninsula, etc. Again, this section is not complete and should be expanded. [Kenichi Matsuoka]	Noted – because of space limit only exceptional new findings are mentioned.
4-576	A	22:1		... I prefer "mass loss" to "mass turnover" here. [Jerry Mahlman]	Rejected – it is explicitly “mass turn over” which is addressed here.
4-577	A	22:1		increaseD [Neville Nicholls]	Accepted. Corrected.
4-578	A	22:5		There are studies showing different variability in mass balance depending on climate regions. Sentence: There are studies that glaciers under monsoon climate (annual distribution of precipitation is strongly concentrated in warm season) is much more sensitive to temperature change than regions where winter precipitation is moderate or high (Fujita and Ageta, 2000) Reference: Fujita, K. and Ageta, Y. Effect of summer accumulation on glacier mass balance on the Tibetan Plateau revealed by mass-balance model Journal of Glaciology, 46(153), 244-252. 2000 [Tetsuo Ohato]	Taken into account. Text modified
4-579	A	22:7	22:23	It needs to be made what the assumptions of Oerlemans (2005) are. To me they assume the winter precip doesn't change. Vincent et al. (2005) explain some long European glaciers with a winter precip increase of 25% over 1760-1830. These two papers need to be reconciled with each other. In a forthcoming paper in JGR by Ethymiadis et al. winter precip during 1800-1830 over the Alps does show more (10%) than later in the 19th century. [Philip Jones]	Taken into account – paragraph reorganize, goes partly to Chapter 6
4-580	A	22:7	22:23	This is interesting, but it doesn't seem to speak to the assessment-centric theme of this Chapter 4. [Jerry Mahlman]	Taken into account – paragraph reorganized, goes partly to Chapter 6
4-581	A	22:7	22:23	I object strongly to the way Oerleman’s work is presented without a much more critical assessment. He does not account for changes in precipitation and other things. It is not global and the Fig 4.5.3 is objectionable, please see Chapter 3. [Kevin Trenberth]	Taken into account – paragraph reorganized, goes partly to Chapter 6

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4-582	A	22:7	33:	I like the way Oerlemans 05 is discussed, particularly the caveats regarding pre-1900 and complications due to precipitation changes. I can only suggest some more explicit cross-referencing with chap 6, which has a Box on glacier variations back in time. Nice job, however. [Jonathan Overpeck]	Noted – paragraph reorganized, goes partly to Chapter 6
4-583	A	22:8	22:8	Write "… as 1600 in a few cases, and based on geomorphological records - moraines, dated trees uncovered by retreating ice - even through past millennia) …" [Wilfried Haeberli]	Noted – paragraph reorganized, goes partly to Chapter 6
4-584	A	22:9	22:9	The Oerlemans curve should be included in the Spagahetti plot in Ch 6. If Ch 6 don't use itm which they don't, I wouldn't have thought this chapter should use it. [Philip Jones]	Noted – paragraph reorganized, goes partly to Chapter 6
4-585	A	22:11	22:11	"This shows…" at the end of the line should perhaps be replaced by "This analysis suggests…"? "shows" is too definite, given the possible shortcomings of the approach, discussed later. [James Renwick]	Noted – paragraph reorganized, goes partly to Chapter 6
4-586	A	22:12	22:12	Don't use the term global warming, or when it started without defining it [Philip Jones]	Noted – paragraph reorganized, goes partly to Chapter 6
4-587	A	22:14	22:16	Would it be possible to restate this without presupposing that there was a well-defined "Little Ice Age" period, since in 4.5.4 you discuss this in more detail. [Jonathan Gregory]	Noted – paragraph reorganized, goes partly to Chapter 6
4-588	A	22:15	22:15	Terms like the LIA and MWP need to be defined consistently across chapters, especially with Ch 6 [Philip Jones]	Noted – paragraph reorganized, goes partly to Chapter 6
4-589	A	22:15	22:15	The 'Little Ice Age' is considered to have its maximum in 1700 not 1850. The date of 1850 is inconsistent with with Chapter 6, cf fig 6.10. A cool period did occur around 1805 but this probably should not be referred to as 'the little ice age'. Since the T asia record does not extend back to 1700 it seems premature to associate this local minima with 'the little ice age'. [Jeff Ridley]	Noted – paragraph reorganized, goes partly to Chapter 6
4-590	A	22:17	:21	As already mentioned above, Casty et al. (Temperature and precipitation variabiltiy in the european Alps since 1500, Int. J. Climatology, in press) find no such increase in the observations. [Christophe GENTHON]	Noted – paragraph reorganized, goes partly to Chapter 6
4-591	A	22:27	22:33	This is true, but it may not be appropriate in the AR4. I don't think we are generally including statements about research needs. [Jonathan Gregory]	Taken into account – paragraph reorganized

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4-592	A	22:27	22:33	I suggest that this small section be deleted, or expanded [Jerry Mahlman]	Noted – paragraph reorganized
4-593	A	22:40	4:41	Taylor glacier effect is rather special and likely to be a bit of a glaciological curiosity [Richard Hindmarsh]	Noted – paragraph reorganized but paragraph is dedicated to SPECIAL regional features
4-594	A	22:40		It would be helpful if the significance of the different regions mentioned could be clarified for the reader. E.g. What are their areas or approximate mass of ice involved, etc. I would assume that the Himalayas are more significant in scale than Svalbard - but I might be wrong. [Martin Manning]	Taken into account - Text reorganized
4-595	A	22:43	22:43	Insert something like "of physical processes" after "Careful evaluation." [Melinda Marquis]	Taken into account – text modified
4-596	A	22:48	22:48	.....Oscillation (NAO) (Nesje et al., 2000), started to retreat.... [Atle Nesje]	Accepted. Text modified
4-597	A	22:50	22:53	This information needs some more explaining text. From the sentence above we can read about melting glaciers and then we can see that the cold surface layer of Storglaciären has lost 8 m (22% of its thickness!). In the 1990s approximately 15% of the total mass of Storglaciären held temperatures below the freezing point. between 1989 and 2001 the glacier toungue grew thicker but at the same time the ice became warmer. The warm part grew at the expense of the cold part. This is important proxy climate information, but it needs some more words to be understandable. [Per Holmlund]	Taken into account. Text reworded
4-598	A	22:55	22:55	"Coastal Scandinavia" is wrongly mentioned in the paragraph about New Zealand glaciers. [Roxana Bojariu]	Rejected – authors believe text is clear
4-599	A	22:55	22:55	As for coastal.." change to "As with coastal.. [Thomas James]	Accepted . Text changed
4-600	A	22:55		I thought New Zealand glaciers had first retreated, then advanced in recent decades, until about 2000? [Neville Nicholls]	Taken into account – text modified
4-601	A	23:3	23:3	The Swiss have estimated how much of their glaciers have disappeared in 2003. It would be useful to include this number [Philip Jones]	Noted – number is included (2.5 m W.E.)
4-602	A	23:5	23:5	Remove the word "negative" [James Renwick]	Accepted. Text changed
4-603	A	23:6	23:7	"This was caused by extraordinary high air temperature over a long period". Please be	Noted. Text modified

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				more specific, what extraordinary and a long period means. [Christof Appenzeller]	
4-604	A	23:11	23:11	I would suggest to add a reference of Fujita, K., Nakawo, M., Fujii, Y., and Paudyal, P. (1997), entitled "Changes in glaciers in Hidden Valley, Mukut Himal, Nepal Himalayas" appeared in J. Glaciol. 42, 583-588. It says that three Himalaya glaciers shrink at much faster rates than Tianshan glaciers, whose rate has been mainly used for assessing the rate of shinkage in Asian glaciers. [Masayoshi NAKAWO]	Noted. Paragraph reorganized
4-605	A	23:11		Is there any work indicating whether warming is causing the retreat of Himalayan glaciers? [Neville Nicholls]	Noted. Paragraph reorganized
4-606	A	23:12		and/or to thickened" delete: "to [Hartmut Grassl]	Accepted
4-607	A	23:15	23:31	In the Tropical Glaciers section, there is no explicit reference to Andean glaciers although I think there exist published papers. [Anny CAZENAVE]	Rejected – there are 5 references relating to the tropical Andes
4-608	A	23:15	23:31	This paragraph about tropical glaciers is interesting, and useful to include in order to set the record straight about Kilimanjaro. However, since tropical glaciers are only a tiny part of the cryosphere and its sea-level contribution, the paragraph could perhaps be a bit shorter. [Jonathan Gregory]	Noted, but sea level is one feature of the topic. Tropical glaciers provide climate change information from where little else information is available i.e. the tropical mid to high troposphere
4-609	A	23:15	23:31	It is very interesting that the tropical glaciers exhibited retreats into the1940s and continued again beyond the 1980's, thus following the trends in global-mean surface temperature quite well Clearly, the glacier melting was surprisingly coincident with the globally averaged surface air temperature. [Jerry Mahlman]	Noted
4-610	A	23:15		I cannot understand why an important paper of Kaser et al. is not discussed (G. Kaser, D. R. Hardy, T. Mölg, R. S. Bradley and T. M. Hyera (2004) "Modern glacier retreat on Kilimanjaro as evidence of climate change: observations and facts," Int. J. Climatol., 24, 329-339.) This paper maybe deals with a special case where the glacier retreats because of moisture deficiency, but it can suggest how you can work out a countermeasure for the reservation of the important glacier. [Kiminori Itoh]	Noted, but Mölg et al. (2003) and Mölg and Hardy (2004) cover the topic sufficiently.
4-611	A	23:16	23:16	relative" should be "relatively [Thomas James]	Accepted . Text changed
4-612	A	23:19		I suggest adding the word "regional" in front of changes to avoid some readers being	Accepted. Text changed

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				puzzled at the connection with statements elsewhere in the report that humidity is increasing (globally) leading to a possible expectation of glacier increase (everywhere). [Martin Manning]	
4-613	A	23:21		what does "hygric" mean? Maybe use a more accessable word? [Jonathan Overpeck]	Taken into account – terminology changed
4-614	A	23:27		Solar radiation is identified as the main driver for the retreat of the vertical walls (Moelg et al., 2003). Are there other references? What about the long-wave radiation? One publication is a weak argument given the fact that other publications cite the temperature increase and precipitation as the major cause for the retreat of the ice masses on Kilimanjaro. [Konrad Steffan]	Rejected – no in situ measured climate data based analyses have been performed on Kilimanjaro other than those by Mölg et al.
4-615	A	23:31		What period is this "decreasing retreat rate"? [Neville Nicholls]	Taken into account – text modified
4-616	A	23:35	23:57	This section belongs in Chapter 6, which provides a much fuller discussion of the LIA and MWP. [Lenny Bernstein]	Taken into account – section goes to Ch 6
4-617	A	23:35	23:57	Section 4.5.4: Please cross check with chapter 6 for consistency. Why should changes around the world be synchronous? We know they are not today as atmospheric waves play a major role, and the way this is written up is misleading by assuming this. [Kevin Trenberth]	Taken into account – section goes to Ch 6
4-618	A	23:35		Little Ice Age - needs cross-reference to Chapter 6 [Melanie Fitzpatrick]	Taken into account – section goes to Ch 6
4-619	A	23:35		It would be useful if you could check whether this section is consistent with what is said about the LIA in Box 6.3 and the MWP in Box 6.4. In my comments on chapter 6 I will suggest that they should omit discussion of glacier fluctuations associated with the LIA since it is covered by you. [Jonathan Gregory]	Taken into account – section goes to Ch 6
4-620	A	23:37	23:57	This whole section needs to be reconciled with Ch 6. You have the LIA starting before the MWP ends according to Ch 6. I think it is better if all the time you just refer to calendar years for all presumed warm or cold periods in the past. Ch 3 refers to all periods by years, even the so-called early 20th century warm episode. This section also refers ro glaciers so should be reconciled with 4.5.2. [Philip Jones]	Taken into account – section goes to Ch 6
4-621	A	23:37	23:46	Work by Steve Porter should be referenced here [Michelle Koutnik]	Taken into account – section goes to Ch 6
4-622	A	23:38	23:38	Much of the Canadian Rockies are in British Columbia, so this sentence reads somewhat	Taken into account – section goes to Ch

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				strangely. Suggest changing "…from Alaska, British Columbia, Canadian Rockies, and Patagonia Luckman" to "…from Alaska, coastal British Columbia, the Canadian Rockies, and Patagonia, Luckman..." [Thomas James]	6
4-623	A	23:40	23:40	Use of the term 'Little Ice Age' is rather controversial. It is emotive and was originally invented based on largely anecdotal evidence. However it is a useful term to describe a particular period with specific regional (if not global) climate attributes. I would recommend that it is placed in inverted commas in the text and "so called" or something similar added before it, to emphasize it is a colloquial term e.g. (like the so called "Little Ice Age", LIA, 13th-19th Century) [Gareth S. Jones]	Taken into account – section goes to Ch 6
4-624	A	23:44	23:44	I did not find the definition of the accronym ELA. [Philippe Tulkens]	Taken into account – section goes to Ch. 6
4-625	A	23:48	23:48	Start with: "Detailed information on length and mass balance changes of mountain glaciers during the past millennia is available in exeptional cases only (Haeberli and Holzhauser 2003). The overall evidence from glacier fluctuations during the beginning of ...". Reference: Haeberli, W. and Holzhauser, H., (2003): Alpine glacier mass changes during the past two millennia. Pages News, 1/11, 13-15. [Wilfried Haeberli]	Taken into account – section goes to Ch. 6
4-626	A	23:48	23:57	Section 4.5.4: Medieval Warm Period assessment of glaciers with regard to climate change and recent trends is scarcely developed - delete? [Ola M. Johannessen]	Taken into account – section goes to Ch. 6
4-627	A	23:48	23:49	Use of the term 'Medieval Warm Period' is rather controversial. It is emotive and was originally invented based on largely anecdotal evidence. However it is a useful term to describe a particular period with specific regional (if not global) climate attributes. I would recommend that it is placed in inverted commas in the text and "so called" or something similar added before it, to emphasize it is a colloquial term. e.g. (the so called "Medieval Warm Period", MWP, 10th -13th Century) [Gareth S. Jones]	Taken into account – section goes to Ch. 6
4-628	A	23:48	23:57	Did Alaska participate in the little Ice Age? If so, this is interesting since the continental U. S. appeared not to, at least according to the proxy records. [Jerry Mahlman]	Taken into account – section goes to Ch. 6
4-629	A	23:52	23:52	lake sediments evidences" should be "lake sediment evidence [Thomas James]	Taken into account – section goes to Ch. 6
4-630	A	23:56	23:57	Sentence starting "Despite the low…" is strangely written and difficult to understand.	Taken into account – section goes to

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				[Neville Nicholls]	Ch. 6
4-631	A	24:13	24:14	I would suggest to modify this sentence at the later part: ……..Andes (Kaser et al., 2003), Himalaya, and semi arid areas in central Eurasia (Sakai, et al., submitted). Information of the literature: Sakai, A., Fujita, K., Duan, K., Pu, J., Nakawo, M. and Yao, T. submitted to J. Glaciol.: Five decades of shrinkage of July 1st Glacier, Qilian Mountains, China. [Masayoshi NAKAWO]	Taken into account – text modified, reference not yet available
4-632	A	24:18	24:26	I'm wondering a little if this section (4.5.5.2) would belong better in WG2 - impacts and adaptations? [Thomas James]	Noted – will try to submit to WG2
4-633	A	24:19	24:19	Start with: "An overview of glacier-related hazards is provided by Haeberli and Burn (2002)". (Reference already contained in the reference list) [Wilfried Haeberli]	Rejected. The paper has a short paragraph summarizing the processes related to glacier lake outbursts and is not appropriate for the assessment of observations.
4-634	A	24:22	24:22	The acronym "GLOFs" is superfluous since it is never again used [Garry CLARKE]	Accepted
4-635	A	24:22	24:22	GLOF abbreviation is not needed, as it's not used elsewhere in the chapter. [Thomas James]	Accepted
4-636	A	24:22	24:22	Abrievation 'GLOFs' not required as is not used later [Jeff Ridley]	Accepted
4-637	A	24:25	24:26	This is a bit of a run-on sentence. If this section (4.5.5.2) is retained, suggest changing the part "….and 24 in Bhutan (Yamada, 1998), several…..vigilance." to "and 24 in Bhutan (Yamada, 1998). As well, several glaciers in Peru require monitoring." [Thomas James]	Accepted
4-638	A	24:25	24:26	I would suggest to add the following sentence at the end of Line 26. "A computer simulation (Naito et al, 2000) suggests that the lake formation is prgressed at the present climate condition, and the risk is considered increase with the warming climate. Information of the liertature: Naito, N., Nakawo, M., Kadota, T., and Raymond, C. F. (2000): Numerical simulation of recent shrinkage of Khumbu Glacier, Nepal Himalayas. IAHS Publication, 264, 245-254. [Masayoshi NAKAWO]	Rejected – this would go beyond "observations"
4-639	A	24:26	24:26	Add: "… recommending vigilance. Modern concepts for hazard recognition and assessments using remote sensing and geoinformatics have recently been developed by Huggel et al., 2003, 2004)." (References: Huggel, C., Kääb, A., Haeberli, W. and Krummenacher, B. (2003): Regional-scale GIS-models for assessment of hazards from glacier lake outbursts: evaluation and application in the Swiss Alps. Natural Hazards and	Rejected – this goes beyond "observations"

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				Earth System Sciences, 3 (6), 647-662. Huggel, C., Haeberli, W., Kääb, A., Bieri, D. and Richardson, S. (2004): An assessment procedure for glacial hazards in the Swiss Alps. Canadian Geotechnical Journal, 41, 1068-1083. [Wilfried Haeberli]	
4-640	A	24:26	24:26	If it's known that glaciers in Peru need monitoring, what about the Patagonian ice fields? Should the statement be changed to be somewhat more general? [Thomas James]	Rejected – they do not form hazardous lakes
4-641	A	24:28	32:50	Group the climate change information about Greenland together and the same for Antarctica. For instance, the information about the Jakobshavn Isbrae appears in several places. Most scientists think of climate change impacts on regions, not on components of glaciers (e.g. ice shelves, ice sheets, etc.) [Anne Nolin]	Accepted. Text modified.
4-642	A	24:28		Section 4.6. I find this section is somewhat confusingly arranged, because the division into "mass balance" and "causes of changes" is not clear and leads to some repetition of ideas, as well as alternation between ice sheets and ice shelves. I suggest that you could call 4.6 "Mass balance and stability of ice sheets and ice shelves", with background as preamble, then subsections 4.6.1 long-term (currently 4.6.3.2 - though maybe this subsection would be better in chapter 6 - see later comment), 4.6.2 techniques (currently 4.6.2.1), 4.6.3 mass balance of ice sheets (4.6.2.2 and 4.6.3.1), 4.6.4 mass balance of ice shelves (4.6.2.3 and 4.6.3.4), 4.6.5 recent ice shelf collapse and ice flow acceleration (4.6.3.5 and 4.6.3.3), 4.6.6 issues in modelling of balance changes (which would follow quite nicely from 4.6.5). I suspect that where subsections are combined in this way, some shortenings could be made. [Jonathan Gregory]	Noted. Text extensively modified for clarity and brevity.
4-643	A	24:28		Section 4.6: Substantial and generally well written, but ensure that the order of treatment of Greenland and Antarctica is consistent within each sub-section and sub-topic, and make sure that the assessment comes through all of the detail. [Ola M. Johannessen]	Noted.
4-644	A	24:28		No mention of observed changes to the area of the ablation zone in Greenland made by SSM/I and associated runoff estimates. [Jeff Ridley]	Accepted. Additional comment added.
4-645	A	24:32	24:32	Please check consistency of "over 60 m" with Table 4.1.1. [Jonathan Gregory]	Accepted.
4-646	A	24:32	24:33	Now, this is truly a gratuitious reference. Even high school students have learned this 60 meters of global sea-level rise from the melting ice caps number! [Jerry Mahlman]	Noted.
4-647	A	24:32		.. raise of sea level over 60 m.	Noted; discussion improved.

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				Does this account for isostatic adjustment of the ice floor and the land masses? [Konrad Steffan]	
4-648	A	24:36		Greenland and Antarctica ice tongues …., few terminate on land. What is the percentage? In certain parts of Greenland most glaciers terminate on land. [Konrad Steffan]	Noted.
4-649	A	24:37		typo, run ground on. Remove "a" [Kenichi Matsuoka]	Accepted.
4-650	A	24:40	24:41	Is this statement really correct as to model the response of ice sheets accurately one needs to model the formation of ice streams and the response of grounding lines to forcing etc. Am not sure that such a strong statement can be made see eg Vieli, A., and A. J. Payne (2005), Assessing the ability of numerical ice sheet models to simulate grounding line migration, J. Geophys. Res., 110, F01003, doi:10.1029/2004JF000202. [William Hare]	Noted; text clarified.
4-651	A	24:40	24:40	of the slow-moving" change to "of slow-moving [Thomas James]	Noted,; text revised.
4-652	A	24:42		TAR is not clear for me. Many readers just scan the report and such jergon (maybe) should be avoided. [Kenichi Matsuoka]	Accepted. Text revised.
4-653	A	24:49	24:50	Provide a sign post for reader to relevant section of WGII report by editing this sentence as follows: "The current state of balance is discussed here, with consideration of possible future changes deferred until Chapter 10 of WGI and Chapter 19 of WGII. [Michael Oppenheimer]	Accepted.
4-654	A	24:50		TAR again. What is it? [Kenichi Matsuoka]	Accepted. Text revised.
4-655	A	24:52		Section 4.6.2.1 is good. It appears to be about ice sheets but not ice shelves. [Jonathan Gregory]	Noted.
4-656	A	24:52		Section 4.6.2.1 COMME NT: This section does not refer to the techniques used for estimating ice sheet mass changes based on gravitational anomalies and the uncertainties in these compared to the more traditional methods described. For completeness this seems essential See also comment on S 4.6.2.2 . Recent literature includes eg Nakada, M. and J. Okuno (2003). "Perturbations of the Earth's rotation and their implications for the present-day mass balance of both polar ice caps." Geophysical Journal International 152(1): 124-138. , Tosi, N., R. Sabadini, et al. (2005). "Simultaneous inversion for the Earth's mantle viscosity and ice mass imbalance in Antarctica and Greenland." J. Geophys. Res. 110(B7): 1-14., Velicogna, I. and J. Wahr (2005). "Greenland mass	Accepted; text added.

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				balance from GRACE." Geophys. Res. Lett. 32(18): 1-4. [William Hare]	
4-657	A	24:52		section 4.6.2 does not include ice-penetrating radar, which recently used over the Antarctica and greenland o see very shallow (<10 m) isochrones. [Kenichi Matsuoka]	Noted; discussion improved.
4-658	A	24:53	25:8	This paragraph fails to make the central point, which is that "mean net annual surface mass balance", (to give it its full title!) is actually very hard to measure, which sufficient spatial and temporal sampling to make it representative, and so the errors on our estimates of regional means is not going down. Lots of papers recently have shown that snow accumulation is highly spatially-variable, and actually making point measurements and assuming they are representative of regions is potentially mis-leading. The only way forward with this, will in my mind be the use of ice-cores tied to radar surveys for layering. [David Vaughan]	Accepted, text updated.
4-659	A	24:55		explain this point here: altimetry gives a good estimate of volume change, because surface snow density does not change with time so much. [Kenichi Matsuoka]	Noted; discussion of changes in surface snow density included.
4-660	A	25:0	26:	4.6.2.2 This section recites results but does not carry out an assessment. [Kevin Trenberth]	Noted. Attempt to improve assessment made in revised draft.
4-661	A	25:1		The accumulation map of Giovinetto and Zwally, which is the best available (opinion also expressed by Charlie Bentley) is not cited. It should be cited because this is the only one that incorporates glaciological knowledge in the data interpolation. [Eric Rignot]	Noted; updated citations for surface mass balance added.
4-662	A	25:2		interval"s" [Kenichi Matsuoka]	Noted; text reworded to avoid ambiguity.
4-663	A	25:7		Recent work worth citing in this context is Krinner, G., O. Magand, I. Simmonds, C. Genthon and J.-L. Dufresne, 2006: Simulated Antarctic precipitation and surface mass balance of the end of the 20th and 21st centuries. Climate Dynamics, (submitted). [Ian Simmonds]	Noted; additional work published since FOD now cited.
4-664	A	25:10	25:10	No clear statement of how depth dependence of *horizontal* velocity estimated. Do we need to mention seismic techniques for depth measuremetn? [Richard Hindmarsh]	Accepted; text clarified.
4-665	A	25:10		depth variaiton of the speed is too much detail, I think. Why don't you use just "depth-averaged speed" and explain that the surface speed is roughly 90% of depth-averaged speed (90% comes from non-sliding assumption)	Noted; text clarified.

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				[Kenichi Matsuoka]	
4-666	A	25:11	25:11	remove comma after "radar" [Thomas James]	Noted; text changes in response to other comments removed this particular wording.
4-667	A	25:11		Grounding line is a reasonable out-flow gate, because once ice becomes afloat, it is already contributed to rise sea level. I like to mention this concept here. [Kenichi Matsuoka]	Accepted; text added.
4-668	A	25:13		Ian Joughin et al. 2002 is not the most representative reference on InSAR observations of glaciers. [Eric Rignot]	Noted; referencing updated to reflect most-recent papers published since FOD.
4-669	A	25:14		should quote the digital elevation models of Bamber, which are much better in quality that the RAMP dem of Liu et al. 1999, which is a well stiched DEM with lots of stiches. [Eric Rignot]	Accepted.
4-670	A	25:14		What reference demonstrates that basal melting of grounded ice is an important part of the glacier mass budget? I would think on the contrary that this is only contributing less than a few percent of the total ice discharge. [Eric Rignot]	Accepted; text modified.
4-671	A	25:17	25:17	replace 'not widely "ground truthed"' with 'poorly validated' [Jeff Ridley]	Taken into account; this text did not appear at this point in the document (typographical error on comment), but was clarified where it appears.
4-672	A	25:20		fast"-flow" glacier [Kenichi Matsuoka]	Taken into account, in rewording for clarity.
4-673	A	25:27		the work of Wingham et al. Science 1998 should be cited. [Eric Rignot]	Noted; most-recent papers published since FOD added.
4-674	A	25:27		Thomas et al in press is not in the reference. Thomas et al. 2004 would be appropriate [Eric Rignot]	Taken into account; references updated and edited for consistency.
4-675	A	25:27		The chapter misses the results from GRACE in Greenland published by Velicogna and Wahr (2005). This milestone paper is the first demonstration of the usefulness of gravity measurements for estimating the mass balance of an ice sheet. Their result is larger than the value derived from partial surveys of the Greenland ice sheet with laser altimetry. It should not be ignored. GRACE may be the best technique for measuring the mass balance of an ice sheet in coming years. [Eric Rignot]	Accepted; reference and text added.
4-676	A	25:28	25:29	The phrase "Gravity data can contribute to correction for isostatic changes in bedrock elevation" should be replaced with a new sentence. Surface gravity measurements on	Taken into account. Additional text on space-geodetic measurements and their

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				bedrock of sufficient accuracy to measure glacio-isostatic uplift are extremely demanding to make and require substantial correction for other mass change effects (especially in an ice-covered region!). Similarly, if this phrase is referring to satellite gravity (e.g., GRACE), the integrated mass change (from both bedrock uplift and ice mass change) will be measured and job will be to to separate the two effects. Instead, I would suggest replacing the phrase with the sentence "Space geodetic (Global Positioning System) observations have the potential to provide corrections for isostatic changes in bedrock elevation (e.g., Raymond et al., 2004), but have not yet been widely deployed in the Antarctic interior." This sentence would be followed by "Field data and models can contribute to corrections for density changes (Cuffey, 2001)." as originally given in the sentence. The new reference is "Raymond, C.A., E.R. Ivins, M.B. Heflin, and T.S. James, Quasi-continuous global positioning system measurements of glacial isostatic deformation in the Northern Transantarctic Mountains, Global and Planetary Change 42, 295-303, 2004." [Thomas James]	contributions to determining mass balance added.
4-677	A	25:32	25:32	suggest changing "argued" to "suggested" [Thomas James]	Taken into account in text revision.
4-678	A	25:33		A nice reference which might be included here. Bentley, C. R. and J. M. Wahr (1998). "Satellite gravity and the mass balance of the Antarctic ice sheet." Journal of Glaciology 44(147): 207-213. [Kenichi Matsuoka]	Noted. Indeed, a nice reference, but the rather strict limits on reference numbers preclude including all nice references.
4-679	A	25:35	25:57	The measured balance of the Greenland ice sheet is based on the estimated ice flow output (Reeh et al., 1999; Bauer 1962; 1968; and Weidick 1984). Given the large uncertainty of these estimates, it would be appropriate to quote that the mass balance of Greenland is not known. What is the estimated uncertainty of the ice flow output? Do we even know the sign of the mass balance given this uncertainty? [Konrad Steffan]	Taken into account in updated assessment including new papers published since FOD.
4-680	A	25:35	26:51	Please, discuss recent significant changes in Central Asian glaciation (possible citations: 1. Aizen, V.B., E. Aizen, K.J.Kreutz, K.Fujita, L.D. Cecil, S.A. Nikitin. 2004: Approaches for Ice-Core Climatic Reconstruction in Central Asia. KluwerAcademic Publishing, in Book: Editors: L.D. Cecil, L.G. Thompson and J.R. Green. “Earth Paleoenvironments: Records Preserved in Mid- and Low Latitude Glaciers. Developments in Paleoenvironmental Research (Editors: L.Cecil. L.Thomson, G. Green), Volume 9, 2004, 248 pp..; and 2. Khromova et al. (2003, publication in GRL). [Pavel Groisman]	Noted; this comment is out of place, as this section deals only with ice sheets, not mountain glaciers.
4-681	A	25:35		Section 4.6.2.2. It might be helpful to have subsections on Greenland and Antarctica. [Jonathan Gregory]	Taken into account in reorganization for succinctness and clarity.

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4-682	A	25:35		Section 4.6.2.2 COMMENT: The estimates from gravitational anomalies need to be included in this discussion. The Greenland estimates are close to those made by more traditional means and the AIS mass balance is somewhat larger. See for example Nakada, M. and J. Okuno (2003). "Perturbations of the Earth's rotation and their implications for the present-day mass balance of both polar ice caps." Geophysical Journal International 152(1): 124-138. , Tosi, N., R. Sabadini, et al. (2005). "Simultaneous inversion for the Earth's mantle viscosity and ice mass imbalance in Antarctica and Greenland." J. Geophys. Res. 110(B7): 1-14., Velicogna, I. and J. Wahr (2005). "Greenland mass balance from GRACE." Geophys. Res. Lett. 32(18): 1-4. [William Hare]	Taken into account; Veliconga and Wahr and other references added.
4-683	A	25:35		most of Section 4.6.2.2 discuss Greenland ice sheet, although the title says "ice sheets". I suggest to subdivide into Greenland Ice Sheet and Antarctic Ice Sheet so that little knowledge on Antarctic ice sheet will be emphasized. [Kenichi Matsuoka]	Taken into account in reorganization for succinctness and clarity.
4-684	A	25:36	25:51	The whole paragraph has to be rewritten considering the satellite data observations (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356). Older Krabil's data are spotty in time and space and not confirm by satellite observations of the whole ice sheet. Also modeling results of Box et al are in disagreemnet with satellite observations suggesting that models are not yet good enough to reproduce the current state. [Petr Chylek]	Noted; results of Johannessen cited, and included in full discussion. Box et al compared to Hanna et al.
4-685	A	25:37	25:37	In the references list, Alley et al (2005a) is quoted as submitted [Anny CAZENAVE]	Accepted and updated.
4-686	A	25:37	25:37	Surely there must be an upper bound on the Greenland mass loss from observations. For the sea level budget, we need a range. [Jonathan Gregory]	Taken into account; estimates provided.
4-687	A	25:37	25:37	The ratio of SLR mm to ice mass here and on line 48 are not consistent with mass but with ice volume. [William Hare]	Taken into account; units standardized throughout discussion.
4-688	A	25:37	25:37	Alley, et al (2005a) is no longer "in press", the reference is: ; needs changing in references section as well [Michelle Koutnik]	Accepted.
4-689	A	25:38	25:39	New results from satellite data (1992-2003) disagree with the fact that Greenland ice-sheet is near balance at high elevations (Ola M. Johannessen, Kirill Khvorostovsky, Martin W. Miles, and Leonid P. Bobylev, 2005: Recent Ice-Sheet Growth in the Interior of Greenland. Science. DOI: 10.1126/science.1115356). [Roxana Bojariu]	Noted; discussion by Johannessen and newer work by Zwally et al. Discussed.

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4-690	A	25:39	25:39	No longer up to date. Add a sentence such as: "However, a continuous dataset of Greenland Ice Sheet altimeter height from European Space Agency ERS-1 and ERS-2 satellites, 1992–2003, has been analyzed (Johannessen et al., 2005). Thickening of 6.4 ± 0.2 cm/year is found in the vast interior areas above 1500 m, in contrast to the earlier reports of high-elevation balance." [Ola M. Johannessen]	Noted; discussion is included, as is discussion of Zwally et al obtaining smaller inland thickening using updated analysis of essentially the same data set.
4-691	A	25:43		This section will have to be revised based on the ice discharge results of Rignot and Kanagaratnam submitted for publication in Science and presently in review. Their paper shows that the mass losses are dominated by ice dynamics. About 2/3 of the loss is from ice dynamics, 1/3 from melt. The new estimates of mass loss are much higher than published by Krabill et al. 2004. [Eric Rignot]	Accepted; text and reference added.
4-692	A	25:43		Box et al. used the calf-ice production from Reeh et al. 1999 which itself is based on an old compilation from Reeh, 1985 which assumed an ice sheet in a state of mass balance. This estimation of the mass balance of the Greenland Ice Sheet is a disguise of the true mass balance of the ice sheet. Ice discharge is missing and yet is essential. This does not disminish the value of Box et al's work but the limitation of their estimate ought to be clearly pointed out. [Eric Rignot]	Accepted, and is clearly pointed out in text.
4-693	A	25:43		Table 11.5 of TAR is based on old data. None of them include quality estimate of ice discharge. The ice discharge estimates of Rignot and Kanagaratnam are in fact the first of its kind. As a result, the conclusions of this entire section of chapter 4 are no better than those of TAR. There are actually wrong. The work of Hanna et al. and Box et al ONLY addresses runoff and accumulation, it does not address ice discharge, which is the most important factor! [Eric Rignot]	Taken into account; updated estimates from Rignot and Kanagaratnam; from Zwally et al., and from Velicet al., all accepted since FOD, added to discussion.
4-694	A	25:43		Are four references needed from Box to state that similar conclusion were reached? [Konrad Steffan]	Accepted; number of references reduced.
4-695	A	25:47	25:47	The estimate cited from Reeh (1999) for the ice flow output includes basal melting and ice berg calving. As far as can be determined the latter is a derived balance term assuming the ice sheet is in mass balance. (see eg Reeh N., Calving from Greenland glaciers: Observations, balance estimates of calving rates, calving laws, in Workshop on the calving rate of West Greenland glaciers in response to climate change, 13-15 September 1993, Copenhagen Denmark, edited by N. Reeh, pp. 85-102, Danish Polar Center, 1994.) Bigg made estimates based on observations Bigg, G. (1999). "An estimate of the flux of iceberg calving from Greenland." Arctic, Antarctic, and alpine research 31(2): 174-178. which are	Taken into account in updated assessment.

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				used by Mote et al 2003 as an upper and lower bound (170-270 Gt/yr) [William Hare]	
4-696	A	25:50	25:50	How many extra Gt and mm yr-1 SLE would that doubling add? [Jonathan Gregory]	Taken into account in updated text including new papers since FOD.
4-697	A	25:50	25:51	It took me some time of reading to infer that Jacobshavn Isbrae is not actually a human being after all! [Jerry Mahlman]	Taken into account.
4-698	A	26:0		It is not true that no agreement has been found for Antarctica. Rignot and Thomas, Science 2002 clearly states that West Antarctica is losing mass. So is Wingham et al. Science 1998. So is Davis et al. Science 2005. [Eric Rignot]	Taken into account in updated assessment. This statement apparently refers to line 26:14, which in turn discusses Antarctica as a whole, not just West Antarctica. Rewritten for clarity.
4-699	A	26:1	26:11	The measured balance of the Greenland ice sheet is based on the estimated ice flow output (Reeh et al., 1999; Bauer 1962; 1968; and Weidick 1984). Given the large uncertainty of these estimates, it would be appropriate to quote that the mass balance of Greenland is not known. What is the estimated uncertainty of the ice flow output? Do we even know the sign of the mass balance given this uncertainty? [Konrad Steffan]	Taken into account in updated discussion in text.
4-700	A	26:2	26:4	What is the expected reason for enhanced runoff following the major volcanic eruptions? [Takashi Yamanouchi]	Taken into account. The comment has the sign of the perturbation wrong (reduced runoff is stated after the volcanic eruptions), but text rewritten for clarity.
4-701	A	26:3	26:4	volcanic eruptions dates are incorrect (Mt Agung is 1963; El Chichon is 1982, Pinatubo is 1991) [Anny CAZENAVE]	Noted; the detail on dates of eruptions was removed from the revised text.
4-702	A	26:3	26:4	age of eruption should be given with AD [Kenichi Matsuoka]	Noted; the detail on dates of eruptions was removed from the revised text.
4-703	A	26:3	26:3	Correct the spelling of El Chichon. [Anne Nolin]	Noted; the detail on dates of eruptions was removed from the revised text.
4-704	A	26:4	26:6	Please could you compare these SMB numbers those in TAR Table 11.5 and comment on any differences. [Jonathan Gregory]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-705	A	26:5		increasing rather than "rising" [Neville Nicholls]	Taken into account; revision of the text has removed the detailed discussion

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					referenced.
4-706	A	26:6	26:6	New paragraph before "Using" perhaps? Maybe for the uninitiated reader it would be helpful to give formulae for how the various terms are combined in the ice sheet mass budget, as in the footnotes to TAR Tables 11.5 and 11.6. Please could you state the mass balance in SLE as well so chapter 5 can pick it up. For the sea level budget we would like to use periods 1961-1998 and 1993-2003. [Jonathan Gregory]	Taken into account; inter-chapter discussion in writing meeting led to modification of these requirements.
4-707	A	26:6	26:6	An average of the TAR numbers from Table 11.5 may not the best: The estimates of Weidick (1984) and Reeh (1999) which is based on Reeh (1984) were or appear to have been made assuming the ice sheet is in mass balance with the calving as being the residual term. The other estimates (Benson and Bauer) appear to be better based on observations. In addition there is the range from Bigg 1999 (170-270 Gt/yr). The "average" would not change much. Also there is ionly one estimate for basal melting. [William Hare]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-708	A	26:8	26:8	What is not statistically significant? [Jonathan Gregory]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-709	A	26:9	26:9	"Three highest…" should be "The three highest….") [Thomas James]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-710	A	26:9	26:11	A statement regarding the importance of climate variability and extremes could be added. [Sharon Smith]	Accepted.
4-711	A	26:11	26:11	See also Velicogna and Wahr, 2005: "Greenland mass balance from GRACE" GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L18505, doi:10.1029/2005GL023955 [David Parker]	Accepted; reference and text added.
4-712	A	26:11		What is "negative surface mass balance" - does this eman that no year exhibited a decline in mass of ice-sheet? [Neville Nicholls]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-713	A	26:13	26:18	The tone of this paragraph seems surprisingly disparaging of the data, compared with the situation for Greenland. Why is it so much less certain? [Jonathan Gregory]	Taken into account; text modified to better represent uncertainties.
4-714	A	26:13	26:17	Maybe appropriate to mention difference between plateau (SVP) and coastal accumulation styles, in contex of difficulties of estimating accumulation [Richard Hindmarsh]	Noted; additional discussion of accumulation added.
4-715	A	26:13	26:17	This paragraph is carefully and honestly stated.	Noted.

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				[Jerry Mahlman]	
4-716	A	26:13	26:17	No sentences on Antarctic Peninsula. [Kenichi Matsuoka]	Accepted, and text added on Antarctic Peninsula.
4-717	A	26:17		What does “ground-truth” mean here? [Eric Rignot]	Taken into account; wording changed.
4-718	A	26:17		There is the implication that the Davis et al, paper (and similar) are not sufficiently ground-truth and that there is uncertainty about the veracity of their results, is this what is mean to be said? [David Vaughan]	Taken into account; wording changed.
4-719	A	26:21	26:41	Please compare with the estimates in TAR Table 11.6 and comment on any differences. We need a range for the recent sea-level contribution from Antarctica. As you know, we had one in the TAR, and we adopted one of 0.0+-0.1 mm yr-1 (1 stdev) in chapter 5 for the sea level budget for 1993-2003, and 0.10+-0.15 for the 20th century. These ranges belong in chapter 4, to which chapter 5 should refer. For the sea level budget we would like to use periods 1961-1998 and 1993-2003. [Jonathan Gregory]	Taken into account; inter-chapter meeting at writing meeting has modified required budget numbers.
4-720	A	26:21	26:41	The study by Frezzotti et al on detail observation based estimates of SMB in a part of the EAIS is worth citing here as it adds a lot of new informatiom compared to the work of Vaughan cited. See in particular Table 2 which compares SMB estimates with those of Vaughan and that of Gioveneof Frezzotti, M., M. Pourchet, et al. (2004). New estimations of precipitation and surface sublimation in East Antarctica from snow accumulation measurements. Climate Dynamics. 23: 803-813. [William Hare]	Taken into account; additional recent references added.
4-721	A	26:21	26:41	Three comments 1) Splitting the para into two one on WAIS and one EAIS may help understanding and 2) Expansion of the points on the Davis study in brackets may help the reader 3) a comment on why the overall mass balance estimate here is so different from the gravitational anomaly inferences would complete the review. [William Hare]	Taken into account; revision of the text has removed the detailed discussion referenced.
4-722	A	26:21	26:41	Since Chapter 5 is citing this section as the source of its figures on Antarctic and Greenland contributions to recent sea level rise, I would like to sea those numbers derived explicitly. [David Vaughan]	Taken into account; text modified and updated to more clearly explain sources of figures.
4-723	A	26:21		Vaughan et al. 1999 acccumulation map is not the only accumulation map. Do not forget Giovinetto and Zwally’s. Rignot and Thomas 2002 and Bentley’s book chapter on mass balance published in 2004 clearly explain why the second one is more reliable. [Eric Rignot]	Taken into account; text updated to include more-recent references.

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4-724	A	26:21		The study of Jacobs et al. 1992 is way too old and out of date to be mentioned here. [Eric Rignot]	Accepted; reference eliminated
4-725	A	26:26	26:26	change "non-floating" to "grounded" [Thomas James]	Accepted; text changed.
4-726	A	26:28	26:28	change "most probable at" to "at" (the quoted uncertainty removes the need need to say "most probably") [Thomas James]	Taken into account; text updated.
4-727	A	26:29	26:29	change "losing mass" to "losing ice mass" [Thomas James]	Taken into account; text updated.
4-728	A	26:30	26:30	I would suggest that this part of the paragraph be broken out into a new paragraph. The present paragraph is very long as it presently stands. [Thomas James]	Taken into account in text redrafting.
4-729	A	26:30	26:37	This section is two run-on sentences (with one of them in brackets - why?). Suggested rewording: "ERS-1 and ERS-2 monitoring of the interior of Antarctica during the 1990's showed that much of East Antarctica thickened slightly (Davis et al., 2005; Vaughan, 2005; Figure 4.6.3). Elevation changes do not map directly to ice sheet mass change because the history of recent snow accumulation, and conversion of the snow to denser ice, affects the elevation history. With certain assumptions, however, the elevation change is equivalent to growth of about 45 Gt a-1, and contributes to sea-level fall of about 0.12 mm/yr (Davis et al., 2005). Owing to satellite coverage, these results do not include the area with 900 km of the south pole, nor are they reliable in regions of steep topography near the coast." [Thomas James]	Taken into account; text was rewritten for clarity and succinctness.
4-730	A	26:33	26:37	Here is the lead candidate for the prize presented to the person who has written the longest sentence in the 1644 pages of the WG1 IPCC Assessment's First Order Draft. Now, we need to identify the winning candidate to present him or her the well earned prize- an opportunity to read the entire sentence without breathing! [Jerry Mahlman]	Accepted; text modified.
4-731	A	26:38	26:41	Run-on sentence. Suggested rephrasing: "Reanalysis of ice input……indicates slight thickening (Joughin and Bamber, in press) that is consistent with the altimetry. An earlier analysis for this region indicated little change (Rignot and Thomas, 2002), and suggests a recent increase in the accumulation rate." [Thomas James]	Accepted; text modified.
4-732	A	26:40		The results of Joughin and Bamber on Foundation ice stream are in opposition to flux measurements made by Lambretch et al. 1999 which are based on in-situ data. The difference explains why Joughin and Bamber erroneously concluded on the thickening of this region.	Taken into account; discussion updated.

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				[Eric Rignot]	
4-733	A	26:43	26:43	Figure 4.6.3. Is this the *surface* mass balance? Is there a key for the size of the symbols? What period does it apply to? [Jonathan Gregory]	Taken into account; figures modified.
4-734	A	26:43		Fig 4.6.3: what is period for this? [Kevin Trenberth]	Taken into account; figures modified.
4-735	A	26:45	26:51	I suggest this belongs in 4.6.3.3, since it is about acceleration of grounded ice. [Jonathan Gregory]	Taken into account in rewriting.
4-736	A	26:45		Are these of concern because they make trend estimates unreliable, or some other reason? [Neville Nicholls]	Taken into account; text clarified.
4-737	A	26:47	26:48	The Siple Coast sentence is misleading because it implies that the slow-down of one ice stream caused the entire coast to thicken. Would it be correct to say that it caused the Siple Coast to "thicken overall"? [Thomas James]	Accepted; text rewritten for clarity.
4-738	A	26:49		The speed up of Pine Island and Thwaites was reported in Rignot et al. Ann. Glaciol 34, 2002. Joughin et al. 2003 only took the same data and added results from Rosanova and Scambos to extend the time period. Rignot et al. Ann. Glaciol. is the first report of the acceleration. A more recent summary of the evolution of these glaciers from 1975 to 2005 is in press by Rignot, in the Proc. Roy Soc. London, 2005. Acceleration has increased fivefold in the last 30 years. [Eric Rignot]	Noted; the difficulty of meeting the limited number of allowed references has required omission of many important papers.
4-739	A	26:49		While it is correct that the periods of observations are short, it is important to stress that accelerating glaciers have NOT been seen to slow down. Once pushed out of equilibrium they are not coming back. This is a major point! While we do not know how long these acceleration will live, we have not seen them die yet. [Eric Rignot]	Noted.
4-740	A	26:54	27:13	But why should we care about ice shelves? Because of their putative effect on ice sheets, and their impact on the oceans… [David Vaughan]	Noted.
4-741	A	26:57	26:57	Should this regional specific comparison 'as big as spain' be made just here? Stick with numbers. [Jeff Ridley]	Accepted.
4-742	A	27:0	48:	Section 4.6.3; Seems a bit repetitious and unduly long. [Kevin Trenberth]	Taken into account; text shortened and “tightened up”.
4-743	A	27:1	27:34	There seems to be a very interesting punchline here: We are still lacking an Antarctic punchline about monitoring human-induced climate change(in either direction!).	Taken into account in text revision.

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				[Jerry Mahlman]	
4-744	A	27:2	27:2	change "becomes afloat" to "starts to float" [Thomas James]	Accepted.
4-745	A	27:7		Summer melting is observed on Antarctic ice shelves as well. For instance Pine Island shelf, Fimbul ice shelf (pers. comm.. from norvegians), and Amery. If the authors are not convinced, I suggest a trip to these ice shelf, with long boots and a good strategy for rapid evacuation. [Eric Rignot]	Accepted; text on melting modified.
4-746	A	27:7		Along the Peninsula, major summer melting creates rivers of melt water discharge. Same on Fimbul ice shelf. Some of these rivers have been photographed by P. Skvarca. [Eric Rignot]	Accepted and text modified. (One at least of the authors has also photographed such features.)
4-747	A	27:9	27:11	Run-on sentence. Suggested rephrasing: "Basal freezing, if it occurs, typically takes place at slow rates, although the amount of frozen-on ice is significant towards the fronts of a few ice shelves. Basal melting is more usual and melting rates can be as large as several tens of meters per year." [Thomas James]	Taken into account; revision of text removed individual sentence cited.
4-748	A	27:9	27:9	Sublimation would cause thinning not thickening. [David Parker]	Accepted.
4-749	A	27:11	27:11	Reference for high basal melt rates? [Richard Hindmarsh]	Accepted; Shepherd et al. And other references added.
4-750	A	27:12	27:12	Replace "Terminology" with "The terminology". [Thomas James]	Taken into account; “terminology” removed in rewriting.
4-751	A	27:15	27:15	Figure 4.6.4 is very beautiful, but is it useful? [Jonathan Gregory]	Figures are redone.
4-752	A	27:23		This summary misses the important point that more than half of the ice that crosses the grounding line melts before it reaches the ice front. Forget the refreezing and ice-front melting, all the action is near the grounding line! [Eric Rignot]	Accepted; focus on basal melting sharpened.
4-753	A	27:25		Fig 4.6.5: not very readable. [Kevin Trenberth]	Figures are redone and improved.
4-754	A	27:29	27:29	After "thinning," insert "ice shelves" and then a period, not a comma. [Melinda Marquis]	Taken into account; detailed wording changed in rewriting.
4-755	A	27:30	27:34	There are suggestions that the break-up of Larsen-B can be attributed to warming. Is it appropriate to note this here? [Neville Nicholls]	Accepted and noted.

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4-756	A	27:31	27:34	This sentence is about acceleration of grounded ice. I suggest it belongs in 4.6.3.3. [Jonathan Gregory]	Taken into account; rewriting removed this particular sentence-section mismatch.
4-757	A	27:34		This is the paragrapf that also one of the most amazing topic is discussed. More sentences should be added to explain the results of the ice shelf breakup. [Takashi Yamanouchi]	Accepted.
4-758	A	27:41	27:41	The information about the glacier (fastest flowing, 7 km yr-1) isn't relevant here. [Jonathan Gregory]	Accepted. Information removed here.
4-759	A	27:48	27:51	Awkward sentence. Suggested rephrasing "Moreover, recent Antarctic Peninsual ice-shelf changes are much larger than any earlier changes that took place over the past few millennia (Pudsey and Evans, 2001; Brachfield et al., 2003). As well, recent changes to the Jakobshavn ice tongue are the largest over more than 40 years (Sohn et al., 1998). [Thomas James]	Taken into account; rewriting of section for clarity and succinctness removed this issue.
4-760	A	27:49	27:49	This sentence about Antarctic ice shelf changes would belong better at line 31, rather than here, since this paragraph is otherwise about Greenland. There's a related remark about collapse of Antarctic ice shelves in Section 6.3.2 (page 6-20 lines 21-22). I don't think it should be in chapter 6, because it's a subject of chapter 4, and I've said that in my comments on chapter 6. Wrt what chapter 6 writes, I would commen+G68t that we can't say the warming is anthropogenic, in fact, since (a) we haven't successfully simulated the Peninsula warming (b) it is only an inference, rather than measured, that oceanic warming is relevant in the Amundsen Sea. [Jonathan Gregory]	Taken into account; section rewritten for clarity.
4-761	A	27:49	27:50	Clarify statement: Which ice-shelf changes are unprecedented compared to what? [Melinda Marquis]	Accepted; “unprecedented” removed.
4-762	A	27:49		I note another paper of significance… Domack, E., D. Duran, A. Leventer, S. Ishman, S. Doane, S. McCallum, D. Amblas, J. Ring, R. Gilbert, and M. Prentice, Stability of the Larsen B ice shelf on the Antarctic Peninsula during the Holocene epoch, Nature, 436 (7051), 681-685, 2005. [David Vaughan]	Taken into account; referencing updated.
4-763	A	27:51		This chapter ought to report on the acceleration of Kangerdlussuaq, Helheim, southeast Greenland, and other west Greenland glaciers published by Rignot and Kanagaratnam, in review. [Eric Rignot]	Accepted; reference to Rignot and Kanagaratnam (which is now accepted but which was not available for FOD) has been added.
4-764	A	27:53	31:23	Section 4. 6. 3 is too long and discussions are complicated. Since this report is not a scientific original paper, discussions should be much simple and clear-cutt. [Takashi Yamanouchi]	Taken into account; section rewritten for clarity.

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4-765	A	27:55		Section 4.6.3.1. As suggested above, this could be combined with 4.6.2.2. Box and Hanna are mentioned in that subse+G79ction as well, for instance. [Jonathan Gregory]	Taken into account; section rewritten for clarity.
4-766	A	27:56	27:57	Box 2002 was published before the end of the claimed period 1991 to 2003, and also the ERA-40 does not go to 2003. [Kevin Trenberth]	Accepted; referencing simplified and updated.
4-767	A	28:0		there is widespread acceleration of glacier flow on areas south of 70 deg north in Greenland. See Rignot and Kanagaratnam 2005. [Eric Rignot]	Accepted; reference to Rignot and Kanagaratnam (which is now accepted but which was not available for FOD) has been added.
4-768	A	28:1	28:24	This material uses reanalyses but those are now known to have problems, see Chapter 3 for instance. A more critical assessment is needed. [Kevin Trenberth]	Noted; assessment added, but a detailed consideration of reanalysis products is beyond the scope of the chapter. The model results for many of the studies cited have been calibrated against surface data, which is now noted in text.
4-769	A	28:2	28:2	Not complete. After the sentence "North Atlantic Oscillation (NAO)", add a sentence such as: "Moreover, winter elevation changes derived from ERS-1/ERS-2 altimeter heights have shown to be linked to the NAO index (r ~ –0.88, lagged one month) during a study period 1992-2003 (Johannessen et al., 2005)." [Ola M. Johannessen]	Noted; the chapter interprets only multi-annual trends, so seasonal amplitudes are not discussed directly.
4-770	A	28:2	28:2	....North Atlantic Oscillation (Nesje et al., 2000). General warming.... [Atle Nesje]	Taken into account; text rewritten for clarity.
4-771	A	28:4	28:4	Do all these papers by Box need to be referenced? They are ALL referenced just a few paragraphs before as well. It disturbs the flow of the paragraph to see this multiple times. [Michelle Koutnik]	Accepted; referencing simplified.
4-772	A	28:5		Warming was not observed in 1930s. Maybe temperature change was? [Kevin Trenberth]	Noted.
4-773	A	28:10	28:12	This is a very useful punchline because it gives us some perspective about the levels of challenge that we face concerning Antarctic climate trends. [Jerry Mahlman]	Noted.
4-774	A	28:14	:24	Satellites also provide data on recent temperature (Comiso, J.C. 2000. Variability and trends in Antarctic surface temperatures from in situ and satellite infrared measurements. J. Clim 13, 1674-1696) or temperature-related (e.g. melt: Torinesi O. et al. 2003, Interannual variability and trend in the Antarctic ice sheet summer melting period from 20	Taken into account; text updated.

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				years of spaceborne microwave data, J. Clim. 16, 1047-1060.) trends [Christophe GENTHON]	
4-775	A	28:16	28:16	Change "Higher accumulation..." to "A higher …" [Thomas James]	Taken into account; detailed wording changed in rewriting.
4-776	A	28:17	28:17	brackets around "2002" [Thomas James]	Taken into account; detailed wording changed in rewriting.
4-777	A	28:24	28:24	change "across the Antarctic in" to "across Antarctica in" [Thomas James]	Taken into account; detailed wording changed in rewriting.
4-778	A	28:26		"past" is a bit vague. Does it mean glacial-interglacial? [Jonathan Gregory]	Accepted; text modified.
4-779	A	28:26		Section 4.6.3.2: This paragraph says that "we don't know much" about very long-term trends. Maybe just state this, and delete this subsection. [James Renwick]	Accepted; text shortened.
4-780	A	28:27	28:38	For sea level projections, we need a range for this ongoing dynamic response. As you know, we had one in the TAR. The TAR range used the same modelling results but also geological information such as discussed in Section 6.3.3 and Section 6.4.1 (page 6-24 line 33). Are there any recent geological references which could be cited, in addition to the modelling. Please could you confirm the TAR range or give a new assessment. Alternatively, since the information doesn't really come from observations of the cryosphere, perhaps it would be better to incorporate the material from Section 4.6.3.2 into chapter 6. [Jonathan Gregory]	Noted. A strong effort has been made to provide useful estimates.
4-781	A	28:27	28:38	I think that the evidence from outcrop exposure age-dating will prove key to answering this question and Stone has published what appeared to be compelling case for continued thinning of part of WAIS at around 4 cm per year. Stone, J.O., G.A. Balco, D.E. Sugden, M.W. Caffee, L.C. Sass III, S.G. Cowdery, and C. Siddoway, Holocene deglaciation of Marie Byrd Land, West Antarctica, Science, 299, 99-102, 2003. [David Vaughan]	Noted. The authors agree on the value of the data, but note that they do not provide an ice-sheet-wide assessment.
4-782	A	28:45	28:45	Comment about "slowdown" a bit lame - it is of interest because it highlights the problem of distinguishing between internal variability and external forcing [Richard Hindmarsh]	Taken into account; text rewritten.
4-783	A	28:50		add a semicolon between Dupont and Alley, 2005" "Payne et al." [Hartmut Grassl]	Accepted.
4-784	A	28:54	28:54	As Thomas (1979) is rather an old reference, I wonder whether the ideas are all covered by your later refs (Thomas, Payne, Dupont). [Jonathan Gregory]	Accepted. Ref. deleted.

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4-785	A	28:57	28:57	"advective-diffusive" is jargon [Richard Hindmarsh]	Accepted; wording changed.
4-786	A	29:0	30:	Section 4.6.3.4 seems unduly long. [Kevin Trenberth]	Accepted; shortened.
4-787	A	29:1	29:10	There are two issues here - the initial rate and the final response. Payne et al. and the other papers deal with former, but either don't compute final response or do not show large final response. [Richard Hindmarsh]	Taken into account; text clarified.
4-788	A	29:9		important for the flow" instead of: "important in the flow [Hartmut Grassl]	Accepted.
4-789	A	29:10		This paragraph seems to skirt round a central point which is that the models are not sufficiently well developed to really be a tool for prediction. Its not clear if stability of marine grounding lines is real, or simply a function of model design and this debate is far from over. [David Vaughan]	Taken into account; text rewritten for clarity.
4-790	A	29:12		The section on melting of ice shelves is well written but conspicuously out of scale (too long) [Garry CLARKE]	Accepted; text shortened.
4-791	A	29:12		Section 4.6.3.4. As suggested above, this could be combined with 4.6.2.3. I would suggest also that the combined material could be shortened considerably. At present there is more text on ice shelves than ice sheets, which may not correctly reflect their relative importance e.g. to sea level. [Jonathan Gregory]	Taken into account; text reorganized and shortened, but not identically as suggested by reviewer.
4-792	A	29:13	29:13	typo, missing "a" after "appear to be at least in part" [Michelle Koutnik]	Taken into account; “responses” should follow the phrase noted.
4-793	A	29:16	29:17	This is a quite useful assertion, and it gives some perspective from which to compare future studies on Antarctic Ice Sheet balances, or lack therof. [Jerry Mahlman]	Noted.
4-794	A	29:20	29:20	replace semi-colon with a full stop. [Thomas James]	Accepted.
4-795	A	29:20		The more impressive statistic from the Cook et al work is that 87% of tide-water glaciers are in retreat over the last 30 years [Alan Rodger]	Noted.
4-796	A	29:21	29:22	What isotherm is being referred to - the summertime warmest month? [Thomas James]	Accepted. Temperature is mean annual; text added
4-797	A	29:23	29:23	use the word 'years' rather than unit 'a'	Accepted.

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				[Jeff Ridley]	
4-798	A	29:23		'50 a' to '50 years' [Ian Simmonds]	Accepted.
4-799	A	29:26		Evidence indicates that the circulation changes themselves (rather than solely through their effect on regional temperatures) probably had a significant impact on the timing and nature of the Larsen disintegrations (Simmonds, I., 2003: Regional and large-scale influences on Antarctic Peninsula climate. Antarctic Peninsula Climate Variability: A Historical and Paleoenvironmental Perspective. AGU Antarctic Research Series, Volume 79. E. Domack, A. Leventer, A. Burnett, R. Bindschadler, P. Convey and M. Kirby, Eds., American Geophysical Union, 31-42.) [Ian Simmonds]	Noted.
4-800	A	29:27	29:27	Repetition of sentence [Rowan Fealy]	Accepted. Text revised.
4-801	A	29:27	29:29	This has already been said (page 27 line 31-34). I think it belongs in 4.6.3.3. [Jonathan Gregory]	Accepted. Text modofied.
4-802	A	29:31	29:33	Awkward sentence. Suggested rephrasing "Despite an increased ice supply from tributary glaciers, ice shelves on the Amundsen Sea coastline are observed to be thinning. This suggests that increased basal ice melting is responsible for the thinning (Shepherd et al., 2003; 2004) and focusses attention on sub-ice shelf oceanographic conditions." [Thomas James]	Accepted.
4-803	A	29:31	29:32	sentence needs some commas, should read: "Observed thinning of Amundsen Sea coast ice shelves, despite increased ice supply from tributary glaciers and in the absense of notable surface melting, implicates increased…" [Michelle Koutnik]	Taken into account.
4-804	A	29:32	29:32	Are not "implicates" and "implicating" (line 40) synonyms for "implies" and "implying" in these sentences? [Garry CLARKE]	Taken into account.
4-805	A	29:36		I don't believe that either Jacobs or Robertson reported ocean warming in the Amundsen/Bellingshausen sector, and so this is misleading. [David Vaughan]	Accepted; reworded.
4-806	A	29:39	29:39	air temperatures warmer than recently [David Parker]	Accepted.
4-807	A	29:41	29:41	I don't like the phrase "changes in the ocean's thermal structure, which is itself driven by the ocean circulation". This may be partly a grammatical problem but the "which" can only refer to the "ocean's thermal structure" and not to the "changes" (due to the singular). I would certainly not have thought that the "ocean's thermal structure" is "driven by the	Taken into account; reworded for clarity.

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				ocean circulation" (isn't it driven by the Sun?), while SOME changes in the structure are due to re-distribution of the temperature field by the ocean circulation. Basically, I don't see the point of this phrase -- it should probably be omitted. [John Hunter]	
4-808	A	29:42	29:57	The process described here may not be universal, with some areas being driven by supply of warm water, and others where the thermohaline circulation is driven internally. [David Vaughan]	Taken into account; reworded for clarity.
4-809	A	29:50	29:50	change "from grounding line to ice front" to "from the grounding line to the ice front" [Thomas James]	Taken into account; other rewriting removed this particular wording.
4-810	A	30:1	31:23	Although it is very interesting and informative to me, it seems that much of the discussions concerning Antarctica are avoiding the bottom-line story about Antarctica: It just does not seem to be participating co-operatively in helping us penetrate the global warming problem quantitatively. It just may have something to do with our, roughly speaking. "500-year bank" of high heat capacity in the Circum-Antarctic ocean. In climate models, this high heat capacity subworld makes it very challenging to equilibrate or initilize the climates of Antarctica and the Circum-Antarctic Ocean. In other words, it seems most appropriate to me that we admit the magnitude of the future challenges provided to us by this very interesting physical subsystem. [Jerry Mahlman]	Taken into account in rewriting.
4-811	A	30:27	30:27	Called Ronne-Filchner before rather than just Filchner here. [Jeff Ridley]	Accepted.
4-812	A	30:31	30:32	Is this sentence a correct sense of this process? Williams seems to argue that it is possible that the replacement of HSSW by warmer MCDW could happen relatively quickly see the discussion on page 2741 of the paper. If it is of relevance in this discussion a recent paper by Hemer argues that that there was a large retreat of the AIS in the mid Holocene Hemer, M. A. and P. T. Harris (2003). "Sediment core from beneath the Amery Ice Shelf, East Antarctica, suggests mid-Holocene ice-shelf retreat." Geology 31(2): 127-130. [William Hare]	Taken into account; text rewriting for clarity and succinctness has removed the detailed discussion referenced.
4-813	A	30:36	30:42	In this section it would be useful to cite the observed deep ocean warming trends in the region eg Robertson, R., M. Visbeck, et al. (2002). "Long-term temperature trends in the deep waters of the Weddell Sea." Deep Sea Research Part II: Topical Studies in Oceanography 49(21): 4791-4806. [William Hare]	Accepted.
4-814	A	30:40	30:42	drlete the brackets [Hartmut Grassl]	Accepted.
4-815	A	30:45	30:45	True HSSW is absent...' rephase as not standard English. [Jeff Ridley]	Accepted.

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4-816	A	31:0	32:	Section 4.6.4 also seems unduly long. [Kevin Trenberth]	Accepted.
4-817	A	31:1	31:2	Iceberg calving (as distinct from discharge of ice across the grounding line) isn't particularly important, is it? - perhaps it could be omitted from the subsection title and first sentence dropped. [Jonathan Gregory]	Taken into account. The implication that iceberg calving is unimportant is wrong in many cases; because of side-drag or basal-drag on pinning points, ice shelves provide back-stress on grounded ice, and loss of important parts can have great feedbacks on ice sheets. Text clarified.
4-818	A	31:1	31:11	This section fails to point out the climate related aspect of ice shelf retreat and collapse/break-up. And in this regard it would be good to point out the difference between "retreat" and the final stage "collapse". I believe we have argued strongly that climate-driven retreat on the AP has been going on for, many decades, and perhaps a century. In a few cases, notably Larsen A and B, this has culminated in a "collapse" that was at least in part driven by a dynamic repsonse. [David Vaughan]	Taken into account.
4-819	A	31:21	31:23	As there is little to say about this and it doesn't really belong with ice-shelf collapse, perhaps it could be omitted, or moved to an introductory section somewhere as background information. [Jonathan Gregory]	Accepted.
4-820	A	31:22	31:22	Remove the word "Only" [James Renwick]	Accepted. Text revised.
4-821	A	31:23	31:23	Include reference to: Gladstone RM, Bigg GR, Nicholls KW Iceberg trajectory modeling and meltwater injection in the Southern Ocean JOURNAL OF GEOPHYSICAL RESEARCH-OCEANS 106 (C9): 19903-19915 SEP 15 2001 [Jeff Ridley]	Noted. Removal of the preceding text (in response to point raised in 4-819) removed the need for this reference.
4-822	A	31:25	31:25	I would say these are issues for "modelling" of balance changes, rather than "projection" specifically. As you say, they relate to understanding of the relevant processes. [Jonathan Gregory]	Taken into account. The requirement of shortening the text has led to removal of some text and changes in headings.
4-823	A	31:25	32:49	A summary of the issues raised in this section should appear in this chapter's Executive Summary and in the higher level summaries that will be developed for this report. The potential for abrupt climate change and sea level rise because of ice sheet collapse has been widely discussed, but with little or no understanding of the difficulties involved in assessing ice sheet stability. Summary statements that explain these difficulties would be	Taken into account. Executive Summary updated.

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				of great use to policymakers in helping them understand the uncertainties involved in projection of ice sheet changes. It is important that both the level of uncertainty and the reasons for that uncertainty be stated. [Lenny Bernstein]	
4-824	A	31:25	32:49	There has been a large amount of disucssion of the potential for abrupt climate change and sea level rise because of ice sheet collapse, but little disucssion of the quality of the underlying science. This section makes it clear that it is very difficult ot assess ice sheet stability. A summary of this information shoudl appear in the Chapter's Executive Summary and in WG I's SPM to help policymakers undertand the uncertainties in projections of ice sheet changes. Both the level of uncertainty and its causes should be explained. [Jeffrey Kueter]	Taken into account. Executive Summary updated.
4-825	A	31:25		Section 4.6.4 COMMENT. This is a useful section but it does not seem complete and it does not give the reader a sense of the main issues in terms of what is more important. For example ice stream formation and behaviour and response to ice shelf collapse and or grounding line retreat is one of the main uncertainties and is not really capture in any of the subsections properly. For a useful review see ISMASS Committee . (2004). "Recommendations for the collection and synthesis of Antarctic Ice Sheet mass balance data." Global and Planetary Change 42(1-4): 1-15. [William Hare]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-826	A	31:31	31:31	Is it really correct that ice flow models work with "considerable accuracy" in the context of ice sheet modelling (as opposed to say valley glacier flow)? [William Hare]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-827	A	31:31	31:40	This para doesn't seem to fit its title. [David Vaughan]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-828	A	31:35	31:35	change "have order(s)-of-magnitude effects" to "have an orders-of-magnitude effect" [Thomas James]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-829	A	31:35	31:36	change "order(s)-of-magnitude" to "orders-of-magnitude" [Thomas James]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-830	A	31:43	31:53	I would add that the short-period (hours to weeks) fluctuations in ice-stream velocity are potentially confounding in our measurement of change, and need to be understood in greater depth to make the measurements of change valid. [David Vaughan]	Taken into account. Rewriting of the section has changed the details referenced in the comment.

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4-831	A	31:56	32:4	The purpose of this part is unclear to me. It is partly introductory, and partly overlaps with 4.6.4.1 perhaps. Again, it is not just "projection" which presents problems, but modelling in general. [Jonathan Gregory]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-832	A	32:0	38:	The organization of section 4.7 could be improved. This reviewer advocates putting some of the definitions toward the front of the material. Otherwise, the reader is left until the end to wonder about the definition and significance of some phenomena introduced introduced in earlier passages. For example, "thaw settlement" and "thermokarst" are defined at the very end of the section (p. 38, lines 8-14), but are used much earlier (p. 35, lines 24-32). [Frederick Nelson]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-833	A	32:0	38:	Three of the four figures pertaining to frozen ground appear to have come from the publications of one of the chapter's authors. This reviewer believes that integrative figures that incorporate data from a variety of sources would be superior. [Frederick Nelson]	Noted; this comment is not properly located, as the section is not on frozen ground.
4-834	A	32:0	38:	I have restricted my commentary to the section of Chapter 4 concerned with frozen ground, my own specialty. I have, however, examined the chapter as a whole. It constitutes a very large improvement over the zero-order draft. The document is in need of close editing to ensure consistency in writing, but this will of course be accomplished in time. The authors are to be commended for their hard work on what is sure to be a benchmark review when it reaches its final stages. [Frederick Nelson]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-835	A	32:5		Wwe all need to check out the new paper soon coming out in Science by Ken Miller (Rutgers - he told me two weeks ago that the paper was going to be published) - I have seen an early version of the paper and have talked to Ken - he makes an reasonable case that there WAS ice volume back into the time of the dinos - in the late Cretaceous (100 my) to Eocene period, he asserts ca. 30m sea level variations due to small south polar ice sheets. Ken is argueably the world's best longterm sea level person, so this must be taken seriously. [Jonathan Overpeck]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-836	A	32:7	32:9	One of the main issues in terms of the last deglaciation is to explain the large rates of sea level rise which are known to have occurred and which may be linked to ice sheet instabilites. A recent paper by Calov et al could be cited here as it adds additional information to the Huybrechts et al work in relation to this instability question: Calov, R., A. Ganopolski, et al. (2002). "Large-scale instabilities of the Laurentide ice sheet simulated in a fully coupled climate-system model." Geophysical Research Letters 29(24).	Taken into account. Rewriting of the section has changed the details referenced in the comment.

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				[William Hare]	
4-837	A	32:9	32:12	Other papers may be cited in support of a Greenland mass loss in this period of order 2-4.5 metres in a time frame of order a few centuries Stirling, C. H., T. M. Esat, et al. (1998). "Timing and duration of the Last Interglacial: evidence for a restricted interval of widespread coral reef growth." Earth and Planetary Science Letters 160(3-4): 745-762. and Lhomme, N., G. K. C. Clarke, et al. (2005). "Tracer transport in the Greenland Ice Sheet: constraints on ice cores and glacial history." Quaternary Science Reviews 24(1-2): 173-194. [William Hare]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-838	A	32:12	32:14	This sentence doesn't belong in chapter 4; it's a projection issue, for chapter 10 (which does go beyond the 21st century). [Jonathan Gregory]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-839	A	32:13	32:14	Restate this internally redundant sentence. How about? ... determine the longer-term (beyond the year 2100) behavior of the ice sheets. [Jerry Mahlman]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-840	A	32:16	32:16	This is a rather atypical discussion of glacial hydrology - for a start it should mention that it is very poorly understood, and it isn't understood except in very qultitative terms how it interacts with sliding. Most glaciologists would regard this para. as very micjh more speculative than the rest of the chapter [Richard Hindmarsh]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-841	A	32:16		use: "lubrication" instead of "Lubrication" [Hartmut Grassl]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-842	A	32:19	32:19	I had understood the link between drainage of surface meltwater and change in ice-flow to be circumstantial rather than demonstrated. [Jonathan Gregory]	Taken into account; the evidence is rather strong, but rewording has affected the presentation so that the comment is not as directly relevant as before.
4-843	A	32:20	32:21	The sentence about Parizek and Alley doesn't belong in chapter 4; it concerns projections (and is covered in chapter 10). [Jonathan Gregory]	Taken into account. We note that the statement does fall within the purview of Chapter 4, as discussed at an inter-chapter meeting, but that rewording of the section has changed the text so as to remove the possible difficulty.
4-844	A	32:20	32:20	Insert something like "of such increased ice-flow velocity" after "Inclusion."	Taken into account. Rewriting of the

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				[Melinda Marquis]	section has changed the details referenced in the comment.
4-845	A	32:22	32:22	Is there not uncertainty in some glaciologists' minds about *whether* water can drain through 1 km of cold ice? [Jonathan Gregory]	Taken into account. The simple answer is that there is strong evidence that this process occurs. However, rewording has changed the text so that the comment is not directly relevant.
4-846	A	32:29	29:32	stabilizing" instead of "stabilizin [Roxana Bojariu]	Accepted. Text revised.
4-847	A	32:29	32:29	Typo [Richard Hindmarsh]	Accepted. Text revised.
4-848	A	32:29	32:29	should be "stabilizing" [Thomas James]	Accepted. Text revised.
4-849	A	32:29	32:29	typo: "stabilizin" should be "stabilizing" [Michelle Koutnik]	Accepted. Text revised.
4-850	A	32:29	32:29	Possible stabilizing feedbacks. [Andrew Lacis]	Accepted. Text revised.
4-851	A	32:29	32:29	Fix spelling of "stabilizing" in subheading. [Melinda Marquis]	Accepted. Text revised.
4-852	A	32:29	32:29	..Stabilising feedbacks [Atle Nesje]	Accepted. Text revised.
4-853	A	32:29	32:29	Change "stabilizin" to "stabilizing" [Anne Nolin]	Accepted. Text revised.
4-854	A	32:29	32:29	Middle word in title should be "stabilizing" [James Renwick]	Accepted. Text revised.
4-855	A	32:29	32:29	a "g" is missing in "stabilizin" [Philippe Tulkens]	Accepted. Text revised.
4-856	A	32:29		correct to: "stabilizing" [Hartmut Grassl]	Accepted. Text revised.
4-857	A	32:29		Section 4.6.4.5 COMMENT This section seems too specific as the question of destabilizing feedbacks has not been discussed. There are also possible destabilizing feedbacks: Should these have a separate section too? Perhaps not as the same processes can give rise to self limiting events or to positive feedbacks depending on a variety of parameters. Given its content which is mainly on the ice shelf-ice stream relationship (and by implication also the effects of grounding line migration) it would be better to rename this as something like "ice shelves, grounding line migration and ice stream	Taken into account. Rewriting of the section has changed the details referenced in the comment.

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				response". The text could then be focused on papers in the literature which deal with this issue eg Vieli, A. and A. J. Payne (2005). "Assessing the ability of numerical ice sheet models to simulate grounding line migration." J. Geophys. Res. 110(F1): 1-18. [William Hare]	
4-858	A	32:29		4.6.4.5 Possible stabilizing feedbacks (repair of a simple typo) [Jerry Mahlman]	Accepted. Text revised.
4-859	A	32:29		stabilizing [Neville Nicholls]	Accepted. Text revised.
4-860	A	32:30	32:32	The way this is written presupposes that there are initially unstable positive feedbacks which are at some later point restrained by negative feedbacks. It is also possible that there is no instability in the first place, isn't it? That would be a more parsimonious explanation. [Jonathan Gregory]	Taken into account. Positive feedbacks are well-known and documented in the chapter. However, rewriting of the section has changed the details referenced in the comment.
4-861	A	32:30	32:31	suggest changing first sentence to "It should be kept in mind that despite the climate warming since the last ice age, which led to substantial shrinking of the Greenland and Antarctic ice sheets, the ice sheets still persist." [Thomas James]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-862	A	32:30	32:38	Stabilising feedbacks can be investigated through coupled ice sheet - climate models (eg. Lunt et al., 2005; Ridley et al., 2005).Lunt DJ, de Noblet-Ducoudre N, Charbit S Effects of a melted greenland ice sheet on climate, vegetation, and the cryosphere CLIMATE DYNAMICS 23 (7-8): 679-694 DEC 2004. Ridley JK, Huybrechts P, Gregory JM, et al. Elimination of the Greenland ice sheet in a high CO2 climate JOURNAL OF CLIMATE 18 (17): 3409-3427 SEP 1 2005 [Jeff Ridley]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-863	A	32:31	32:31	"they still persist" -- It is not clear what "they" matches to (presumably the ice sheets but from the way the sentence is written "warming" and "sea-level rise" are possible candidates as well) [Garry CLARKE]	Rejected. Grammatically, the pronoun refers to the immediately preceding noun. No ambiguity exists. We note, however, the rewriting of the chapter has removed the sentence.
4-864	A	32:40	32:40	Who calls it full-stress-tensor modelling? Something like "modelling with more complete mechanics" [Richard Hindmarsh]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-865	A	32:47	32:49	These sentences on stress tensor components are over-specialised. All that needs to be said is that it requires more computing time. [Richard Hindmarsh]	Taken into account. Rewriting of the section has changed the details referenced in the comment.

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4-866	A	32:48	32:48	suggest change "quite difficult" to "computationally intensive". I don't believe there are any significant theoretical or applied computational advances required to implement a full stress-tensor solution, simply the application of high-performance computational facilities. [Thomas James]	Taken into account. Rewriting of the section has changed the details referenced in the comment.
4-867	A	32:51		Within the permafrost section there is no mention made of sub-sea permafrost; it is not discussed in the text, nor is it included in Figures 4.1.1. and 4.8.1. This is aerially quite important, and of course the melt-out of shelf-bottom permafrost would have effects on gas release and potentially sea-bed erosion. The seasonality and mechanisms for sub-sea permafrost destruction would be very different from sub-aerial permafrost. [Robie Macdonald]	Accepted. A “Subsea Permafrost” section is added.
4-868	A	32:55	32:56	change text to "Frozen ground, in a broad sense, includes near-surface soil affected by short-term freeze/thaw cycles, seasonally frozen ground, and permafrost. The permafrost regions occupy approximately …" [Frederick Nelson]	Accepted. Text modified.
4-869	A	32:55	32:55	Near-surface soil freeze/thaw cycle -- does this refer to occaisional (short-term) frost on the ground? Some clarification is required to explain how this differs from seasonally frozen ground which also exists because of a freeze /thaw cycle. [Sharon Smith]	Accepted. Text modified.
4-870	A	33:0	33:	Section 4.7.2 ("Changes in Permafrost") needs a little more text explaining why and how permafrost is important. A great deal of space is taken up in subsequent sections detailing changes in permafrost temperature, but the section on p. 33 does little to prepare the reader for understanding why such changes may be important. [Frederick Nelson]	Noted. The importance of permafrost, or frozen ground in general, discussed in the background section 4.7.1.
4-871	A	33:0	35:	Section 4.7.2.2 - Since rates of temperature change are summarized in the table, perhaps the discussion could be cut down and present only broad regional trends. [Sharon Smith]	Accepted. Text modified.
4-872	A	33:2	33:2	Change the word "mass" to "area" [James Renwick]	Accepted. Text modified.
4-873	A	33:3	33:4	And, by implication, does this mean the most vulnerable part of the cryosphere as well? [Jerry Mahlman]	Accepted. Text revised
4-874	A	33:6	33:7	The 1st sentence is poorly written and it would be better to say: The presence of frozen ground depends on the ground temperature which is controlled by the surface energy balance. While the atmospheric climate is an important factor determining the distribution of frozen ground, local factors are also important such as vegetation conditions, snow cover, thermal properties and moisture conditions of the underlying materials. [Sharon Smith]	Accepted. Text modified.

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4-875	A	33:7	33:8	The link should be made between changes in temperature at the ground surface and the deeper temperatures. Deeper temperatures can provide a record of changes in temperature at the ground surface that may be related to climate change. [Sharon Smith]	Accepted. Text modified.
4-876	A	33:8	33:8	typo: "Mashall" should be "Marshall" [Michelle Koutnik]	Accepted. Text modified.
4-877	A	33:8		correct: "Marshall" [Hartmut Grassl]	Accepted. Text modified.
4-878	A	33:9	14:	need more coordination with chapter 6 - there is evidence that the WAIS also collapsed to some degree in the late Quaternary - most likely ca. 130,000 years ago. The important thing is to make sure the two chapters are in agreement. [Jonathan Overpeck]	Taken into account. The referenced line addresses permafrost, not WAIS, but an inter-chapter meeting was arranged with chapter 6 and others to improve coordination on WAIS.
4-879	A	33:10	33:10	hypen missing, "role in the land-surface energy…" [Michelle Koutnik]	Accepted. Text modified.
4-880	A	33:11	33:15	Changes in moisture fluxes, surface and subsurface hydrology (drainage) should also be mentioned -- these will have implications for vegetation and also carbon budgets. [Sharon Smith]	Accepted and text revised.
4-881	A	33:22	33:22	3.2 m is presumably a borehole depth but this is not clear in the context of the actual sentence [Garry CLARKE]	Accepted and will add more info.
4-882	A	33:25	33:26	delete: "China" and the comma before [Hartmut Grassl]	Accepted. Text modified.
4-883	A	33:26		... 1960s, while continuois permafrost ... [Jerry Mahlman]	Accepted. Text modified.
4-884	A	33:27	33:28	What is meant by deep temperatures. In Canada, most ground temperatures measured are at depths of 20 m and at a few sites temperatures down to depths of 50 to 100 m are measured. [Sharon Smith]	Accepted and text revised.
4-885	A	33:28	33:28	Smith et al (2005) does not appear in the reference list. Full reference will be provided with comments on reference section. [Sharon Smith]	Accepted
4-886	A	33:28		delete: "the" in "in the Europe" [Hartmut Grassl]	Accepted. Text modified.
4-887	A	33:31	33:35	This could be re-written as : The various monitoring programs such as those described above contribute to the permafrost thermal monitoring component of the Global	Accepted. Text modified.

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				Terrestrial Network for Permafrost (GTN-P). The GTN-P was initiated in the 1990s by the International Permafrost Association (IPA) to provide long-term observations of permafrost conditions in order to characterize their spatial and temporal variability (Brown et al. 2000; Burgess et al. 2000). Burgess et al. 2000 needs to be added to the reference list. [Sharon Smith]	
4-888	A	33:35	33:37	If the previous suggestion is used, this sentence can be removed and comments related to active layer monitoring can be added to the discussion on seasonally frozen ground. [Sharon Smith]	Accepted. Text modified.
4-889	A	33:37	33:39	It is more correct here to say that the thermal monitoring component (or Thermal State of Permafrost) will be further developed as part of IPA's contribution to the Internaional Polar Year. [Sharon Smith]	Accepted. Text revised.
4-890	A	33:37	33:37	Romanovsky et al 2000 is not in reference list. [Sharon Smith]	Accepted. Text revised.
4-891	A	33:38	33:39	It is the information that is generated from the monitoring programs that provide evidence of climate -induced changes not the programs themselves. The statement should be: Information generated from the GTN-P can be used to provide evidence of climate-induced changes. [Sharon Smith]	Accepted. Text revised.
4-892	A	33:41	33:52	What depth of measurement? [Jerry Mahlman]	Noted. We refer to the permafrost surface temperature as stated in the text.
4-893	A	33:42	33:43	? [Hartmut Grassl]	
4-894	A	33:42	34:47	Much of the indicated text mirrors the information in Table 4.7.1 and could be reduced substantially. The remaining text should concentrate on providing an integration of measurements at individual locations, with emphasis on explaining similarities and discrepancies and the factors lying behind them. As a review document, it is critically important for this section to present an integrated assessment of changes in permafrost temperature. [Frederick Nelson]	Accepted. Text revised.
4-895	A	33:43	33:45	It is important that the conclusions of Lachenbruch and Marshall are based on reconstructions of surface temperature rather than observations of permafrost temperature over a 100 year period. It is also important to note that this 50-100 year time period does not include the last 20 years as the work referred to was published in 1985 -- saying the last 50 to 100 years therefore is not a correct statement. The authors need to be careful	Accepted and text revised.

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				when discussing time periods and need to more specific. It would also be useful to add a comment about temperatures propagating down from the surface with deeper temperatures containing a record of past changes in temperature at the ground surface. [Sharon Smith]	
4-896	A	33:45		Insert "a" after "even" [Neville Nicholls]	Accepted. Text revised.
4-897	A	33:46	33:46	write "…borehole …" (not norehole) [Wilfried Haeberli]	Accepted. Text revised.
4-898	A	33:46	33:46	norehole" should be "borehole [James Renwick]	Accepted. Text revised.
4-899	A	33:46		change: "norehole" to "borehole" [Hartmut Grassl]	Accepted. Text revised.
4-900	A	33:48	33:49	Over what time period has this 2C increase occurred? [Sharon Smith]	Accepted. Text revised.
4-901	A	33:50	33:52	The reference to "increased snow cover" in Alaska seem inconsistent with statements made elsewhere (p. 10, lines 7-22) about declines and reductions. [Frederick Nelson]	Accepted. Text revised.
4-902	A	33:56	33:57	This statement should be: Data from the northern Makcenzie valley in the continuous permafrost zone indicates that permafrost temperature between depths of 20 and 30 metres has increased by about 1deg C in the 1990s (Smith et al. 2005) - note correction to publication year as well as depth of measurement and amount of change. [Sharon Smith]	Accepted. Text revised.
4-903	A	34:0		Table 4.7.1. Recent Trends in Permafrost Temperature. Table row 27: Site name typed wrong: Correct is: "Juvvasshøe, Southern Norway" [Ketil Isaksen]	Accepted. Text revised.
4-904	A	34:0		Table 4.7.1. Recent Trends in Permafrost Temperature. Table row 27: Table column "Period of Record": "Past 60–80 years" is wrong. Correct is: "Past 30–40 years" [Ketil Isaksen]	Accepted. Text revised.
4-905	A	34:0		Table 4.7.1. Recent Trends in Permafrost Temperature. Table row 27: Table column "Reference": "Isaksen et al., 2001" is missing in the reference list. Please include this - full reference: Isaksen K, Holmlund P, Sollid JL, Harris C. 2001. Three deep alpine-permafrost boreholes in Svalbard and Scandinavia. Permafrost and Periglacial Processes 12: 13-25. [Ketil Isaksen]	Accepted. Text revised.
4-906	A	34:0		Table 4.7.1. Recent Trends in Permafrost Temperature. Table row 28: Table column "Reference": "Isaksen et al., 2001" is wrong. Correct is "Isaksen et al., 2000". This	Accepted. Text revised.

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				reference is already included in the reference list. [Ketil Isaksen]	
4-907	A	34:3	34:3	The reference should be Smith et al. 2005 as it has been published [Sharon Smith]	Accepted. Text revised.
4-908	A	34:3	35:44	Too much detail here. [Sharon Smith]	Noted.
4-909	A	34:6	34:6	A sentence could be added which indicates that warming of permafrost in the Canadian eastern and high arctic has occurred later than that in the western arctic. [Sharon Smith]	Accepted. Text revised.
4-910	A	34:7		eastern Arctic" insert: "eastern Canadian Arctic [Hartmut Grassl]	Accepted. Text revised.
4-911	A	34:8	34:8	Reference is Smith et al 2005 [Sharon Smith]	Accepted. Text revised.
4-912	A	34:10	34:10	Allard et al. 2002 is not in reference list. [Sharon Smith]	Accepted. Text revised.
4-913	A	34:16	34:17	A bit of repetition [Sharon Smith]	Accepted. Text revised.
4-914	A	34:19		There are measurements made in Mongolian Region. Adding below may be appropriate. Sentence: The average thickness of active layer depth and mean annual temperature in Central Mongolia in the recent 30 years at depth 10-90 m increased 0.1-0.6cm and 0.05-0.15 deg. per decades, respectively. Similar or smaller values than Central Asia and European Territories (Sharkhuu, 2003) Reference: Sharkhuu N. (2003): Recent changes in the permafrost of Mongolia. Proceedings of the 8th International Conference on Permafrost, Zurich, Switzerland, 1029-1034. [Tetsuo Ohato]	Accepted and text revised.
4-915	A	34:21	34:21	Indicate time perod over which measurements made. [Sharon Smith]	Accepted. Text revised.
4-916	A	34:23	34:23	Wrong reference (Isaksen et al., 2000) --> Please use: Isaksen et al., 2004 (Full reference: Isaksen K, Sollid JL, Humlum O, Harris C. 2004. Evidence of significant secular and recent warming of permafrost on Svalbard. Abstract S4-10, Bjerknes Centenary 2004: Climate Change in High Latitudes, 1-3 September, Bergen, Norway) [Ketil Isaksen]	Accepted. Text revised.
4-917	A	34:24	34:24	Site name missing, please include "Southern Norway" --> ground temperature monitoring in Juvvasshøe, Southern Norway, indicate that the…	Accepted. Text revised.

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				[Ketil Isaksen]	
4-918	A	34:25	34:25	Better to say between 1999 and ?? [Sharon Smith]	Accepted and text revised.
4-919	A	34:31	34:33	Comments on snow are repetitive as this will be similar for all regions. Can influence of snow cover be summarized in a sentence some where? [Sharon Smith]	Accepted. Text revised.
4-920	A	34:35	34:44	The sentences are monotonously structured, each beginning with "Permafrost temperature(s)" [Garry CLARKE]	Accepted. Text revised.
4-921	A	34:47		correct: "Lanchenbruch" to "Lachenbruch" [Hartmut Grassl]	Accepted. Text modified.
4-922	A	35:0	35:	Table 4.7.1 - data for southern Mackenzie valley also comes from Smith et al 2005. Also note that Iqaluit is spelled wrong. [Sharon Smith]	Accepted. Text modified.
4-923	A	35:0		figure above: change: "Smith et al., 2005" to "2003"; correct: "Vonder Muhll" to "Mühll" [Hartmut Grassl]	Accepted. Text modified.
4-924	A	35:0		Spelling needs work in section 4.7 [Neville Nicholls]	Accepted. Text modified.
4-925	A	35:0		Table 4.7.1: no temperature change is given in the N Quebec 1996-2001 row. [David Parker]	Accepted. Revised.
4-926	A	35:2		I have not seen anywhere in Draft 1.00 any mention of methane release from melting permafrost. In previous assessment reports I believe this has been regarded as a minor issue but there are published reports, perhaps of unreviewed work, in newspapers and science magazines that are quite hysterical about this (see www.guardian.co.uk/climatechange/story/0,12374,1546824,00.html). The issue needs to be addressed somewhere in AR4 --- if only to discount the claims. Since it is a cryospheric response leading to a positive feedback, Chapter 4 would seem like the right place. The end of section 4.7.2.3 touches on carbon flux into the Arctic Ocean but this is not the only carbon flux to result from permafrost melting. [Garry CLARKE]	Accepted. Taken into account.
4-927	A	35:4	35:5	This sentence seems to imply that only what are in effect volumetric changes in permafrost qualify as "degradation." It is well known that the bearing capacity of ice-rich permafrost decreases as its temperature increases; this can also reasonably be construed as "degradation." It is also important to stress (at the outset of Section 4.7) that permafrost is defined on temperature/duration criteria; it follows that a sentence should be inserted at the location indicated here to the effect that degradation of _ice-rich_ permafrost is what	Noted. Considering permafrost temperature increase as permafrost degradation is arguable. Accepted for the second part of the comment.

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				really matters in terms of impacts on ecosystems and human infrastructure. [Frederick Nelson]	
4-928	A	35:4	35:15	Much of this paragraph repeats old results that were likely cited in TAR. To cut down on length, the section should focus on recent results. [Sharon Smith]	Accepted. Text revised.
4-929	A	35:4	35:15	A recent reference that could be added to this section is Beilman and Robinson 2003 (ref: Beilman, D.W. and Robinson, S.D. 2003 Peatland permafrost that and landform type along a climate gradient. Permafrost - Proceedings of 8th Int. Conf. on Permafrost. M. Phillips, S.M. Springman and L. Arenson ed. p. 61-65). The paper examines changes in peatlands and their vegetation related to permafrost degradation. Beilman and Robinson (2003) present some preliminary results which show that 10to 50% of original frozen peat plateaus have degraded (with associated changes in vegetation cover) over the last 50 years in the discontinuous permafrost zone in western Canada. Recent results that show permafrost degradation over the last 50 years in the Mackenzie Valley are also presented. Climate is the dominant trigger for the sites studied. [Sharon Smith]	Noted.
4-930	A	35:5		What does "discontinuous" mean here? Is the meaning of the sentence changed if the word is deleted? [Neville Nicholls]	Accepted. Text clarified.
4-931	A	35:12	35:12	extent" instead of "extend [Roxana Bojariu]	Accepted. Text modified.
4-932	A	35:12		correct: "areal extend" to "extent" [Hartmut Grassl]	Accepted. Text modified.
4-933	A	35:13		Is "taliks" defined in the glossary? Is it different to permafrost? [Neville Nicholls]	Yes, it is in the glossary.
4-934	A	35:17	35:22	This paragraph contains too little detail to be very useful. Two examples are given, but both are from relatively marginal permafrost environments. It should be emphasized that basal thawing is not rapid in areas with thick permafrost. [Frederick Nelson]	Accepted. Text revised.
4-935	A	35:17	35:19	Statement is a bit unclear. [Sharon Smith]	Accepted. Text modified.
4-936	A	35:21	32:22	when current permafrost surface warming continues --- Do you mean when warming that occurred at surface reaches greater depth? [Sharon Smith]	Accepted. Text revised for clarification.
4-937	A	35:24	35:24	explain 'thermokarst topography' [Anny CAZENAVE]	Accepted. Text modified.

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4-938	A	35:24	35:25	A more detailed definition of the term "thermokarst" and explanation of the phenomenon's significance is needed here. Are enough data available in the recent literature to construct an illustration or table showing increases in areas affected by thermokarst processes? [Frederick Nelson]	Accepted. Text modified.
4-939	A	35:25	35:25	Thermokarst development a better term than thermokarsting? [Sharon Smith]	Accepted. Text modified.
4-940	A	35:30	35:30	The reference should be Smith et al. 2004 if it is the one that appears in the reference list. [Sharon Smith]	Accepted.
4-941	A	35:31	35:31	discontunous" should be "discontinuous [James Renwick]	Accepted. Text revised.
4-942	A	36:0	37:	Perhaps all of 4.7.4 "Consequences" should be in WG2? [Neville Nicholls]	Accepted and text shortened.
4-943	A	36:4		change: "total of carbon" to "total carbon" [Hartmut Grassl]	Accepted. Text revised.
4-944	A	36:4		Wow! Four-digit accuracy! [Jerry Mahlman]	Noted.
4-945	A	36:5	36:5	"lowering in permafrost stability" - what is meant by this? Does this refer to a reduction in the strength of the frozen material? [Sharon Smith]	Accepted and text revised.
4-946	A	36:6		? [Hartmut Grassl]	
4-947	A	36:12	36:13	It would be better to say that it is the portion of the soil above the permafrost that freezes and thaws seasonally. [Sharon Smith]	Accepted and text revised.
4-948	A	36:13		delete "of" [Neville Nicholls]	Accepted. Text revised.
4-949	A	36:14	36:17	Vegetation is also an important factor. [Sharon Smith]	Noted.
4-950	A	36:17	36:19	I suggest the following wording for this passage: "Interannual and spatial variations in thaw depth at point locations can be large, an artifact of year-to-year and microtopographic variations in both surface temperature and soil moisture. It is therefore important to obtain robust, replicate samples of active-layer thickness for monitoring purposes." [Frederick Nelson]	Accepted. Text revised.
4-951	A	36:17	36:19	Using depth of thaw as an indicator of long-term climate change is difficult. In addition	Accepted and text revised.

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				the results have shown that difficulties in using active layer as a climate indicator also arise when settlement of ice-rich sediments occurs. The active layer thickness may vary little from year to year although thaw may be progressing to deep layers in the ground due to the accompanying surface settlement that occurs (see for example: Smith, S.L., Burgess, M.M. and Nixon, F.M. 2001. Response of active-layer and permafrost temperatures to warming during 1998 in the Mackenzie Delta, Northwest Territories and at Canadian Forces Station Alert and Baker Lake, Nunavut; Geological Survey of Canada Current Research 2001-E5, 8 p.) [Sharon Smith]	
4-952	A	36:23	36:29	This paragraph seems out of place and should perhaps come later when recent changes in thaw depth are discussed or various regions. [Sharon Smith]	Accepted. Text revised.
4-953	A	36:23		...monitoring of the active layer .... [Jerry Mahlman]	Accepted. Text modified.
4-954	A	36:27		change: "because of most ecological" to "because most" [Hartmut Grassl]	Accepted. Text modified.
4-955	A	36:27		change: "a few was of early" to "a few as early as" [Hartmut Grassl]	Accepted. Text modified.
4-956	A	36:29		"thawing index" (?) [Hartmut Grassl]	Accepted. Text modified.
4-957	A	36:29		what is "thawing index". Can unfamiliar terms like this be changed for more familiar terms, especially where they are only used a few times? [Neville Nicholls]	Accepted. Text modified.
4-958	A	36:33	36:34	CALM currently operates more than 125 sites at locations including the Arctic, the Antarctic, and several midlatitude mountain ranges. [Frederick Nelson]	Noted.
4-959	A	36:33	36:34	This sentence should also mention that CALM contributes to GTN-P (it is the active layer monitoring component of GTN-P) [Sharon Smith]	Accepted. Text modified.
4-960	A	36:33	36:44	Some references other than Brown et al. (2000) should be given here as this only gives information from sites that contribute to CALM and only includes data up to 2000. More recent papers should also be cited including some of those that were published in the proceedings of the 8th Int. Conf. on Permafrost. For Canada, 3 papers are suggested: Nixon, M., Tarnocai, C. and Kutny, L. 2003. Long-term active layer monitoring: Mackenzie Valley, northwest Canada. Proceedings of the 8th International Conference on Permafrost, July 2003, Zurich Switzerland. M. Phillips, S.M. Springman and L.U.	Accepted.

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				Arenson (eds.), A.A. Balkema, Lisse, the Netherlands, p. 821-826. Tarnocai C., Nixon, F.M. and Kutny, L. 2004.Circumpolar-Active-Layer-Monitoring (CALM) sites in the Mackenzie Valley, Northwestern Canada. Permafrost and Periglacial Processes, vol 15, p. 141-153. Mackay, J.R. and Burn, C.R. 2002. The first 20 years (1978-79 to 1998-1999) of active-layer development, Illisarvik experimental drained lake site, western Arctic coast, Canada. Canadian Journal of Earth Sciences 39: 1657-1674. [Sharon Smith]	
4-961	A	36:38	36:39	Did the minimum thaw depth in Canada not also occur in 1996 which was a cold summer? [Sharon Smith]	Text clarified.
4-962	A	36:39	36:40	Some other references could be added. For Canada, the importance of subsidence is shown in Smith et al. 2001 (Smith, S.L., Burgess, M.M. and Nixon, F.M. 2001. Response of active-layer and permafrost temperatures to warming during 1998 in the Mackenzie Delta, Northwest Territories and at Canadian Forces Station Alert and Baker Lake, Nunavut; Geological Survey of Canada Current Research 2001-E5, 8 p.) [Sharon Smith]	Taken into account.
4-963	A	36:40		what is "thaw subsidence"? [Neville Nicholls]	Accepted. Text revised.
4-964	A	36:46	36:46	Change "area" to "areas" [Anne Nolin]	Accepted. Text revised.
4-965	A	36:46	37:8	The discussion of seasonally frozen ground outside the permafrost regions is severely limited. Only Russia and Tibet are mentioned. There should be an integrated discussion of seasonally frozen ground that incorporates literature from other regions. Results from satellite remote sensing should be included. [Frederick Nelson]	Noted. There is a small section on freeze/thaw cycle using satellite remote sensing data.
4-966	A	36:47	36:48	The definition of seasonally frozen ground given here is at odds with that provided earlier (p. 33, line 1). This reviewer agrees strongly with the the inclusion of the active layer as a form of seasonally frozen ground. [Frederick Nelson]	Accepted. Text revised.
4-967	A	36:48	36:51	Here you say that the thickness of seasonally frozen ground is "primarily controlled by" the increase in winter air temperature and snow depth (analogous statements are made elsewhere). By this, do you mean that in a statistical sense, the changes are most highly correlated with temperature and snow depth (of the available variables), or do you mean that physical or analytical modelling shows that these are the most important variables? It would be useful to explain what is meant, in a little more detail. [James Renwick]	Accepted.

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4-968	A	36:49	36:51	This seems to be quite credible. [Jerry Mahlman]	Accepted.
4-969	A	37:0	38:	Section 4.7.4 - This section on consequences seems to be a discussion of impacts that would be more appropriate for Working Group 2. This section could probably be much shorter with a great deal of the material moved to the appropriate chapter (such as polar chapter 15) in WG2. If a discussion of consequences is to be added there are a few other references that could be added such as couple of Canadian ones: Smith, S.L. and Burgess, M.M. 2004. Sensitivity of permafrost in Canada to climate warming. Geological Survey of Canada Bulletin 579. Mackenzie River Basin Board, 2004. Mackenzie River Basin State of the Aquatic Ecosystem Report 2003. [Sharon Smith]	Accepted. Text shortened
4-970	A	37:0	38:	Section 4.7 also unduly long and no so relevant to WG I. [Kevin Trenberth]	Accepted. Text shortened (4.7.4)
4-971	A	37:1		correct: "plays" to "play" [Hartmut Grassl]	Accepted. Text modified.
4-972	A	37:10		Fig 4.7.4: The values plotted appear to be anomalies (since some numbers are negative). Explain in the caption what the values are anomalies from. [James Renwick]	Accepted. Caption revised.
4-973	A	37:22	38:26	This section does better than the previous in at least attempting to draw a few connections between changes in permafrost and effects on, for example, the carbon cycle and humans (e.g., section 4.7.4); I would recommend parallel text for the other components of cryospheric change. [Robie Macdonald]	Noted. Section remains, but is shortened. Impacts are the responsibility of WG II.
4-974	A	37:22		section 4.7. If relevant, the consequences of permafrost melting on methane emissions should be given here. [Philippe Tulkens]	Accepted. Text modified.
4-975	A	37:27	37:57	This is a very nice summary of the likely impacts and consequences of permafrost melting. [Jerry Mahlman]	Noted.
4-976	A	37:27	37:43	Links between permafrost (and other components of the cryosphere), hydrological systems and climate change were discussed in a recent document produced by the Canadian Government: Brown, R.D.Demuth, M.N., Goodison, B.E., Marsh, P., Prowse, T.D., Smith, S.L. and Woo, M.-K. 2004. Climate Variability & Change - Cryosphere. Chapter 14 in Threats to Water Availability in Canada; NWRI Scientific Assessment Report Series No. 3 and ACSD Science Assessment Series No. 1, National Water Research Institute, Meteorological Service of Canada, Environment Canada, p. 107-116 [Sharon Smith]	Accepted. And text revised

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4-977	A	37:27	:57	Is all of this detail necessary? [John Church]	Accepted. Text shortened
4-978	A	37:35	37:35	change 'impacted' to 'affected' [Thomas James]	Accepted. Text modified.
4-979	A	37:35	37:35	There will be increased winter baseflow which will sustain winter stream flow. [Sharon Smith]	Noted. Text revised.
4-980	A	37:43	37:43	Replace "contributing" by "contributed". [Martin Stendel]	Accepted. Text modified.
4-981	A	37:45	37:46	over thin/thick permafrost' change to 'with thin/thick permafrost' [Thomas James]	Accepted. Text modified.
4-982	A	37:45	37:45	Changes in surface water etc. will also have impacts on terrestrial and aquatic ecosystems. [Sharon Smith]	Accepted. Text modified.
4-983	A	37:45	37:45	This repeats material in previous paragraph. [Sharon Smith]	Accepted. Text modified.
4-984	A	37:45	37:52	No references are provided. Specific examples would be a good idea. [Sharon Smith]	Accepted. Text revsied.
4-985	A	37:50	37:52	These two sentences (about villages) probably belong in WG2, and should be removed. [Thomas James]	Accepted. Text revised.
4-986	A	37:51	37:51	Change "is" to "are." [Melinda Marquis]	Accepted. Text modified.
4-987	A	37:53	37:53	Thawing of decomposed plant materials -- referring to peat? [Sharon Smith]	Text improved.
4-988	A	37:54		length of unfrozen" add: "lenght of the unfrozen [Hartmut Grassl]	Accepted. Text modified.
4-989	A	37:56	37:57	This is not of any real value unless it would alter the global CO2 budget quantitatively. I doubt this, because CO2 is a very long-lived greenhouse gas which is close to being globally well mixed. Strong exhalations at the microscale could be relevant at the microscale. [Jerry Mahlman]	Noted.
4-990	A	37:58		Suggest the annual totals are plotted in 4.7.4 rather than a misleading colour interpolation of data - needs further discussion in text [Melanie Fitzpatrick]	Accepted and figure revised.
4-991	A	38:0	39:	I think 4.8.1 could be deleted without any loss to the chapter [Neville Nicholls]	Accepted. Text rewritten.
4-992	A	38:0	39:	Section 4.8 Synthesis - Other things could be considered here. It is important to consider	Accepted. Text rewritten.

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				climate change not just climate warming or an air temperature change. There will also be precipitation changes which can result in changes to snow amounts. In the case of permafrost - changes in permafrost conditions (temperature, active layer, permafrost thickness and distribution) will reflect the combined effects of temperature and precipitation (especially snow). [Sharon Smith]	
4-993	A	38:0		Climate and variability and the state of the cryosphere is briefly mentioned. A mulitdisiplinary project in Canada investigated the impact of the warming in 1998 on the Canadian arctic cryosphere. A paper summarizing results is in press: Atkinson, D.E., R. Brown, B. Alt, T. Agnew, J. Bourgeois, M. Burgess, C. Duguay, G. Henry, S. Jeffers, R. Koerner, A.G. Lewkowicz, S. McCourt, H. Melling, M. Sharp, S. Smith, A. Walker, K. Wilson, S. Wolfe, M-k. Woo, K. Young, 2005: Canadian cryospheric response to an anomalous warm summer. Atmosphere-Ocean The results were also released in a report: CCAF Summer 1998 Project Team 2001The state of the Arctic Cryosphere during the Extreme Warm Summer of 1998: documenting cryospheric variability in the Canadian Arctic, , CCAF Final Report, Available at http://www.socc.ca [Sharon Smith]	Noted.
4-994	A	38:1	38:39	Again, these microscale effects do not appear to be globally relevant unless someone can make a quantitative case for it. [Jerry Mahlman]	Accepted. Text rewritten.
4-995	A	38:8	38:10	This definition of thermokarst terrain should come earlier, where thermokarst is first mentioned (page 35). [James Renwick]	Accepted. Text rewritten.
4-996	A	38:8	38:11	Differential settlement is the process referred to. Note that Smith and Burgess (2003) examine the thaw settlement in response to surface disturbance but it does illustrate the concept of differential settlement (Burgess, M.M. and Smith, S.L. 2003. 17 years of thaw penetration and surface settlement observations in permafrost terrain along the Norman Wells pipeline, Northwest Territories, Canada; Proceedings of 8th International Conference on Permafrost, July 2003, Zurich Switzerland, p. 107-112) [Sharon Smith]	Noted.
4-997	A	38:10	38:10	particular' should be 'particularly' [Thomas James]	Accepted. Text modified.
4-998	A	38:10	38:10	second-last word should be "particularly" [James Renwick]	Accepted. Text modified.
4-999	A	38:28		Section 4.8, Synthesis: The remarkable changes in the arctic sea ice cover, and the interplay between thermodynamics and dynamics, seem rather lost in this text. [Ola M. Johannessen]	Accepted. Text rewritten.

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4-1000	A	38:30		Section 4.8.1. Since this is an observational chapter, and the modelling it mentions is specifically of the cryosphere, I think you have to be *extremely* cautious about attributing cryospheric changes to other aspects of climate change. To be sure of such attributions, one needs coupled modelling, and it might be more appropriate to defer the discussion to chapter 9. [Jonathan Gregory]	Accepted. Text rewritten.
4-1001	A	38:35	38:35	After "frozen ground," and a comma. Change "it" to "the motion of sea ice." [Melinda Marquis]	Only partially accepted. Text rewritten ("it" replaced by "transport").
4-1002	A	38:35	38:35	change to read "…lake ice, while for frozen ground it is of minor importance." [James Renwick]	Accepted. Text modified.
4-1003	A	38:41		I question this assumption that temperature is the most important variable. If there is extra heating then temperature will go up along with the changes in the cryosphere, but it does not mean that temperature changes are responsible. On the contrary in the tropical glaciers it is clearly not. Precipitation changes are also important and not independent. This should be much more carefully worded. [Kevin Trenberth]	Accepted. Text modified.
4-1004	A	38:42	38:43	Does "high southern latitudes" mean "south of 65S"? If so, the first phrase can be omitted. [Jonathan Gregory]	Accepted. Text modified.
4-1005	A	38:42	38:42	for the marine' should be 'for marine' [Thomas James]	Accepted. Text modified.
4-1006	A	38:48	38:48	Referring to seasonally frozen ground in non permafrost areas only? [Sharon Smith]	No. As defined in the text, seasonally frozen ground includes the active layer in permafrost regions.
4-1007	A	38:52	38:52	Replace "20th" with "Twentieth" [James Renwick]	Accepted. Text modified.
4-1008	A	38:53	38:54	Is this consistent with 4.5.3, which says that the retreat on Kilimanjaro is driven by radiation, not warming? [Jonathan Gregory]	Text clarified.
4-1009	A	38:54		Hygric? Its not in my dictionary. [Jerry Mahlman]	Accepted. Text modified.
4-1010	A	38:54		What is "hygric". The term "global warming" is vague and should not be used here. [Kevin Trenberth]	Accepted. Text modified.
4-1011	A	38:56	39:4	Not covered here, or in the main text, the issue of surging glaciers/ice streams and natural variability associated with ocean temperatures and ice stream speeds. Why do ice streams surge? [Jeff Ridley]	Accepted. Text added.

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4-1012	A	38:57	38:57	Is there any evidence that basal melting has *increased*? [Jonathan Gregory]	Taken into account. There is evidence that thinning occurred with increased ice supply, hence increased ice loss, and no evidence that that mass loss is through the upper surface, so basal melt is implicated. Text clarified.
4-1013	A	39:9	39:10	This is compellingly true poleward of, say, 50 degrees North latitude. [Jerry Mahlman]	Noted.
4-1014	A	39:14	39:34	The synthesis section 'cryospheric contributiond to sea level change' does not exactly reflect the discussion presented in 4.5. For example in 4.5, the value presented as the best estimate is the mean (0.36 mm/yr) of 3 different studies while in 4.8.2 only one study result is given. If the Dyugerov&Meier estimate is to be prefered, then why not say it and then why not give the value for the longest period (1960-2003)? [Anny CAZENAVE]	Taken into account. Numbers on glacier and ice-sheet mass balances and contributions to sea-level change have been updated.
4-1015	A	39:14	39:34	The "past five years" should be given a time frame (year x to year y). [Thomas James]	Accepted. Text modified.
4-1016	A	39:14	:32	Suggest a table of sea level contributions (with various periods and uncertainties) is required for clean handoff to other chapters. [John Church]	Taken into account. Numbers on glacier and ice-sheet mass balances and contributions to sea-level change have been updated.
4-1017	A	39:14		Section 4.8.2. I suggest that this section should be omitted. It is covered by chapter 5, which is responsible for the synthesis of factors affecting sea level. For chapter 4 it is repetition. [Jonathan Gregory]	Rejected. Cryospheric contributions to sea level will remain part of the syntheses. But the synthesis will be rewritten.
4-1018	A	39:14		Section 4.8.2 COMMENT In the next draft this section would benefit from some expansion as it is too brief at present and in particular a complete (not long) summary of the information used in table 5.5.2 in the following Chapter would help the reader. [William Hare]	Accepted. Text modified.
4-1019	A	39:19	39:19	After "contribution" insert "to sea level rise." [Melinda Marquis]	Accepted. Text modified.
4-1020	A	39:21		Why can't this be updated at least through 2004? [Kevin Trenberth]	Taken into account. Numbers on ice-sheet mass balances and contributions to sea-level change have been updated.
4-1021	A	39:22	39:22	Table 4.5.3 appears to include Antarctica. Refer to comment on page 21 line 3. [David Parker]	Accepted. Text modified.
4-1022	A	39:26	39:27	While technically consistent with the conclusion in Chapter 5, there are very different	Taken into account. Numbers on ice-

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				implications to the two chapters' conclusions. Chapter 5 (Pg.3, lines 8-9) states: The contribution from the Greenland and Antarctic ice sheets during 1993-2003 is assessed as 0.0 +/- 0.2 mm/yr, where this chapter concludes that the Greenland and Antarctic ice sheets made a positive contribution to sea level rise of up during that period, which rose ot 0.2 mm/yr over the last five years. The two conclusions should be made consistent. [Lenny Bernstein]	sheet mass balances and contributions to sea-level change have been updated. Interchapter meetings were held to inmprove coordination.
4-1023	A	39:26	39:27	This conclusion is inconsistent with the conclusion in Chapter 5 (Pg.3, lines 8-9) which states: The contribution from the Greenland and Antarctic ice sheets during 1993-2003 is assessed as 0.0 +/- 0.2 mm/yr. The two chapters should be sending the same message. [Jeffrey Kueter]	Accepted. Text updated and transferred to Ch. 5.
4-1024	A	39:29	39:30	Not all of this water will end up in the sea - some will be retained as pore water and some may end up in new lakes and ponds created by ground subsidence (and available for evaporation). [Sharon Smith]	Taken into account.
4-1025	A	39:31		in Russian Arctic" add: "in the Russian [Hartmut Grassl]	Accepted. Text modified.
4-1026	A	39:34	39:34	Suggested rewording: "This suggests that melting ice has contributed 1 mm/yr to global sea-level rise from year x to year y. This is an increase from the 0.2-0.4 mm/yr estimate given in the Third Assessment Report, and is indicative of increased rates of melting of glaciers and ice caps." [Thomas James]	Taken into account. Numbers on ice-sheet mass balances and contributions to sea-level change have been updated.
4-1027	A	39:34	39:34	Could indicate in this closing sentence that this value of 1 mm/yr comes from data over the past decade (or less?). If warming continues then there will be a cryospheric contribution of more than 1 mm/yr in the future. [Michelle Koutnik]	Taken into account. Numbers on ice-sheet mass balances and contributions to sea-level change have been updated.
4-1028	A	39:34	39:34	What is the error estimate on this 1mm/yr? [Jeff Ridley]	Taken into account. Numbers on ice-sheet mass balances and contributions to sea-level change have been updated.
4-1029	A	39:34	39:34	Do you mean the potential contribution if the ice were to thaw? [Sharon Smith]	No, this is the current contribution of melting ice. Text improved.
4-1030	A	39:34	39:34	The arithmetic on how the figure of 1 mm/year was calculated is not fully explained. It should be detailed as, from the figures given in section 4.8.2 (p. 39), different interpretations may emerge. [Philippe Tulkens]	Taken into account. Numbers on ice-sheet mass balances and contributions to sea-level change have been updated.
4-1031	A	39:34	39:34	The figure of 1 mm/year corresponds to an average increase over which time period ? It should be specified and checked with the figure given in Ch 5 executive summary (p 5-3,	Accepted. Text modified.

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				L7). [Philippe Tulkens]	
4-1032	A	39:34		What does "current" mean? [John Church]	Accepted. Text modified.
4-1033	A	40:0	53:	References - The following are missing from the reference list: Smith, S.L., Burgess, M.M., Riseborough, D. and Nixon, F.M. 2005. Recent trends from Canadian permafrost thermal monitoring network sites. Permafrost and Periglacial Processes 16: 19-30 Romanovsky, V. Burgess, M., Smith, S., Yoshikawa, K. and Brown, J., 2002. Permafrost temperature records: indicators of climate change; EOS, Transactions of the American Geophysical Union, vol 83, p. 589, 593-594. Burgess, M.M., Smith, S.L., Brown, J., Romanovsky, V. and Hinkel, K. 2000. Global Terrestrial Network for Permafrost (GTNet-P): permafrost monitoring contributing to global climate observations; Geological Survey of Canada, Current Research 2000-E14, 8 p. Allard, M., Fortier, R. and Duguay, C. 2002. A trend of fast climate warming in northern Quebec since 1993. Impacts on permafrost and man-made infrastructures. Eos Trans. AGU 83(74), Fall Meeting Supplement Abstract Burn, C.R. 1998. The response (1958-1997) of permafrost and near-surface ground temperatures to forest fire, Takhini River valley, southern Yukon Territory. Canadian Journal of Earth Sciences 35:184-199. [Sharon Smith]	Reference list will be updated according to text requirements.
4-1034	A	40:1		Reference, add: Belchansky, G. I., D. C. Douglas, I. V. Alpatsky, and N. G. Platonov, 2004. Spatial and temporal multiyear sea ice distributions in the Arctic: a neural network analysis of SSM/I data, 1988– 2001, J. Geophys. Res., 109, C10017, doi:10.1029/2004JC002388. Reference, add: Bjørgo, E., Johannessen, O. M. and Miles, M. W., 1997. Analysis of merged SMMR-SSMI time series of Arctic and Antarctic sea ice. Geophys. Res. Lett., 24, 413–416. Cavalieri, D. J., Gloersen, P., Parkinson, C. L., Comiso, J. C. and Zwally, H. J., 1997. Observed hemispheric assymetry in global sea ice changes. Science, 278, 1104-1106 Reference, add: Deser, C., J. E. Walsh and M. S. Timlin, 2000. Arctic sea ice variability in the context of recent atmospheric trends. J. Clim. 13, 617–630. Reference, add: Gloersen, P. and Campbell, W. J., 1991. Recent variations in Arctic and Antarctic sea-ice covers. Nature, 352:33-36. Reference, add: Johannessen, O. M., L. Bengtsson, M. W. Miles, S. I. Kuzmina, V. A.Semenov, G. V. Alekseev, A. P. Nagurnyi, V. F. Zakharov, K. Hasselmann and H. Cattle, 2004. Arctic climate change – observed and modelled temperature and sea ice.	Rejected. References are only added when required by text modifications.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Tellus 56A, 328–341. Reference, add: Johannessen, O. M., K. Khvorostovsky, M. W. Miles and L. P. Bobylev, 2005. Recent Ice Sheet Growth in the Interior of Greenland. Sciencexpress/20 October 2005/10.1126/science.1115356. Reference, add: Johannessen, O. M., M. W. Miles and E. Bjørgo, 1995. The Arctic's shrinking sea ice, Nature 376, 126–127. Reference, add: Johannessen, O. M., E. V. Shalina and M. W. Miles, 1999. Satellite evidence for an arctic sea ice cover in transformation, Science 286, 1937–1939 Reference, add: Serreze, M. C., J. A. Maslanik, T. A. Scambos, F. Fetterer, J. Stroeve, K. Knowles, C. Fowler, S. Drobot, R. G. Barry and T. M. Haran. 2003. Record minimum sea ice cover in the Arctic Ocean for summer 2002. Geophys. Res. Lett., 30, 1110–111. Reference, add: Stroeve, J. C., M. C. Serreze, F. Fetterer, T. Arbetter, W. Meier, J. Maslanik and K. Knowles, 2005. Tracking the Arctic's shrinking ice cover: another extreme September minimum in 2004. Geophysical Research Letters 32, L04501, doi:10.1029/2004GL02810. [Ola M. Johannessen]	
4-1035	A	44:1		Hanna E., … Steffens should be Steffen [Konrad Steffan]	Accepted. Text modified.
4-1036	A	44:44	44:44	Replace "permafrostat" by "permafrost at". [Martin Stendel]	Accepted. Text modified.
4-1037	A	44:46	44:	Reference missing, please include: Isaksen K, Holmlund P, Sollid JL, Harris C. 2001. Three deep alpine-permafrost boreholes in Svalbard and Scandinavia. Permafrost and Periglacial Processes 12: 13-25. [Ketil Isaksen]	Reference list will be updated according to text requirements.
4-1038	A	47:32	47:32	Nesje, A., Ø. Lie and S.O. Dahl 2000: Is the North Atlantic Oscillation reflected in Scandinavian glacier mass balance records? Journal of Quaternary Science 15, 587-601. [Atle Nesje]	Reference list will be updated according to text requirements.
4-1039	A	48:45	48:45	insert reference "Raymond, C.A., E.R. Ivins, M.B. Heflin, and T.S. James, Quasi-continuous global positioning system measurements of glacial isostatic deformation in the Northern Transantarctic Mountains, Global and Planetary Change 42, 295-303, 2004." See my comment #31 [Thomas James]	Reference list will be updated according to text requirements.
4-1040	A	51:28		1888 should be 1878. [Takashi Yamanouchi]	Accepted. Text modified.
4-1041	A	54:0		p. 54 The quality of some of the figures needs to be improved (make lines and text clearer) [Atle Nesje]	Accepted. Figures improved.

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4-1042	A	54:10	54:18	Factors to control snow albedo should be discussed: "The factors to change the spectral albedo of the snow surface are essentially divided into two categories: (1) the physical parameters of snow and (2) the external parameters such as atmospheric conditions and solar zenith angle (Aoki et al., 1999). The important factors in the snow physical parameters are snow grain size and concentration of snow impurities. The near infrared albedo decreases with an increase of snow grain size (Wiscombe and Warren, 1980) and the visible albedo is reduced by snow impurities (Warren and Wiscombe, 1980). It was shown that the effects of these snow physical parameters on spectral albedo (Aoki et al, 2000) and broadband albedo (Aoki et al, 2003) from their in situ measurements." References Aoki, Te., Ta. Aoki, M. Fukabori, and A. Uchiyama, Numerical simulation of the atmospheric effects on snow albedo with a multiple scattering radiative transfer model for the atmosphere-snow system, J. Meteor. Soc. Japan, 77, 595-614, 1999. Aoki, Te., Ta. Aoki, M. Fukabori, A. Hachikubo, Y. Tachibana and F. Nishio, Effects of snow physical parameters on spectral albedo and bidirectional reflectance of snow surface, J. Geophys. Res., 105, 10219-10236, 2000. Aoki, Te., A. Hachikubo, and M. Hori, 2003: Effects of snow physical parameters on shortwave broadband albedos. J. Geophys. Res., 108, 4616, doi: 10.1029 / 2003JD003506. Warren, S. G., and W. J. Wiscombe, A model for the spectral albedo of snow, II: Snow containing atmospheric aerosols, J. Atmos. Sci., 37, 2734-2745, 1980. Wiscombe, W. J., and S. G. Warren, A model for the spectral albedo of snow, I: Pure snow, J. Atmos. Sci., 37, 2712-2733, 1980. [Teruo Aoki]	Page and line numbers wrong. Obviously refer to p. 8 Rejected. This is an assessment of observed changes, not a treatise on snow physics.
4-1043	A	55:0		Figure 4.1.1 is an excellent schematic - suggest it remain in report [Melanie Fitzpatrick]	Noted.
4-1044	A	55:0		Fig. 4.1.1 Lines and letters in graphic are fuzzy. Improve quality of lettering in graphic. [Melinda Marquis]	Accepted. Figure improved.
4-1045	A	55:0		Figure 1. Divide the upper and lower half clearly. [Kenichi Matsuoka]	Accepted. Figure improved.
4-1046	A	55:0		What does dashed line over ice sheet ad mountains indicate? [Kenichi Matsuoka]	Snow cover. Figure improved.
4-1047	A	55:0		Characters in Fig. 4. 1. 1. should be much larger. [Takashi Yamanouchi]	Accepted. Figure improved.
4-1048	A	56:0		Fig 4.2.1 Lines and letters in graphic are fuzzy. Improve quality of lettering in graphic.	Accepted. Will fix.

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				Need citation for NOAA data set. [Melinda Marquis]	
4-1049	A	56:0		Explanation for thick line is missing. [Takashi Yamanouchi]	Accepted. Figure improved.
4-1050	A	56:2		the bold line in the figure should be explained [Hartmut Grassl]	Accepted. Figure improved.
4-1051	A	57:0		Numbers (minus sign) in legend is not clear for Fig. 4. 2. 2 and should be written with %. [Takashi Yamanouchi]	Accepted. Figure improved.
4-1052	A	57:6	57:6	In caption of Figure 4.2.2, negative (not positive) values indicate greater extent in the earlier portion of the record. [David Parker]	Accepted. Figure improved.
4-1053	A	57:6	57:7	Surely positive values indicate greater extent in the latter portion of the record - i.e. growth of snow cover? [Jeff Ridley]	Accepted. Figure improved.
4-1054	A	57:6		There appears to be a sign error here compared with the text. [John Church]	Accepted. Figure improved.
4-1055	A	58:0		Figure 4.2.3 gray scale is difficult to see - is there an alternate way to represent altitude? [Melanie Fitzpatrick]	Accepted. Figure improved.
4-1056	A	58:0		Fig. 4.2.3 Caption states: " Dependence of trends in snow on elevation." Is it elevation or temperature? The left graph doesn't show elevation, does it? Graph (a) seems to show that decreasing temp is correlated with decreasing SCE; is this correct? In right graph (b), what are diagonal lines showing? SCE? Add units to X and Y axis of both graphs. Add (a) and (b) labels to graphs, to relate them to the caption. In the right graph (b), label X and Y axes (move from inside graph to along axes). [Melinda Marquis]	Accepted. Figure improved.
4-1057	A	58:0		Fig. 4. 2. 3 is not easy to understand. What is the curve in left portion? [Takashi Yamanouchi]	Accepted. Figure improved.
4-1058	A	58:1	58:11	In the left panel, we see mainly the scattering of the data. Are there any means to simplify the figure or highlight the dependency of the change with the temperature ? By the way, the scattering is not surprising because the SWE is also related to précipitation. [ERIC MARTIN]	Accepted. Figure improved.
4-1059	A	59:0		Figure 4.2.4 REMOVE this Figure. It is too localized. It is a single dataset that does not represent a region or area. [Melanie Fitzpatrick]	Accepted. Removed.
4-1060	A	59:0		meaning of black band across the image is unclear [Michelle Koutnik]	See response to 4-1059.

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4-1061	A	59:0		Fig. 4.2.4 Clarify units of X axis (metres?). [Melinda Marquis]	See response to 4-1059.
4-1062	A	60:0		Figure 4.3.1 Keep this figure in the report [Melanie Fitzpatrick]	Noted.
4-1063	A	60:0		poor quality figure reproduction [Michelle Koutnik]	Accepted. Figure improved.
4-1064	A	60:0		Fig. 4.3.1 Both the data points and the lettering on the graphic are fuzzy. Please improve quality of graphic. [Melinda Marquis]	Accepted. Figure improved.
4-1065	A	61:0		Figure 4.3.2 This Figure could be simplified – do not need all the panels. Could for instance cut to show 1967-1996 only. Also if 10% is not significant then why not leave out altogether and only show significant trends? Significance should be discussed in text. [Melanie Fitzpatrick]	Noted. There is a strong desire to show long records, although sparse. Showing only locations with significant trend would be misleading. This will be considered during revisions.
4-1066	A	61:0		Fig. 4.3.2 Difficult to see different triangles in graphic. Please improve quality of graphic. [Melinda Marquis]	Accepted. Figure improved.
4-1067	A	61:0		Figure 4.3.2: Top row suggests earlier freeze-up so disagrees with text. [David Parker]	Accepted: text modified
4-1068	A	61:0		Figure 4.3.2: Not an easy diagram to see: Figure 4.3.3 is clearer. Recommend omitting 1957-96 and 1947-97 panels from Figure 4.3.2 as they have few data; then the 1967-96 panels can be expanded. [David Parker]	Noted. There is a strong desire to show long records, although sparse. Showing only locations with significant trend would be misleading. This will be considered during revisions.
4-1069	A	62:0		Figure 4.3.3 LEAVE OUT. Don't need so many Figures to show the insignificance of break up / freeze trends. Is significance at the 10% level worth showing? [Melanie Fitzpatrick]	Rejected. This is one of the few summaries of lake ice available.
4-1070	A	62:0		Fig. 4.3.3 Are upward and downward triangles used in the same way they're used in Fig. 4.3.2? If so, consider using the explanation for them in Fig. 4.3.2, which is clearer. Add reference in caption to (a) freeze-up and (b) break-up. [Melinda Marquis]	Accepted. Captions revised.
4-1071	A	62:0		Fig. 4. 3. 3 is too small, it is difficult to read legends. [Takashi Yamanouchi]	Accepted. Figure improved.
4-1072	A	63:0		What are the dashed lines? [John Church]	Explained in revised caption. Dashed lines are SMMR data, rather than SSM/I

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4-1073	A	63:0		Figure 4.4.1 Excellent - keep this figure in the report [Melanie Fitzpatrick]	Noted
4-1074	A	63:0		Fig. 4.4. 1 In caption, insert comma before "whereas." [Melinda Marquis]	Accepted
4-1075	A	63:0		Figure 4.4.1. The caption should say that the dashed portions of the plots are based on an earlier (microwave) instrument. [David Parker]	Explained in revised caption.
4-1076	A	64:0		Figure 4.4.2 Excellent - keep this figure in the report [Melanie Fitzpatrick]	Noted
4-1077	A	65:0		Figure 4.4.3 How useful is the Koch index? Either explain it adequately in the text (it is not at this stage) or else leave it out. [Melanie Fitzpatrick]	Accepted. Koch Index time series deleted
4-1078	A	65:0		poor quality figure reproduction [Michelle Koutnik]	Accepted. Figure improved.
4-1079	A	65:0		Fig. 4.4.3Difficult to print. Both the data points and the lettering on the graphic are fuzzy. Please improve quality of graphic. [Melinda Marquis]	Accepted. Figure improved.
4-1080	A	66:0		Figure 4.4.4 In this Figure the years between 1950-1960 are most likely within the spin-up times of the models – should these years then be excluded from the plot? Explain this in the text or truncate the plot. Make clear that these are all computations – it is ambiguous in the text as to whether K+G 2003 are from observations of some sort. [Melanie Fitzpatrick]	Taken into account. Models are generally spun-up in some way prior to the starting year shown on the figure, but to some extent the first few years (maybe 5, not 10) are potentially influenced by whatever the initial condition was.
4-1081	A	66:0		poor quality figure reproduction [Michelle Koutnik]	Accepted. Figure improved.
4-1082	A	66:0		Fig. 4.4.4 Remove stray close-parenthesis in caption after "anomaly," [Melinda Marquis]	Accepted.
4-1083	A	66:0		No explanation for each color curves in Fig. 4. 4. 4. [Takashi Yamanouchi]	Rejected. Aim of plot is to show general pattern only, and reader is referred in caption the the original source.
4-1084	A	66:8	66:8	Reference to Koeberle and Gerdes (2003) is not listed. [David Parker]	Accepted. Reference added.

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4-1085	A	67:0		Fig. 4.4.5 Units: cm s-1 [Melinda Marquis]	Figure has been deleted.
4-1086	A	68:0		Figure 4.4.6. Aren't around more than one NAO index? If this the case, then, please, specify which one was used, I guess it is Hurrell, J. W., 1995: Decadal trends in the North Atlantic Oscillation: regional temperatures and precipitation. Science, 269, 676-679. [Paolo Cherubini]	Taken into account – Hilmer and Jung used Hurrell NAO.
4-1087	A	69:0		Figure 4.5.1 Expand SLE and G+IC in words in the Figure Caption [Melanie Fitzpatrick]	Taken into account. Figures rearranged
4-1088	A	69:0		Fig. 4.5.1 Add labels to X and Y axes. Explain abbreviations in caption. [Melinda Marquis]	Taken into account. Figures rearranged
4-1089	A	69:6	69:7	Figure 4.5.1. Are the data for the entire globe? If so it would be useful to state it more explicitly. [Christof Appenzeller]	Taken into account. Figures rearranged
4-1090	A	69:26	69:27	These two sentences are not really well worded: the point is several recent observations (the acceleration of ice streams previously buttressd by ice shelves (Antarctic Peninsula, Jakobshavn Isbrae and possibly Pine Island Glacier), groundline retreat coupled with deep inland acceleration of ice streems and due to deep ocean warming of the Amundsen sea glaciers) tends to confirm old but debated theories over the role of ice shelf buttressing and ice stream stability. [William Hare]	Wrong page numbers. Comment presumably refers to the ice sheet section. The rewritten text addresses theses issues.
4-1091	A	69:28	69:32	The point is that recent observatons (of a "limited area" - which if it is meant PIG and Thwaites holds about 1.5 m of SLE in ice) and some modelling (Vieli and Payne) appear to support important parts of the mechanism hypothesized for the possible disintegration of the WAIS (ice shelf collapse leading to ice stream motion and rapid and possibly unstable discharge of ice). The question of whether collapse of the basins containing the PIG and Thwaites ect would affect the stability of the WAIS as a whole is another question but the point of the observations is that this could have implications should the Ross Ice Shelf disintegrate from eg surface meltwater formation which could occur by the end of the 21st century or changes in ocean temperature and circulation affecting eg the FRIS or Amery ice shelves. [William Hare]	Wrong page numbers. Comment presumably refers to the ice sheet section. The rewritten text addresses theses issues.
4-1092	A	69:32	69:33	Is this all that can be said about the level of risk: this is what was said in the TAR? Do the new observations and grounding line/ice stream model results tell us anything more about the risk as we understand: is it more or less than was said in the TAR? [William Hare]	Wrong page numbers. Comment presumably refers to the ice sheet section. The rewritten text addresses theses issues.

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4-1093	A	70:0		Figure 4.5.2 Is area-weighting (presumably for decrease in area towards poles) and/or spatial interpolation used for this plot? If so, include in description – as it is unclear how these values were calculated for different regions. [Melanie Fitzpatrick]	Noted. The comment is not clear but the Figure is rearranged
4-1094	A	70:0		Fig. 4.5.2 Add labels to X and Y axes. Explain abbreviations in caption. [Melinda Marquis]	Taken into account. Figures rearranged
4-1095	A	71:0		Fig. 4.5.3 In caption, refer to a, b anc c graphs. For c, consider a clearer label for Y axis. [Melinda Marquis]	Taken into account. Figures rearranged
4-1096	A	71:0		Axis label for Panel C should be revised. "# of glaciers" makes more sense. [Kenichi Matsuoka]	Taken into account. Figures rearranged
4-1097	A	71:10		(70-90 year) add: (70-90 per year) [Hartmut Grassl]	Taken into account. Figures rearranged
4-1098	A	72:0		Figure 4.5.4 How were the weightings 0.5, 0.1, 0.15 etc arrived at? This is not explained in the text. Change “combing” to “combining”. [Melanie Fitzpatrick]	Taken into account. Figures rearranged
4-1099	A	72:0		poor quality figure reproduction [Michelle Koutnik]	Taken into account. Figures rearranged
4-1100	A	72:0		Fig. 4.5.4 In caption, fix typo: Change "combing" to "combining." [Melinda Marquis]	Taken into account. Figures rearranged
4-1101	A	73:0		Fig. 4.5.5 Add labels and units to X and Y axes of all four graphs. In caption, delete "(appr. 1990)" because earlier in the sentence, it says already "around 1900." [Melinda Marquis]	Taken into account. Figures rearranged
4-1102	A	74:0		Fig. 4.5.6 Interpretation of data in graph ("Moderate summer discharge … due to …in particular" probably best discussed in text of chapter, rather than in figure caption. [Melinda Marquis]	Taken into account. Figures rearranged
4-1103	A	75:0		poor quality figure reproduction [Michelle Koutnik]	Figures in section 4.6 are all redone.
4-1104	A	75:0		caption is probably from the reference and then include unnecessary information (last sentense) and un-defined ATM. [Kenichi Matsuoka]	Figures in section 4.6 are all redone.
4-1105	A	75:5		You might mention that this is a map of Greenland [Vincent Gray]	Figures in section 4.6 are all redone.

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4-1106	A	75:5		Figure 4.6.1. - could be followed by new results from Johannessen et al.. (2005), their Fig. 1, as well as a figure showing the recent progression of melt areas over Greenland, from K. Steffen and coworkers. [Ola M. Johannessen]	Figures in section 4.6 are all redone.
4-1107	A	75:10		correct: "Gletscher" to "Glacier" [Hartmut Grassl]	Figures in section 4.6 are all redone.
4-1108	A	76:0		Figure 4.6.2 Expand legend “P-E” and “SMB” or explain in caption. [Melanie Fitzpatrick]	Figures in section 4.6 are all redone.
4-1109	A	76:0		Fig. 4.6.2 Explain abbreviations (e.g., P-E, SMB). [Melinda Marquis]	Figures in section 4.6 are all redone.
4-1110	A	76:5		Figure caption is too short. [Takashi Yamanouchi]	Figures in section 4.6 are all redone.
4-1111	A	77:0		Figure 4.6.3 Caption needs expanding to explain symbols and shading [Melanie Fitzpatrick]	Figures in section 4.6 are all redone.
4-1112	A	77:0		Fig. 4.6.3 Add to caption that "+" means area of increased mass of ice sheet, whereas "-" means area of decreased mass of ice sheet. Over what time period? [Melinda Marquis]	Figures in section 4.6 are all redone.
4-1113	A	77:5		Legend of Figure 4.6.3 needs more information. What is the meaning of the different blue shading? [Jefferson Cardia Simões]	Figures in section 4.6 are all redone.
4-1114	A	77:5		Explain in the legend that brown areas represent zones with some rock outcrops. [Jefferson Cardia Simões]	Figures in section 4.6 are all redone.
4-1115	A	77:5		Figure caption is too short, captions from original reference should be shown. [Takashi Yamanouchi]	Figures in section 4.6 are all redone.
4-1116	A	78:0		What is the purpose of this DEM? Seems superfluous to the text (it is actually inserted in a discussion of ice shelves not continental ice sheet thickness). Suggest it be removed. [Melanie Fitzpatrick]	Figures in section 4.6 are all redone.
4-1117	A	79:0		Fig. 4.6.5 Please put color bars where they can be seen, e.g., not superimposed on the images. [Melinda Marquis]	Figures in section 4.6 are all redone.
4-1118	A	79:0		Figure 4.6.5: These maps are difficult to see. [David Parker]	Figures in section 4.6 are all redone.

No.	Batch	Page:line From	Page:line To	Comment	Notes
4-1119	A	79:0		Fig. 4. 6. 5 is not easy to read, it should be more clear. [Takashi Yamanouchi]	Figures in section 4.6 are all redone.
4-1120	A	79:2		the colour bar is not included [Hartmut Grassl]	Figures in section 4.6 are all redone.
4-1121	A	80:0		Fig. 4.6.6 In (b), what does the dashed arrow refer to? [Melinda Marquis]	Figures in section 4.6 are all redone.
4-1122	A	81:0		Figure 4.7.1 is a useful and clear figure, but this reviewer questions how and why it was chosen. The IPCC assessments should be integrative, but this figure serves primarily as an example of what is discussed in section 4.7.2.2. It may be appropriate to include an entirely new figure that provides a graphical integration of information from more than one region. There is no need for color in this type of figure; a black-and-white rendering with appropriate symbolization would serve equally well. [Frederick Nelson]	Noted. Due to different length of time series, it is difficult to make a graphical integration.
4-1123	A	82:0		Figure 4.7.2 is a useful graphic, but it is used here only as an example. As a synthesizing document, the IPCC report should go a step or two further. The CALM program has produced several dozen publications and it may be appropriate to employ (or synthesize) some of the graphics from this international global-change monitoring program. [Frederick Nelson]	Noted. This is a composite figure with data from 31 Russian stations.
4-1124	A	82:6		correct: "Frauenfield" to "Frauenfeld" [Hartmut Grassl]	Accepted. Text modified.
4-1125	A	84:0		Figure 4.7.4 is difficult even for an expert to understand. It suggests unreasonable interpolation of data across years and seasons - very easy to take the colour scheme as representative of reliable and extensive data existing (for example from 1900s-1920s) when in fact the data is very sparse. What is the data source? Why not plot annual means instead in a much clearer and simpler representation. Suggest this plot is altered or removed. [Melanie Fitzpatrick]	Accepted. Figure improved.
4-1126	A	84:0		Fig. 4.7.4 Add label to color bar: Seasonally frozen ground. [Melinda Marquis]	Accepted. Figure improved.
4-1127	A	84:0		The datum used to derive positive and negative values in Figure 4.7.4 is not indicated in the caption. It should also be noted that this graph does not include seasonally frozen ground over permafrost (active layer). [Frederick Nelson]	Noted. It indeed includes active layer.
4-1128	A	84:1	84:8	I am not sure this figure is useful. In the corresponding text the comment is "there was a little change in the area extent of seasonnally…, page 37, line 8. Are there any way to simplify this figure and highlight the decrease seen in the last decade ? (see the higher left	Noted. Will make it consistent.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				part) [ERIC MARTIN]	
4-1129	A	85:0		Significance levels required on figure 4.8.1. [John Church]	Accepted. Text modified.
4-1130	A	85:0		Figure 4.8.1 Excellent summary figure - keep in report [Melanie Fitzpatrick]	Noted.
4-1131	A	85:0		Fig. 4.8.1 This summary graphic is very helpful. [Melinda Marquis]	Noted.
4-1132	A	86:0		Box 4.1 Figure 1 Excellent Figure – keep this in chapter 4 Change “VA” anf “FA” labels on plot to more effectively communicate ideas – ie. “including feedback” and “no feedback” [Melanie Fitzpatrick]	Noted. Box and figure deleted
4-1133	A	86:0		Latitude is preferred to be vertical. [Kenichi Matsuoka]	Noted. Box and figure deleted
4-1134	A	86:7		delete: "models" [Hartmut Grassl]	Noted. Box and Figure deleted.
4-1135	A	87:0		The caption does not adequately define the curves in this figure. [John Church]	Accepted. Figure and caption modified.
4-1136	A	87:0		Question 4.1 Figure 1 Labels needed (a), (b) etc Excellent Figure – keep this in the report. May need additional explanation for the lay reader. [Melanie Fitzpatrick]	Accepted. Figure and caption modified.
4-1137	A	87:0		Box. 4.1, Fig. 1Order graphs in same order as caption describes them, e.g. top to bottom, or bottom to top. [Melinda Marquis]	Accepted. Figure and caption modified.