Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-1	13	0	0	0	0	The chapter is still very good, and many changes have made it better. Because I reviewed it during the first round, I shall confine my comments to things that changes or issues that remain open from my first set of comments. Most of my first-round comments have been adequately addressed. [James G Titus, United States of America]	Noted
13-2	13	0	0			The chapter is still misusing the term "sea level". Sea level is the average water level. Therefore the term "extreme sea level" is a misnomer. Because other terms are available (e.g. flood levels, extreme water levels) this chapter should use them rather than propogage a misnomer that has not yet enterred the lexicon of all the nations that participate in IPCC. [James G Titus, United States of America]	Rejected. As suggested by the reviewer, we generally use "sea level" for Mean Sea Level, which removes shorter period variability, and for which the global average is Global Mean Sea Level. However, we point out in Section 13.1.2 that we also specifically consider "sea level" for high frequency changes in ocean surface height, which are referred to as extreme sea levels. We believe that the adjective "extreme" makes it clear that in this context, the use of "sea level" differs from "mean sea level" as used elsewhere in the chapter.
13-3	13	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. This is critical for chapter 13, being an 'integration chapter'. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - we will check for consistency within the chapter as well as between chapters.
13-4	13	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - we will follow this format.
13-5	13	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
13-6	13	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - will recfer to specififc chapter section.
13-7	13	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - reference to earlier assessments will be specific.
13-8	13	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
13-9	13	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
13-10	13	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II -	Done

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						Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	
13-11	13	0				There are several glaring problems with Chapter 13, Sea Level Change, which I pointed out in my FOD review, but which have not been adequately corrected in the SOD. [David Burton, United States of America]	Noted - see below
13-12	13	0				1. There are no full-length graphs of representative high-quality tide gauges. Fig. 13-17 has graphs going back only to 1970, despite the fact that the best tide gauges go back over 150 years, and despite the fact that the literature indicates that at least 60 years of tide gauge data are needed to determine a robust SLR trend (see http://tinyurl.com/slr60yrs for literature references). FAQ13.1 Figure 1a (p.89) has a few such graphs, but shrunk to the size of postage stamps, and only back to 1950. Good quality graphs of full-length tide gauge records from high-quality tide stations are absolutely essential for "grounding" the reader's understanding of sea level, in particular the (lack of) response (thus far) in rate of SLR to GHG forcings, and the amounts and timescales of typical fluctuation in relative sea level, and the variation in relative sea level trends between locations. The omission of such graphs appears calculated to hide the fact that, thus far, sea level rise has not increased in response to GHG forcings, and will surely be powerful ammunition for critics of the IPCC and its Reports. Here's a good example of such a graph, from one of the longest, best-quality tide stations in the world: http://tidesandcurrents.noaa.gov/sltrends/sltrends_global_station.shtml?stnid=120-012 [David Burton, United States of America]	Rejected. The numerous tide gauge records are properly the area of |Chapter 3 where they are extensively discussed . The examples shown in the two figures in Chapter 13, as well as in Figure 3.12, provide a good representation of the regional variability in sea level. One of the major points of these figures (and this chapter) is to illustrate that one cannot infer global sea level rise from individual tide gauge records due to regional variability.
13-13	13	0				2. There is pervasive confusion between satellite-measured sea levels (over the open ocean) and tide-gauge-measured sea levels (at the coasts). They are two very different quantities, and conflating them as if they were measurements of the same quantity is a gross error, which confuses the reader and calls into question all associated conclusions. [David Burton, United States of America]	Rejected - The ocean responds dynamically such that sea level rise from expansion of the deep ocean and mass changes cause water to flow onto the shelf and thus affect coastal sea level. We have now included a brief discussion of this process, with supporting references, in section 13.1.3. There is also very good agreement between tide gauge and satellite records over the period of common measurements. We have also added a section on fundamental definitions and concepts in an attempt to overcome this misunderstanding
13-14	13	0				3. There is no admission of the fact that, over the period during which there have been large GHG emissions (roughly, the last 2/3 to 3/4 century), the rate of coastal sea level rise, as measured by tide gauges, has not accelerated. See http://tinyurl.com/slracc1 for numerous literature references. (Likewise, the rate of mid-ocean sea level rise, as measured by satellites, apparently has not accelerated, though there are significant problems with the satellite sea level data.) [David Burton, United States of America]	Rejected. We address the question of an acceleration, and this is fully assessed in Chapter 3, where they conclude that it is necessary to look at records that extend back at least to 1900 so as to account for multi-decadal variability. Moreover, it is necessary to look at the contributions from individual components in order to understand the response of the integrated global sea level record to forcing. The observed rate of GMSL rise can be accounted for in various budget periods as a sum of contributions derived from observations and models of the contributions. The text has been revised in order to address these topics. The observatrions have been compared witht he models and both show similar rates and acclerations.
13-15	13	0				4. There is no mention of the fact that, over the nearly 20 years for which we have data, satellite measurements of global MSL have exhibited a deceleration in rate of sea level rise; nor is it mentioned that the various satellites differ significantly from one another in the rates of SLR they are measuring; nor is it mentioned that the satellite which had the broadest coverage of the world's oceans, Envisat, has had ex post facto revisions to its data so dramatic that the corrections more than tripled the "measured" rate of sea level rise. Instead, it is claimed that the satellites (as if they were unanimous!) have been measuring about 3.2 mm/yr SLR. That is misleading. Moreover, the reference to agreement between different data processing groups, without mentioning either the disagreement between the data from the different satellites or the huge	Rejected. The latest satellite measurements show that after a brief (~2-year) deceleration, sea level has begun to rise again and is consistent with the 20-year mean. This is an example of short-term variability supermposed on the longer term mean. We have now added a figure and corresponding text to discuss this most recent part of the satellite records. Regarding different satellites providing differing measurements of

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						calculated adjustments, conveys a deceptive impression that the reliability of the result is much stronger than it really is. [David Burton, United States of America]	SLR, this is also rejected. First, AR5 does not use data from Envisat, which does have problems with data processing. AR5 (Chapters 3 and 13) uses TOPEX, Jason-1, and Jason-2 to compute GMSL, because data from other missions have more problems and larger errors. Figure 13.3 shows comparisons of different groups processing the TOPEX, J-1, and J-2 data - the differences are not large. These issues are specifically addressed inthe chapter and chapter 3
13-16	13	0				5. There is pervasive confusion regarding the effect of ocean density changes on satellite-measured mid-ocean sea levels, and how it differs from (and does not affect) coastal sea levels. [David Burton, United States of America]	Rejected - The ocean responds dynamically such that sea level rise from expansion of the deep ocean causes water to flow onto the shelf and thus affect coastal sea level. We have now included a brief discussion of this process, with supporting references, in section 13.1.3.
13-17	13	0				6. There is pervasive understatement of the uncertainties regarding sea level projections. [David Burton, United States of America]	Rejected - there is very careful and considerable attention applied to uncertainties. The reviewer provides no specific examples.
13-18	13	0				7. There is extensive discussion of, and credence given to, Rahmstorf's discredited "semi-empirical models." The SOD says that there's no consensus about the reliability of that approach, but that grossly understates the problem. The truth is that the approach has been thoroughly discredited, and does not belong in the AR5. Rahmstorf, himself, has pecuniary conflicts of interests through his connections with reinsurance giant Munich Re. The material about semi-empirical models should be deleted. [David Burton, United States of America]	Rejected - the reviewer provides no reference to any literature that has discredited semi-empirical models. Our stated assessment of no consensus and low confidence in these models is a fair representation of the literature and expert opinion. To omit discussion of them would be ignoring an important part of the literature.
13-19	13	0				8. There is excessive reliance on modeling (which suggest dramatic accelerations in SLR should be happening because of the large increase in GHGs), and far too little reliance on measurements (which show no such accelerations in SLR), throughout. [David Burton, United States of America]	Rejected - there is considerable comparison of measurements with model results in order to assess the reliability of models used for projections.
13-20	13	0				9. The glossary is an excellent addition to the SOD! However, it still needs some work. The various cross-references ("see also" etc.) should be hyperlinks. Also, and more important, the definition of Mean Sea Level is incomplete: it makes no distinction between two very different kinds of global mean sea levels: averaged coastal sea level (measured by tide gauges), and the level of the open ocean (measured by satellite altimetry). The two kinds of global mean sea levels are affected in different ways, and to different extents, by different factors. To avoid confusion by the readers (and, probably the authors!), two different terms should be used throughout for these two different quantities: perhaps OOGMSL (Open-Ocean Global Mean Sea Level) and CAMSL (Coastal Average Mean Sea Level). [David Burton, United States of America]	Rejected - see response to comment 13-13.
13-21	13	0				10. Despite the fact that the central purpose of this report is forecasting climate, and the purpose of this chapter is forecasting sea level, there are no references at all to any of the literature from the discipline of Forecasting. In fact, the word "forecast" doesn't even appear in the SOD. http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1099-131X [David Burton, United States of America]	Rejected - the central purpose of the report involves projections of sea level change.
13-22	13	0				The most important goal for AR5 should be to restore the credibility of the IPCC. The SOD will not do that. A report which goes on for 110 pages about sea level, without showing any proper graphs of real sea level records, and without ever admitting the most basic and important facts about sea level rise, will not be taken seriously by anyone who is not already a "true believer." Everyone else will justifiably conclude that the reason graphs like this http://tidesandcurrents.noaa.gov/sltrends/sltrends_global_station.shtml?stnid=120-022 and this http://tidesandcurrents.noaa.gov/sltrends/sltrends_global_station.shtml?stnid=680-140 do not appear is that they make it too obvious that sea level rise is not accelerating, and the reason the Report doesn't admit sea level rise hasn't accelerated in response to anthropogenic GHGs is that ideology was allowed to trump	Rejected - see response to comment 13-12.

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						evidence. [David Burton, United States of America]	
13-23	13	0				It is difficult to understand why sections on historical sea level change would reside in Chapters 3 and 5 rather than in this chapter, which specifically focuses on sea level change. While SL changes are clearly important oceanic observations and palaeoclimatic reconstructions, I would have thought that these considerations would be trumped by the presence of Chapter 13. I suppose the rationale is that Ch 13 deals with processes and attribution, whereas Ch 3, (4), and 5, deal with observations or reconstructions. I would still have preferred to see all of the evidence in one chapter when it is as important as this one. In part, this is because I regard SL change as a first-order impact of climate change rather than a component of climate change, which underpins many critical higher-order impacts described in Working Group II. That said, the authors of these chapters appear to have tried to co-ordinate their sections, though it is odd that I noted only two names that are in common between chapters 3 and 13: Mark Hemer (CA on both) and Philip Woodworth (RE for Ch 13 and CA on Ch 3). Moreover, in spite of the efforts of the authors, some statements emanating from those two chapters are still not fully consistent (see my ES comments). [Timothy Carter, Finland]	Noted. Chapter 13 is a synthesis chapter, and thus is synthesizing the information and assessment of historical sea level from Chapters 3 and 5.
13-24	13	0				My thanks to the author team for a clear review of the post-AR4 literature and, in particular, for the nuanced evaluation of the basis for and limitations of semi-empirical models. The incorporation of ice sheet dynamic losses based on recent advances is welcome, as is the coverage of regional variance and extremes. [Donald Forbes, Canada]	Thank you.
13-25	13	0				To illustrate the comment about Chapter 9, the projections of the supposed anthropogenic forcing on sea level rise throughout this chapter appear exaggerated essentially because the main forcing, the greenhouse effect of anthropogenic CO2, is itself exaggerated as discussed above and in ROFOD as well. Consider also that the projections do not fit the time dependence of the level of the Pacific Ocean level which remained roughly flat since 10 years as measured by JASON (in absence of GIA and other questionable corrections). One may also wonder, therefore, about the slope of the satellite ENVISAT data which have been multiplied in April 2012 by an enormous factor of 4.6, from 0.5 mm per year to 2.3 mm/year. How to convince of the reliability of the new slope which intrinsically would imply an enormous error on the preceding one ? [François Gervais, France]	Rejected. The reviewer provides no reference to literature which supports the notion that the greenhouse effect is exaggerated. We also note that future projections will not overlap with with historical observations. Lastly, AR5 does not use results from Envisat (see response to comment 13-15).
13-26	13	0				Cont. – Anyway, it is instructive to consider the average of altimetric data in the available file MSL_Serie_MERGED_Global_IB_RWT_NoGIA_NoAdjust.txt. A simple linear regression to fit the 1993-2006 data gives a slope of 3.2 mm per year for the sea level rise. This is consistent with the ascending phase of the oscillation of 60-years period observed in GMSL data of Fig. 3.14, and in many other proxies as well, as detailed throughout this reviewer report. However, note that the slope is reduced to 2 mm per year in the period 2006-2012 and even to 1 mm per year in the period 2008-2012, viz. less than during the 20th century. This is consistent with the onset of the decreasing phase of the oscillation of 60-years period observed in GMSL data of Fig. 3.14. [François Gervais, France]	Rejected - because of natutral variability, we do not think one can place any confidence in rates based on just several years of measurements. Regarding the low rate referred to for 2008-2012, note that the satellite measurements that now extend to 2013 show a rapid rate of rise, We have added new text to discuss this recent variability in sea level and its likely cause. Recent literature has also explored the principal reasons for the larger rate of recent rise.
13-27	13	0				FAQs in this chapter are embedded within sections of the chapter rather than placed all together at the end like other chapters. Suggest making this consistent and placing the FAQs at the end. [Government of Canada]	Noted. The arrangement of the FAQs in the chapter will be consistent with the full report.
13-28	13	0				While I find the discussion of the semi-empirical models to be correct I find the authors bias too much towards having confidence in the process-based ice sheet modelling as there is no complete modelling of the ice dynamics. The SMB is well treated and there is a high confidence in the results from the SMB. [European Union]	Rejected. We assign specific levels of confidence to each of the various process-based models.
13-29	13	0				This chapter is in general in a good state, however, some issues need to be addressed. In particular the numbers of the contribution of ice sheets to sea level change are not consistent within the chapter and with Chapter 4. This needs to be revised and reviewed again. The lack of projection using process-based modelling for the contribution of ice sheets to sea level changed was discussed to some extend. However, I suggest discussing the requirements for future modelling approaches more in detail, as this allows the reader to understand how far off the projections currently might be and what complexity semi-empirical models try to capture. As an example, the capability of models to capture grounding line migration was studied in a model intercomparison project MISMIP3D (Pattyn et al.,	Accepted - we will make more use of MISMIP. Re: inconsistancy: These will be addressed, but in the case of glacier observed loss rates, because of the lack of complete data for 2010, the reporting periods for glaciers cannot be extended past 2009, so the comparisons of glacier rates with other components cannot be perfectly parallel. However, this wasn't explained in the text. This will be corrected and will

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						submitted). I suggest inserting an assessment of their findings in order to show the current state of the art and the challenges for AR6. Furthermore, the section dealing with SMB modeling could enhance its outcome by assessing more in depth the grid-size dependence and the required scale for capturing processes driving melting and refreezing. The general goal would be what scale is required for process-based modeling of the SMB as a recommendation for future modeling approaches, including AOGCMs. I would appreciate a bit more in depth discussion about the ability of the semi- empirical models to give reliable projections. The numbers of those models are given in great detail, while the assessment is often done very briefly [European Union]	clearly noted the text. Re: assessment of semi-empirical models, we disagree that it was done very briefly when there are ~2.5 pages devoted to this very thing.
13-30	13	0				The substantive change between this chapter and AR4 is the attempt to quantify the contribution of large ice sheets, and in particular the marine ice-sheet instability. This is an important step forward, but there is still a wide disparity in the results. My belief is that this has a lot to do with which physical processes are included in the model, out of grounding line mechanics, the shelf/buttressing representation and how the shelf might be affected by melt from underneath. There is insufficient discussion in the chapter about this, and how it might be affecting uncertainty; in my view, improving these areas of models is crucial to getting better predictions, and it is important to be clear now about the omissions of various models, so that there is some guidance on how to focus efforts. [European Union]	Noted - we agree with this sentiment but these is considerable discussion about this in the chapter text, box and FAQ.
13-31	13	0				This chapter should concentrate on observation, understanding and projection of mean sea level, and possibly be a bit shorter. In some parts it looks rather like a self standing scientific paper than a summary analysis of publications. [Government of France]	Noted - we attempted to shorten the chapter but there are many issues to doscuss.
13-32	13	0				It is difficult to understand why mean sea level observations are addressed both in Chapter 3 (3.7) and in Chapter 13. Considering that measurements of sea level are subject to polemics (e.g. Dörfer), it would be wise to capture the observational aspects (incl. references and VLM impact) and evidence in one single chapter. This may lead to exclude mean sea level from "ocean: observations") and to address the full subject in Chapter 13. [Government of France]	Noted - see response to comment 13-23.
13-33	13	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	The information is now proded as afootnote. This information is provided in Chapter 1, and space limitations prevent the luxury of repeating it in each chapter.
13-34	13	0				There are so many abbreviations that it is hard to keep them all in mind . The chapter needs a glossary of abbreviations. [Government of United States of America]	Noted. There will be a glossary of acronyms for the entire volume. Each acronym will be provided parenthetically and the term it represents spelled out at first usage in each chapter.
13-35	13	0				Figure references are missing. [Government of United States of America]	Unclear what this comment is referring to.
13-36	13	0				The chapter writing is difficult to read. There and many parenthesis in sentences that make reading difficult and many long sentences. The writing should be more concise. [Government of United States of America]	Accepted - there was a concerted effort to shorten the chapter (not particularly successful) and make the writing more concise.
13-37	13	0				The figures are not well integrated into the chapter text. Some of the figures are mealy placed in parenthesis in the text. Each figure should be identified in the text with a discussion of the major points revealed by the figures. [Government of United States of America]	Taken into account - this will be done to the extent possible.
13-38	13	0				The chapter did not consistently use itallics for the conclusionary remarks. There are several instances of the use of "likely", for example, where it is not italisized. Chapter 3 should be used as a model for the structure and use of statements about certainty of the current state of knowledge. [Government of United States of America]	Accepted - attempted to italicize the likelihood language .
13-39	13	0				The chapter relies heavily on the discussion of model outputs with minimal correlation back to observations. The discussions could be strengthened by more rigorously including observations to support the model results. [Government of United States of America]	Rejected - there is considerable comparison of measurements with model results in order to assess the reliability of models used for projections This has also been strengthened.

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13-40	13	0				I note that many references cited are “Submitted.” Is it IPCC policy to include such references? This means that the report is not based only on peer-reviewed results. Referees cannot comment on the results of these papers, since they are not yet publicly available. [James Gower, Canada]	Noted. All papers listed as "submitted" are required to be "accepted" or "in press" by March 15, 2013, in order to be included in the chapter.
13-41	13	0				Some sections are very heavy on acronyms (and/or initializations) and hence much harder to read. Most acronyms, except perhaps those commonly used throughout the document, need to be spelled out. [James Gower, Canada]	Noted. There will be a glossary of acronyms for the entire volume. Each acronym will be provided parenthetically and the term it represents spelled out at first usage in each chapter.
13-42	13	0				Acronyms SLE and SLR are used at various points without definition. Text needs to spell out, e.g. “sea level rise.” [James Gower, Canada]	Accepted. Noted. Each acronym will be provided parenthetically and the term it represents spelled out at first usage in each chapter.
13-43	13	0				Very interesting chapter, really adding to the report. Difficult subjects, eg how to interpret semi-empirical model results well covered. Unfortunately I ran out of time partway through… [Gabriele Hegerl, United Kingdom]	Noted.
13-44	13	0				Given the challenges involved in modeling ice sheet behaviors, it's not obvious that current process-based models should be expected to perform better than semi-empirical models. It's therefore not clear why the chapter choses to relegate these models to second-class status, excluding them from key figures (13.8-13.10, 13.21) and tables (13.5 and 13.8). I understand the desire to keep process-based and semi-empirical estimates separate, so I would suggest adding separate bars/curves to the figures and a row to the tables to indicate the semi-empirical projections. [Robert Kopp, United States]	Taken into account. A new figure comparing projections using semi-empirical models has been added.
13-45	13	0				In my opinion, quantitative estimates of the ice sheet dynamic contribution must be made and the authors should be congratulated for their brave steps in that direction. While criticism of the judgements used to construct projections is inevitable, it is less important that they are "right" (as even other sources of SLR -- e.g. glaciers -- are subject to structural errors) than that they are traceable. The chapter is not organized in a manner that is conducive to this traceability. I am concerned about the breakout of the projections -- and the assumptions that are chosen to generate them -- as an appendix. My preferred organization would be to move the extensive text of 13.4 (except p 34 lines 28-48 and p 40 line 4-40) to the appendix. I would then combine 13.5 and the appendix with p 34 lines 28-48 and p 40 line 4-40 to document the assumptions made in the projections and the judgements as to which studies inform the likely range. My guess is that it may be difficult to change the structure of the document. But I strongly suggest that the chapter includes a table for both Greenland and Antarctica that explicitly states which studies they consider in denoting the "likely" range of outcomes. There is an analogous table used in Chapter 4 which assigns a value H, M, L to the reliability of data. It's unclear to me from the text which simulations/methods are included in their assessment of likely. Assuming the main text of 13.4 and 13.5 remains where it is, the table can serve as an organizing tool -- i.e. discuss high/medium/low reliability studies together, along with the judgement as to why they are in the location they are in. [Christopher Little, United States of America]	Partially Accepted. Those sections in the chapter discussing projections are being reorganized to make it clear how the projections were developed. However, the discssion in section 4 is important to the assessment.
13-46	13	0				As always, initial drafts are too long and I expect that subsequent versions will be more concise. Several concepts are repeatedly defined and described, and this opens the door up for contradictions. This is of particular importance w/r/t to the traceability of subjective judgements involved in projections in this chapter. Stating them once clearly, and clearly separating them from the literature review is vital. [Christopher Little, United States of America]	Taken into account. An attempt to shorten the chaper was made. .
13-47	13	0				Suggest intense efforts to rationalize scope of work and conclusions with Chapter 4. Seems like many sections of Ch 13 could be shifted and/or consolidated in Ch 4. [Christopher Little, United States of America]	Rejected We have not changed the framework for the two chapters. Chapter 13 is a synthesis chapter, and must stand alone, but assessments from observtions are based on earlier chapters.
13-48	13	0				RE: judgements about what comprises the likely ranges of ice sheet dynamic contributions to SLR (p 34 lines 28-48 and p 40 line 4-40). I would include additional discussion in the main text on why certain models/methods are included/not included and how they are parameterized in the projections (i.e. answering questions like: are only process based models able to be used for quantitative estimates? How are regional models and continental scale models combined? What constitutes a “reasonable” forcing? Why can we	Accepted. We have made an attempt ot do this.

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						generate a likely range – down to a 17% exceedance -- from the models that were chosen, but not an upper bound, or a full pdf?). This last question may revolve around the difference between likelihood and confidence – if so that could be revisited briefly here in the context of this application. Regardless of where a detailed discussion of these subjective judgments are located, they should be made explicit. Currently some studies are discounted more than others, it's up to the reader to imply how heavily they're weighted and why that was decided. [Christopher Little, United States of America]	
13-49	13	0				This is a very comprehensive chapter that clearly sets out the key processes, the state of the science and the uncertainties associated with sea level rise projections. [Kathleen McInnes, Australia]	Thank you.
13-50	13	0				In addition to providing SLR projections for different times, it would also be useful to provide the results from the perspective of a threshold exceedence of particular sea level rise. [Kathleen McInnes, Australia]	Noted. We have discussed this issue at length but decided not to include these values.
13-51	13	0				From the impacts community point of view, it would be good to be able to state if possible an upper threshold that GMSL by 2100 is very unlikely to exceed. Often it is low probability but high impact scenario that may be highly relevant for impact assessments within some sectors. [Kathleen McInnes, Australia]	Taken into account. We have added new text to address this issue, and conclude that there is insufficient confidence to assign an upper limit, as discussed inthe text.
13-52	13	0				An overall comment is that the treatment of semi-implicit models is not well integrated into the chapter structure as it stands and their role/purpose in this assessment is not clear. Given the larger uncertainties that this approach appears to generate, perhaps confidence in the methodological approach needs to be assessed in 13.5.1.2 as low with a clear statement that they are therefore not being used in the projections. Maybe an additional subsection that assesses the general methodological approaches to developing projections (e.g. process based, semi-empirical, analog) is needed. [Kathleen McInnes, Australia]	Taken into account. We believe that the treatment of semi-empiical models is well integrated into the chapter insofar as we conclude that there is no consensus about their reliability, which contributes to our assignment of medium confidence for our projections. We have added a figure that compares projections by semi-empirical models with those from process-based models.
13-53	13	0				Chapter 13 is well written and make a thorough review of the recent litterature on Sea level Change. It is a really good job in my opinion and I have actually very few comments. [Benoit Meyssignac, France]	Thank you.
13-54	13	0				I congratulate the authors of chapter 13 for their efforts, and especially for advancing the assessment of future sea level rise. Nevertheless, sections 13.4 and 13.5 require further clarification. The discussion in these sections and especially on p.40 obscures some of the key reasons for the judgments about likely ranges and how they were selected, rather than providing a traceable, transparent account of the expert judgments involved. The appendix doesn't help in this regard. It is critically important that this shortcoming be rectified. Finally, it is disappointing to see yet another switch in WGI use of baseline and projection-year dates as well as uncertinty range (likely vs very likely). Adding the A1B results in the text to table 13.5 would help immensely. [Michael Oppenheimer, United States of America]	Accepted. Sections 13.4, 13,5, and the Appendix are being reorganized to make it clearer how the projections were developed.
13-55	13	0				The revised version of the chapter is much better than the original one. Consequently, I have only some minor technical comments. [Mirko Orlic, Croatia]	Thank you.
13-56	13	0				As I waded through Chapter 13 I was very positively impressed by the thorough review of different processes and methods, including the semi-empirical methods, and the fact that an estimate of ice sheet dynamical changes is now included in the total predictions of sea level rise. However, in some sections I had a feeling that the main messages are drowned by the amount of detail (for example, sections 13.4.3 and 13.4.4). Thus, I generally suggest clarifying the main messages and their justification wherever possible. [Hilkka Pellikka, Finland]	Taken into account. We attempted to shorten the chapter with more concise discussion particularly in the sections identified by the reviewer.
13-57	13	0				FIRST MAIN COMMENT to Ch 13: by the time I had arrived to page 46, I had become increasingly surprised that all the uncertainties seem to be symmetrical. This is a feature throughout this chapter, but I find it surprising because it looks quite artificial to me. Surely a distribution of solutions from the models would not necessarily be symmetrical (especially given that climate sensitivity and feedbacks are not symmetrical)? This may be a simplification that you have introduced, but that simplification is never expressed. I think that some attention is needed to explain the nature of the distribution of uncertainties, rather than pretending that all uncertainties are symmetrical about the mean. This may seem a rather detailed, science-focussed comment, but proper representation of uncertainties is important to achieve the best communication of aspects that are	Accepted that more clarity is required. Symmetric errors are not assumed - in fact some are assymetric.

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						understood versus aspects that remain highly uncertain. At least, the selected approach should be explained somewhere. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	
13-58	13	0				SECOND MAIN COMMENT to Ch 13: the chapter seems to automate any reference to Meehl et al. (2012 Nature Climate Change). This is surprising to me given that that paper deals with some very similar aspects to this chapter, and that it must have been submitted, reviewed and revised well before the cut-off deadline. The paper has attracted a lot of attention, and it would be odd (to say the least) if it remained outside the assessment in this chapter. I do not know what the motivation was to keep this paper outside, but I do know that serious questions will be asked if this were kept in that way. I recommend that it is given significant attention wherever that is warranted. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	paper now referred to
13-59	13	0				This chapter overemphasizes physical models for sea level rise projections and underemphasizes semi-empirical models. Each approach has significant strengths and drawbacks, and should be presented as such in balance, with projections from each approach featured in the Executive Summary and within the figures. The key challenge with physical models – which remain the ideal approach – comes from modeling ice sheet behavior. Our ability to do so feels oversold in the chapter, especially the Executive Summary, which hails the advance of including contributions from ice-sheet flow, without explicitly admitting that modeling has not advanced far enough to offer different projections for different emissions scenarios, a critical shortcoming. Ice sheet decay is by far the largest term in potential sea level rise, so projections with special weakness in this area need to be appropriately qualified. Considering this weakness, it is curious that simple extrapolation of recent ice sheet decay trends appears to have been excluded from consideration, as employed, for example, within the US National Research Council (2012), resulting in ranges substantially larger than the value used in the second order draft. In the same spirit of humility with which physical model based projections should be treated, given the state of ice sheet modeling, I would argue that we cannot exclude semi-empirical projections – especially considering that they achieve superior hind-casts of sea level than physical models do. More strictly physical models are more appealing at a philosophical level, but the fact is that we are extrapolating into uncharted waters no matter what approach we take, given the speed and extent of warming we project. Suppressing projections from semi-empirical models suppresses the extent of our uncertainty. There may be no consensus on their reliability – but there may also be no consensus on the utility of incomplete physical models. There is mounting evidence that IPCC reports have a bias toward under-projection. This may seem “conservative” from a scientific perspective, but it also seems reckless from the perspective of advising nonscientists concerned with safeguarding lives, property and livelihoods. This chapter should more evenly present both sets of projections with their respective strengths and weaknesses clearly stated; convey humility about the state of our knowledge; and help policymakers prepare for a future where seas may rise either more quickly or more slowly than we expect. References: US NRC 2012. Sea-Level Rise for the Coasts of California, Oregon and Washington: Past, Present and Future. Committee on Sea Level Rise in California, Oregon, and Washington; Board on Earth Sciences and Resources; Ocean Studies Board; Division on Earth and Life Studies; National Research Council [Benjamin Strauss, United States]	Rejected. We provide a balanced discussion of the strengths and weaknesses of both modeling approaches, from which we assign medium confidence to ice-sheet models and conclude that there is no consensus on the reliability of semi-empirical models. We now more explicitly address the fact that there is too little evidence to have different projections from ice sheet models for different emission scenarios. We reject using the approach of a simple extrapolation given that we do not understand the extent that the trends may be influenced by variability. Finally, the superior hindcast of semi-empirical models is by design and is thus not a validation of them.
13-60	13	1	1	1	1	This chapter is looking in very good shape. The approach to discussing semi-empirical (SE) modelling is an accurate reflection of sentiment in the community. The dissussion of the history of publication, comment and response regarding SE models justifies the statement that there is ongoing disagreement regarding them. I don't think it is as easy to use the term 'lack of consensus because ice dynamics also lacks consensus. I wonder if part of the difficulty here is dealing with an approach to understanding the hazard posed by SLR which may fit in better to WG2 within WG1. As the 'physical science basis', WG1 is not well suited to dealing with the SE approach that is presented here. On the other hand I don't think a stronger case could be made for moving beyond SE modelling than is made in CH13. It may be worth more explicitly noting that the discrepancy between SE and physical approaches has motivated physical modellers to redouble their efforts. That would recognise that SE models have contributed indirectly to the progress in physical modelling noted in the report. [Mark Siddall, United Kingdom]	Rejected - we disagree that the development of physical models has been motivated by semi-empirical models. Instead, the primary motivation has been the observations of ice dynamical changes.

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13-61	13	1	1	1	1	I imagine that there has been considerable discussion among authors on whether or not to include a figure of SE models. This is a very hard call and I see arguments both ways. Clearly including a figure gives a powerful communication of results which the text argues against and risks giving too much weight to the SE approach. On the other hand the lengthy discussion of SE estimates in the text is clunky and would be better presented as a figure. Can the disagreement in the approach be expressed within a figure. For example the physically based estimates could be in bold, the SE estimates feint. Full vs dashed lines could be used and text expressing the skepticism explicilty shown on the figure. This approach feels less like an effort to reduce communication of the SE result and more like expressing what is in the text. Any effort to reduce communication of the SE result could be open to criticism. [Mark Siddall, United Kingdom]	Taken into account. We will be adding a figure comparing projections from semi-empirical models to those from process-based models.
13-62	13	1	1	65	57	What is the notation for year (or per year) 'yr' and 'a' are both used here. [Neil White, Australia]	Taken into account. We will be consistent in this notation.
13-63	13	1	1	70	30	The . Chapter should be about Sea Level,and it should describe the observations and their reliability. Instead the whole subject has been distorted in order to build a case that Sea Level Change complies with the absurd IPCC climate models.tt attempts to claim that change must be assocaited with increased melting of ice, but fails to note that there has been no increase in the supposed global temperature, as measured by the so-called mean global surface temperature anomaly for over ten years,. There have never been any surface temperature measurements on ice or glaciers and the most likely explanations of any melting are either changes in ocean currents (with the Arctic) or changes in precipitation. [Vincent Gray, New Zealand]	Rejected. These are unsubstantiated statements. Changes in sea level are due to well-documented measurements of ice melting as well as other causes, as thoroughly assessed in Chapters 3 and 4.
13-64	13	1	1	70	30	For land surfaces the "Sea Level" is Relative, that is to say it is the difference between the level of the sea and the position of the equipment attached to the land..Both the sea and the land change from time to time and from one measurement to another.The level of the sea may change because of storm protection measures, dredging of the harbour, or measures to increase local water level to enable larger ships to enter. The land position can change in many ways. The extent of the land may increase by reclamation frpm the sea, or decrease by erosion. It may subside from removal of mnierals or ground water or from weight of buildings.The equipment and its ground attachment tend to be damaged by storms and replaced in a different place. Many records are fragmented for this reason. Most of these changes cause an upwards bias, so it is wrong, as is claimed in this Chapter, to claim that they are necessarily related to changes on the climate. or to supposed increases in the level of the ocean caused by melting ice..It also means that the comprehensive amalgamation of sea level "changes"carried out in this Chapter is not a reliable guide to future sea level. In any case it is wrong to attempt to derive "trends" from time series where every point in the graph is different and where the ealiuer figures are the least relaible. The most modern measuring equipment (SEA FRAME) and the levelling equipment, based on GPS availability gives greater reliability means that the best method for future prediction is to extrapolate the last few relaible and uniform fifures. Examples are the recent Pacific Island series, described at http://scienceandpublicpolicy.org/images/stories/papers/originals/southpacific.pdf, which show no change for the past ten years afetr the GPS levelling was installed. A similar result can be found for Australia.The claim in this Chapter that sea level is rising is unsupported by individual measurements carried out by reliable equipment. [Vincent Gray, New Zealand]	Rejected - all recent work has accounted for these issues. Also, we emphasize throughout the chapter that one cannot derive a global record of sea level from individual records, and that regional variability can strongly modulate the global signal. We have added a section on fundamental definitions and concepts.
13-65	13	1	1	70	30	Accuracy figures are omitted for most of your figures. Previous IPCC Reports gave very high inaccuracy levels for sea level measurements [Vincent Gray, New Zealand]	Rejected - unsubstantiated claim regarding inaccuracies.
13-66	13	1	1	110	70	I wanted to make a general comment here to congratulate all the authors on producing such an authorative and comprehensive draft, given the short timescale and huge number of papers submitted close to the 31/7 deadline that you have incorporated in the m/s. My comments should be taken in the light of the acknowledgement of the tremendous effort involved. [Jonathan Bamber, United Kingdom]	Thank you.
13-67	13	1	1			The authors are commended for providing an excellent overview over the current knowledge about sea-level rise. I have few small technical comments of secondary importance, provided at the appropriate places. However, I see fundamental problems with the treatment and communication of uncertainties in future sea-level rise, which I will try to explain below. 1. The draft provides only a "likely" range for future sea-level rise, unlike the AR4 which gave a “very likely” range. This is most directly evident when comparing the new Fig. 13.8 to the old Fig. 10.33 of the AR4, where	Rejected. The AR4 range in Fig 10.33 is a model spread only. They explicitly stated they were "not able to assess the liklihood". Thus the AR5 presentation of a likely range is an advance since the AR4. We have extensively discussed the issues of a very likely range and an upper bound. Despite significant advances since the AR4 allowing the specification of a likely

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						the AR4 graph shows 5-95% ranges and the equivalent AR5 draft graph gives “likely” ranges. A key statement on this is found on page 51: “Our likely ranges are narrower, in order to be more useful, but they are consequently accompanied by lower confidence.” I am afraid this is a great misunderstanding of what is “useful” to users of the IPCC reports. I have a lot of contact with coastal managers, and I can assure you that a “likely” range is not what is needed in coastal planning. A "likely" range is basically useless for coastal planning since it implies a 17% chance of being exceeded (even if the range is accurate as such), which is an insufficient safety level for practical planning purposes. IPCC is refusing an important societal demand here. At the very least the report (including the SPM) should provide a "very likely" range, as it did in the AR4. Preferably also a “plausible upper limit”, like the US Army Corps of Engineers provides in its coastal guidance (2m by 2100). Different users have different levels of risk aversion - you might be happy with a 5% chance of your holiday house getting flooded, but not for critical infrastructure like a port, airport or nuclear power station, which also typically have the long planning horizons and life times that require sea level rise to be taken into account. 2. Quite apart from the lack of practical usefulness, I think the switch from very likely to likely is extremely bad from a public communications and transparency point of view. After the IAC review of IPCC the procedures should become more transparent, not less. This switch, however, obscures the fact that uncertainties are now much larger than presented in the AR4. Some people might even wonder whether this is in fact why this was done. The excuses for not providing a “very likely” range, found scattered around the chapter, sound rather flimsy and indeed just like excuses. The public is certainly not going to understand the subtleties, the media will simply compare the new range to the old range without qualification. But by the switch to “likely” range and a different time span of the projection (now 95 years, in the AR4 it was 105 years), this comparison is made difficult to misleading and the numbers are artificially brought down. 3. This practice obscures the rather large differences in the AR5 projections, which are in fact much higher than the AR4 projections for the same scenario. As stated on page 47, for A1B the old range was 21-48 cm, while the new likely range is 44-75 cm. This hardly overlaps with the old range and on average is almost twice as high. That really is a major finding of the AR5! For the same scenario you now expect almost double the sea level rise! In the interest of transparency, and since differences to the previous assessment are supposed to be highlighted, this should be prominently stated, including in the SPM. 4. The report displays (again) in my view an unwarranted over-confidence in the process models. E.g. on page 50, where the different approaches are compared, about process-based projections one merely finds the statement: “Confidence in this approach comes from our understanding of the modelled physical processes, the consistency of the models with wider physical understanding of those processes as elements of the climate system, the agreement of modelled and observed contributions, and the agreement of observed and modelled GMSL”. So basically all is perfect: we understand the processes and the models reproduce the past sea-level rise. One has to look elsewhere in the chapter to find more honest statements like: “Before we can project outflow over the 21st century with any confidence, we need to better simulate ice flow” (p.43) – so maybe the processes can’t be modelled so well after all? Or the fact that the sum of modelled processes accounts for only 70% of the observed 20th Century sea-level rise (p. 23). What if it will also account for only 70% of the 21st Century sea-level rise? Then the actual rise would end up 43% greater than the projection. Just for the fun of it: if you add 43% to the RCP4.5 projection range from Table 13.5 it becomes 59 – 102 cm. Now you’re right in the semi-empirical range shown in Table 13.6 for this scenario. I am not suggesting this is the “true answer”. But the “big picture” of this chapter is: process-based projections are now much higher than in the AR4, which goes a long way towards reconciling the discrepancy between process-based and semi-empirical projections. But unlike semi-empirical models, the process-based models still underestimate 20th Century sea-level rise. This “big picture” is rather well hidden. A cursory read of the chapter gives a completely different impression. 5. The uncertainties are now larger than they were in the AR4, but the public is given quite the opposite impression. The range for the highest emissions scenario is 58 +/- 16 cm. Who really believes that we can forecast sea-level rise for such a massive, unprecedented warming to within less than +/- 16 cm? I think the draft very seriously understates the true uncertainties that we have about future sea-level rise, and this is	range in the AR5, we have come to the assessment that the science to specify a very likely range is not available in the scientific literature at the present. An upper bound requires the specification of a confidence value to accompany it. Such an upper bound would be in the upper tails of a PDF and we have insufficient knowledge to specify such a value and tha associated confidence level.

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						because of the switch to a “likely” range and shorter projection interval, the mentioned overconfidence in process models and thus the exclusion of semi-empirical models from the forecast range. The IPCC draft dismisses all the higher results from semi-empirical models on the grounds that "current scientific understanding is insufficient for evaluating the probability of higher values" (so it says in the SPM) - this is why these results are simply not included in the projected uncertainty range. This is illogical - I would have thought that if we're uncertain about these models, then this logically is something within the current uncertainty. I think the semi-empirical models have their limitations (as the process models do) but they have a certain amount of credibility, e.g. via the validation studies mentioned. Just writing (p. 50) that “there is no consensus about the reliability of semi-empirical model projections” is no reason to exclude them, because the same can also be said about the process-based projections. Note for example that the "ice dynamics" contribution from Greenland and Antarctica included is scenario-independent - i.e. IPCC assumes it is going to be the same, regardless of whether we get 1 or 5 °C global warming! As if continental ice discharge does not care about warming ocean waters, loss of ice shelves, meltwater percolating down etc. That is clearly unrealistic (as is said in the chapter somewhere) and simply reflects that our understanding of these processes is so limited that simple ad-hoc assumptions are being used - but why should such assumptions and the semi-empirical mountain glacier melt estimates be included, but well-calibrated semi-empirical models of sea-level rise be excluded? I would strongly advocate that IPCC uses a more even-handed approach, that at the same time is more honest about the overall uncertainties that we still have about future sea level, and follows assessments like the US National Climate Assessment, the coastal guidance of the US Army Corps of Engineers or the recently published World Bank Report in presenting a blend or synthesis of both modelling approaches, as the overall uncertainty range. [Stefan Rahmstorf, Germany]	
13-68	13	1		110		My overall impression about this chapter is very positive. It is balanced in presentation of the progress and remaining problems. Lots of new staff compared to AR4. My only concern is numerous "submitted" publications. Naturally, some of them will not be accepted before the deadline. This would leave some statements without backing from perreviewed publications. [Andrey Ganopolski, Germany]	Noted. We are aware of the deadline regarding papers being accepted and the need to revise the chapter if paper is not accepted in time.
13-69	13	1		110		I find this chapter to be a useful statement on our current understanding of sea-level rise that addresses to varying degrees all the components of sea-level rise. I have reviewed it from the perspective of users of the information i.e. impact and adaptation analysis. In this regard, there are some significant improvements and clarifications that are possible. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Noted.
13-70	13	1		110		I am surprised that the paper by Pickering et al (2012) on changes in tidal range due to sea-level rise in north-west Europe is not in the references as I think this is an important issue which might influence extreme sea levels. Pickering, M.D., Wells, N.C., Horsburgh, K.J. and Green, J.A.M. (2012) The impact of future sea-level rise on the European Shelf tides. Continental Shelf Research, 35, 1-15. (doi:10.1016/j.csr.2011.11.011). [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Noted - this paper was examined.
13-71	13	1		110		I am surprised that the UKCP09 scenarios developed by Haigh et al (2009) are not cited as they are important statements on sea-level rise scenarios, including both the AR4 guidance and the higher H++ scenario which has been used in the planning of the future of London's flood defences. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	We disagree. Our role as IPCC lead authors is to make an assessment of the peer-reviewed scientific literature, not to assess other assessments. Hoever, this report does get referred to
13-72	13	1		110		In general there is little if any consideration of sea-level rise scenarios that are being developed for impact and adaptation analysis. These reflect the translation of earlier IPCC reports and related literature and some assessment of these interpretations by these collected experts would be very useful. This includes the UKCP09, but there are other efforts such as the Economics of Adaptation to Climate Change by the World Bank which used scenarios including some derived from the work of Rahmstorf and colleagues. There are also papers that consider extreme marine scenarios for coastal design purposes such as Wilby et al (2011). There is a danger that this area of scenario development falls between the stools of WGI and WGII and at the least the division of these efforts should be discussed and agreed. This will allow for a statement of clear scope for this Chapter, as well as the corresponding Chapters. While I realise that much of this material probbaly belongs in WGII, some of the scientific aspects are best reviewed by the experts in this Chapter.	see 13-71

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						REFERENCE Wilby, Robert, Nicholls, R.J., Warren, Rachael, Wheater, Howard, Clarke, D. and Dawson, Richard (2011) New nuclear build: adaptation options over the full life-cycle. Proceedings of Institution of Civil Engineers [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	
13-73	13	1		110		As a general additional comment regarding assessment of extreme values of sea level rise from measured data, I believe that the use of extrapolation for extremes when done is not justified using the Peak over Theshold method for waves, winds and sea levels, since the world is changing its behavior due to the global warming and changing climate so one can not look at them as belonging to quasi stationary processes. Consequently, I believe a more appropriate distribution for assessing extremes would be using Gumbel or Asymptote III distributions. [Sergiu Dov ROSEN, Israel]	Noted. After consulting with an extreme-statistics expert, it is our understanding that the Peak over Threshold method can be used to examine non-stationary processes.
13-74	13	1		110		The chapter does not deal with more enclosed basins such as the Mediterranean sea levels. However, it seems to me that one potential anthropogenic impact, apparently at first local has been disregarded. I refer to the increased inflow in the Mediterranean via the Suez Canal particularly since the 1990's when the Canal was deepened and widened (with futher increases of both planned for the near future) as well as the cessation of a huge volume of sweet water from the Nile river since 1965, due to completion of the High Asswan dam. The former impact led to entrance in the Mediterranean of a volume of about 110 cubic kilometers per annum (more than that of Danube's yearly discharge) with higher salinity that both that of the Mediterranean and the Red Sea, due to the passage of the Canal trough the salty Bitter Lakes, getting also warmer during their pass through the Suez Canal. These waters sink to the bottom of the Easter Mediterranean, affect its circulation and water layers exchange and induce a flow of saltier waters eventually leaving the Mediterranean trough the Gibraltar affecting the North Eastern European coasts of the Atlantic. I believe an updated modelling simulating this contribution, may also affect the forecasted climate change impact estimates. As an outcome of these, the sea level data gathered at the GLOSS station 80 -Hadera, operated by IOLR since 1992, indicate that during the last 20 years a larger average sea level rise than that of the global average, namely of 6 mm/year occured. I attach the graph derived for this GLOSS station in an additional worksheet here, and wish also to mention that the higher rise since April 192 through March 2001 is attributed mainly to the Eastern Mediterranean Transient event which happend during this period. [Sergiu Dov ROSEN, Israel]	Noted. Unfortunately the models used for the regional projections are unable to capture the processes that influence these enclosed basins.
13-75	13	1		110		The whole chapter on Sea Level Change is very good. [Terje Wahl, Norway]	Thank you.
13-76	13	1		200		19. This paragraph refers to the entire Chapter 13. Chapter 13 reviews some of the published information on the topic "Sea Level Change". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, data interpretation and projections, predictions and scenarios are based exclusively on the same IPCC climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitute a fraud. [Igor Khmelinskii, Portugal]	Rejected. Unsubstantiated claims and personal opinions.
13-77	13	3	1	5	25	Executive Summary: There should be no abbreviations in the Summary [Government of United States of America]	Noted.
13-78	13	3	1	5	25	This Chapter provides an excellent overview of what we know about past sea level and its causes. Its Executive Summary of findings is extremely useful, particularly as the Chapter is long and unlikely to be read in full by many. [Robert Thomas, United States of America]	Thank you.
13-79	13	3	3	3	11	Why are these topics selected -- this states what the chapter addresses, but why? Is the goal to provide	Rejected. This is an executive summary, and this

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						information on sea-level rise, or information on sea-level rise that is relevant for impact and adaptation work (WG II). In the case of the later, relative sea-level rise is important, including local effects. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	paragraph summarizes chapter topics. We think it is self-evident that the goal is to provide information on sea-level change, not impacts.
13-80	13	3	4	3	6	Calling "expansion/contraction of the ocean as it warms/cools" a primary contributor to global averaged sea level change conveys a false impression, because it fails to note the critical fact that thermal expansion primarily affects sea level of the open deep ocean, not coastal sea levels. As any competent physicist can confirm, thermal expansion of the upper ocean cannot affect coastal sea levels at all. That's very basic physics: gravity balances mass, not volume. That's why icebergs stick up in the air, and why displacement is measured in units of mass, not volume. (Note that density changes in the ocean depths can theoretically affect coastal sea levels, but not by much, because the temperature and density of the deep ocean is very stable.) Density changes (e.g. thermal expansion) in the upper ocean can affect (satellite-measured) mid-ocean sea levels, but those effects have no practical consequences. Such changes don't affect shorelines or increase flooding at the coasts. (One of the problems with AR4 is its failure to note the distinction between coastal sea-level rise and mid-ocean sea-level rise.) Note that there is widespread confusion about the effects of water density changes. For instance, for over six years the National Science Foundation had a false statement on their "sea ice" page on their web site, saying, "melting sea ice also raises worldwide sea levels, with potentially significant effects for coastal cities and towns." After I informed them that was incorrect, and they confirmed I was right, they replaced the false statement with this mea culpa: "[Editor's note: An inaccurate statement about sea ice and rising sea levels has been deleted. We regret the error.]" http://www.nsf.gov/about/history/nsf0050/arctic/seaice.htm [David Burton, United States of America]	Rejected - see #13-13.
13-81	13	3	4	3	6	The sentence should be rewritten, especially the part concerning the transport of water, which is not complete. In ist generality it should also include the transport of water to glaciers and ice sheets. I suggest to rephrase the second part. New formulation: ... transfer of water between ocean and land, particularly through growing or melting glaciers and ice sheets. [Peter Lemke, Germany]	Taken into account - ES extensively rewritten
13-82	13	3	5	3	5	Suggested change "the transfer of water to/from the ocean/land," -> "the transfer of water between ocean and land," [Aslak Grinsted, Denmark]	Taken into account - ES extensively rewritten
13-83	13	3	8	3	10	The sentence is a bit unclear when "solid Earth which itself may be moving" erase "itself" [Charlotte Sparrenbom, Sweden]	ES extensively rewritten
13-84	13	3	13	3	13	Executive Summary: Suggest to use the term "Past Sea Level Change" as for section 13.2. As is "Sea Level Observations" also includes paleo information, which is not really reflected in the title. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-85	13	3	15	3	17	The bolded line seems to conflate the Mid Pliocene and LIG results in a way that is confusing. I suggest highlighting the LIG in the bolded line, and perhaps emphasizing polar in additon to global temepratures since it is these which are the key aspect of the analog for ice sheet contribution (I note that polar temperatures are absent from the p.12 discussion however). Then Mid Pliocene, which has lower confidence, can be mentioned secondarily. [Michael Oppenheimer, United States of America]	Taken into account. The reviewer is correct that the results are conflated. ES extensively rewritten
13-86	13	3	15	3	22	"Medium-to-high" is excessively confident. [David Burton, United States of America]	Rejected. This is based on multiple lines of evidence. ES extensively rewritten
13-87	13	3	15	3	22	Sea level measurements are not conducted in a representative random fashion all over the earth, so you cannot have a :"global mean". There seems no point in any sort of average, either. Each set of measurements has to be considered seperately [Vincent Gray, New Zealand]	Rejected. The studies on which this statement is based account for regional variability in individual measurements.
13-88	13	3	15	3	22	We do not know the average temperature of the earth's surface today so it is unbelievable that you can cliam that it was 2-3 degrees warmer a million years ago. Your claim that only three measurements can give a "global average" is absurd. [Vincent Gray, New Zealand]	Rejected. There are many more than three measurements that are the basis for the average temperature estimate.
13-89	13	3	15	3	22	"More than 6m" is too vague. Please say what the "and less than X" meters was. Personally, I'm not sure the 10m Chap 5 talks about is solid enough to warrant "high confidence" but you need to engage them, decide on what the consensus is among your LA's and report it here (as well as in Chap 5). [Jonathan Overpeck, United	Taken into account. We report the conclusions from Chapter 5's assessment. ES extensively rewritten

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						States of America]	
13-90	13	3	15	3	22	LIG global temp 2-3 warmer? Medium to high confidence? No way. LIG models don't show this do they? Obs don't support thi, do they? The global temp of the LIG relative to now was 1 degree warmer max, and could have been cooler than today.See McKay et al., 2011: McKay N. P., J.T. Overpeck, B.L Otto-Bliesner. (2011). (and there are other papers which you need to read/cite).The role of ocean thermal expansion in Last Interglacial sea level rise, Geophyscial Research Letters 38: L14605, doi:10.1029/2011GL048280. Of course, this isn't the big point - the big point was what were the polar temperatures needed to get this amount of ice sheet decay and sea level rise?? This is tricky, but there is a new paper by Otto-Bliesner that discusses this - ask her, she's a chap 5 LA. The Antarctic may have been 5-6 degrees warmer than today, but only after the loss of signficant ice mass - see next comment. Otto-Bliesner et al., 2006 (and yer new paper just mentioned) nail Arctic tems. Suggest you update the prose to be more in line with the literature and chap 5, and you asign confidence that is realistic. [Jonathan Overpeck, United States of America]	see #13-85. The numbers for the Pliocene and LIG were conflated. This will be revised.
13-91	13	3	15	3	29	would be nice to use only one units mm/yr or m/kyr (even if they are the same). [Olivier Gagliardini, France]	We agree. See 13-96.
13-92	13	3	15	3	30	It would be much easier to read this section if it started with the most recent observations (i.e. switch the first and second paragraphs), and provide some clear context on the current rate of sea level rise. [Government of Australia]	Rejected. We think the more logical progression is to work from the past to the present.
13-93	13	3	15	3	30	These two paragraphs should be easily comparable. One talks about rates in m kyr-1 and the other in mm yr-1. Can the same units be used? [Government of Australia]	We agree. See 13-96.
13-94	13	3	15	3	30	I understand that kyr is a more appropriate unit than yr when presenting paleo changes, but to see two different units for the same thing in successive paragraphs, and working out (albeit quickly) that m/kyr is the same as mm/yr, disturbed the flow of my reading of these two paragraphs. [Adrian Simmons, United Kingdom]	We agree. See 13-96.
13-95	13	3	18	3	19	These statements are based on informal reanalysis of Kopp et al., 2009, and therefore may be harder to trace. It may be better to rely on the more formal reanalysis in Kopp et al. (submitted); the author is happy to provide a copy of this manuscript on request. [Robert Kopp, United States]	Taken into account. The new study by Kopp will be used as the basis for these statements.
13-96	13	3	19	3	19	I find it confusing that units of m kyr^-1 are used in this paragraph, and mm yr^-1 are used in the next. They are exactly the same units. There is no scientific convention that says that m kyr^-1 implies observations over m or kyr, or that mm yr^-1 implies observations over mm or years. The appropriate way to state these rates is to select a consistent combination of units (mm yr^-1 would be fine) and end the sentence with a phrase like "....over timescales of millennia", or ".... over timescales of centuries" etc. [John Hunter, Australia]	Accepted. We will follow this convention.
13-97	13	3	19	3	19	I am not sure what to do with units here. It is clearly a little odd to use different units (m kyr^-1) compared to elsewhere in the text. On the other hand these units express the fact that this is the millennial average rate. I suggest that you write 'a millennial average rate of xx common units' to get around that. That would, in one phrase also express that this is the millennial average. [Mark Siddall, United Kingdom]	We agree. See 13-96.
13-98	13	3	19			See comments below in regard to p.13 lines 14-17 [Michael Oppenheimer, United States of America]	Noted. This statement has been deleted.
13-99	13	3	20	3	21	In stead of "limited analogy" which is unclear, suggest : "forcing factors are very different" [Gilles RAMSTEIN, France]	Accepted - text will be revised.
13-100	13	3	21	3	22	What I think this sentence intends to say is that that the rates of SLR from the last interglacial period cannot be used to calculate an upper bound of global mean sea level rise for this Century. [Kathleen McInnes, Australia]	Noted. This statement has been deleted.
13-101	13	3	22	3	22	Can paleoproxies give any information about likely ranges? [Christopher Little, United States of America]	Taken into account. This statement has been deleted.
13-102	13	3	23	3	24	In a view that the range from ice sheet dynamics contrubution is not even scenario-dependent (which has no physical sense) makes to my view "medium confidence" over-optimistic. [Andrey Ganopolski, Germany]	Noted. The ranges we have given are inclusive of the range of scenarios. Because we give a wider range, we think medium confidence is appropriate.
13-103	13	3	23	3	30	Care needs to be taken here in discussing the issue of acceleration in MSL which has been a keenly debated by numerous researchers, in particular over the past 2 years or so. The advised accelerations are based on simple quadratic curve fitting which apply the curve fit to the length of the data resulting in an average rate of	Point taken. However, the point here is to average through these "inflexions" and observe the longer term acceleration that is more likely associated with

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						acceleration over the whole time period. The fitting of a quadratic does not facilitate breaking the record down to distinguish periods of high or low (even negative) acceleration. Acceleration terms defined using these techniques are particularly sensitive to the time period chosen, especially during the 20th century where large inflexions in the 20's - 40's and again post 1990 are separated by periods of deceleration. Yes there are clearly periods of acceleration, however, it is not consistent or sustained throughout the 20th century and certainly doesn't manifest as a smooth average. [Phil Watson, Australia]	anthropogenic effects. ES extensively rewritten
13-104	13	3	24	3	24	This sentence is true, but incomplete. Change it to read: "It is very likely that the rate of global mean sea level rise has increased during the last two centuries, but not during the last 80 years." [David Burton, United States of America]	We disagree with this modification. The tide gauge data show that the change of rate has been ongoing over the last 80 years.
13-105	13	3	24	3	30	I suggest that the total change in SL is given, not only the rates of change. [Jan Fuglestvedt, Norway]	The rates represent the average change over the given time period and can be easily scaled to give the average total sea level rise over the same. We have given the total change as well.
13-106	13	3	24	3	30	Only global mean sea level rise is mentioned. To present comprehensive understanding of sea level rise, it is suggested that Local Sea Level Change be described here. [Ke Xiu LIU, China]	Local sea level changes are not discussed because the land motion would be needed to assess the local change.
13-107	13	3	26	3	26	"modern rates" -> not clear what is meant here by "modern". Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Change "modern" to "moderate"
13-108	13	3	26	3	28	Second, and more importantly, the fact is that both tide-gauge and satellite measurements show NO acceleration at all in the rate of sea-level rise. The "2.8-3.6" mm/yr number is from satellites, and the 1.7 number is from averaged tide gauges (both inflated by GIA). Conflating measurements of sea level in different places is a fundamental error, which can create the illusion of acceleration where no true acceleration exists. The best and most comprehensive studies of averaged tide gauge data show deceleration, rather than acceleration, over the last ~3/4 century. The satellites also show deceleration rather than acceleration (though only a slight deceleration, rather than a large deceleration, since the Envisat numbers were dramatically revised). The claim that the rate of sea level rise has approximately doubled since the early 1990s is a colossal error, trivially refuted by examination of the recorded sea level at almost any GLOSS-LTT tide gauge. [David Burton, United States of America]	The tide gauges and the altimetry record are in reasonably good agreement, so we can consider the tide gauge record, which shows a small acceleration over the last hundred years.
13-109	13	3	26	3	28	This statement differs from the equivalent statement in Ch 3 Executive Summary. Here global mean sea level (GMSL) rise over the 20th century is given as ~1.7 [1.5 to 1.9] mm yr-1 (very likely) whereas Ch 13 gives 1.4 to 2.0 mm yr–1 (virtually certain). For the period from 1993, this chapter gives 2.8 to 3.6 mm yr-1 (very likely), while Ch 3 gives gives 2.7 to 3.7 mm yr-1 (virtually certain). It might be expected that a widening of the range would increase confidence, but collectively the messages conveyed are confusing. [Timothy Carter, Finland]	Ch 13 will be revised to use the rates reported by Ch 3.
13-110	13	3	26	3	29	Only sea level changes per year are given. Please consider also to give the range of total sea rise from 1900 and from 1993, as this is considered more informative. [Government of NORWAY]	Noted - toatl change also reported now
13-111	13	3	26	3	30	The last two sentences of this paragraph are absolutely false. First of all, the ~1.7 mm/year claimed rate for the 20th century is exaggerated by the addition of Peltier's GIA adjustment for presumed enlargement of ocean basins. Sea level, as defined conventionally and in the SOD, is the level of the surface of the sea, which means that you can't legitimately subtract off factors (like Peltier's 0.3 mm/yr GIA) which lower sea level. Such arithmetic is useful for mass budget calculations, but the result of that subtraction isn't sea level! [David Burton, United States of America]	Rejected. The 1.7 mm/year rate does not have a 0.3 mm/year correction applied. Instead, GIA corrections are applied to the individual tide gauge records.
13-112	13	3	26	3	30	The confidence level used for this paragraph is different to that used in Chapter 3, noting that the SPM uses Chapter 3 values. This will be really confusing. Suggest changing the observational values here to be consistent with Chapter 3. [Government of Australia]	This will be made consistent with Ch 3.
13-113	13	3	26			The statement, “It is very likely that global mean sea level has risen ~1.7 [1.5 to 1.9] mm/yr during the 20th century, and between 2.8 and 3.6 mm/yr since 1993.”, is misleading. The estimated error on the trend of sea level from the altimetry era (+/- 0.4 mm/year) is has been attributed to uncertainties in the vertical land motion of the network of tide gauges used to verify the global mean sea level time series from altimetry data. This	The land motion error maps into the tide gauge record and the altimetry record in different ways, such that the error is not the same in the rates of sea level change.

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						error assumes that there is an unknown linear vertical land motion error at each gauge that has been constant over the 20th century. By definition, because this error is linear, it has no acceleration. It is a systematic uncertainty common to both the gauge-measured 20th century rate of sea level rise and the rate from altimetry. By assuming that the errors are independent, the summary concludes that the increase from 1.7 to 3.2 mm/year is "very likely". Because the error is linear and common to both, the increase is virtually certain. [Government of United States of America]	
13-114	13	3	26			do you mean moderate instead of modern? [Kathleen McInnes, Australia]	Yes - changed.
13-115	13	3	26			It is suggested to substitute "modern" by "moderate". [Klaus Radunsky, Austria]	Yes - changed.
13-116	13	3	27	3	27	Main range of sea level increase is 1.5 to 1.9 whereas in chapter 3 (page4, line 32) it is given at 1.4 to 2.0, as well as in SPM. [SYLVIE JOUSSAUME, France]	Changed to be consistent with Ch 3.
13-117	13	3	27	3	28	Please note that these reported ranges of SLR during the 20th century are slightly (0.1 mm yr-1) inconsistent with the numbers reported in Chapter 3. Please check for consistency. [Thomas Stocker/ WGI TSU, Switzerland]	Changed to be consistent with Ch 3.
13-118	13	3	28	3	29	The statement that sea level has accelerated is misleading. An accelaration of sea level rise means that there is a consistent positive second derivative, which is not the case, if at all, only for a short period. Looking at the graph, it seems to me that there are two linear trends, a smaller one up to 1940 and a larger one from 1940 to today. This cannot be labelled as acceleration throughout the entire record. There is no acceleration during the satellite era. I find this statement very misleading. From 1940 onwards there is definitely a linear trend, which is superimposed by fluctuations of various frequencies. The increased (constant) rate since the 1940s I would not term "acceleration". [Peter Lemke, Germany]	As long as "acceleration" is defined as the quadratic term, then this should still be correct.
13-119	13	3	28	3	29	Fitting a quadratic term is not very informative, except when a quadratic is indeed a good model for the observed time evolution (which it rarely is). See detailed explanation in the article "Don't estimate acceleration by fitting a quadratic…" at http://www.realclimate.org/index.php/archives/2012/11/dont-estimate-acceleration-by-fitting-a-quadratic/ [Stefan Rahmstorf, Germany]	This is true, however the quadratic term is still useful, especially when averaged over a long time period.
13-120	13	3	28	3	30	"It is likely that global mean sea level has accelerated since the early 1900s" is a plainly false statement (and it's missing the word "rise"). Change this to "it is unlikely that global mean sea level rise has accelerated since the early 1900s," or "Tide gauge measurements indicate that global mean sea level rise has not accelerated since the early 1900s." [David Burton, United States of America]	We disagree with this statement.
13-121	13	3	29	3	29	mm yr-2 should be mm yr -1 [Charlotte Sparrenbom, Sweden]	These are acceleration terms, so the units are correct as used.
13-122	13	3	29	3	29	The text 0.000 to 0.013 [-0.002 to 0.019] is difficult to understand: a double uncertainty interval. [Hans Visser, The Netherlands]	ES extensively rewritten
13-123	13	3	32	4	4	This part actually discusses the "Global" Mean Sea Level Change. So it's suggested to replace the title "Understanding of Sea Level Change" with "Understanding of Global Mean Sea Level Change", or present some description of "Understanding of Local sea level change" in this part. [Ke Xiu LIU, China]	Accepted.
13-124	13	3	34	3	34	I think that there should be a comma after "expansion" in order to get the sense right. It may be better, in fact to replace "ocean warming and" with simply "thermal". [John Hunter, Australia]	Accepted.
13-125	13	3	34	3	44	The last sentence of this summary paragraph should be first. The current first sentence does not sum up the rest of the paragraph [Mark Siddall, United Kingdom]	Rejected. The current first sentence is the key one in terms of explaining what we know best about the recent budget.
13-126	13	3	39	3	39	Thermosteric' should be defined in Exec Summary. [Government of Australia]	Accepted
13-127	13	3	39	3	39	Define theromosteric here [European Union]	Accepted
13-128	13	3	39	3	41	The sentence "Reservoir impoundment … rate of impoundment." needs to be extended to explain the significance of that fact. I suggest this replacement: "Reservoir impoundment exceeded groundwater	Rejected. The sentence is specific to the contribution from land water storage. Also ES extensively

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						depletion for the majority of the 20th century but the rate of groundwater depletion has increased and now exceeds the rate of impoundment, which could be expected to cause accelerated sea level rise; the fact that no such acceleration has been observed suggests that net meltwater contribution to sea level rise has decreased." [David Burton, United States of America]	rewritten
13-129	13	3	41	3	44	This paragraph talks about the closure of the sea level rise budget, however provides no indication of why we now know this, and does not clearly explain the components that make up the budget. This is a really important concept to communicate and having a simple summary of it in the Executive Summary would be really useful. (The introduction does this well). [Government of Australia]	Taken into account. The paragraph does provide information on how we know this, but we will consider clarifying this. Also ES extensively rewritten
13-130	13	3	41	3	44	The last sentence of this paragraph states that "the closure of the observational budget for recent periods within uncertainties represents an advance since AR4, however it does not clarify what has improved, other than on pg. 4 line 3-4, which mentions improvements in modelling of sea ice dynamics. Is this the only improvement? [Government of Australia]	accepted - ES extensively rewritten
13-131	13	3	41	3	44	Please clarify the advance from AR4 or remove the reference to AR4: The message was essentially the same in AR4. In AR4 ch 5 p 387 (ES), it is explained that for the period > 1993, "the sea level budget (...) is closed within known errors". In AR5 the text refers to "recent period" instead of 1993, and to "uncertainties" rather than "known errors", but this seems very similar. [Philippe Marbaix, Belgium]	accepted - ES extensively rewritten
13-132	13	3	46	3	57	Why not give the components of the budget here ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	No thiat would be too detailed - ES extensively rewritten
13-133	13	3	46			Don't use acronyms without definitions (AOGCM) in important summary statements - most general readers won't know what you are talking about [Terrence Joyce, United States of America]	accepted
13-134	13	3	46			Page 3, line 46. I find this paragraph to be rather convoluted/complexly written. Especially the sentence after the bold sentence, starting “the difference between" seems to me too complicated. It needs to be simplified and/or broken up. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted - ES extensively rewritten
13-135	13	3	48	3	49	It is not clear why "and 1993-2010" is added. Does this mean that there was no sea level rise between 1971 - 1993? [Government of NORWAY]	accepted - ES extensively rewritten
13-136	13	3	48			Global Mean Sea Level should not be capitalized. [Government of United States of America]	accepted - ES extensively rewritten
13-137	13	3	49	3	51	The sentence "the difference … is consistent with zero, but is potentially explained…" sounds somewhat strange to me. Obviously, what follows after that explains why in fact the difference is not zero. [Andrey Ganopolski, Germany]	accepted - ES extensively rewritten
13-138	13	3	49	3	53	Sentence is very unclear. Reformulate [European Union]	accepted - ES extensively rewritten
13-139	13	3	49	3	53	This sentence is very long and difficult to read. Please consider to split it in two. [Government of NORWAY]	accepted - ES extensively rewritten
13-140	13	3	50	3	50	This statement is a little confusing. It refers to the difference between observations and model estimates as being "consistent with zero", which suggests that the error margin bounds zero. However, it then goes on to explain why there is a difference (seemingly an unexplained modelled shortfall in SLR that may be due to various factors). The zero difference doesn't appear to be believable to the authors, even if it falls within the margin for error. So, should the reader come away with the message that the models are now robust enough to match the observations, or should the message be that even though the model results could be interpreted as matching the observations, actually this is misleading and in fact they shouldn't because there are several factors not accounted for (e.g. volcanic forcing, glacier losses in the 1930s)? [Timothy Carter, Finland]	accepted - ES extensively rewritten
13-141	13	3	50	3	53	This section poorly organized and extremely confusing. Better to first go into details about the reasons for discrepancy in individual terms of the model/observation comparison, then add that the difference between modeled and observed is also consistent with zero. [Michael Oppenheimer, United States of America]	accepted - ES extensively rewritten
13-142	13	3	53	3	54	the sentence “Model-based estimates of ocean thermal expansion and glacier contributions increase from 1990, in agreement with the global mean sea level record” seems unfinished [Pavel Tkalich, Singapore]	accepted - ES extensively rewritten

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13-143	13	3	53			mass loss from glaciers during the 1930s due to unforced climate variability' Ch10 concludes that the early 20th century warming (over first half of century) was very likely forced in part - please make consistent - even specific anomalies eg over the 30th then would at least have a forced contribution? [Gabriele Hegerl, United Kingdom]	accepted - ES extensively rewritten
13-144	13	3	54	3	54	Please insert "are" after "1990". [Government of NORWAY]	accepted - ES extensively rewritten
13-145	13	3	54	3	56	There is no "observed increase in rate" (of sea level rise). Rather, the best and most comprehensive studies have found either a linear rate or slight deceleration. If AR5 is to be taken seriously by serious people, it can't make assertions that can be seen to be obviously untrue by anyone who takes even a few minutes to look at PSMSL or NOAA tide gauge graphs. [David Burton, United States of America]	Rejected.
13-146	13	3	55			Does 'loss of mass of ice sheets not taken into account' mean self- gravitation and/or GIA? This needs to be stated more explicitly. [European Union]	accepted - ES extensively rewritten
13-147	13	3	56			The statement about scientific advance since AR4 seems a bit odd and misleading in the context of the executive summary. It is difficult to decipher from the text whether sea-level rise by ice sheets matter from the AOGCMS, which don't capture ice sheet dynamics. Further, I don't see how these models provide increased confidence in future projections, given they still don't capture ice sheet dynamics, which is arguably the largest source of uncertainty in future sea-level changes. They may be capturing the processes important for 20th century sea-level changes, but that doesn't mean they provide confidence for projections given the possibility of bifurcations and thresholds. [Ryan Sriver, United States of America]	accepted - ES extensively rewritten
13-148	13	3		110		the chapter is politically and not scientifically correct [albert parker, australia]	Please explain why. It is not possible to respond to this sort of general criticism.
13-149	13	3		110		the authors misrepresent the reality of measurements all suggesting a lack of acceleration [albert parker, australia]	Observations of tide gauge records of sea level rise are assessed in Chapter 3, and we rely on that assessment as well as paleo observations from Chapter 5. Together these clearly indicate the rate of rise has increased.
13-150	13	3		110		there is no tide gauge record of enough length and quality that suggest something different from oscillations about a smooth linear trend of constant sea level rise [albert parker, australia]	Observations of tide gauge records of sea level rise are assessed in Chapter 3, and we rely on that assessment - There assessment disagrees with this assertion.
13-151	13	3		110		all the long term tide gauges show no positive acceleration since the end of the 1800s [albert parker, australia]	Rejected. This statement is incorrect - see chapter 3 and also - (need to look up reference and perhaps adjust response).
13-152	13	3		110		same all the tide gauges more than 60-70 years [albert parker, australia]	Rejected. This statement is incorrect - see chapter 3 and also - (need to look up reference and perhaps adjust response).
13-153	13	3		110		short tide gauges show unrealistic low or high sea level rises because of the multidecadal oscillations [albert parker, australia]	Agreed - individual short records have large uncertainty in trends, and thus in this chapter we do not rely on individual short records alone.
13-154	13	3		110		the satellite radar altimeter reconstruction only covering 2 decades is free of any positive acceleration [albert parker, australia]	Agreed. No statement is made to the contrary.
13-155	13	3		110		the authors deliberately neglect all the scientific literature questioning their claims based on models and arbitrary reconstructions rather than true measurements [albert parker, australia]	Rejected. Observations of tide gauge records of sea level rise are assessed in Chapter 3, and we rely on that assessment as well as paleo observations from Chapter 5.
13-156	13	3		110		climate models and arbitrary reconstructions do not have more value than the true measurements [albert parker, australia]	Agreed, but we have not observations of the future, only models that can inform us of potential future

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							conditions.
13-157	13	3		110		our group published 12 peer review papers on the subject. All of them have been deliberately censored [albert parker, australia]	These papers now referred to
13-158	13	3		110		other groups also published papers against the positive acceleration claims. All of them have been censored. [albert parker, australia]	Rejected. Observations of tide gauge records of sea level rise are assessed in Chapter 3, and we rely on that assessment as well as paleo observations from Chapter 5. Together these clearly indicate the rate of rise has increased.
13-159	13	3		110		this is not science. This is politics. [albert parker, australia]	Please explain why. It is not possible to respond to this sort of general criticism
13-160	13	3		110		A BORETTI, "SHORT TERM COMPARISON OF CLIMATE MODEL PREDICTIONS AND SATELLITE ALTIMETER MEASUREMENTS OF SEA LEVELS", Coastal Engineering, Volume 60, February 2012, Pages 319-322. doi:10.1016/j.coastaleng.2011.10.005. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-161	13	3		110		A BORETTI and T Watson, "The inconvenient truth: Ocean Levels are not accelerating in Australia or over the world", Energy & Environment , 2012, volume 23 number 5, pp. 801-817. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-162	13	3		110		A BORETTI, "Is there any support in the long term tide gauge data to the claims that parts of Sydney will be swamped by rising sea levels?", Coastal Engineering, Volume 64, June 2012, Pages 161-167. DOI:10.1016/j.coastaleng.2012.01.006. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-163	13	3		110		A BORETTI, "Discussion of J.A.G. Cooper, C. Lemckert, Extreme sea level rise and adA Parkertation options for coastal resort cities: A qualitative assessment from the Gold Coast, Australia, Ocean & Coastal Management, In Press, Accepted Manuscript, Available online 18 April 2012", Ocean & Coastal Management, In Press, Accepted Manuscript, DOI: 10.1016/j.ocecoaman.2012.05.031. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-164	13	3		110		A BORETTI, "Discussion of 'Dynamic system model to predict global sea-level rise and temperature change' by Aral, M.M., Guan, J., Chang, B., Journal of Hydrologic Engineering, Volume 17, Issue 2, 7 March 2012, Pages 237-242.", ASCE's Journal of Hydrologic Engineering, , In Press, Accepted Manuscript. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-165	13	3		110		A BORETTI, "Discussion of Christine C. Shepard, Vera N. Agostini, Ben Gilmer, Tashya Allen, Jeff Stone, William Brooks and Michael W. Beck, Reply: Evaluating alternative future sea-level rise scenarios, Natural Hazards, 2012, DOI: 10.1007/s11069-012-0160-2.", Natural Hazards, In Press, Accepted Manuscript. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-166	13	3		110		A BORETTI, "Discussion of Natalya N. Warner, Philippe E. Tissot, Storm flooding sensitivity to sea level rise for Galveston Bay, Texas, Ocean Engineering 44(2012); 23-32", Ocean Engineering, In Press, Accepted Manuscript, DOI: 10.1016/j.oceaneng.2012.06.030. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-167	13	3		110		A BORETTI, "The measured rate of rise of sea levels is not increasing and climate models should be revised to match the experimental evidence", Royal Society Publishing eLetter Published July 12, 2011. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-168	13	3		110		http://rsta.royalsocietypublishing.org/content/369/1934/161.short/reply#roypta_el_118 [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-169	13	3		110		A PARKER, "Comment to Shepard, C.C. , Agostini, V.N., Gilmer, B., Allen, T., Stone, J., Brooks, W., Beck, M.W., Assessing future risk: Quantifying the effects of sea level rise on storm surge risk for the southern shores of Long Island, New York, Natural Hazards, Volume 60, Issue 2, January 2012, Pages 727-745", Natural Hazards, In Press, Accepted Manuscript, DOI 10.1007/s11069-012-0314-2. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-170	13	3		110		A PARKER, "Oscillations of sea level rise along the Atlantic coast of North America north of CAPE Hatteras", Natural Hazards, In Press, Accepted Manuscript, DOI: 10.1007/s11069-012-0354-7. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here

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13-171	13	3		110		A PARKER, "Comment to M Lichter and D Felsenstein, Assessing the costs of sea-level rise and extreme flooding at the local level: A GIS-based A Parkerproach, Ocean & Coastal Management 59 (2012) 47-62.", Ocean & Coastal Management, In Press, Accepted Manuscript, dx.doi.org/10.1016/j.ocecoaman.2012.08.020. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-172	13	3		110		A PARKER, "SEA LEVEL TRENDS AT LOCATIONS OF THE UNITED STATES WITH MORE THAN 100 YEARS OF RECORDING", Natural Hazards, In Press, Accepted Manuscript. [albert parker, australia]	This paper is relevant to Chapter 3 and also referred to here
13-173	13	4	1			"methods for projections" of what? [Michael Oppenheimer, United States of America]	Taken into account - "of sea level" will be added.
13-174	13	4	2			Page 4, line 2. After “clearer account" please specify account of what exactly. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this will be clarified.
13-175	13	4	6	4	13	It would be useful here in Exec Summary to state something like that in section 13.4.1 'that 'over 90% of the net energy increase of the climate system on multi-annual timescales is stored in the oceans'. [Government of Australia]	Noted. This is really a chapter 3 issue, where this is stated.
13-176	13	4	15	4	48	Now that we've talked about the Last Interglacial in Comments 3 and 4, there is a big logic gap in in this section. How could the LIG have had a sea level 6-10m above present, when it was 2-3 degrees warmer (again, I don't see how that is right, since no models can simulate this amt of warming, but I'll leave it to you to decide how open to post-plenary controversy you want to risk). But lets say you change our ES to reflect the literature and note that very little global warming apparently triggered 6-10m of sea level rise during the LIG. How much of that came from thermal expansion? Almost none (see McKay et al 2011). How much came from Greenland? Current estimates are in the 2m range (e.g., Overpeck and Otto-Bleisner 2006 papers in Science said more, but comments argued less, and then these guys weighed in with new evidence: Colville, E.J., Carlson, A.E., Beard, B.L., Hatfield, R.G., Stoner, J.S., Reyes, A.V., and Ullman, D.J., 2011, Sr-Nd-Pb isotope evidence for ice-sheet presence on southern Greenland during the last interglacial: Science, v. 333, p. 620-623. So, 6-10m, and yet, you can't get more than 3-4 (other polar ice, glaciers) without turning to the Antarctic. And you need up to 7m more to be internally consistent. Did it all come from the WAIS? No way. Last estimate I heard from WAIS modelers was 3-4m? But, there are parts of the EAIS that are also grounded below sea level. You guys know this. But the point I'm trying to make is that there is a chance (medium confidence or better if you're going to stick to the literature on the LIG sea level rise) that the WAIS and EAIS each lost several meters. All with a global temperature that was at most 1 degree warmer than today. Logic dictates that you at least have a bullet on the potential future role of the AIS, like you do w/ the GIS. [Jonathan Overpeck, United States of America]	Noted. This discussion on paleo is in section 13.2, and referred to in 13.5.2.
13-177	13	4	15	4	48	comment 5 continued… The Paleo record makes a strong case that there is some possibility of that global warming beyond several degrees will warm the poles to the temperatures they were at during the LIG, and thus ensure a long-term rise of sea level of meters from AIS, not just the GIS. Is it reversable? On what time scale? What's the top end sea level rise possible, including this dynamical ice retreat? What's the possible top end rate? The literature has many papers that say more than a m/100 years and yet this chapter doesn't seem to want to let the policy makers know what the scientists are talking about. PLEASE provide all this detail, and if you are not sure how strong the evidence and agreement is, use confidence language accordingly. But to not mention the AIS is really selling this excellent chapter short. Policy makers clearly asked the WGI (some of you were at the scoping) for this, and this is why there is a sea level chapter - to hear the whole story, with numbers rather than "larger" etc. Please get more specific and comprehensive. [Jonathan Overpeck, United States of America]	see #13-177
13-178	13	4	15	4	48	Just to be really sure you get the point, how could you possibly not consider 6-10m sea level rise, plus that due to thermosteric, over the next several centuries to millennium, if we warm the earth more than the temp reached (think polar) during the LIG. And, to not mention that this could take place at rates exceeding 1m/100 years not only ignores important literature, it also assumes you know things that we still don't really know. The flip side is that I'm not suggesting "high confidence" on all this... but you can't just ignore it. [Jonathan Overpeck, United States of America]	see #13-177
13-179	13	4	15	4	48	Overall I think the authors have done an admirable job in describing and evaluating the evidence that can provide information on future SLR. I have no problems with their evaluation and discussion of individual	Taken into account by indicating where we have less than high confidence (likely ranges can be given with

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						elements, but I do believe that their final conclusions and presentations of likely future ranges and confidence are logically inconsistent and open to misinterpretation. This is spelled out in individual comments which hopefully make the problem clear and offer ways to resolve it. [Andy Reisinger, New Zealand]	medium confidence, according to the guidelines), and by clarifying the meaning of the likely range for values above the range. The latter appears to be the reviewer's concern about logical inconsistency in his comments elsewhere.
13-180	13	4	17	4	21	This paragraph is nonsense. It is unlikely (not "very likely") that the rate of global mean sea level rise will significantly exceed the rate observed over the last 3/4 century. The stability or slight decline in rate of measured coastal sea level rise coincident with at least 80 ppm CO2 increase over that period is strong evidence that additional CO2 and other anthropogenic GHGs have very little effect on the rate of sea level rise. [David Burton, United States of America]	Rejected. Our confidence in this statement arises from the satisfactory explanation of observed global mean sea level rise in terms of contributions that are modelled in similar ways to those used to make projections. See Section 13.3.
13-181	13	4	17	4	21	These ranges seem pretty limited to me given the paleo results cited above this text. With global average temperature going up at a rate far above the rate for the paleo periods, the rate of rise of sea level being projected gives no indication to me that this much faster rate is not even being considered. This is very hard to understand, seeming to me to be far too tied to the results of models where we have very limited verification of the models and, at the same time, processes like meltwater forming moulons, etc. that are just not being adequately represented in model simulations (indeed, we don't really know well, what the consequences might be other than taking a lot of heat down into the ice sheet--it is hard to see how that could not be a large amplifying process). Note--this is just a comment based on reading the Executive Summary--by this point, I have run out of time to read the chapter in detailand am not up on all references, but nonetheless think based on my history with the issue and other input that these estimates are just too small. At the very least, one thing to do here is to indicate with what confidence you are suggesting these are the limits--is the range, for example, the innermost 50% range--I would certainly hope it is no more than that. [Michael MacCracken, United States of America]	The reviewer makes good points, but all these issues are thoroughly discussed in Sections 13.4 and 13.5. Moreover in 13.5 and in the Exec Summ paragraph to which this comment relates we already state the level of confidence in the projections (medium), as the reviewer requests, and we would remind the reviewer that a likely range, such as we give here, corresponds notionally to 2/3 probability. Hence we reject the comment as no changes appear to be needed.
13-182	13	4	17	4	27	While I applaud this chapter in general for a much needed comprehensive update on sea level, I fear that the AR5 is falling into the same undesirable place as the AR4 on sea level. It is our job to let the policy-makers know what scientists are thinking, even IF there is not a firm concensus. Thus, it is not satisfactory to just say there is "no consensus about the reliability of semi-empirical models, which give higher projections than process-based models." Instead, I strongly urge you to say what "higher" means. Its just to vague, and the semi-empirical model results have been published in multiple peer-reviewed venues. Tell the policy-makers what higher is, and then then keep your note about lack of consensus. [Jonathan Overpeck, United States of America]	Taken into account by including more explicit statements that the probability of GMSL rise exceeding particular levels above the likely range cannot be quantified, meaning that there is currently no method for giving an "upper bound" with confidence. We do not therefore think that we should suggest any numbers above the likely range, even as illustrative, because of the danger that they would be misinterpreted.
13-183	13	4	17	4	27	The likelihood and confidence statements given for SLR projections are logically inconsistent. Lines 17-21 give likely SLR ranges, but lines 26-27 state that one cannot assess probabilities for values higher than these ranges. You cannot have both: EITHER you give a likely range, which means the probability of higher values is less than 33% (if so, say so, this is important!; i.e. "As a result, larger values for sea level rise cannot be excluded, but they are unlikely; however, current scientific understanding is insufficient for evaluating their probability in more detail."); OR if the authors feel they cannot make a judgement that higher values are "unlikely", then frankly it is impossible and logically inconsistent to give a "likely" range for SLR. Chose one or the other - you cannot have both. [Andy Reisinger, New Zealand]	See 13-1259.
13-184	13	4	17	4	27	The combination of a likelihood range and medium confidence level should give the authors pause to question whether this is really the most appropriate way of characterising their findings. Two reasons: likelihood statements are meant to convey a quantification of the probability of outcomes in the real world, not the statistics of available model results. If the authors have only medium confidence, are they sure that the likely range should be just the range resulting from current process-based models and not wider than that? Essentially, by equating the process-model range with the 'likely' range, what you are saying is that the process-based models are correct and other models are not - but that seems to contradict the rather more nuanced and careful discussion of semi-empirical models found in the body of the chapter. If there is any possibility of semi-empirical models having a grain of truth in them, then the likely range of real outcomes cannot be the same but must be greater than the 66% range that comes out of the process-based models. The second reason for my concern is that numerical ranges take on a life of their own, and I'm not convinced	We appreciate this thoughtful comment, which we have taken into account by trying to clarify our reasoning. However, we do not think that any material change is required. We would point out that the likely range is the 5-95% range of models, consistent with the projections of global mean surface air temperature change in the AR4 as well as ch12 of this assessment, and this makes it wider than if we took the 1/6-5/6 range of models. We do suggest any number above the likely range for precisely the reason the reviewer gives i.e. the danger that it would take on

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						that decision-makers will take great note of the 'medium confidence' qualifier. This implicit caveat should made stronger and clearer. [Andy Reisinger, New Zealand]	a life of its own. This could result in a number for which no probability can be assessed, in which we would not have any confidence, being used for policy purposes instead of the likely range. We would rather avoid that outcome. See also 13-182.
13-185	13	4	17			Model numbers will have no meaning to policy makers. Define models with generic characteristics. [Government of United States of America]	Rejected because we cannot work out what this comment refers to.
13-186	13	4	18	4	18	Define RCP the first time it appears in the text here….. [Charlotte Sparrenbom, Sweden]	It should be in the list of acronyms.
13-187	13	4	18	4	21	…global mean sea level rise is likely to be in the range 0.29-0.55 m for RCP2.6, … and 0.48-0.82 m … for RCP8.5. I am aware that in the current U.S. National Assessment being drafted, the sea level rise range in the same period is being assessed as 1 to 4 feet (0.31 m to 1.22 m) by 2100 compared to 2000, with the possibility of even 6.6 feet (2.01 m). I would like to reconcile the vastly different assessment between the IPCC and the U.S. National Climate Assessment of the upper end of the range. Has the reference of "Rahmstorf, S., G. Foster, and A. Cazenave, 2012: Comparing climate projections to observations up to 2011. Environ. Res. Lett., submitted" been fully considered? Has the reference of "Jevrejeva, S., Moore, J.C. and Grinsted, A., submitted. Are we overestimating sea level projections by 2100 with semi-empirical models. Journal of Geophysical Research" been fully considered? [Arthur Lee, United States of America]	Taken into account already in the text. We have an extensive discussion of semi-empirical models in 13.5.2 and 13.5.3.
13-188	13	4	18			Same as above: RCP [Terrence Joyce, United States of America]	There is a glossary
13-189	13	4	20	4	21	It is unclear as to what the bracketed numbers here are referring to, and the reference to rates is confusing. Is the rate of rise at the end of the 21st C greater than earlier in the decade or the same? [Government of Australia]	Accepted. Reworded
13-190	13	4	21	4	22	The phrase “unlike in the AR4, these projections include a contribution from changes in ice-sheet outflow” is not correct, although it is a common claim that the AR4 projections omitted any “contribution from changes in ice-sheet outflow”. The AR4 effectively produced two sets of projections – one without any contribution from ice-sheet outflow, and the other including a contribution called “scaled-up ice sheet discharge”. Although this latter term came from a very simple model, which scaled the discharge with the projected temperature, it is nevertheless a “projection” (table 10.7 in the AR4) – to pretend it is not is just playing with words. [John Hunter, Australia]	Taken into account by deleting "Unlike in the AR4". In fact the AR4 did not intend the scaled-up dynamical ice-sheet discharge to be interpreted as a projection, so our statement as it stands is correct. However, those illustrative numbers have been regarded as projections by some users of the AR4.
13-191	13	4	21	4	22	the projection provided (0.11m) is not clear for which period “changes in ice-sheet outflow, for which the central projection is 0.11 m” [Pavel Tkalich, Singapore]	Taken into account by repeating "for 2081-2100".
13-192	13	4	22	4	23	Is medium confidence defined anywhere? And how should i interpret that the "likely" range with medium confidence. (That said I fully support that you state your level of confidence, but i just want to ensure that "medium confidence" is defined somewhere in AR5.) [Aslak Grinsted, Denmark]	Yes, "medium confidence" is defined by the uncertainty guidelines, which permit a likely range to be given with less than high confidence if the confidence level is stated, as we have done. We believe that this addresses the reviewer's concern and no change is required.
13-193	13	4	22	4	23	I think it would be better to do what SREX chapter 3 did and only quote likelihoods when there is high confidence [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Rejected. In that case we would be unable to give likely ranges at all. We could still give a "model-based range" with no indication of likelihood, but we expect that users of the report would find this unhelpful, and we do not think it would be the most informative assessment that we can make.
13-194	13	4	22	4	27	This statement seems very strange. Saying that you will not put any reliance on semi-empirical approaches because there is not a consensus (that normally means very high agreement--since when is it the case that IPCC gives no credence to results that do not have everyone in agreement?). Either the results of semi-empirical approaches has to be considered, or the phrasing (and likely the chapter) has to be changed giving a much better reason for why the results are not being considered, because not having a consensus is just not	Taken into account by including more of the confidence assessment from 13.5.3 in the Exec Summ.

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						a reason fo exclude consideration of a view. For IPCC, consensus has actually meant unanimous agreement--well, there are lots of areas where there is not unanimous agreement and material is included, giving a range that includes that view. [Michael MacCracken, United States of America]	
13-195	13	4	25	4	26	I wonder if it would be helpful to state by how much (approximately) the projections from semi-empirical models exceed those from process-based models. Is it by 10%, 50%, double, or what, based on the published literature? Would this be any less valid to report than the projections on long-term SLR by 2300 in the next summary point (L35-37), from just a handful of models? [Timothy Carter, Finland]	Rejected. We think there is a substantial danger that doing this would produce a number that might be taken as an "upper limit", despite it not being possible to assign it any confidence or likelihood. See also 13-184.
13-196	13	4	26	4	26	change "cannot" to "can" [David Burton, United States of America]	Rejected. Such a statement (the opposite of our assessment) would not be consistent with current scientific knowledge.
13-197	13	4	26	4	27	But because the likely range is defined -- doesn't this imply that these larger values have a less than 17% likelihood of occuring -- i.e. there's an implicit assessment of their probability? [Christopher Little, United States of America]	Taken into account by explaining more clearly that we mean it is not possible to assign a probability to particular levels above the likely range.
13-198	13	4	26	4	27	If larger values cannot be excluded this is not very useful from an impact or adaptation perspective. It means that if somebody advocates a 5 -m rise occurring during the 21st century it is possible and must be considered. Hence clearer statements here would be useful for this debate. Policymakers focus extensively on this upper bound and this makes sense from a risk and decision perspective. What about the UKCP09 approach (Lowe et al., 2009) which developed the H++ scenario as a bounding case for project apprasial. Is this useful or not? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account by including more material from 13.5.3 in the Exec Summ, to make clear that we think it would not be useful to indicate an "upper bound" to which we would not be able to assign a probability with any confidence.
13-199	13	4	26	4	27	The last sentence is KEY, again you should quantify "larger values" What 50m by 2100? No. 1m? Surely possible, 2M, well, your call. The point is that terms like "larger" are too vague and do the policy-maker or other user of the IPCC no favors. I would strongly urge you to turn this sentence into a BOLD statement in your ES, quantify it, and attach confidence language. Even if there is low confidence (you clearly do a nice job describing why) larger values ARE possible and this is IMPORTANT information for those out in the real world who may be looking to spend billions of $$ to build coastal protection based on IPCC results!!! [Jonathan Overpeck, United States of America]	See 13-182 and 13-198.
13-200	13	4	26			Not withstanding limited scientific understanding on the probability of higher values of sea level rise, it would be useful to estimate an upper threshold that sea levels are very unlikely to exceed by 2100. Such thresholds are often valuable to consider in impact assessment. [Kathleen McInnes, Australia]	Taken into account by including more material from 13.5.3 in the Exec Summ, to make clear that we think it would not be useful to indicate an "upper threshold" to which we would not be able to assign a probability with any confidence.
13-201	13	4	29	4	37	I think it can also be said with confidence that the rate over the 22st and 23nd century will likely exceed the rate in the 21st (ofcourse that depends on scenario, but i think it is pretty clear it will be so for all ECPs). [Aslak Grinsted, Denmark]	Rejected. An acceleration beyond the year 2100 without increasing forcing is not uniformly supported by models nor by basic physical reasoning.
13-202	13	4	29			It is difficult to understand why global mean sea level rise beyond 2100 "… unless global temperatures decline" is only "very likely." Section 13.5.2 states that "For increasing GMT, sea level is virtually certain to continue to rise beyond the year 2500 …". [Government of United States of America]	accepted. The text is changed accordingly.
13-203	13	4	30	4	31	This sentence ("Longer-term sea level rise depends on future emissions") is unsupportable. Not only does it assume that GHG emissions are the only fact which can affect temperatures and sea levels, which is transparently false, it ignores the measured data. GCM models assume a strong linkage between GHG emissions and sea-level rise, but the measured data says otherwise. Thus far, > 2/3 century of high GHG emissions and dramatically increasing GHG levels have not resulted in any measurable increase in the rate of sea level rise. [David Burton, United States of America]	rejected. The reviewer does not provide published literature to support his statement and we are not aware of such literature.
13-204	13	4	30	4	31	Please rephrase for clarification. What is meant by "longer term"; what is meant by "future"? [Government of Germany]	accepted. The text is changed accordingly.
13-205	13	4	31			Is there a possibility of composing the future sea level rise of the commitment one and the one due to future	The basis for doing that is not currently available

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						emissions? [Gabriele Hegerl, United Kingdom]	
13-206	13	4	32	4	32	The volume of all mountain glaciers ist given as ~0.6m SLE. The numbers in Chapter4 are: 0.52m (in the Executive Summary) and 0.6m in the body of the capter (p.7). This number has to be made consistent in both chapters. [Peter Lemke, Germany]	Taken into Account. These inconsistancies will be rectified, and the range of estimated total SLE will be discussed.
13-207	13	4	32	4	33	Unclear what is meant by "glacier contribution decreases over time as their volume (currently ~0.6 m sea level equivalent) decreases" [Charlotte Sparrenbom, Sweden]	Noted. This will be elaborated.
13-208	13	4	32			The given estimate for the glacier contribution (0.6 m) is most likely a systematic overestimation because of the problems related to area-related thickness/volume estimates for glacier complexes instead of individual glaciers. Much more realistic values are provided from flux- und slope-dependent high-resolution ice thickness/volume calculations (Huss, M. and Farinotti, D. (2012): Distributed ice thickness and volume of all glaciers around the globe. Journal of Geophysical Research 117, F04010, doi:10.1029/2012JF0025239). The part of ice below sea level (which must be subtracted from the toatal glacier volume, because it does not contribute mass to the ocean when melting) is probably a few centimetres sea level equivalent: this obvious and non-negligible effect had been ignored so far and must now be correctly mentionenned (cf. Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion). A best estimate for the overall glacier coztribution is probably now 0.4 m. [Wilfried Haeberli, Switzerland]	Accepted
13-209	13	4	34	4	35	I believe the joint use of "low confidence" and a likelihood expression is not consistent with the IPCC uncertainty guidance note (Mastrandrea et al 2010). If the authors' confidence in projections is "low", then there is no credible basis for giving a quantified probabilistic assessment ("likely") to projected changes in the real world - it just says something about the statistical distribution across the "ensemble of opportunity" of available model runs - which is a very different issue. Delete the 'likely' and make clear this is based on process-based models, whereas semi-empirical models give higher numbers (e.g. Schaeffer et al, Nature Climate Change, 2012). "The few available results from process-based models indicate a global mean sea level rise by 2300 of less than 1 m for greenhouse gas concentrations ... etc". Consider adding something about higher results from semi-empirical models, with appropriate caveat. [Andy Reisinger, New Zealand]	accepted. The text is changed to focus on a specific time period.
13-210	13	4	34	4	37	I would suggest a verification of the statement that it is likely that sea-level will remain below 1m after 2100 with < 500 ppm. This concentration level implies a warming near 2.5°C for a medium sensitivity, assuming that little aerosols remain in the long-term. There are several publications that suggests that this could be near a tipping point for Greenland melting. In these conditions can it be "likely" that sea-level would remain below 1m? Besides, I am not sure that it is consistent with the guidance on uncertainty that this paragraph provides a "likely" range for a statement that has low confidence. The values are of course informative and useful, but maybe the probabilistic statement is not appropriate. I thank the authors for the verification of this. [Philippe Marbaix, Belgium]	accepted. The text is changed to focus on a specific time period.
13-211	13	4	35	4	35	Modify as follows: "indicate global mean sea level rise DUE TO ICE SHEET MELTING by 2300 is likely …" [Denis Gilbert, Canada]	accepted. The text is changed accordingly.
13-212	13	4	35			It seems that some words have been omitted. The sentence might read: ..indicate additional global mean sea level rise by 2300 compared to 2100 to be less than … [Klaus Radunsky, Austria]	accepted. The text is changed accordingly.
13-213	13	4	39	4	40	I'm not sure that I understand why it is necessary to have 3.1 [1.9 - 4.6] degC warming to trigger a negative SMB on the Greenland ice sheet, when elsewhere in the chapter it is reported that Greenland may already be close to being a positive contributor to GMSL rise (cf. Figure 13.6), and projections all show positive contributions during the 21st century (cf. Table 13.4), for seemingly less warming than the thresholds cited. Or is it possible for the ice sheet to be a net positive contributor to GMSL rise without it having a negative SMB? For example, if precipitation were to increase, there could be greater accumulation but also greater ablation (with higher temperatures) than previously. More meltwater could result, even though the ice sheet might continue to grow. Forget this comment if I'm barking up the wrong tree, but if there's something in it, I think it would be useful to clarify this point about SMB versus net GMSL rise for the less well versed, like me! [Timothy Carter, Finland]	accepted. This is related to the different assumptions that are used in different studies to determine the threshold. This is now explained in both the chapter and less detailed in the ES.

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13-214	13	4	39	4	40	This range (3.1 [1.9–4.6]) looks strange in comparison to the text in the chapter: it appear identical to an estimate published in 2006 (before AR4), while more recent estimates are provided (also excluding a possible topographic feedback), two times at 2.5°C. Why aren't these recent studies reflected in the 3.1 °C central estimate above ? In addition, is the topographic feedback the sole reason for the lower figure in Robinson (2012), as suggested here? [Philippe Marbaix, Belgium]	accepted. ES extensively rewritten
13-215	13	4	39	4	43	From the order of the sentences it appears that the first estimate (3.1) more valid the the second one (1.6). Are there scientific reasons why not to combine the estimates to a range of 0.8-4.6°C? [Government of Germany]	accepted. This is related to the different assumptions that are used in different studies to determine the threshold. This is now explained in both the chapter and less detailed in the ES.
13-216	13	4	39	4	43	It is suggested to keep also the second sentence in bold as it seems very important to consider the change in sea level in any such estimate. [Klaus Radunsky, Austria]	accepted.
13-217	13	4	39	4	43	I find it unjustified how the study by Robinson et al (Nature Climate Change 2012) is excluded from the given temperature range in line 40, although it arguably is the best of all studies. Other studies simply conclude the threshold is where the surface mass balance turns negative, but that is a simplistic assumption and not a result. At best it is an upper limit for the threshold. The key finding of Robinson et al is entirely plausible, namely that through ice flow, the ice sheet already gets drawn down if the SMB becomes negative over a large part of it, like one quadrant. [Stefan Rahmstorf, Germany]	accepted. The different approaches to determine the threshold are now mentioned.
13-218	13	4	39	4	48	It is confusing that only surface mass balance, and not ice sheet dynamics, are discussd here. [Robert Kopp, United States]	accepted. It is now mentioned in accordance with the discussion on the different measures for the threshold.
13-219	13	4	39			It is not clear what is meant by "is projected". Reading of the statement in bold seems like a consensus or majority agreement, but the follow up statement refers to only one study. [Ryan Sriver, United States of America]	ES extensively rewritten
13-220	13	4	42	4	42	I do not think this paragraph correctly summarizes what is written on the page 35. The numbers from Gregory and Huybrechcts (2006) and from Robinson et al. (2011) cannot be directly compared - the first are the temperature range for which GIS surface mass balance turns negative while Robinson's range for temperature is the threshold for complete deglaciation of Greenland. The meaning of "decay" and "threshold" is not clear in this context. [Andrey Ganopolski, Germany]	accepted. This is related to the different assumptions that are used in different studies to determine the threshold. This is now explained in both the chapter and less detailed in the ES. ES extensively rewritten
13-221	13	4	47	4	48	The wording here is unnecessarily vague ("could"). Why not put in some specific examples from 13.5? [Michael Oppenheimer, United States of America]	accepted.
13-222	13	4	47			Recent studies suggest a lower value of 2.5 C yet in the next sentence it states one study estimates a lower threshold of 1.6. if the difference is that the 2.5 C studies don't have ice sheet dynamics, then perhaps make this clearer. [Kathleen McInnes, Australia]	accepted. This is related to the different assumptions that are used in different studies to determine the threshold. This is now explained in both the chapter and less detailed in the ES. ES extensively rewritten
13-223	13	4	48	4	48	Shouldn't there be a statement here about the possibility for a long-term and/or locked-in response of dynamic changes on both ice sheets, especially Antarctica with the possibility of marine instability; as noted in Chapter 4 and later in this Chapter? [Christopher Little, United States of America]	accepted in principle, but we know less about this. ES extensively rewritten
13-224	13	4	50	5	8	In line 4 of page 5, the number "75% and 80%" doesn't agree with "72% and 77%" in line 46 of page 60 (chapter 13.6.7). [Ke Xiu LIU, China]	Taken into account - revised and made consistent with section 13.6.
13-225	13	4	50	5	8	What are the mechansims that lead to regional sea-level change patterns. The mechanisms are unclear [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Noted - the mechanisms are all described in section 13.1 and in detail in section 13.6.
13-226	13	4	52	4	54	Is this regional vs. global contrast different from past behaviour? [Isabel Andreu-Burillo, Spain]	Noted - Most likely the behaviors have not differed.
13-227	13	4	52			Here and elsewhere through the chapter, why is 'very likely' and 'high confidence' stated within this sentence, surely one will do. [Kathleen McInnes, Australia]	Taken into account - stating only "very likely" is required when high confidence.
13-228	13	4	54	4	57	The regional pattern will also result from regional tectonics. [Robert Kopp, United States]	Noted - as specified in section 13.1, this is not subject of our discussion.

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13-229	13	4	55			suggest using "ocean circulation" change here rather then dynamical ocean… [Terrence Joyce, United States of America]	Accepted - text was changed.
13-230	13	5	1	5	1	Insert after the period: ", and in many places local factors such as PGR and land subsidence have larger effects than does global sea level change. E.g., at approx. 1/4 of GLOSS-LTT locations, relative sea level is falling, rather than rising; an increase in the rate of global sea level rise would actually reduce the amount of sea level change observed at most of those locations" [David Burton, United States of America]	Rejected - this level of detail is not suitable for the Executuve Summary, but the general point about regional variability is clearly made, and the context of the variability discussed here is specifically related to changes from climate versus PGR and land subsidence.
13-231	13	5	1	5	1	"more than 100%" is confusing to me. Is this on the positive side, or does it include a SL fall? Perhaps just say of opposite sign? [Christopher Little, United States of America]	Accepted - rephrased in the text.
13-232	13	5	1	5	4	This appears to be a statement about relative rather than absolute sea level change. Most of the preceding discussion relates to absolute sea level, so this needs to be explicit here. [Timothy Carter, Finland]	Rejected - this text is explicitly related to regional sea level change, occurring under the heading "Regional Sea Level Projections.
13-233	13	5	2	5	6	Is this result sensitive to the breaksown of sea level components (i.e. ice loss from different ice sheets vs. thermosteric?) [Christopher Little, United States of America]	Noted - result is not sensitive, especially not over next 100 years.
13-234	13	5	2	5	8	"Very likely" should be "unlikely," or else the rest of the paragraph needs to be rewritten. Like much of the rest of Chapter 13, this paragraph confuses sea level of the open deep ocean ("95% of the ocean") with coastal sea level ("coastlines"). What happens to sea level in the open ocean has no practical consequences, only coastal sea level matters. [David Burton, United States of America]	Rejected - see response to comment 13-13
13-235	13	5	4	5	6	The claim that 75-80% of global coastlines will experience sea level change within 20% of the global average rate is wildly at odds with current measured data. Of 159 NOAA-selected GLOSS-LTT tide gauges, just 13% (20 locations) meet that criterion. The median rate of relative sea-level rise was 1.09 mm/yr, and the geographically-weighted average was 1.133 mm/yr. 20% of 1.133 mm/yr is .227 mm/yr, so the +/-20% range around that average is 0.906 to 1.360. Of the 159 stations, just 20 were in that range. 71 had lower rates of RSL rise, and 68 had higher rates. (http://tinyurl.com/noaa159slrSorted) [David Burton, United States of America]	Statement considered. The chapter consider short term variability as well
13-236	13	5	6	5	6	Without reading the main part of this chapter, the meaning of “probability density function” is unclear – from the context, it appears to be a spatial distribution function – but, if this is the case, it is unclear why the arithmetic mean should differ from the global mean. This needs clarification. [John Hunter, Australia]	Taken into account.
13-237	13	5	6	5	6	probability density function - please check that probability density distribution is not the correct term here and elsewhere in the document. My understanding is that PDD may be correct [Mark Siddall, United Kingdom]	Rejected. The guidance from the TSU is that PDD is non-standard and we should use pdf in AR5.
13-238	13	5	6	5	7	avoid jargon - this won't mean anything to anyone but a statistician! [Terrence Joyce, United States of America]	The point of the text is important, but will be rewritten for clarification.
13-239	13	5	6	5	8	This seems to be a fairly uninformative statement. What is the significance of the mode being greater than the mean? It presumably indicates that the distribution is skewed, with many coastlines having SLR at or a little above average and a long tail of SLR projections across a wide range of low or even negative SLR values. It is difficult to know how to interpret these results without knowing the method used to calculate them. It might also be interesting to know if some regional subdivision of these PDFs is possible (e.g. can coastal areas in low-, mid- and high-latitudes be discriminated?) [Timothy Carter, Finland]	See #13-238
13-240	13	5	6	5	8	This should be phrased in a clearer manner in the summary. [Aslak Grinsted, Denmark]	See #13-238
13-241	13	5	6	5	8	This sentence is almost incomprehensible. [Robert Kopp, United States]	See #13-238
13-242	13	5	6	5	8	The wording is again unnecessarily convoluted and should be simplified and clarified. [Michael Oppenheimer, United States of America]	See #13-238
13-243	13	5	6	5	8	The sentence need to become clearer "In both cases the maximum of the probability density function is larger than the global 7 mean sea level, however, the arithmetic mean is lower than the global mean (0.3 m versus	See #13-238

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						0.37 m for 8 RCP4.5; 0.56 m versus 0.68 m for RCP8.5, respectively)." [Charlotte Sparrenbom, Sweden]	
13-244	13	5	7	5	8	0.37m for RCP4.5 and 0.68m for RCP8.5 don't seem to correspond to numbers on page 13-4 lines 17-21 ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account - the numbers will be made consistent.
13-245	13	5	12			The current wording does not give credit to the significance of the expected change in future extreme sea levels. It is suggested to insert "significant" before "increase". [Klaus Radunsky, Austria]	considered
13-246	13	5	13	5	13	In the first sentence of this paragraph, change "very likely" to "unlikely," and change "an" to "a significant." The rewritten sentence reads, "It is unlikely that there will be a significant increase in the occurrence of future extreme sea level and flooding events." A greater than 80 ppm CO2 increase over the last 3/4 century has resulted in no detectable increase in the frequency or severity of flooding events, so the best evidence is that future GHG emissions will also cause little if any increase in the frequency or severity of flooding events. Speculation that future GHG emissions will have radically different consequences than past GHG emissions cannot reasonably be characterized as "very likely." [David Burton, United States of America]	Analysis of the past tide gauge data as well as projections of extreme sea level showchanges in extreme sea level are linked to changes in mean sea level rise. As mean sea level rises the retun periods of extreme events decrease. This has been shown for different regions.
13-247	13	5	13	5	13	The word "determine" is not a good choice here,because there are also other factors such as the topography can affect the extremes. It's better to replace "determine" with some other word such as "affect". [Ke Xiu LIU, China]	considered
13-248	13	5	14	5	14	What "extremes" are implied in this high confidence statement? Does the "will increase" refer to frequency or magnitude or both? [Timothy Carter, Finland]	Return periods will decrease for a given level
13-249	13	5	15	5	16	How is a high "impact" on exceedance return period to be interpreted, because it is rated as "likely"? Shouldn't this be re-phrased into a statement about projected changes in return period of a given (specified) extreme, with a confidence rather than likelihood statement attached to that? [Timothy Carter, Finland]	considered
13-250	13	5	15	5	18	The final sentence of this paragraph is nonsense. There's no evidence at all to support the claim that "100 year" flooding events will become 10x or 100x more frequent due to future GHG emissions. Putting such nonsense in AR5 will be great for Munich Re's profits, but it is scientifically indefensible. [David Burton, United States of America]	the phenomenon is region dependent and thier is good evidence to support it. The phrase is modified mentioning the regional dependence.
13-251	13	5	15	5	18	The statement on change in extreme sea levels does not link to the driver -- for what magnitude of climate change or sea-level rise? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	there are only limited studies that deal with driver. However, most of the studies point out that mean sea level rise will contribute to the changes in extremes
13-252	13	5	15	5	18	The reason for a decrease in the return period of a given return level is rather unclear. This could be driven by sea-level rise or by a change in surge characteristics. Which is more important? My experience is that mean sea-level rise is the biggest driver of change and clarity on these issues would be useful. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	considered as in the above comment
13-253	13	5	15	5	18	Another key factor which controls the change in return period is the shape of the exceedance curve -- can this be discussed? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	strong regional dependence is included
13-254	13	5	15	5	18	The wording here (and in the underlying text in 13.7.4) is also vague. Is it meant to be a generic global statement? Can more specificity be provided? [Michael Oppenheimer, United States of America]	strong regional dependence is included
13-255	13	5	16	5	16	It is strange to say "likely to be high". Likely has a very specific meaning, but "high" has not. [Aslak Grinsted, Denmark]	ES extensively rewritten
13-256	13	5	16	5	17	This is important stuff: coastal floods of the century will occur yearly! But the jargon obscures this again. [Terrence Joyce, United States of America]	noted
13-257	13	5	16	5	17	What is a return period? [Charlotte Sparrenbom, Sweden]	defined in Glossary
13-258	13	5	16	5	18	About the sentense "for example with current 100-year return period events decreasing to 10-year and possibly 1-year events by the end of the 21st century.", it's better to avoid this kind of statement in the ES part because there is no strong argument to support it. [Ke Xiu LIU, China]	modified by indicating regional dependence
13-259	13	5	16			"Exceeding given threshold levels" is confusing; please rewrite or provide an example of a threshold level.	considered

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						[Government of United States of America]	
13-260	13	5	16			define TW [David Sauchyn, Canada]	agreed
13-261	13	5	17	5	17	"100 year return period events" -> not clear what 'events' are here - extreme sea levels?. Please clarify, because paragraph talks of extreme sea level, flooding, and storminess. [Thomas Stocker/ WGI TSU, Switzerland]	will be clarified
13-262	13	5				Reference is made here to projected changes in surface wave height in the Southern Ocean, due to projected strengthening of Westerlies, and the South Pacific, due to projected strengthening of austral winter easterly trade winds. It needs to be specified whether this means increased wave intensity (this is implied but not made explicit). [Government of Australia]	An explicit statement will be included
13-263	13	6	1	6	1	Text on figure is too small, also Fig. 13.5 [Peter Clift, United States of America]	Font size will be modified.
13-264	13	6	1	6	52	I commend the authors for this text - the context for the ongoing scientific struggle here is vital, given the disagreements after AR4. It is a very good idea to include this history and it is well written [Mark Siddall, United Kingdom]	Noted - Thank you
13-265	13	6	1	8	5	This introductory section and definitional section are really good, easy to understand and communicate advances in knowledge really well. [Government of Australia]	Noted - Thank you
13-266	13	6	1			Would it be helpful to show the past track record of SLR projected by the earlier IPCC WG1 reports compared to observations? I appreciate there are many issues and maybe just thermosteric changes could be shown. However I think the figure could be helpful. [Ronald Stouffer, United States of America]	Noted - This material is in Chapter 1
13-267	13	6	5	6	6	This sentence is unclear; sugest revision [Government of United States of America]	Accepted - text revised
13-268	13	6	5			Page 6, line 5. Delete “the” before “many factors". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
13-269	13	6	6			Please clarify "contributing make it an integral..." [Government of United States of America]	Accepted - text revised
13-270	13	6	6			Page 6, line 6. Insert “to" before “make it". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
13-271	13	6	7	6	7	Need to distinguish between coastal and mid-ocean sea level here. "The expansion of the ocean as it warms" affects the later, but has little effect on the former. Expansion of water in the deep ocean would affect coastal sea levels, but not expansion of surface water, which rises in place, like floating ice, and does not cause lateral flows, because gravity balances mass, not volume. [David Burton, United States of America]	Rejected. This statment is incorrect - see section 13.1.2 for new text addressing this comment.
13-272	13	6	8	6	8	Church et al is not an original reference for glacier and ice sheet contributions [Mark Siddall, United Kingdom]	Accepted. Reference moved so that it applies to all contributions, and additional references added.
13-273	13	6	9			Page 6, line 9. Insert “that" after “indicates”. (Please note that the Anglo-Saxon contraction that is made by omitting “that" is grammatically correct, but makes text hard to follow for non-native speakers.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
13-274	13	6	23	6	39	The relevant constituent chapters in the AR4 should be cited in this chapter. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
13-275	13	6	23			Page 6, line 23. Remove the space before the comma before “coupled". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted
13-276	13	6	26	6	27	The adjective "global" should be the adverb "globally", since it modifies the adjective "averaged". Alterately, "global average" would also be correct, as then the adjective would be modifying a noun. [Robert Hallberg, United States of America]	Accepted
13-277	13	6	30	6	31	"…time-variable spatial distribution…" awkward statement, meaning ? [Government of United States of America]	Accepted - deleted "time-variable"

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13-278	13	6	31	6	31	"the past" after over [Christopher Little, United States of America]	Accepted
13-279	13	6	33	6	33	I believe that "(Rahmstorf et al., 2007; submitted)" should be "(Rahmstorf et al., 2007; Rahmstorf et al., submitted)" [Robert Hallberg, United States of America]	Accepted - Rahmstorf et al. 2012 now published
13-280	13	6	33	6	33	Rahmstorf et al. (subm) is now Rahmstorf et al. 2012, http://iopscience.iop.org/1748-9326/7/4/044035/pdf/1748-9326_7_4_044035.pdf [Stefan Rahmstorf, Germany]	Accepted
13-281	13	6	33	6	33	The references to Rahmstorf is unclear, there is apparently a citation to 2007 and an submitted one? [Ernst Schrama, Netherlands]	Accepted - Rahmstorf et al. 2012 now published
13-282	13	6	33	6	33	Please check status of this reference. Year 2007 and status "submitted" would be odd. [Ralf Weisse, Germany]	Accepted - Rahmstorf et al. 2012 now published
13-283	13	6	33	6	34	Please delete: "although in general the uncertainties were large enough that there was no significant contradiction." Justificaiton: First, given the assertion on page 46 line 45 that "1.8 ± 0.5 mm yr–1, and the sum of terms as 1.1 ± 0.5 mm yr–1", then assuming no correlation in the two sources of information, the difference between the two would be 0.7± 0.7 mm yr–1, so there is a contradiction in spite of the uncertanties. Second: One could argue that point either way: Yes, if the error bounds were viewed as confidence intervals, one could not rejct the null hypothesis that the components explained the totals. But there were certainly articles, review comments, and government comments suggesting that, in fact, something fundamental was missing. If you had more space, you might explain that issue in detail--most important, the projections were showing less of a contribution 100 years hence than the data showed was already occurring! But lacking space, and recognizing that self-reflection is not necessarily what assessments do, the more expedient thing to do is just delete this clause, which tends to undermine the strength of the "first" point being made. [James G Titus, United States of America]	Accepted - phrase deleted.
13-284	13	6	33			Rahmstorf, 2007 is surely not submitted. The semi-empirical models are discussed later in the chapter in detail. Consider moving this paragraph further below. [European Union]	Rejected - Rahmstorf et al. 2012 now published. This is not about semiempirical models.
13-285	13	6	33			still submitted or published in 2007 - it can't be both. Although some papers do take a long time to get published! [Terrence Joyce, United States of America]	Noted - Rahmstorf et al. 2012 now published
13-286	13	6	33			reference to Rahmstorf et al. 2007 states submitted? Please correct. [Thomas Stocker/ WGI TSU, Switzerland]	Rejected. There are two publications, 2007 and 2012. Rahmstorf et al. 2012 now published
13-287	13	6	36	6	39	This is a very important sentence as I feel it is not readily apparent in the AR4 that this was the case in Meehl et al (2007). A casual reader would certainly not note this point. The clearest statement in the AR4 is in the IPCC Synthesis Report and this should be cited here. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account - a reference added to Solomon et al. (2007), but we ratin the reference to Meehl et al. as well, since the wording is in their chapter.
13-288	13	6	37			Please clarify what is accelerating.. "observations of ice sheet accelerations" [Government of United States of America]	Taken into account - wording revised.
13-289	13	6	39	6	39	"…future contributions" would be more clear if it read "…future contributions to sea level rise." [Government of United States of America]	Accepted
13-290	13	6	41	6	46	The case is made that the sea-level projections have been remarkably similar between the four previous Assessment Reports, and from the cited numbers this also applies to AR5. For AR4, this is only the case when the ice-sheet dynamical changes are included in the main projections (which they were not). This statement ('remarkably similar') seems contradictory with p 51, lines 38-50, where it is stated that 'the largest increase is from changes in Greenland and Antarctic ice sheets'. A clear communication around this issue will be very important to the credibilty of the process (the question whether the AR4 and AR5 are now very similar or not should have a crystal clear answer). [Philippe Huybrechts, Belgium]	Taken into account - reworded for AR4. Later in chapter, we clearly draw the distinction between AR4 projections without ice-sheet dynamics and those made here, which include ice-sheet dynamics.
13-291	13	6	41	6	46	suggest to refer to Chapter 1, Figure 1.11 presenting the projected SLR from FAR to AR4 and a comparison to observed SLR estimates [Thomas Stocker/ WGI TSU, Switzerland]	rejected. The sentence draws the comparison for projections for 2100, whereas Figure 1.11 only compares to present.

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13-292	13	6	43			avoid colloquial use of "likely" since it's official IPCC uncertianty language [Michael Oppenheimer, United States of America]	accepted - phrase deleted
13-293	13	6	44	6	45	The progressive decrease in predicted SLR and its uncertainty with each Assessment is surprising. Each IPCC Assessment appears to under-estimate critical predictions, and I would endorse the recent suggestion by Ramstorf that ""... the IPCC needs to have a critical look at why they have underestimated certain developments, like sea-level rise or the loss of Arctic sea ice, and make sure this does not happen again in its forthcoming report,In areas where we do not yet have reliable models, as is the case for sea level, the existence of large uncertainties about future impacts of global warming should be honestly acknowledged." My impression is that as we learn more, we also realize just how much more we need to learn, and if we seek credibility, we should reflect this awareness by trying to make realistic estimates of uncertainty. Simply accepting published uncertainty estimates is not good enough. This is my major concern about this chapter. [Robert Thomas, United States of America]	Noted. Later in this chapter we attempt to do what is requested.
13-294	13	6	44			FAR, SAR and TAR should be defined here, or included in the glossary. [Government of United States of America]	Accepted.
13-295	13	6	45	6	46	Does the parenthetical expression refer to AR4? Please clarify. [Government of United States of America]	Accepted - reworded
13-296	13	6	48	48		This introductory section has been re-written and some things may have got lost. For instance, the definition for "CMIP5" (which is extensively used throghout the whole chapter) appeared in the FOD, but it does not appear in the SOD. [Belén Martín Míguez, Spain]	Noted - CMIP5 is not used here but will be defined when it is first used.
13-297	13	6	51	6	51	"reveal" instead of constrain? [Christopher Little, United States of America]	accepted
13-298	13	6	51	6	52	"……constrain glacier etc……" is unclear [Government of United States of America]	accepted - reworded
13-299	13	6	55			as -> those [Kathleen McInnes, Australia]	Accepted - reworded
13-300	13	6	55			Page 6, line 55. Replace “as" by “that". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted - reworded
13-301	13	7	14	7	15	Rahmstorf's so-called "semi-empirical" approach is junk science: fitting arbitrary parameters to noise, and then using the resulting formula, not grounded in any physical basis, to make wildly implausible projections. [David Burton, United States of America]	Rejected. We have to assess the full range of literature.
13-302	13	7	15			What about beyond the 21st Century ? [Kathleen McInnes, Australia]	Accepted - wording generalised
13-303	13	7	17			Not clear. Suggest that this be rewritten. [Government of United States of America]	Accepted - this sentence is now deleted
13-304	13	7	20	7	23	Why are these topics not addressed. From an impacts perspective they are very important. The reason they are not included should be stated explictly. Do you for example expect WG II chapter on coasts to address? Have their been discussions between the CLAs of this chapter and the CLAs of the relevant WGII chapter to address this issue? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Rejected - outside the scope of this chapter and this assessment
13-305	13	7	20	7	23	I am surprised that Nicholls et al (link below) is not cited here as the goal of this document is to act as a bridge between WGI perpsectives and WGII perspective concerning sea-level rise. See http://www.ipcc-data.org/docs/Sea_Level_Scenario_Guidance_Oct2011.pdf. This comes back to the issue in an earlier comment/query about the role of this chapter in supporting impact and adaptation analysis of sea-level rise, or is it just about sea-level science? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted - Reference added
13-306	13	7	23	7	23	add to the end of the final sentence: ", factors which increase measured RSL, tending to cause globally averaged sea level rise measurements to overstate true global sea level rise." [David Burton, United States of America]	Rejected. This statement is about future projections. An assessment of past sea level change is primarily covered in Chapter 3.
13-307	13	7	25	7	28	The idea of climate caused SLR is missing here. The slow increase makes a given location more vulnerable to natural variability. [Ronald Stouffer, United States of America]	Accepted - reworded.
13-308	13	7	32			It is stated that "is measured either with respect to the surface of the solid Earth (relative sea level) or a geocentric reference such as the reference ellipsoid geocentric sea level)." However, if the sea level is	Rejected. This text refers only to direct, site-specific measurements of sea level. Global mean sea-level is

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						expressed in terms of volume change allowing for lowering of the ocean floor, this sea level is with respect to the lowering of the ocean floor. [Robert Dean, United States of America]	not directly measured as described in the following paragraph.
13-309	13	7	35	7	41	statement “The temporal average, known as Mean Sea Level (MSL; see Glossary), is used to remove the effects of tides, storms and waves” has several problems with respect to waves. (1)It seems from the writing, that effect of waves could be delineated from tide-gauges or satellite altimetry, which is not a conventional method and might be hardly possible with the current technologies. (2) Why and how the “effect of waves” seams separated from “effect of storms”? (3) There have been any literature which proven effect of waves on regional sea level? Unless we are talking about coastal processes, which are beyond of resolution of current GCMs. IPCC AR5 estimates wave height but don't estimate wave setup which really may cause MSL variability. The difference in contribution of wave setup rather than wave height into MSL must be clearly stated. [Pavel Tkalich, Singapore]	Acccepted. Text changed to "A temporal average, known as Mean Sea Level (MSL; see Glossary), is applied to remove shorter period variability"
13-310	13	7	37	7	38	May be helpful to note, here or elsewhere, that "geocentric sea level" is what is projected as "sea surface height" in climate models. [Robert Kopp, United States]	Climate models do not compute geocentric sea level as changes in gravity are not considered.
13-311	13	7	38	7	38	add to the end of the final sentence: ", but the data remains inferior to tide gauge measurements." [David Burton, United States of America]	Rejected. The reviewer does not state in what way these data are inferior nor provide any supporting literature.
13-312	13	7	40	7	40	I suggest '...in an effort to remove tides....' here [Mark Siddall, United Kingdom]	Accepted. See response to comment 309.
13-313	13	7	40	13	42	MSL is defined as the average of hourly values over a period of 19 continuous years. Using MSL here is not clear if it is using this definition or not. If yes, then one needs to use estimates of MSL change in step intervals of 19 years.Otherwise, one must account for the impact of sea level rise with time. A possible solution could be to define a new characteristic parameter, the Yearly Mean Sea Level (YMSL) or to clearly diferentiate names a Yearly Average Sea Level (YASL) based also on hourly averaged values, and look to its fluctuation in time, over the years. Furthermore, for obtaining a proper averaging, it should be done not over a Gregorian calendar year (January-December), but on a hydrologic year, for example from October of year 1 to September of year 2, as this covers over most of the globe a year including the main yearly wave storms season, the yearly monsoon/rainy season and the main hurricane season. This would enable that the storm and hydrologic events within one such year can be considered almost independent from those of the following hydrologic year, and a correct statistical assessment can be performed. Given that approach, it is estimated that the yearly average values of the sea level will change relatively to what has been calculated and presented so far in this chapter and so may be also the obtained sea level change trends. [Sergiu Dov ROSEN, Israel]	Rejected. The definition of MSL in the Glossary is clear.
13-314	13	7	40			Page 7, line 40. In this section I find there is scope for confusion between ocean volume and Ocean basin volume. Perhaps it would be better to speak of the volumetric capacity of the ocean basins rather than Ocean basin volume? It just may make things easier to follow. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted. This distinction is now made explicit by writing "ocean water volume" or "ocean basin volume."
13-315	13	7	41	7	41	Should include longer term atmospheric effects such as wind-driven variations in the Baltic Sea. [European Union]	Rejected. Specific longer-term processes are described later where appropriate. Main purpose of this paragraph is to define MSL and GMSL and how the latter relates to ocean volume Accepted in the sense that this statement is now more general (see response to comment 309.
13-316	13	7	43	7	44	“integrated…. relative sea level change (or GMSL) gives the change in ocean water volume” [Isabel Andreu-Burillo, Spain]	Rejected - based on our understanding, integrating relative sea level change over ocean area does provide an estimate of of ocean water volume change.
13-317	13	7	43	7	44	this would be a change in the way the volume of water occupies the basin, but wouldn't necessarily imply [Isabel Andreu-Burillo, Spain]	Rejected - based on our understanding, integrating relative sea level change over ocean area does provide an estimate of of ocean water volume change.
13-318	13	7	43	7	44	a change in the volume of water, would it? The volume of water would imply in/out-flow of water through	Rejected - based on our understanding, integrating

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						[Isabel Andreu-Burillo, Spain]	relative sea level change over ocean area does provide an estimate of of ocean water volume change.
13-319	13	7	43	7	44	Increased precipitation/other or a change in the water proterties. [Isabel Andreu-Burillo, Spain]	Rejected - based on our understanding, integrating relative sea level change over ocean area does provide an estimate of of ocean water volume change.
13-320	13	7	43	7	50	Some meanings of the sentences are not clear or so much compact. I recommend to reconsider if more easier or detailed description can be possible for the various levels of potential readers. [Sok Kuh Kang, Republic of Korea]	Accepted. Text revised in an effort to improve clarity and readability.
13-321	13	7	44	7	44	"GMSL change" [Christopher Little, United States of America]	Accepted. This text has been revised (see response to comment 320).
13-322	13	7	45	7	48	Sentence starting with “In contrast...” is complicated. [Isabel Andreu-Burillo, Spain]	Accepted. See response to comment 320.
13-323	13	7	45	7	48	This sentence is tough to parse. I recommend a rewrite. [Stephen Griffies, United States of America]	Accepted. See response to comment 320.
13-324	13	7	45	7	49	these two sentences will appear contradictory; you need to elaborate on what correction the second refers to. [Michael Oppenheimer, United States of America]	Accepted. The last sentence of this paragraph has been expanded to remove this apparent contradiction.
13-325	13	7	45	49		I find these sentences confusing and the reference to Tamisiea not precise enough (in his work, he suggests that different corrections must be applied depending on the dataset). I suggest the following reformulation:"In contrast, integrating geocentric sea level change over an ocean area in particular does not necessarily result in ocean volume change because there are changes in ocean basin volume caused, largely, by the on-going deformation of the Earth to the most recent global deglaciation (Mitrovica and Peltier, 1991). A small correction (-0.15 to -0.5 mm yr-1) is required to estimate ocean volume change (Tamisiea, 2011) from altimetry estimates." [Belén Martín Míguez, Spain]	Partially accepted. The text has been shortened given space constraints but elements of the reviewer's suggested wording have been adopted.
13-326	13	7	46	7	46	“due to” is clumsy/ambiguous here; you don’t mean the ocean volume changes due to ocean basin changes, but that the mathematical procedure is incorrect. Sentence should be made more precise. [European Union]	Accepted. See response to comment 320.
13-327	13	7	48	7	49	change the sentence "A small correction… is required to estimate ocean volume change" to read "A small correction… is required to estimate ocean basin volume change, for water mass budget calculations (but should not be used to adjust measured sea level rise)." [David Burton, United States of America]	Rejected. Although note that this text has been changed in response to comment 320..
13-328	13	7	48			Is this due to changes in the reference ellipsoid itself? Better explanation needed. [Terrence Joyce, United States of America]	Rejected. Space constraints limit the level of detail that can be provided. Please see cited literature.
13-329	13	7	52	7	57	Have to tone down the statement ""“Any process that causes vertical motion of the ocean surface or ocean floor will result in relative sea level change” or it will rise several questions: (1) What about tsunami-genic earthquakes and tsunami? They cause vertical motion of ocean floor and ocean surface, does it mean it must be included in regional sea level estimations. (2) Swells (which cause vertical motion of ocean surface) unlikely affect sea level, unless as part of coastal processes, which is not part of Chapter 13 according to statement at Page 9 line 23. (3) The same for gravity-capillary waves. It must be qualified the scales (time-space) of processes considered in the Chapter and those neglected. [Pavel Tkalich, Singapore]	This paragraph has been removed due to space limitations.
13-330	13	8	1			It seems to me that shelf loading due to larger deep ocean steric SLR compared to on the shelf is important to be discussed here. See Yin et al. 2010. [Ronald Stouffer, United States of America]	This paragraph has been removed due to space limitations.
13-331	13	8	5	8	5	Add that isostatic adjustment occurs over longer timescales than the process mentioned in lines 2-3. [European Union]	This paragraph has been removed due to space limitations.
13-332	13	8	10			Some grounded ice of glaciers is below sea level and does not contribute: Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion. [Wilfried Haeberli, Switzerland]	Accepted - changed "ice grounded on land" to the more general term "land ice". This is more in line with the general nature of the statement being made.

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13-333	13	8	20	8	25	"Geocentric sea level" in Fig.13.1 needs to be explicitly plotted from the reference ellipsoid (or explained), not from ceratin depth in the ground. Or additional explanation is to be given. [Sok Kuh Kang, Republic of Korea]	Accepted. Additional explanation has been added to the figure caption.
13-334	13	8	27	8	31	Pehraps the first two sentences are too much detail. Can they be removed and substituted with a reference in the sentence that begins "Changes in temperature…"? [Christopher Little, United States of America]	Accepted.
13-335	13	8	27	8	41	I suggest more citation is needed here, e.g. Papers by Wunsch but many other physical oceanographers [Mark Siddall, United Kingdom]	Rejected. We believe the number of papers referenced is appropriate and in balance with other paragraphs.
13-336	13	8	30	8	30	“ocean mixing” -> “multi-scale ocean mixing processes”? Otherwise slightly ambiguous. [European Union]	Due to space constraints, this text was removed.
13-337	13	8	31	8	31	After the 2nd sentence of the paragraph, you need to note here the time scale: the Atlantic Conveyor transit time is about 1000 years (which is one of the reasons that anthropogenic factors cannot yet have much affected the ocean depths). [David Burton, United States of America]	Due to space constraints, this text was removed.
13-338	13	8	31	8	31	change "affect sea level" to "affect sea level of the open ocean (but does not much affect coastal sea level)" [David Burton, United States of America]	Due to space constraints, this text was removed.
13-339	13	8	34	8	34	Suggestion: “and sea level far from the location of the initial change” [Isabel Andreu-Burillo, Spain]	Accepted.
13-340	13	8	35	8	35	Add s reference 'Marcos et al., 2012' (Prog in Oceanogr, 105, 4-21) after 'Churgh et al., 2010)'. [Jae Hak Lee, Republic of Korea]	Accepted.
13-341	13	8	44	8	46	Not true in regions of sea level fall due to gravitation, solid earth effects. [Christopher Little, United States of America]	Accepted. Changed "sea level rise" to "sea level change"?
13-342	13	8	50	8	53	“Water mass exchange... results in contemporary sea level change due to vertical movement of the ocean floor...” [Isabel Andreu-Burillo, Spain]	See reponse to following comment.
13-343	13	8	50	8	53	Contemporary? What are the time-scales of the vertical adjustment of the ocean floor? [Isabel Andreu-Burillo, Spain]	Accepted. The word "contemporary" has been removed.
13-344	13	8	50	8	56	The description of finger-printing will leave many or most non-specialists bewildered and would benefit greatly from a somewhat expanded explanation, including the current gravitational anomaly, the meaning of the inertia tensor and how this and Earth's rotation affect sea level. [Donald Forbes, Canada]	Rejected. Our intention is only to outline key processes in this section. Space constraints do not permit a more detailed overview. The reader is expected to refer to the cited literature for more information.
13-345	13	8	54	8	56	an explanation to the "sea level fingerprints" is needed [Charlotte Sparrenbom, Sweden]	Rejected. See response to comment 344.
13-346	13	9	1	9	35	This is all a bit ambiguous, owing to the large number of time-scales involved as past climate change merges into current climate change – at the moment I’m having to interpret this rather than understanding the way you separate processes. I think you’d be better of quantifying these time-scales, though I appreciate this might cause space problems. [European Union]	Rejected. This section has been shortened and merged into previous section due to space considerations.
13-347	13	9	1	9	35	The title of this section - Sea level Change Not Caused by Comtemporary Climate Change - can be argued to be inappropriate and should be changed. Likewise the opening sentence. Sea-level is a variable (in GCOS parlance an essential climate variable) that in part defines the state of the Earth's climate in a broad sense, just as near-surface temperature or precipitation does. It is one of the variables for which adaptation to change will be needed. From this viewpoint, sea-level change is part of climate change, not something caused by climate change. Where this section uses "not caused by contemporary climate change" other chapters use "external forcing" of climate change (or variability). There is much discussion in other chapters of impact of volcanic activity on atmospheric temperature for example. It can be argued that there is not much logical difference between this and sea-level change due to other geological processes, even though for the case of volcanic activity the mechanism involves aerosols that perturb the radiative balance, and thus have to be considered alongside radiative changes of anthropogenic (and solar) origin. See also comment 166. [Adrian Simmons, United Kingdom]	Accepted. This section and the previous one have been shortened and merged and these changes have addressed the issue raised here.

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13-348	13	9	1			Section 13.1.4 - "Contemporary Climate Change" is a curious term, ambiguous and undefined. The reader will likely assume it means "anthropogenic climate change," but that doesn't seem to be the case. It needs to be defined, and the document needs to make it very clear that the rate of sea level rise has not measurably increased in response to anthropogenic forcings. [David Burton, United States of America]	This heading has been removed.
13-349	13	9	3			Page 9, line 3. Replace “are" by “is". (There is grammatical confusion throughout the text about this. When talking about a number, you use plural, and when talking about for example a combination, you use singular. The correct approach is to use singular forms of the verb, because the subject is singular, or to decide to use an implicit plural and to then be consistent throughout.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	For space considerations, this text has been removed.
13-350	13	9	5			Page 9, line 5. I think it would be helpful to insert “ongoing" before “deformation". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	For space considerations, this text has been removed.
13-351	13	9	12			Not clear. Suggest that this be rewritten. [Government of United States of America]	Accepted. Text has been revised to improve clarity.
13-352	13	9	15	22	47	According to our research, we're not convinced that evapotranspiration in the case of replacing vegetated land with a water storage body would have a significant effet. The authors appear to assume that all dams divert water from a natural course, but this is not the case. Only a portion of dams do this, and this effect will be very site- and purpose-specific. [Tracy Lane, United Kingdom]	Rejected. Text states that these processes can "potentially cause sea level change" and so does not contradict the reviewer's findings.
13-353	13	9	23	9	23	Why are these processes excluded? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	They are excluded because these processes are not sufficient to impact regional or global sea level trends over the time periods that are the focus of this chapter (decades to centuries). The text has been revised to make this point more explicit.
13-354	13	9	23			please define coastal processes. I think of storm surges as a coastal process, and that IS a part of this chapter. [Terrence Joyce, United States of America]	Accepted. Wording changed to "sediment transfer and compaction in the coastal zone".
13-355	13	9	25			should be "0"? [Government of United States of America]	See response to following comment
13-356	13	9	25			It is not clear if what (<O[0.1 mm/yr])nomenclature means. How does one read/interpret this value? [Government of United States of America]	Accepted - nomenclature removed
13-357	13	9	29	9	30	“... which causes further sea level change” Through redistribution mechanisms? [Isabel Andreu-Burillo, Spain]	This text has been removed due to space considerations.
13-358	13	9	32	9	35	Does WG II consider these processes -- is there discussion between WG I and WG II on this point? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Yes and a reference to the appropriate chapter has been included.
13-359	13	9	37			Section 13.1.5 - GCMs are less useful than actual measurements, for projecting sea level rise. When there is no actual, measured data then GCMs can be useful, but it is a huge mistake to prefer model-derived pseudo-data to actual, measured data. W/r/t sea level rise, we have excellent measured data going back > 150 years, which tells us how sea level responds to GHG forcings. To the extent that model-derived results diverge from the measured reality, the models are, by definition, wrong. [David Burton, United States of America]	Noted.
13-360	13	9	39	9	40	It states here that AOGCMS include cryospheric processes yet in the analyses that follow none of them are used to assess glacier/iceh sheet changes directly. Instead, AOGCM o/p is used to drive glacier or ice sheet models. This statement could, therefore, be misleading or require clarification. [Jonathan Bamber, United Kingdom]	Rejected. The text does not state that AOGCMs include cryospheric processes, only that they include a component representing the cryosphere.
13-361	13	9	43	9	43	"Nino" needs a tilde over second "n". [Stephen Griffies, United States of America]	Editorial
13-362	13	10	1	10	7	This seems very pedantic - 'text booky'. Is this level of detail about models necessary? [Terrence Joyce, United States of America]	Accepted. This text has been removed.
13-363	13	10	9	10	9	I suggest 'conceptual' here. I am note sure that this falls into a rigorously defined 'statistical relationship' - correlation is not causation and all that [Mark Siddall, United Kingdom]	Rejected. Given the general nature of the discussion, the statement made is appropriate and accurate.

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13-364	13	10	9	10	17	Rahmstorf's so-called "semi-empirical" approach is junk science: he has enough tweakable arbitrary parameters that he can make the elephant wiggle its trunk (von Neumann). [David Burton, United States of America]	Noted.
13-365	13	10	9	10	17	Why bring up semi empirical models - what is the benefit of it? [Charlotte Sparrenbom, Sweden]	They are in the refereed literature and so therefore within the remit of the AR5.
13-366	13	10	16	10	16	confused by the statement that a response time is infinite in a semi-empirical model integrated over a finite time-span. Surely an infinite response time means nothing happens? [European Union]	See response to following comment.
13-367	13	10	16	10	16	"infinite" response time: actually the paper says: "The equilibration time scale is expected to be on the order of millennia. Even if the exact shape of the time evolution H(t) is not known, we can approximate it by assuming a linear increase in the early phase; the long time scales of the relevant processes give us hope that this linear approximation may be valid for a few centuries." So the idea here is not that it is infinite, but that it is long compared to a couple of centuries, the time scale of interest. Consequently, when we turned our attention to longer time scales (in Kemp et al) we explicitly included this time scale. [Stefan Rahmstorf, Germany]	Accepted. Text revised to "This response time could be considerably longer than the timescale of interest".
13-368	13	10	25	10	30	Perhaps a better term is "bottom-up" instead of process based. No model truly represents all processes. The aggregated effect of some of them is parameterized (perhaps a better word than semi-empirical here, as it is more commonly used in climate modelling). Some of these parameterizations are able to be calibrated with data. [Christopher Little, United States of America]	Partially accepted. Sentence changed to "that aim to simulate the underlying processes and interactions, in contrast to …, which do not."
13-369	13	10	25	10	30	nicely put but references needed - where are SE models used within process models? [Mark Siddall, United Kingdom]	Accepted. Reference to paper by Braithwate et al. (199?).
13-370	13	10	26	10	26	put “semi-empirical models” in inverted commas. [European Union]	This is not approriate everywhere
13-371	13	10	32			seems odd to have just one 4th level heading in this section. [Kathleen McInnes, Australia]	OK
13-372	13	10	34	10	35	cf previous remark. May be a cross reference is needed here. [Jonathan Bamber, United Kingdom]	accepted - need to clarify what is in and out of AOGCMs
13-373	13	10	38	10	52	Important points for sea level projections beyond 2100. Important point for policy makers that should be emphasised. [European Union]	noted
13-374	13	10	40	10	40	"melt" should be "melt and sublimation" [David Burton, United States of America]	editorial
13-375	13	10	40	10	42	please refer to the relevant sections in Ch4, WGI AR5 [Thomas Stocker/ WGI TSU, Switzerland]	Noted
13-376	13	10	40		42	As Chapter 4 shows the ice sheets are not in steady state and SMB and discharge not in balance! [European Union]	taken into account - satement is close to steady state not in steady state
13-377	13	10	41	10	42	"Contemporary" What does this mean here? Surely not now, as in 2012?? Very low rates of SLR until mid/late 20th century suggests they may heve been close to mass balance for a some centuries, but recent observations show strongly negative balance for Greenland, and modestly negative balance for Antarctica. [Robert Thomas, United States of America]	taken into account - give imbalance as a %age throughput
13-378	13	10	42			italics should be used on calibrated likelihood statements 'very likely' [Kathleen McInnes, Australia]	agreed
13-379	13	10	42			SMB stands for "surface mass balance". So in what sense can "SMB and outflow" be "nearly in balance". The correct statement would be that surface mass change (or imbalance) and outflow are nearly in balance. [Adrian Simmons, United Kingdom]	rejected - this is simply mass conservation
13-380	13	10	43	10	43	Might be useful to write down sea-level equivalents of throughputs for Antarctic ice-sheet (5-6mm?) and Greenland ice-sheet (2mm?) for context. [European Union]	accepted

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13-381	13	10	49	10	49	"experiment"?!? Vizcaino et al didn't perform any experiments! All they did was play with computer models. That's not an experiment, that's a calculation. You should NEVER confuse the two! [David Burton, United States of America]	rejected - this is common terminology.
13-382	13	10	51			Page 10, line 51. “Antarctic” should be “Antarctica”. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted
13-383	13	10	55	10	55	The base state is important and choosing it wrong will bias the resulting projections. For Greenland it seems that our best evidence says the ice sheet has been out of balance throughout most of the 20th century. See Bjørk et al. for the SE outlet glaciers. [Aslak Grinsted, Denmark]	taken into account - this point is made in next sentence
13-384	13	11	1	11	2	These are placeholders for the published papers, but they must be published before this is accepted! There are LOTS of these 'submitted' references in this chapter. [Terrence Joyce, United States of America]	Accepted - have had to delete a number of papers not accepted in time
13-385	13	11	6	11	9	Not really a GIS/AIS distinction here -- the basis is whether the ocean is responsible for the changes in ice mass. The distinction is between marine based and non-marine based (although regional oceanography may influence Non-marine based as well). [Christopher Little, United States of America]	noted - not attempted to contrast simply to give examples
13-386	13	11	10	11	10	what is the grounding line position? [Isabel Andreu-Burillo, Spain]	accepted - need to move definition
13-387	13	11	11	11	11	The sentence talks about 'change in outflow' and 'changes in grounding line position' without specifying the direction of the change. Therefore SLR (sea level rise) should be substituted by the neutral term SLC (sea level change). [Philippe Huybrechts, Belgium]	accepted - need to specify
13-388	13	11	15	11	17	Remove -- used later and too much detail here. [Christopher Little, United States of America]	accepted
13-389	13	11	16	11	16	“Major source” – quantify proportion with respect to total ice-sheet mass balance. [European Union]	accepted but will omit
13-390	13	11	16			Van den Broeke et al., 2009 and Lenaerts et al., 2012 is missing - RACMO2 is the most up-to-date model for surface mass balance processes, including melting, refreezing in the snowpack. Van den Broeke M, et al. Partitioning recent Greenland mass loss. Science. 2009;326:984-986 Lenaerts, J.T.M., van den Broeke, M.R., Déry, S.J., van Meijgaard, E., van de Berg, W.J., Palm,S.P. and J. Sanz Rodrigo (2012): Modeling drifting snow in Antarctica with a regional climate model: 1. Methods and model evaluation. Journal of Geophysical Research 117, D05108, doi:10.1029/2011JD016145. [European Union]	Noted. While RACMO is the besy model available, it does not have the capacity to model heterogeneous infiltration and piping. This point will be clarified.
13-391	13	11	18			Page 11, line 18. “are” should be “is". (The subject here is “information", which is singular.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-392	13	11	21	11	28	Important statements on improvements to ice sheet models which are relevant for policy makers. [European Union]	noted -
13-393	13	11	22	11	26	If this statement is true, then this chapter can be greatly reduced. We don't need to know the details or projections of inadequate models. [Terrence Joyce, United States of America]	rejected - no model is perfect
13-394	13	11	24	11	24	"some of" before simulating [Christopher Little, United States of America]	accepted
13-395	13	11	25	11	26	It is stated here that there is not a sufficient basis to provide a "time-dependent" or scenario-dependent assessment of SLR from changes in ice sheet dynamics. However, later on page 45, lines 18-19, you only state that the basis is insufficient to provide scenario-dependent projections, and Figure 3.9 does indeed provide time-dependent information, albeit using dashed lines, rather than solid lines. Please clarify - is there a sufficient basis to provide a time-dependent assessment or not? [Thomas Stocker/ WGI TSU, Switzerland]	accept - need to clarify how time dependence derived.
13-396	13	11	25	11	28	This paragraph should provide more detail. It will appear quite mysterious to anyone who doesn't already know the direction you are headed. [Michael Oppenheimer, United States of America]	accepted - see above.
13-397	13	11	26	11	26	Levermann et al. Submitted-b has been able to make scenario dependent estimates of Antarctic discharge based on SeaRISE results. Faezeh Nick is able to do it for GrIS calving. [Aslak Grinsted, Denmark]	noted - one paper does not an assessment make - and it is not puiblished
13-398	13	11	30	11	30	Ice sheet dynamics is not treated in chapter 4 in such a way that it can be referenced like this. [European	noted - need to specify section number

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						Union]	
13-399	13	11	30	11	30	replace “rely” with “are strongly constrained by” or something similar. [European Union]	editorial
13-400	13	11	30			section on limitations of the models. Include that the bedrock valleys channelling ice into the narrow ice streams around the Greenland ice sheet are not mapped and this limits the ability to model the fast flowing glaciers around Greenland. [European Union]	rejected - too specific in this introduction
13-401	13	11	33	11	33	"surface-melt driven" before ice shelf -- too strong -- we don't know about possible links between basal melting and collapse. Also I think this general topic (of how bottom melt might lead to rapid changes of an ice shelf) has been given too little attention in both Ch 4 and 13. See Vaughan et al. JGR 2012 and Gladish J Glac 2012 as a place to start. [Christopher Little, United States of America]	noted but the observations mainly come from Larsen b, other work is more speculative
13-402	13	11	33	11	33	What about warming of ice and flow enhancement? [Christopher Little, United States of America]	accepted - add reference
13-403	13	11	33			Scambos et al., 2000 were likely the first ones who made that link and shall be cited here. Scambos, T. A., C. Hulbe, M. A. Fahnestock, and J. Bohlander. 2000. The link between climate warming and breakup of ice shelves in the Antarctic Peninsula. Journal of Glaciology 46(154): 516-530, doi:10.3189/172756500781833043 [European Union]	accepted
13-404	13	11	35	11	37	I think that it should be mentioned that changes in sea ice cover will have a strong influence on marginal temperatures (both greenland and antarctica). This is important if you consider what are the driving mechanisms for ocean temps and discharge. It also may influence predictability if we can be confident about future changes in northern hemisphere ice cover. [Aslak Grinsted, Denmark]	accepted to some extent - not clear that sea ice cover perse controls temperature at depth
13-405	13	11	38			Thoma et al. is not a detailed regional model being able to resolve physics down to the level of eddies. [European Union]	noted
13-406	13	11	38			I suggest you add citations: Ice shelf morphology and the efficiency of basal melting (CM Little et al 2009), J. Geophysical Res. 114, C12007, doi:10.1029/2008JC005197. [Michael Oppenheimer, United States of America]	accepted
13-407	13	11	40	11	40	Suggest inserting a sentence about the coupling of melt rates to ice shelf and grounded ice emphasizing that is non-linear and may depend on details of the coupled evolution -- requires detailed models and relatively short timesteps. See the following references:Little, C M and D N Goldberg and A Gnanadesikan and M Oppenheimer (2012). On the coupled response to ice-shelf basal melting. J. of Glaciology, 58 (208), pp. 203-215 Goldberg, D N and C M Little and O Sergienko and A Gnanadesikan and R Hallberg and M Oppenheimer (2012). Investigation of land ice-ocean interaction with a fully coupled ice-ocean model, Part 1: Model description and behavior. J. Geophys. Res., 117, F02037. Goldberg, D N and C M Little and O Sergienko and A Gnanadesikan and R Hallberg and M Oppenheimer (2012). Investigation of land ice-ocean interaction with a fully coupled ice-ocean model, Part 2: Sensitivity to external forcings. J. Geophys. Res., 117, F02038. [Christopher Little, United States of America]	accepted
13-408	13	11	40	11	40	I suggest 'very low' [Mark Siddall, United Kingdom]	editorial
13-409	13	11	45	11	46	This was also observed and evaluated in Pattyn et al. (submitted) [Frank Pattyn, Belgium]	accepted
13-410	13	11	45			The Marine Ice Sheet modeling inter comparison project MISMIP3D studied the ability of models to represent grounding line migration. Pattyn et al., submitted to Journal of Glaciology shall be cited here. [European Union]	accepted
13-411	13	11	48	11	48	It is not corrrect to state upfront that 'grounding-line migration' is likely to be the primary control of the SLR contribution from Antarctica. SMB changes may be at least as likely to cause sea-level changes from Antarctica. Moreover, 'SLR' should be replaced by 'SLC' here as the contribution from Antarctica may also be negative (more so for EAIS than WAIS). [Philippe Huybrechts, Belgium]	accepted - reword main control on mass loss
13-412	13	11	50			The authors might consider adding Cornford et al (submitted) to the list of models with adaptable spatial resolution, since it's referred to a few times later in the section as a model which is able to fully resolve	accepted

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						grounding lines, and the reason it is able to do that is its use of adaptive mesh refinement. [Government of United States of America]	
13-413	13	11	50			Other pertient references include: On the coupled response to ice shelf basal melting (CM Little,et al 2012), Journal of Glaciology, 58, 203-215 doi: 10.3189/2012JoG11J037; Simulation of ocean-land ice interactions through a strongly thermally-forced ice shelf, Part 1: Model description and behavior (Goldberg et al 2012), J. Geophys. Res. E, DOI: 10.1029/2011JF002246; Simulation of ocean-land ice interactions through a strongly thermally-forced ice shelf, Part 2: Sensitivity to external forcings (Goldberg et al 2012), J. Geophys. Res. E, DOI: 10.1029/2011JF002247 [Michael Oppenheimer, United States of America]	accepted
13-414	13	11	52	11	54	Think this might be overstating our understanding and ability to model calving -- there is no statement of the fracture processes involved, and is the biggest issue really the numerical one of tracking the calving front?? [Christopher Little, United States of America]	accepted - will tone down and add other issues
13-415	13	11	57	11	57	Not clear what “this area” refers to in previous sentences – the cited papers are not about computation. [European Union]	accepted - calving models
13-416	13	12	4	12	11	A diagramme showing the different icea processes would be useful. [Isabel Andreu-Burillo, Spain]	noted although i think this would not add much and would take up room - there is a Box on MISI
13-417	13	12	4	12	11	That there is a missing process in many projections of glacier changes needs to be made clear as it is important for policy makers to know. [European Union]	accepted - need to flesh out
13-418	13	12	5	12	6	Can you comment on the spatial distribution of the glaciers for which the mass budgets are known and possible implications for the scaling method? [Government of Germany]	accepted - need to flesh out
13-419	13	12	5			This number of the Randolph inventory differs from the one given in Chapter 4 - additionally, the basis for the evaluation, e.g. the minimum size of the glacier, shall be given. It should be consistent with the numbers in Chapter 4 after the authors of Chapter 4 revised their number. [European Union]	accepted
13-420	13	12	6	12	11	references needed, e.g., to support the "likely to be affected" statement [Thomas Stocker/ WGI TSU, Switzerland]	accepted
13-421	13	12	9			IPCC should be careful with volume-area relations, which are strongly controversial and may even be considered to be misleading statistical data manipulations, because they correlate area (from which volume is calculated) with itself and thereby seemingly suppress the large scatter in the relation between the measured thicknesses and areas. As a consequence, the safety of numbers calculated with such problematic approches has been strongly overestimated. [Wilfried Haeberli, Switzerland]	Rejected. Volume-area power law scaling is a widely accepted and widely used analytical tool with a very solid theoretical basis. While it is somehwat tricky to apply (giving rise to some variable results), the physical basis is very sound. The claim of correlation of one variable with itself is specious, and has been refuted, among other places, in Cogley ("The Future of the World's Glaciers", 2012)
13-422	13	12	13	13	33	This paleo discussion is very well done and helpful, as is the summary figure 13.3! [Terrence Joyce, United States of America]	Noted
13-423	13	12	15			Section 13.2.1 There should be a reference to chapter 5 where a longer section describes the geological records (page 5-39 to 5-45) [European Union]	Accepted - reference to Chapter 5 added.
13-424	13	12	22			Define GIA (Glacial Isostatic Adjustment) [Government of France]	Accepted - defined.
13-425	13	12	28			define Ma [Terrence Joyce, United States of America]	Accepted - defined.
13-426	13	12	32	12	32	10 +- 10: the order of magnitude of the signal is as important as that of the uncertainty? [Isabel Andreu-Burillo, Spain]	text revised following assessment by Chapter 5.

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13-427	13	12	32	12	32	Need a reference for the best estimate : 10 ± 10m [Gilles RAMSTEIN, France]	Text revised following assessment by Chapter 5.
13-428	13	12	33	12	33	It reads as if you are trying to say that there is high confidence in sea level being below 20m. It seems unneccesary given the preceding 10 plus/minus 10 statement. [Aslak Grinsted, Denmark]	Accepted - rewritten.
13-429	13	12	33			that sea level was - Add "global" before "sea level". [Ronald Stouffer, United States of America]	Accepted
13-430	13	12	35	12	35	Assuming an estimate of 10 ± 10 involves, if Greenland and West Antarctic completely melt,for the upper estimats, 10m fromEast Antarctica contribution which is not negligible. [Gilles RAMSTEIN, France]	Noted
13-431	13	12	38			What time period does Marine Isotope Stage 11 refer to? The choice of heading is too specific for the non specialist reader [Kathleen McInnes, Australia]	Taken into account - Marine Isotope Stage 11 is no longer discussed.
13-432	13	12	40	12	41	Please give a reference for this statement. [Government of Germany]	Accepted - reference provided.
13-433	13	12	40			What was the temperature during MIS11 relative to today? [European Union]	Taken into account - Marine Isotope Stage 11 is no longer discussed.
13-434	13	12	41			define ka [Terrence Joyce, United States of America]	Accepted - defined.
13-435	13	12	43	12	43	“requiring” implies you know how ice-sheets contributed sea-level. Need to make this sentence less assertive. [European Union]	Accepted - reworded.
13-436	13	12	44			EAIS is not defined. Please either define here or add to glossary. [Government of United States of America]	Accepted - defined.
13-437	13	12	45	12	45	Please add a MIS11 temperature range also. [Aslak Grinsted, Denmark]	Taken into account - Marine Isotope Stage 11 is no longer discussed.
13-438	13	12	47	13	9	Very important points regarding long-term sea level rise over coming centuries - very high levels cannot be ruled out, this point should be emphasised. [European Union]	Noted.
13-439	13	12	47			If the GIA corrections are essential, how can we say that the results are robust? How can robust evidence be build on a small number of relative sea level reconstructions? It seems to me, that the statement should be given relative to the area to which the sea level reconstructions are representative for. [European Union]	GIA is a relatively well understood concept.
13-440	13	12	49			The time interval about 130-116 ka is defined as representative of the Last Interglacial (LIG). However, the Annex III: Glossary, under ther term "Interglacials or interglaciations" (page AIII-15), informs that the Last Interglacial (LIG) occurred between about 130 and 118 ka before present. [Alejandro Cearreta, Spain]	Text revised.
13-441	13	12	50	12	51	As written, this sentence suggests high agreement that LIG global mean temperature was higher than present; while there is high agreement on the high sea levels, I would be inclined to put medium confidence on the temperatures. [Robert Kopp, United States]	Text revised following assessment by Chapter 5.
13-442	13	12	52	12	52	"form" should be "from" [Stephen Griffies, United States of America]	Accepted
13-443	13	12	52	12	52	Substitution is needed: 'form' to 'from'. [Mirko Orlic, Croatia]	Accepted
13-444	13	12	52	12	52	Typo. Word “form” should be "from". [Phil Watson, Australia]	Accepted
13-445	13	12	52			Change "form" to "from" [Robert Dean, United States of America]	Accepted
13-446	13	12	52			form -> from [Kathleen McInnes, Australia]	Accepted
13-447	13	12	52			Change "generally form" to "generally from" [James G Titus, United States of America]	Accepted
13-448	13	13	1	13	1	95% probability should be "extremely likely" per AR 4, though it does not appear to be defined in the AR5 guidance. [Robert Kopp, United States]	Taken into account - will be revised following assessment by Chapter 5.
13-449	13	13	1	13	2	please delete the brackets explaining the very likely and likely terms ("95% / 67% probability) -- instead refer to the relevant section in Chapter 1 (1.4) and/or the AR5 uncertainty guidance note (Mastrandrea et al. 2010) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted

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13-450	13	13	1	13	9	Include the second reference Dutton and Lambeck and note that the conclusions are based on two publications. [European Union]	Accepted
13-451	13	13	1		9	Can high confidence be built on TWO studies in one region (North Carolina)? Aren't there more sea level reconstructions? [European Union]	Taken into account - additional sites are now included.
13-452	13	13	2	13	2	Probably "at least" +6.6 m ... is meant (?). [Government of Germany]	Accepted
13-453	13	13	4			LIG GMSL more than current [European Union]	Noted
13-454	13	13	5			thus high confidence - The "thus" makes it sound like 2 papers on the subject lead to "high confidence". I assume physical reasoning also led to the high confidence. [Ronald Stouffer, United States of America]	Text revised following assessment by Chapter 5.
13-455	13	13	8	13	8	My impression is that the figure of 2m SL rise from Greenland is more regarded as a minimum by the community, and that others still feel it may have been higher. Not sure if this is dealt with in Chapter 5, but I think that the uncertainty is greater than indicated here. [European Union]	Text revised following assessment by Chapter 5.
13-456	13	13	8			insert 'both' before £1 m [Kathleen McInnes, Australia]	Accepted
13-457	13	13	8			McKay et al GEOPHYSICAL RESEARCH LETTERS, VOL. 38, L14605, doi:10.1029/2011GL048280, 2011 should perhaps be cited here? [Michael Oppenheimer, United States of America]	Accepted
13-458	13	13	11	13	11	Since AR4 an important result is the absence of major climate instabilities during LIG from NEEM is consistent with the sentence Line 11 page 13 [Gilles RAMSTEIN, France]	Noted
13-459	13	13	13	13	16	These statements are based on informal reanalysis of Kopp et al., 2009, and therefore may be harder to trace. It may be better to rely on the more formal reanalysis in Kopp et al. (submitted); the author is happy to provide a copy of this manuscript on request. [Robert Kopp, United States]	Accepted
13-460	13	13	13	13	16	The statement given here, i.e: "Based on the probabilistic assessment of LIG sea level by Kopp et al. (2009), for the time interval in which GMSL was above present, the 1000-year average rate of GMSL rise very likely exceeded 2.0 m/kyr (95% probability), likely exceeded 4.1 m kyr/1 (67% probability), and was unlikely to have exceeded 5.8 m/kyr." ... does not appear to be consistent with the Kopp et al (2009) paper where the abstract actually says: "When global sea level was close to its current level (>= -10 m), the millennial average rate of global sea level rise is very likely to have exceeded 5.6 m/kyr but is unlikely to have exceeded 9.2 m/kyr." This difference should be resolved. [Martin Manning, New Zealand]	Taken into account. These values were derived by Kopp for our chapter from the published results. They will be replaced with new values from paper by Kopp et al. (in press).
13-461	13	13	13	17		It should be explained why this range of rates is used instead of the rates given in Kopp et al 2009. Also see Kopp et al, A probabilistic assessment of sea level variations within the Last Interglacial stage (Kopp et al 2012), submitted to Geophysical Journal International, for an improved approach [Michael Oppenheimer, United States of America]	Taken into account - the new rates from Kopp et al. (in press) will be used.
13-462	13	13	15	13	16	please delete the brackets explaining the very likely and likely terms ("95% / 67% probability) -- instead refer to the relevant section in Chapter 1 (1.4) and/or the AR5 uncertainty guidance note (Mastrandrea et al. 2010) [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
13-463	13	13	16			This value 5.8 mm/yr is being used as a gauge here and earlier, but it is not explained WHY? So please explain where this figure arises. I would like to know too since the current estimates of GMSL rise are 3-4 mm/yr, which is slightly less than this figure. [Terrence Joyce, United States of America]	Accepted - now explained.
13-464	13	13	19	13	19	Superimposed to global sea level rise, there is interesting studies on past and future regional vulnerability of Northeastern Greenland for LIG and furure. For instance The Cryosphere, 6, 1239-1250, 2012 [Gilles RAMSTEIN, France]	Text revised following assessment by Chapter 5.
13-465	13	13	19	13	19	The warm climate are interesting but correspond to equilibrum climates whereas anthropocene corresponds to transient out of equilibrum climate, therefore the MWP1.a (Melt Water Pulse) is more appropriate for comparison to future climate in terms of rapid rise of sea level. Unfortunately in this (excellent) chapter it is not quoted. [Gilles RAMSTEIN, France]	Rejected - MWP1A is not a good analog because of such large differences in ice sheet size and location.

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13-466	13	13	23	13	33	Major Antarctic melting appears to have ceased by about 4,000 years ago. The modern observed Antarctic contribution to sea level is a recent phenomenon based on evidence provided by Yokoyama and others (2012, doi:10.1029/2012GL051983). This work adds important geologic context to the current melting, and I believe it should be included in Chapter 13. [Stephen Obrochta, Japan]	Taken into account - may be revised following assessment by Chapter 5.
13-467	13	13	25	13	27	Is there a way to partition the sea level rise over this interval? All the other sections indicate some partition between ice sheets, glaciers, and thermosteric. This would seem to be a critical period for validating/understanding SEM's. [Christopher Little, United States of America]	Unfortunatelly there is not a way to do so.
13-468	13	13	26			Figure 13.3 not addressed or discussed in text. [Government of United States of America]	Taken into account - now referred to.
13-469	13	13	30	13	33	That 1905-1945 AD range is too recent. The 1840-1920 range is about right. Tide gauge measurements indicate that the acceleration in rate of sea level rise began in the mid or late 1800s, and ceased by about 1930. We have excellent tide gauge data demonstrating that there has been no acceleration in rate of sea level rise since then; certainly not as late as 1945 AD. "Proxies" are nowhere near as reliable as these actual tide gauge measurements. [David Burton, United States of America]	Rejected - it is necessary to assess th whole range of results
13-470	13	13	31	13	33	Is the conclusion about acceleration between 1905 and 1945 credible when it is not apparent in the instrument record, including the data shown in Figure 13.3? Does it not point to questions about the precision of the geological approach? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Rejected - there is some evidence in the instrumental record
13-471	13	13	32	13	32	Gehrels and Woodworth is now published: Gehrels, W.R. and Woodworth, P.L. (2013). When did modern rates of sea-level rise start? Global and Planetary Change 100, 263-277, doi:10.1016/j.gloplacha.2012.10.020. [Roland Gehrels, United Kingdom]	Noted.
13-472	13	13	33	13	33	What is 'CE'? [Philippe Huybrechts, Belgium]	Taken into account - CE means "common era." This is no longer used.
13-473	13	13	35	13	35	Would be very helpful if caption were started with “Figure to illustrate…” with whatever intention you had in including it. [European Union]	Taken into account - caption rewritten.
13-474	13	13	36	13	46	Concerning Figure 13.3 a); the two colors are rather hard to distinguish, especially when printed. [Government of Germany]	Taken into account - figure is being revised.
13-475	13	13	38	13	38	The reconstruction of Kemp et al uses the same upside down Tiljander proxies as Mann 2008. In chapter 5 my advise was to remove all references to Mann (2008, 2009) as long as the issues with the Tiljander proxies have not been resolved. I copy my complaints on Mann 2008: "This is the first time that Mann (2008) is mentioned in the chapter. Mann claimed in this paper that he could get a "hockey stick" with or without tree ring proxies. However, it has been shown very convincingly on Climate Audit that the non-dendro hockey stick in Mann 2008 is the result of using the Tiljander proxies upside-down. This was confirmed by Tiljander herself. McIntyre/McKitrick published a comment in PNAS in which they mentioned this. http://www.pnas.org/content/106/6/E10.full?ijkey=a980e3573c3f58772adf36177c17534023f20dab&keytype2=tf_ipsecsha Mann in his reply http://www.pnas.org/content/106/6/E11.full?sid=cf6aac63-05df-4aa0-aecc-a000276cafd3 denied he had done so and PNAS let him get away with this. McIntyre here http://climateaudit.org/2009/10/14/upside-side-down-mann-and-the-peerreviewedliterature/ summarizes the issues and also explains why in this case apparently the peer reviewed literature is unable to resolve such 'simple' issues whether Tiljander is used upside-down or not. Here http://climateaudit.files.wordpress.com/2009/10/tiljander_rotated2.gif is the graph showing the Tiljander proxies as meant (up is warm). Tiljander also stated that the proxy record is contaminated in the 20th century: "This recent increase in thickness is due to the clay-rich varves caused by intensive cultivation in the late 20th century… There are two exceptionally thick clay-silt layers caused by man. The thick layer of AD 1930 resulted from peat ditching and forest clearance (information from a local farmer in 1999) and the thick layer of AD 1967 originated due to the rebuilding of the bridge in the vicinity of the lake’s southern corner (information from the Finnish Road Administration)." So the 20th century decline in this proxy is not a climatological signal. Mann flipped it though and used the recent hockey stick shape as evidence for anomalous 20th century warming. His dendro hockey stick shape btw is coming from the infamous bristlecone pines of which the NAS Panel in 2006 said they should be "avoided". I urge the authors to remove all references to Mann (2008) as	Rejected. The Kemp et al. sea level reconstruction has no relation to the Tiljander proxies.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						long as Mann doesn't correct the obvious errors in this study." [Marcel Crok, The Netherlands]	
13-476	13	13	44	13	46	What does this tell us about the calibration of the different observations? Or, how does this inform about uncertainty? [Isabel Andreu-Burillo, Spain]	The shift of the time series is simply done in order to align the records over different measurement intervals.
13-477	13	13	46	13	46	Please explain, how the value 150 mm has been derived. [Government of Germany]	As explained in the caption, this was the amount required to align with the paleo data.
13-478	13	13	48	14	26	In my opinion, this section has been reduced too much. I believe that the references to GIA, GPS and satellite altimetry are pertinent. [Belén Martín Míguez, Spain]	Unfortunately, we do not have enough space to expand more . More details are provided in chapter 3
13-479	13	13	50	13	50	Might be good to remind readers tide-gauges/altimetry refer to two different measures, as discussed above. [European Union]	Relative and absolute sea level measurements from tide gauge and altimetry are introduced in section 13.1.2
13-480	13	13	54	14	2	There should be a mention of geoid changes as a significant (and well-understood) source of regional variations in sea-level rise projections. A clause like "changes in the geoid due to redistribution of mass from ice-sheets and glaciers into the ocean, " might be added to the list of causes, or perhaps the phrase "mass changes of the ocean from contributions from glaciers and ice sheets" (line 55) was intended to cover this effect, in which case this phrase might be appended to read "... from glaciers and ice sheets and resultant geoid changes". The body of this chapter already discusses this effect adequately. [Robert Hallberg, United States of America]	The various causes of regional variability are discussed in sections 13.1.3 and 13.1.4; Here we do not discuss the contributions but just the measurements
13-481	13	13	58	13	58	As previously noted, that ~1.7 mm/yr figure is NOT measured tide gauge data. About 1/3 of it is model-derived GIA adjustments, including 0.3 mm/yr that is an erroneously applied adjustment to account for presumed sinking of the ocean floor. There is a great deal of uncertainty w/r/t the GIA numbers (note that they tend to differ strikingly from GPS measurements), but that's not my main objection. The main objection is that if you add Peltier's 0.3 mm/yr GSE for sinking of the ocean floor, the result of that calculation is not "sea level," because sea level isn't defined as the depth of the sea, it's the level of the sea's surface. [David Burton, United States of America]	If you mean 'raw' measurements, yes; But any measurement from any instrument needs to be corrected for unrelated phenomena; This what is usually done to extract the 1.7 mm/yr SLR supposed to reflect climate processes only
13-482	13	13	58			The estimates in GMSL should be 1.7+/- 0.3 mm yr-1, not 0.2, to match Chapter 3, p. 3-32, line 46. [Government of United States of America]	Accepted- corrected
13-483	13	13				In the caption of figure 13.3, the words “twilight", “autumn", and “light…. line" can be deleted so that we are simply left with “blue", “orange", and “green". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-484	13	14	1	14	3	Rewrite for clarity. [Christopher Little, United States of America]	Takn into account - the sentence has been clarified
13-485	13	14	3			Please repeat here potential origins that might be given in Ch.3. [European Union]	Taken into account - examples of discrepancies are given
13-486	13	14	5	14	14	This paragraph obfuscates the most important fact about sea level rise, by comparing contemporary sea level rise (under the influence of at least +80 ppm CO2), to dates long prior to the start of significant anthropogenic GHG emissions. The reported accelerations predate significant anthropogenic CO2, and this paragraph needs to say so. Plus, it cherry-picking studies, ignoring the majority which have found no acceleration, or a slight negative acceleration since the beginning of major anthropogenic GHG emissions. It is very clear from the data that, since the 1930s, there has been no acceleration at all in the rate of sea level rise. See http://tinyurl.com/slracc1 [David Burton, United States of America]	Rejected. Observations of tide gauge records of sea level rise are assessed in Chapter 3, and we rely on that assessment as well as paleo observations from Chapter 5. Together these clearly indicate the rate of rise has increased.
13-487	13	14	5	14	14	The first paragraph on page 13-14, lines 5-14, discusses the difficulty in estimating sea level acceleration in the presence of low-frequency oscillations, noting that the results are sensitive to the choice of the analysis time span. There is clear evidence that some tide gauge records contain multi-decadal variability related to large-scale wind effects (e.g. Miller and Douglas, 2007; Woodworth et al., 2009; Sturges and Douglas, 2011), but the response is complicated. For example, variability on the west coast of Europe propagates as a long Rossby wave across the Atlantic to the east coast of the United States over roughly a 10-year interval. In	We agree that sea level records are contaminated by presence of high- and low-frequency variability associated with large scale atmospheric circulation, ocean dynamics, coastal processes and so on, that what we say in our text. Unfortunately, it is common issue about the noise in observational records. In

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						principle, it makes sense to remove these non-sea level rise signals from the gauge records (e.g. Chambers, 2012), but most of the records are effectively shorter than 60 years in length, making the acceleration error bars a potential issue. [Government of United States of America]	general, we agree with comment that some variability could be removed from tide gauge records, however, there are no published results available to be included in our text.
13-488	13	14	5	14	14	The presence of a 60 year cycle in GMSL rise is weak. Since there is no clear planatary explanation for this cycle, it is unwise to mention it here (Chambers et al. 2012). In fact, one could define a cycle between any two inflexion points. These points are here 1930 and 1990. There are many explanations possible, perhaps a cycle. Note that Chambers et al. use a ? in their title. My advise would be not to mention this hypothesis until more is known. [Hans Visser, The Netherlands]	Taken into account - Text has been changed
13-489	13	14	5	14	14	Same comment as that advised above in comment No. 3 [Phil Watson, Australia]	Cross reference to comment 3 of reviewer was lost when all comments were combined.
13-490	13	14	5			This paragraph repeats some things from Chapter 3 which I find rather questionable, re 60-year cycles and quadratic terms. I've commented on those to chapter 3 - you should perhaps keep an eye on what chapter 3 will actually say in the end. In particular, the fact that chapter 13 shows that the recent rate of rise of the altimeter era is modelled successfully with process-based models, as a result of climate forcing rather than internal decadal variability, argues against the chapter 3 idea that the recent high rate is likely due to multidecadal variability. [Stefan Rahmstorf, Germany]	Agree; this paragraph has been modified
13-491	13	14	6	14	6	Make absolutely clear that these oscillations are not instrumental. [European Union]	Taken itno account - Text has been changed
13-492	13	14	7	14	7	Reference to Chambers is ok, but there is SIGNIFICANT discussion in Journal of Coastal Research (2011 Houston & Dean) [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-493	13	14	7	14	7	2012 Baart, 2012 Boon, etc. on this topic. The uncertainties and disagreement in interpretation of the RATE of sea level [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-494	13	14	7	14	7	rise particulalry since, say, 1990, is a very important issue. I think this should be clarified and expanded upon. [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-495	13	14	7	14	7	Effects of 18.6 yr and approx. 60 year periodicities in sea levels are very important and make it very difficult to accurately [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-496	13	14	7	14	7	identify short term (decadal) changes in rate of sea level rise. I have worked extensively with Halifax and Charlottetown [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-497	13	14	7	14	7	records - the lack of an increasing rate of relative sea level rise is a very difficult issue to reconcile. [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-498	13	14	7	14	7	Baart's 2012 JCR editorial on this issue is a particularly coherent and relevant discussion of this problem. [Michael Davies, Canada]	Noted - this issue is discussed in Chapter 3.
13-499	13	14	7	14	7	Regards, [Michael Davies, Canada]	Thanks for comments
13-500	13	14	7	14	7	Mike Davies, mdavies@coldwater-consulting.com, Ottawa, Canada [Michael Davies, Canada]	Noted.
13-501	13	14	7	14	8	I do not like the term "oscillation" when there is obviously too little data to tell whether it is really is a periodic signal. E.g. A lot of this variaibility could be from aerosol forcing as they have been found to be for north atlantic temperature (Booth et al., Nature, 2012). [Aslak Grinsted, Denmark]	Noted. The whole paragraph has been rewritten
13-502	13	14	9			Does "had continued" refer to rate or acceleration? [Michael Oppenheimer, United States of America]	Noted. The whole paragraph has been rewritten
13-503	13	14	14			Show that 0.01 mm/yr^2 would amount to 5 cm in a century [Robert Dean, United States of America]	Noted.
13-504	13	14	14			Well, if 120 years is taken and it contains two, 60 yr cycles, the effect on the acceleration will be minimized. This is worth mentioning I think. [Terrence Joyce, United States of America]	Noted. The whole paragraph has been rewritten

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13-505	13	14	16	14	26	Conflating satellite data with tide gauge data is simply wrong, and the supposed "higher rate" (line 24) doesn't exist in the tide gauge data. It's not that it doesn't necessarily reflect a recent acceleration; rather, it doesn't exist, and the lack of a higher rate clearly reflects a LACK of recent acceleration. What's more, the satellites have poor agreement with one another (and Envisat even has poor agreement with ITSELF, late adjustments having tripled the rate of "measured" rise), which should be mentioned. What's more, the satellites are showing a deceleration over their 19 year history, which is as significant and worthy of mention as the ~3 mm/yr rate (though 19 years is too short to draw definitive conclusions from either). [David Burton, United States of America]	Concerning the second part of the comment, this answer is : no, there is no discrepancy between the satellite altimetry missions; This has been recently demonstrated by the ESA Climate Change Initiative Project (see http://www.esa-sealevel-cci.org/)
13-506	13	14	16		26	Why does a ranging method like altimetry, need a correction for the changes in ocean basins? [European Union]	Glacial Isostatic Adjustment (or Post Glacial Rebound) produces changes in the gravity field (hence on the mean sea level) and modifies the shape of ocean basins. In the global averaging process of altimetry sea surface height data, the latter factor has a small effect on the global mean sea level
13-507	13	14	16			Note that the deceleration in the satellite datya is large. My analysis of the Colorado data for Release 4 2012, is that the deceleration is 0.107 mm/yr^2 which would amount to a sea level decline of 53 cm in a century. In the interest of balance, this should be noted with appropriate caveats. We are giving weight to trend, why not acceleration? [Robert Dean, United States of America]	Rejected. It's inaapropriate to infer anything about acceleration from such a short record. We also suggest that the result will be different when the reviewer redoes his analysis with the newer 2013 Release 2 (2013-02-27):.
13-508	13	14	17	14	18	Masters et al has been accepted. I don't have further details, beyond the fact that it will be published in marine Geodesy soon. [Neil White, Australia]	Accepted - the reference has been updated
13-509	13	14	19	14	19	Again, it is an error to add Peltier's 0.3 mm/yr GIA and call the result "sea level." [David Burton, United States of America]	Rejected - this effect must be accounted for, as discusse din section 13.1.
13-510	13	14	19			It is stated "correction related to the increasing size of the global ocean basins". Suggest "correction related to lowering of the ocean basins" for clarity. [Robert Dean, United States of America]	Noted - or may be 'change in shape of oceans basins'
13-511	13	14	21			Page 14, line 21. “all source of errors" should be “all sources of error". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-512	13	14	23	14	26	It is odd that the discussion on this is in Chapter 3 - it seems better placed in chapter 13 [Mark Siddall, United Kingdom]	Agree but Chapter 13 is a synthesis chapter that incorporates the assessments from other chapters addressing various observations of sea level.
13-513	13	14	30	14	30	This FAQ is a good place to link with WG II needs concerning sea-level rise -- impacts are due to local sea-level change rather than global-mean sea-level change [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Noted.
13-514	13	14	30	14	30	Is it clear what is meant by local sea level change and is this use consistent throughout the chapter, and ideally across WG I, and even better coordinated with WG II chapters? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	added section on basic definitions
13-515	13	14	32	14	36	What about (local/regional) bathymetry? [Isabel Andreu-Burillo, Spain]	Meaning of comment not clear.
13-516	13	14	35	14	35	are caused by.. [Government of Kenya]	Corrected.
13-517	13	14	35	14	35	"cause" should be "caused" [Stephen Griffies, United States of America]	See response to comment 516.
13-518	13	14	35	14	35	Replace "are cause" by "are caused" [Urs Neu, Switzerland]	See response to comment 516.
13-519	13	14	36	14	36	"highly localised" -- sediment compaction is happening across all large deltas such as the Nile, Ganges-Brahmaputra or Mekong. I would describe as "more localised". [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-520	13	14	42	14	42	"tsunamis" -- do these affect sea level? Directly yes during the tsunami event but afterwards is there any	Tsunamis will have a long term effect where they

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						residual effect? The earthquakes that lead to the tsunami often lead to near instantaneous sea level change as happen in the Japan 2011 earthquake. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	produce significant erosion and/or deposition. They are not the focus of htis chapter
13-521	13	14	42	14	46	The relative role of climate can be debated -- it is a stronger statement if the spatial scale is explicit. Locallyin susceptible locations such as deltas other processes are dominating and have the potential to continue to do so even with climate change. e.g., Nicholls, 2010 in Church et al (2010) book on Understanding Sea Level Rise and Variability; and many recent papers such as Ericson et al (2006) for deltas and more locally cities on deltas such as the recent paper on Shanghai in Climatic Change (apologies but I do not have the precise reference). [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised to make the point that over longer time scales, climate change is the main contributor to sea level change in most areas.
13-522	13	14	48	14	48	The first satellite altimeters were long before 1992. The high quality missions that we normally use started in 1992. This should be made clear. [Neil White, Australia]	Accepted. Given the target audience, a more general statement is used to avoid this issue.
13-523	13	14	48	14	53	FAQ 13.1. Better explanation is required of the central plot of satellite data in Figure 1a. This is presented as the "Mean rate of change in sea surface height for the period 1993-2010 from satellite altimetry." The problem is that the total long term sea level rise, so far, is only about 6cm (3 mm for 20 years) which is smaller than local variations of 10 to 30 cm rms due largely to El-Nino events. Figure 1a is therefore dominated by structure due to occurrence of the major El-Nino in 1997, structure which has nothing to do with the mean rate of change. In another 20 years, the figure equivalent to this will show about half the range of variability, since the relative effect of the ongoing rise will be twice as large. In 100 years, assuming altimetry and a mean 3mm/year rise continue, the structure will be reduced by about a factor 5, converging eventually on a uniform 3mm/year, though with variations as suggested in Fig1b. This change is clearly shown in Figures 13-12, 13-13, 13-15 and 13-16 which show model results for dates near 2100. Figure 1a would give a more accurate picture of "mean rate of change of sea level height" if the years 1997 to 1999 were omitted, but I have never seen this presented. It is important to stress that Figure 1a does NOT mean that long-term sea level rise is expected to show large variations from place to place, as it seems to be often interpreted. [James Gower, Canada]	Accepted. Text revised to make these general points.
13-524	13	14	55	14	56	The authors write that FAQ 13.1-Figure 1 is the result of natural climate variability (El Nino, PDO). I think that some references are needed here to support this assertion because this point has been debated for some regions and I think it is not evident actually. For example in the western Pacific, there have been a debate recently to know whether the local sea level trends (since 1993) could be due to local climate mode of variability (like PDO, NGPO, NiNo) or not (see Feng et al. 2004, Merrifield 2011, Merrifield and Maltrud 2011, Meyssignac et al. 2012, Merrifield and Thompson 2012). [Benoit Meyssignac, France]	IPCC guidelines request that FAQs should not include references wherever possible.
13-525	13	14	55	14	56	The following reference can be cited here, which analysed sea level trend in the Pacific during altimeter period and discussed aliasing of decadal variability. This reference was cited in Chapter 3. (Zhang, X., and J. A. Church (2012), Sea level trends, interannual and decadal variability in the Pacific Ocean, Geophys. Res. Lett., 39, L21701, doi:10.1029/2012GL053240) [Xuebin Zhang, Australia]	See response to comment 524.
13-526	13	14	55			FAQ 13.1: Figure 1: Our recommendation would be to remove panel b and rather have this as a separate figure. This would be consistent with our general preference not to combine observations (panel a) with modelling (panel b) in a single figure, which could be confusing and misinterpreted by the FAQ reader. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-527	13	14	56	15	3	There is discussion of this in a number of papers, including: Bromirski, P. D., A. J. Miller, R. E. Flick, and G. Auad (2011), Dynamical suppression of sea level rise along the Pacific coast of North America: Indications for imminent acceleration, J. Geophys. Res., 116, C07005, doi:10.1029/2010JC006759. I suggest that one or more of these should be referenced. [Neil White, Australia]	See response to comment 524.
13-528	13	15	1	15	3	The text in FAQ 13.1 lines 55-57 on page 13-14, and 1-3 on page 13-15 note that the spatial variation in Figure 1a is due to El Nino etc., but says "The influence of these processes will continue during the 21st century," as if a Fig 1a produced in 2100 will be similar to the present one. In fact as I argue in comment #4, the influence of these processes on Figure 1a will diminish during the 21st century, showing a more uniform average rate of rise, with spatial structure reduced in amplitude by about a factor 5 by 2100. Of course, if one	See response to comment 523.

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						really believed that ocean eddies and other circulation patterns would become 5 times more powerful by 2100, then my statement would not be true. But I don't think anyone believes that. [James Gower, Canada]	
13-529	13	15	5	15	10	Two different issues in a continuous paragraph: 1) vertical land motion, 2) natural climate variability. If this is an [Isabel Andreu-Burillo, Spain]	See response to comment 530.
13-530	13	15	5	15	10	analysis of the figures, rephrasing would help understanding. [Isabel Andreu-Burillo, Spain]	Accepted. Text of variability moved to figure caption.
13-531	13	15	14			It should be noted that extremely high rates of relative sea level fall can be found locally at the ends of some glacial fjords, for instance at Juneau, AK (-12.92 mm/yr) and Skagway, AK (-17.12) (Reference Zervas, 2009) [Government of United States of America]	Taken into account. Sentence added to caption for FAQ 13.1, Figure 2.
13-532	13	15	29	15	29	sediment slumping' should be 'sediment loading' - a quite different process. [Donald Forbes, Canada]	Taken into account.
13-533	13	15	29	15	29	What is "sediment slumping" -- clarify of delete [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
13-534	13	15	33	15	45	the rather–often repeated list of contributory processes is not necessary as the focus is on gravitational effects. [European Union]	Rejected. Land motion is also relevant here as it is included in the figure being discussed.
13-535	13	15	33	15	53	There is a substantial body of literature on this. Why isn't it referenced? [Neil White, Australia]	See response to comment 524.
13-536	13	15	35	15	35	"in spatial variations": locally or at a greater scale? [Isabel Andreu-Burillo, Spain]	Taken into account. Focus of discussion is regional changes.
13-537	13	15	36	15	36	Include reference (e.g. Milne 2009) [European Union]	See response to comment 524.
13-538	13	15	36	15	39	A citation is needed for the model used for generate Figure FAQ 13.1b. [Stephen Griffies, United States of America]	Talen into account. Citation provided.
13-539	13	15	41			from the melting - Add "ice sheet" before "melting". [Ronald Stouffer, United States of America]	Accepted.
13-540	13	15	43	15	43	Slightly misleading. Suggest "would experience a more modest rise than the global average, and some parts would even experience a sea level fall." [Robert Kopp, United States]	Accepted.
13-541	13	15	43	15	45	either … both …ice sheets - The idea of NH and SH needs to be explicit here. [Ronald Stouffer, United States of America]	Taken into account by adding "polar ice sheets"
13-542	13	15	47	15	47	"a variety of processes", an space-time scales [Isabel Andreu-Burillo, Spain]	Taken into account. The spatial and temporal ranges are mentioned in the summary paragraph.
13-543	13	15	47			Page 15, line 47. "drive" should be "drives". (The subject here is "variety", which is singular. Note that in the next sentence, you make a similar construction but then using the word "combination" in which case the correct singular form of the verb is used. This is the lack of grammatical consistency that I referred to before.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial
13-544	13	15	56	16	3	It would be helpful to add the map of the predicted present-day rate of vertical motion of the solid earth (for instance, by Peltier et al.), due to GIA, to FAQ13.1, Figure 1, since people can get insight of SLR by the effect, altogether with the sea level rise by ice melting effect (bottom figure). [Sok Kuh Kang, Republic of Korea]	Rejected. While this is a perfectly reasonable suggestion, this FAQ is already quite long and complex and so the addition of new material would neccessitate removal of an existing figure and related text.
13-545	13	15	57	15	57	Is there a citation to the details of how Figure FAQ13.1a was generated? [Stephen Griffies, United States of America]	No. A similar figure is shown in Chapter 3 but the details of the procedure used to produce it were not the same.
13-546	13	15				Somewhere on this page it may be worthwhile to note that almost all (I can't think of an exception) anthropogenic activities at ports where many tide gauges are located results in increased relative sea level rise. [Robert Dean, United States of America]	Noted.
13-547	13	15				Page 15 Caption of FAQ 13.1 figure 1. This caption needs source citations. [Eelco Johan Rohling, United	Taken into account.

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						Kingdom of Great Britain & Northern Ireland]	
13-548	13	15				FAQ 13.1, figure 1. This caption needs source references. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
13-549	13	16	2	16	3	Suggest clarifying that this is the model of an output of what Kopp et al. (2010) label "static equilibrium" effects, i.e., gravitational-elastic-rotational effects. [Robert Kopp, United States]	Rejected. This addition is too technical for the target audience of FAQs (person with a non-science degree).
13-550	13	16	5	16	5	There appears to be no explicit reference to the role of bathymetry in the regional/local aspects of sea level (change). [Isabel Andreu-Burillo, Spain]	See response to comment 552.
13-551	13	16	5	16	5	The role of bathymetry on Sea Level is well known and FAQ 13.1 Figure 1 a) SL change map appears highly correlated [Isabel Andreu-Burillo, Spain]	See response to comment 552.
13-552	13	16	5	16	5	with it (e. g. West Pacific). [Isabel Andreu-Burillo, Spain]	Rejected. Space limitations preclude making this point.
13-553	13	16	5			Nice figure and discussion [Terrence Joyce, United States of America]	Noted.
13-554	13	16	7			Section 13.3 - This section needs to note that upper ocean density changes do not affect coastal sea levels. Rather, they cause changes in the elevation of the surface of the open ocean, without lateral flows, because gravity balances water mass, not volume. As upper ocean water warms, it rises in place; as it cools, it sinks in place; but neither change affects coastal sea level. (That might be one of the reasons that satellites are measuring higher rates of sea level rise than tide stations.) [David Burton, United States of America]	Rejected - The ocean responds dynamically such that sea level rise from expansion of the deep ocean and mass changes cause water to flow onto the shelf and thus affect coastal sea level. We have now included a brief discussion of this process, with supporting references, in section 13.1.3. There is also very good agreement between tide gauge and satellite records over the period of common measurements.
13-555	13	16	13	16	26	Observations of thermosteric sea level rise. Very important for policy makers as this section sets the scene for the future projections. These results could be emphasised more. [European Union]	Unfortunately we do not have enough space; Thermosteric sea level is discussed in chapter 3
13-556	13	16	13			"thermosteric" needs more here - "observationally inferred" work? [Ronald Stouffer, United States of America]	Unfortunately we do not have enough space; Thermosteric sea level is discussed in chapter 3
13-557	13	16	14	16	14	Explain what XBT data are. These data could be an essential part of the discussion. [European Union]	XBT data are discussed in chapter 3; but we have explicited the acronym
13-558	13	16	14			XBT is not defined. Please define here or add to glossary. [Government of United States of America]	We have explained the acronym
13-559	13	16	16	16	16	“almost gobal coverage” Global 3D coverage? At what scales? [Isabel Andreu-Burillo, Spain]	Clarified
13-560	13	16	21		26	please assess robustness, agreement and likelihood [European Union]	This is discussed in chapter 3
13-561	13	16	22			The numbers do not match Chapter 3, p. 3-4, line 34 (and surrounding text). Please revisit and make sure it matches. [Government of United States of America]	Taken into account - corrected
13-562	13	16	25	16	25	“For the period 2005-2010...” for what depth range? [Isabel Andreu-Burillo, Spain]	Taken into account - Added
13-563	13	16	25	16	25	Change text to "For the period 2005-2010, the thermosteric component is..." [European Union]	Taken into account - corrected
13-564	13	16	25	16	26	Add a vowel in this sentence. [Donald Forbes, Canada]	Taken into account - corrected
13-565	13	16	25	16	26	appears to be an incomplete sentence beginning…".For the period 2005-2010……" [Government of United States of America]	Taken into account - corrected
13-566	13	16	25			Page 16, line 25. Insert “are" before “0.9". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account - corrected
13-567	13	16	31			both of these can be - Add "variables" after "these". [Ronald Stouffer, United States of America]	Taken into account by deleting the phrase.

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13-568	13	16	36	16	36	Surely this should read "…compared GMSL change due to…" [Neil White, Australia]	Accepted.
13-569	13	16	37	16	41	This is confusing - volcanic forcing is invoked, but it is never presented (when did the eruptions occur?) and why shouldn't models with natural AND anthropogenic forcing agree better with the observations. You seem to be saying something different. Isn't the point here that natural variability containing volcanic eruptions can for a certain period, make a substantial global difference in heat storage? If so, then say it simply. If not, what are you trying to say here? [Terrence Joyce, United States of America]	Taken into account by stating that there were several volcanoes during the period specified, and some rewording. We think the point we are making is just the one the reviewer has made.
13-570	13	16	41	16	41	"... by about 10%" what is the figure for the simulations not including the natural forcing? [Isabel Andreu-Burillo, Spain]	the models overestimate the response,as reported in the text
13-571	13	16	43	48		In Figure13.4A, while the CMIP5 generally capture the thermosteric SLR trend during the past ~40 years, they also diverge substantially prior to that. The text focuses on overestimating the trend during the past decade, but the feature that really stands out is the large model spread prior to 1970. I think at least addressing the model spread in the text is needed to enable a clear interpretation by the reader. What's causing the model spread: model drift, structural differences, heterogeneous forcings? [Ryan Sriver, United States of America]	Taken into account by inserting a sentence with references to other chapters.
13-572	13	16	44	16	45	ocean takes up heat more rapidly as the climate warms - This is an incorrect statement as noted larger in the chapter. Delete. [Ronald Stouffer, United States of America]	Taken into account by deleting the sentence. This point belongs better in 13.4.1.
13-573	13	16	44	16	45	"because the ocean takes up heat more rapidly as the climate warms." Why? Does it mean that the ocean heat uptake depends on temperature? It needs explanation here. [Xuebin Zhang, Australia]	See 13-572.
13-574	13	16	45	16	45	Is there a citation for this statement that ocean heat uptake increases as climate warms? One may argue the opposite, based on increased upper ocean stratification shutting down the absorption of heat. [Stephen Griffies, United States of America]	See 13-572.
13-575	13	16	46	16	46	There is no comparison between modeled and observed values in Figure 13.5a. [Sok Kuh Kang, Republic of Korea]	Accepted. Deleted ref to Fig 13.5a.
13-576	13	16	55	16	56	"Gregory..... historical simulations". This sentence is confusing. [Isabel Andreu-Burillo, Spain]	Taken into account by rewording without "spin-up", in case this is the problem.
13-577	13	16	55	17	8	This should be expressed as a summary of the conclusions, rather than a logical explanation; it's not necessary here and merely serves to confuse. [European Union]	Rejected. This is already a summary of the conclusions. We agree that it is hard to understand and have reworded it in the hope of making it easier.
13-578	13	16	55	17	8	Model simulations "spin-up" without volcanic forcing could lead to an underestimate of sea level rise - important that policy makers are aware of this fact. [European Union]	Noted.
13-579	13	16	55	17	8	I'm surprised - we're into the AR5 and still discussing this volcanic spinup issue? The model results that we supplied to the AR4, as matter of course, had the spinup done first with "fictitious" volcanoes - based on the later period for which data were available, and then from 1000 AD onwards to the present with observed volcanic forcing. In our future projections we then again distributed some random eruptions. [Stefan Rahmstorf, Germany]	Noted.
13-580	13	16	56	17	1	This point is very hard to understand. Consider rephrasing to increase clarity. This comment also applies to the respective sentence in the Technical Summary. [Government of Germany]	Accepted and rephrased.
13-581	13	17	1			Volcanic eruptions will cause a forcing (a natural "external forcing") of long-term climate change if there is a long-term trend in volcanic activity. [Adrian Simmons, United Kingdom]	Accepted; the point has been added to the first paragraph.
13-582	13	17	1			volcanoes …give zero long-term forcing, because they are a normal part of the system - "long-term" needs defined better. Time scale is important for this statement to be accurate. [Ronald Stouffer, United States of America]	Accepted; it is now defined by comparison with "several decades" in the first paragraph. In fact it is a rather ill-defined timescale. Complete equilibration requires millennia.
13-583	13	17	3			How about CIMIP5 historical simulations? [European Union]	Taken into account by deleting "CMIP3". The papers cited refer to CMIP3, and there are no papers

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							specifically about CMIP5, but evidence suggests the issue applies to some of the CMIP5 models too.
13-584	13	17	4			Explain spin-up with and without and volcanoes. Is it about a measurable change in the angular momentum when material close to the axis of rotation is quickly moved farther away or some initial numerical model effect as the system achieves a quasi-steady state? [Terrence Joyce, United States of America]	Taken into account by rewording without "spin-up".
13-585	13	17	8	17	8	This sentence refers to material in the Appendix, explaining the implications for projections of the absence of volcanoes in model spin-ups. I failed to see any discussion in the Appendix concerning this point. [Stephen Griffies, United States of America]	Accepted. Reference changed to 13.4.1.
13-586	13	17	14			Please mention that the GIC's of AR4 are now called glaciers. [European Union]	done
13-587	13	17	21	17	21	Should say which part of 19th century to show how certain we are that the decline in volume is not anthropogenic [European Union]	done
13-588	13	17	21	17	23	Aw, come on, just say it: "Little Ice Age." "the result of natural climatic variability" means the end of the Little Ice Age. You can use the words. Michael Mann might get his feelings hurt, but he will get over it. Here's a useful graphic: http://soundwaves.usgs.gov/2001/07/glacierbaymap.gif [David Burton, United States of America]	Accepted. No reason not to. But LIA will have meaning for fewer readers than "natural variability."
13-589	13	17	21	17	23	Taken in isolation, this statement could be misinterpreted as saying that glacier retreat is entirely the result of natural forcing. We suggest adding to the sentence that, as assessed in chapter 10, it is 'likely' that glacier volume has further diminished significantly since the mid-20th century, due to human influence. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. We suggest the following revision: Overall, the combined records suggest that a net decline of global glacier volume began in the 19th century before significant anthropogenic radiative forcing had started, and was probably the result of warming associated with the termination of the Little Ice Age (Crowley, 2000; Gregory et al., 2006; Gregory et al., submitted). Statistically significant acceleration of rates of global glacier decline in the 20th century appear to be the result of anthropogenic forcing (Roe, 2011), and have been recognized as a significant sea level contributor since the earliest sea level assessments (Meier, 1984). As assessed in Chapter 4, observations, combined with improved methods of analysis and a new, globally complete inventory, indicate that global glaciers, including those around the ice sheet peripheries, are very likely continuing to be significant contributors to sea level (at more than 50% of the cryospheric total since about 1990), but are also highly variable on annual to decadal time scales
13-590	13	17	21			Page 17, line 21. In this paragraph you introduce SLE without spelling out what it stands for 1st time, or explaining what it means. This may be done in the glossary already, but it would make the text more readable if it is done here as well when the term is used for the 1st time. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken Into Account. SLE refers to "Sea Level Equivalent," and represents the rise in global mean sea level caused by the addition of a given mass of water. This was not defiined in this draft; this will be corrected.
13-591	13	17	27			While I would not contest this statement, the cited values with error bars for different time periods do not look as if they are statistically different. [Terrence Joyce, United States of America]	Taken Into Account. The time periods shown here do not show the statistical trend clearly. This will be discussed and clarified.
13-592	13	17	28	17	30	numbers not consistent with Ch4. Will need updating. They use 93-2010 also [Jonathan Bamber, United Kingdom]	Taken into account. Numbers will be updated and consistent with Chapter 4.
13-593	13	17	28	17	30	13.3.2.1. Numbers at the end of this section include the undefined acronym SLE. This should be omitted. I assume this stood for Sea Level Elevation until someone realized that “elevation” is ambiguous, meaning either “height” or “rate of change of height.” [James Gower, Canada]	Taken Into Account. SLE refers to "Sea Level Equivalent," and represents the rise in global mean sea level caused by the addition of a given mass of

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							water. This was not defiined in this draft; this will be corrected.
13-594	13	17	28	17	30	The time periods are not the same than those mentioned in Table 13.1 but the SLE are (1900-1990 vs. 1901-1990; 1993-2009 vs. 1993-2010). [Philippe Huybrechts, Belgium]	Taken Into Account. Because of the lack of complete data for 2010, the reporting periods for glaciers cannot be extended past 2009, so the comparisons of glacier rates with other components cannot be perfectly parallel. However, this wasn't explained in the text. This will be corrected and will clearly noted the text.
13-595	13	17	28	17	30	define SLE [Terrence Joyce, United States of America]	Taken Into Account. SLE is used frequently throughout this chapter, and a suitable point in the text.
13-596	13	17	28	17	30	The numbers for the meltwater contribution from glaciers are different from Chapter 4 (Table 4.5). This should be adjusted. [Peter Lemke, Germany]	Noted. The numbers actually are the same, but it's hard to tell. The loss rates are quoted here using the convention decided for Chap 13, as [low range, high range]; in Table 4.5, under "Incl. PG," the same values are written as mean ± variance. The different conventions are confusing, and should be straightened ou. The values listed here will be specified as including Peripheral Glaciers.
13-597	13	17	28			SLE was not defined before; it appears in many other pages [Government of Brazil]	Taken Into Account, and will be corrected. See response to 13-593
13-598	13	17	28			SLE not defined. Please define here or add to glossary. [Government of United States of America]	Taken Into Account, and will be corrected. See response to 13-593
13-599	13	17	34			Please give the number of glaciers that represents the small sample. [European Union]	Taken into account by inserting a reference to chapter 4.
13-600	13	17	37	17	40	"... cannot be independently … against glacier observations" They could be, if a subset was kept [Isabel Andreu-Burillo, Spain]	Taken into account by mentioning cross-validation.
13-601	13	17	37	17	40	for validation purposes. [Isabel Andreu-Burillo, Spain]	(assumed to be the conclusion of the previous comment)
13-602	13	17	42	17	53	This paragraph my be difficult to understand. Suggest re-writing with some sentences broken into shorter ones. [Government of United States of America]	Taken into account by some rephrasing.
13-603	13	17	42			What is 'multi-modal mean simulation' ? [Government of United States of America]	Taken into account by replacing with "ensemble".
13-604	13	17	44	17	44	"historical climate change" as input? " with the same model" output obtained? "using observed climate change" [Isabel Andreu-Burillo, Spain]	Rejected. The statements are compatible. The first statement (now modified) says that simulations of glacier change using observed climate input cannot be evaluated against independent glacier obs unless special techniques are used because all the obs have been used for calibration. The second statement is about the sensitivity of glacier simulation to climate input.
13-605	13	17	44	17	46	" For the model.... and to other obsevational glacier estimates..." How is this comparison compatible with the statement [Isabel Andreu-Burillo, Spain]	(assumed to be the continuation of the previous comment)
13-606	13	17	44	17	46	In lines 37 to 40? [Isabel Andreu-Burillo, Spain]	(assumed to be the conclusion of the previous comment)
13-607	13	17	45	17	45	I think the Marzeion paper has been published, or is, at least, in press. [Neil White, Australia]	Accepted.

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13-608	13	17	50	17	53	What is the uncertainty associated to the results of each of those simulations? Does this estimate take into account [Isabel Andreu-Burillo, Spain]	Taken into account by adding a remark on the comparison between unforced variability and model uncertainty.
13-609	13	17	50	17	53	those undertainties? [Isabel Andreu-Burillo, Spain]	(assumed to be the conclusion of the previous comment)
13-610	13	18	1			Observational total mass balance' - the total mass balance is given in this section. [European Union]	unclear
13-611	13	18	3			sea level changes - Add "observed" before "sea level changes". [Ronald Stouffer, United States of America]	accepted
13-612	13	18	5			I recommend the common terminology 'mass budget method or input¬output method', 'volumetric method' and 'gravimetric method' as repeat altimetry might be used in the mass budget method for ice thickness change estimation as well. [European Union]	accepted
13-613	13	18	12	18	20	The estimates are given in the periods 1993-2009 and 2005-2009 while table 13.1 (page 13-20) and TS-11 line 21-24 used the periods 1993-2010 and 2005-2010. It would greatly improve the report if the same periods were used. [European Union]	accepted - need to ensure effective information exchange with chpt 4
13-614	13	18	12	18	20	The sum of the Greenland and the Antarctic estimates can not be reconstructed by adding the two values listed in the text for either periods? [European Union]	noted - check calculation and if necessary explain logic
13-615	13	18	12	18	20	13.3.3.1. There is an error in the numbers for Greenland's and Antarctica's contributions for global sea level rise. Text shows 0.34+0.06 for Greenland 1993 to 2009, and 0.24+0.09 for Antarctica 1993 to 2009. Contribution for both is then given as 0.42+0.11 1993 to 2009. However, combining the two gives 0.58+0.11. Table 13.1 gives rise rates of 0.34 for Greenland and 0.18 for Antarctica, differing from 13.3.3.1 text. Numbers must agree. [James Gower, Canada]	accepted - need to ensure effective information exchange with chpt 4
13-616	13	18	12	18	20	13.3.3.1. There is an error in the numbers for Greenland's and Antarctica's contributions for global sea level rise. Text shows 0.34+0.06 for Greenland and 0.24+0.09 for Antarctica, 1993 to 2009. Contribution for both is then given as 0.42+0.11 1993 to 2009. However, combining the two gives 0.58+0.11. Table 13.1 gives rise rates of 0.34 for Greenland and 0.18 for Antarctica, differing from 13.3.3.1 text. Numbers must agree. [James Gower, Canada]	accepted - need to ensure effective information exchange with chpt 4
13-617	13	18	12	18	20	There seems to be some inconsistency between numbers given here, and those in Table 13.1. The rate of SLR from Antarctica is given as 0.24 mm between 1993-2009, and 0.40 between 2005 - 2009 (line 17), but is then given in Table 13.1 as 0.18 mm and 0.31 mm respectively. Likewise, for Greenland (1993 - 2009), the value is given as 0.61 here, and 0.63 mm in Table 13.1. The values in Table 13.1 do appear to be consistent with the values coming from Chapter 4 (Table 4.6). Please note also, the combined contributions given here of 0.42 (1993-2009) and 0.73 (2005-2009) also do not seem to make sense, given they are considerably less than the reported individual contributions, and also inconsistent with the combined contributions reported in Chapter 4 (Table 4.6). As a final point, the observational periods also shift by one year between the text here, and the corresponding periods used in Table 13.1 and Chapter 4 further adding to confusion. Please check carefully all values (and the uncertainty ranges!) given here for consistency with Table 13.1, and Chapter 4, and include any explanation in the text if apparent inconsistencies have valid reasons. [Thomas Stocker/ WGI TSU, Switzerland]	accepted - need to ensure effective information exchange with chpt 4
13-618	13	18	12		20	These numbers are inconsistent with Chapter 4 AND inconsistent with Table 13.2.!! This is a crucial point! Please harmonize the time span to 1993 to 2010 or give everywhere in AR5 numbers for 1993-2009. [European Union]	accepted - need to ensure effective information exchange with chpt 4
13-619	13	18	14	18	20	numbers not consistent with Ch4. Will need updating. They use 93-2010 also [Jonathan Bamber, United Kingdom]	accepted - need to ensure effective information exchange with chpt 4
13-620	13	18	14	18	20	The contributions of Greenland and Antarctica to sea level rise are given over 1993-2009 and 2005-2009, but contributions by both in Table 13.1 (page 13-12) are given over 1993-2010 and 2005-2010. I think that the values should be given for the same periods for consistency. [Sok Kuh Kang, Republic of Korea]	accepted - need to ensure effective information exchange with chpt 4
13-621	13	18	14	18	20	The numbers for the ice sheet (Greenland and Antarctica) contribution to sea level change are quite different	accepted - need to ensure effective information

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						from the numbers given in Chapter 4 (Table 4.6). In addition, the periods of data comparison are different (1993-2010 and 2005-2010 in Chapter 4). [Peter Lemke, Germany]	exchange with chpt 4
13-622	13	18	14			The acronym "SLR" is used on line 14 and the full wording "sea level rise" on line 16 below. These should be inverted. [Government of France]	editorial
13-623	13	18	15	18	16	This information is out of date. ICEsat measurements indicate that Antarctica is gaining ice mass, measured at about 0.14 SLE. See Zwally et al: http://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/20120013495_2012013235.pdf [David Burton, United States of America]	noted - this is business for chapter 4 - we only pick up their numbers
13-624	13	18	15	18	19	Pls check again the quantities of SLR-contributions by the GIS and WAIS. There seem to be some inconsistencies by summing up both of the components. [Government of Germany]	accepted - need to ensure effective information exchange with chpt 4
13-625	13	18	17	18	17	0.24 mmyr-1 for 1993-2009 is very different from 0.18 mmyr-1 for 1994-2010 listed in table 13.1 (page 13-20) and TS-11 line 21-24. Can this be true? [European Union]	accepted - need to ensure effective information exchange with chpt 4
13-626	13	18	17	18	17	The value "0.24+-0.09" is not consistent with the value of 0.18 mm per year given in Chapter 4 (for 1993-2010). [Government of Germany]	accepted - need to ensure effective information exchange with chpt 4
13-627	13	18	17	18	20	2009 should be 2010 to be consistent with Table 13.1 [Philippe Huybrechts, Belgium]	corrected
13-628	13	18	18	18	18	The value "0.42" is not given in Chapter 4 for this period. Chapter 4 gives values until 2010. Please check for consistency between the two chapters. [Government of Germany]	accepted - need to ensure effective information exchange with chpt 4
13-629	13	18	18			When I add the 2005-2009 numbers for Greenland (0.61 mm/yr) and Antarctica (0.41 mm/yr) I get 1.02 mm/yr, but here you say 0.73 mm/yr for both ice sheets. How can this be? [Terrence Joyce, United States of America]	noted - check calculation and if necessary explain logic ie that this is done in quadrature NOTE this means central values summed but variances added in quad
13-630	13	18	28	18	29	"... to those employed for projections." What are those observations? [Isabel Andreu-Burillo, Spain]	Taken into account by inserting clarification in the text and the caption of Table 13.2.
13-631	13	18	34	18	34	This range seems to me to be overly rosy on looking at the table. Perhaps the model spread can be cited as well. Also in the accompanying table, it would be helpful to have a column which showed something that could be compared with observations -- either include the accumulation from the models, or the SMB from observations. [Christopher Little, United States of America]	Rejected. The +- is a standard deviation, as the text says at line 30, and the table caption also says.H135
13-632	13	18	34			Page 18, line 34. "give" should be "gives". (This subject here is "set", which is singular.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account by rewording.
13-633	13	18	36	18	36	All "the" models or all "these", i.e. just the models cited in the chapter? Or is it an inclusive list. [European Union]	Accepted. It is "these".
13-634	13	18	36	18	37	SMB stands for "surface mass balance". So in what sense can SMB exhibit a decreasing trend of 3%/yr? Isn't it simply surface (ice) mass that exhibits the trend? Surface mass imbalance can be said qualitatively to be increasing. [Adrian Simmons, United Kingdom]	Taken into account by putting "calculated as accumulation minus runoff" immediately after SMB in the previous para to draw the reader's attention to this definition. It is also in the glossary.
13-635	13	18	37	18	37	"3%/yr" is going to confuse a lot of people! Remember, this Report is supposed to be read by laymen. You need to be clear about what the percentage is of, and you need to relate it to the actual mass of the ice sheet. The Greenland ice sheet obviously is not losing 3% of its mass per year. If its SMB were negative 200 Gt/yr, that would still be only a 0.008% ice loss. [David Burton, United States of America]	See 13-634.
13-636	13	18	40	18	41	Delete this sentence and update it with Shepherd et al., 2012 [European Union]	Rejected. Shepherd et al. do not make a statement on this subject.
13-637	13	18	41	18	48	I found this unclear. It needs rewriting more clearly. [Mark Siddall, United Kingdom]	Taken into account when rewording the paragraph in response to other comments.

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13-638	13	18	42			There is no a priori assumption that the ice sheets were in balance 1992 - in 1992 the satellite era started. (see page 10 lower paragraph) Line 1-42: A section 'Observational surface mass balance' is missing, covering the aspects of precipitation change and surface melting/ablation - these topics are covered in the lower part of 13.3.3.2 'Modelling...' [European Union]	First comment accepted. Second comment rejected. We do not have a subsection on observational SMB because it is really not a quantity which can be obtained directly from observations, owing to sparse data.
13-639	13	18	42			Page 18, line 42. “accounts" should be “account". (The subject here is “dynamics", which is plural.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account by rewording.
13-640	13	18	43	18	44	"total mass" and "observed net rate" needs clarification. [Michael Oppenheimer, United States of America]	Accepted. "total" and "net" have been deleted since they are unnecessary.
13-641	13	18	43	18	46	Sentence beginning "Sasgen et al.(2012) " is confusing for the reader. Maybe rephrase and/or split into two smaller sentences. [European Union]	Accepted.
13-642	13	18	45			Page 18, line 45. The subsentence starting with “and observational estimate" seems to lack a verb. I suggest that “accounts for" may be inserted after “ice outflow". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-643	13	18	46	18	48	I find the spread of SMB values as listed in table 13.2 (page 13-21) to be between 194 and 410 Gtyr-1 larg and larger than the error estimates and do not find the agreement satisfactory. [European Union]	Accepted. Reworded with reference to uncertainies.
13-644	13	18	46			Page 18, line 46. When you say “this satisfactory agreement" I am confused, because in my book 60% +60% does not add up to a normal 100%. I think you mean satisfactory within the uncertainties, but this needs to be made more specific, and it requires a statement about the size of the uncertainties. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reworded with reference to uncertainies.
13-645	13	18	50	19	10	consider referring to the Arctic regional climate change Section in Chapter 14, section 14.7.2, in addition to the Ch12 reference [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-646	13	18	51	18	51	A bracket is superfluous. [Mirko Orlic, Croatia]	Accepted.
13-647	13	18	53			Page 18, line 53. When introducing NAO and AMO for the 1st time, please spell out the full terms with the abbreviations given afterwards between brackets. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account by removing the phrase.
13-648	13	18	55	18	56	It is unclear to me how the RCMs are forced (i.e. in the previos paragraph) that DO show a trend toward a positive GMSL contribution -- reanalyses? [Christopher Little, United States of America]	Accepted. "Reanalyses" has been inserted in a previous para in response to 13-630.
13-649	13	19	1	19	1	This is the first place where “SAT” is used in this Chapter and it hasn't been defined (at least in this Chapter). It is also not on the Glossary (Annexe III). [John Hunter, Australia]	Taken into account by deleting the sentence.
13-650	13	19	1	19	1	Here you use for the first time in the chapter, the acronym 'SAT' for Surface Air Temperature. But you recall what it stands for only on page 26. You should recall it here instead. [Benoit Meyssignac, France]	Taken into account by deleting the sentence.
13-651	13	19	1		10	The grid-size dependence of the AOGCM results need to be discussed here. If no studies exist that are doing that, a sentence shall be included stating that this is missing. [European Union]	Rejected. The inability of the AOGCMs to reproduce the actual historical variability is not because of resolution, but because the variability is chaotic.
13-652	13	19	1			Page 19, line 1. Same as above for SAT. It makes the text more accessible if such an important term is written out when it appears for the 1st time in this chapter. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account by deleting the sentence.
13-653	13	19	4	19	10	Suggestion:Add a sentence about model vs. reality of sea ice decline would be good here. This gives me less confidence in the model projections of warming, and perhaps there is also a common reason for why AOGCMs cannot reproduce recent melt and why they don't get sea ice right. [Aslak Grinsted, Denmark]	Rejected because of lack of information. We are not aware of a literature basis for a link between projections of Greenland temperature and sea-ice decline due to anthropogenic forcing.
13-654	13	19	5	19	9	On the one hand side "there is not yet sufficient evidence to evaluate", on the other hand "we have high confidence… because of qualitative agreement...". To me this does not sound logical. [Andrey Ganopolski,	Taken into account by replacing this sentence with a caveat about uncertainty. Confidence is high in the

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						Germany]	effect, but there is uncertainty about its magnitude.
13-655	13	19	6	19	6	Make clear whether you are talking about the fact of warming or the quantitative values. [European Union]	See 13-654.
13-656	13	19	7	19	8	“qualitative agreement....” Has the uncertainty of those models been assessed? [Isabel Andreu-Burillo, Spain]	See 13-654.
13-657	13	19	18	19	19	“while Box... an the others about zero” in what period? [Isabel Andreu-Burillo, Spain]	Taken into account by rearranging the sentence. Also, for simplicity, the reference period is no longer mentioned.
13-658	13	19	24	19	25	"…surface melting and runoff are negligible in the present climate." Maybe a reference (possibly to Ch 4?) could be good here [Gerhard Krinner, France]	Accepted.
13-659	13	19	26	19	26	replace ", and" with ". Furthermore," [Christopher Little, United States of America]	Taken into account by rearranging the sentence to use the active voice.
13-660	13	19	35			The accumulation rate changes from ice cores are missing here - e.g. doubling of snow accumulation since 1850 at the Antarctic Peninsula (Thomas et al., 2008) Thomas, E. R., G. J. Marshall, and J. R. McConnell (2008), A doubling in snow accumulation in the western Antarctic Peninsula since 1850, Geophys. Res. Lett., 35, L01706, doi:10.1029/2007GL032529 [European Union]	Rejected. This does not seem relevant to the point that precipitation variability is dominant. However, the sentence has been deleted to save space.
13-661	13	19	43	19	43	“rise” -> “change”? [European Union]	Rejected because "change" would be repeated.
13-662	13	19	44	19	45	"Nothing is known about Antarctic mass balance earlier in the 20th century…": a) This paragraph is talking about *surface* mass balance; b) This is a very strong statement. There are some ice core studies looking at past acumulation rates. [Gerhard Krinner, France]	Accepted. Inserted "surface", and modified the statement to restrict it to assessment of SMB for the whole ice sheet (rather than the local evidence from ice cores).
13-663	13	19	47	20	3	consider referring to the Antarctic regional climate change Section in Chapter 14, section 14.7.14 [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-664	13	20	1	20	3	The disagreement in the baseline (p 19, line 30) changes the 1 to 1 relationship between SMB increase and projections of SLR. For instance, Antarctic SMB may increase, but a larger than expected present day negative imbalance might (over) compensate. [Christopher Little, United States of America]	Noted. This does not seem to affect the text, which refers to the Antarctic SMB contribution alone.
13-665	13	20	2	20	2	change the word "incorrect" to "high." Projections can err in either direction. [David Burton, United States of America]	Taken into account by deleting this phrase. It belongs better in 13.5.3.
13-666	13	20	6	20	22	caption wordings could be shortened [Government of Kenya]	Figure redrawn and caption shortened
13-667	13	20	11	20	11	Figure 13.4: (c) Can you explain the change in trend towards the end of 2000s period? [Isabel Andreu-Burillo, Spain]	There was a major event associated eith the 2011 La nina event.
13-668	13	20	11			Figure 13.4: please add source for c) change in land water storage changed; do AOGCMs here refer to the CMIP5 models? If so, we suggest to state this. [Thomas Stocker/ WGI TSU, Switzerland]	Figure redrawn and caption changed. Discussion improved.
13-669	13	20	25			Figure 13.5: The caption is confusing since not only time series from 1993 are shown but some GMSL contributions already start 1986. Please adapt accordingly [Thomas Stocker/ WGI TSU, Switzerland]	Figure redrawn and caption changed. Discussion improved.
13-670	13	20	31	20	33	Figure 13.5: Please clarify where the projections in c) are based on -- we assume this is the CMIP5 multi-model mean and range, but please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Figure redrawn and caption changed. Discussion improved.
13-671	13	20	33	20	33	Figure 13.5: Also shown... I don't understand this completely. [Isabel Andreu-Burillo, Spain]	Figure redrawn and caption changed. Discussion improved.
13-672	13	20	33	20	33	Caption for Fig.13.5. Last line states "Also shown is the observed sea level from (b)" yet the figure has two sets of observations. [European Union]	Figure redrawn and caption changed. Discussion improved.
13-673	13	20	40	20	40	Table 13.1 - This table will be widely used. It is currently confusing and the caption provides little clarity. The caption needs to clearly explain the key messages of the table, and when there is no data it should be clear as	Accepted - Omission of the 2005-2010 period will make this simpler

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						to why. [Government of Australia]	
13-674	13	20	40	20	43	Table caption to be shortened. [Government of Kenya]	Rejected - this is not possible given the request for additional information.
13-675	13	20	40	20	43	At first sight the comparison between observed and modelled contributions to GMSL rise in Table 13.1 seems very odd since the ice sheets are missing from the modelled row. The modelled budget for 1993-2010 can only be closed to within the uncertainty on the ice-sheet contribution because modelled expansion and modelled glaciers are higher than the observations. This undermines the credibility of the table. Moreover, the caption mentions that ice sheet SMB is computed from CMIP5, but those numbers are not included in the Table. [Philippe Huybrechts, Belgium]	Table and tect revised
13-676	13	20	40	20	43	Table 13.1 indicates a difference between the observed contributions and the observed GMSLR widening from 1993-2010 to 2005-2010. Is this primarily due to greater likelihood of short-term variability? Why is there no similar 2005-2010 comparison with modelled contributions consistent with the information available for the other time periods? [Phil Watson, Australia]	The 2005-2010 period has been omitted because of the difficulty of getting meaningful trends over short periods.
13-677	13	20	40		43	Table 13.1: This table nicely displays the various contributors to GMSLR in terms of mm/yr and associated uncertainty ranges. However, a very useful way to present these data would be the relative contributions to the total in terms of percentages – in other words, xx% of total from glaciers, xx% from Greenland, etc. This information could be extracted from this table, and certain pieces do appear in the text, but an additional column in this table or a separate table would be very useful for the community. Further, these percentages might also be nice to include in the executive summary, particularly in the section with Lines 34-44. [Joel Harper, United States of America]	Accepted - some text added to this effect
13-678	13	20	42	20	42	Table 13.1 caption uses word "thermosteric" but row heading in the table uses word "Expansion". Consistent naming required to avoid confusion, especially for non-experts. [European Union]	Accepted - tcaption/table revised
13-679	13	20	43			I see a serious problem with the table 13.1. On the one hand side, the models gives significant contribution (50%) from glaciers and land storage for the period 1993-2010, on the other hand, GRACE gives zero for the sum of these two components for 2005-2010. I cannot imagine any reason why relative contribution from these two sources dropped to zero during 2005-2010. This implies that either models or GRACE are wrong. I believe, this problem should be discussed. [Andrey Ganopolski, Germany]	The 2005-2010 period has been omitted because of the difficulty of getting meaningful trends over short periods. Part of the issue here is that GRACE gives the sum of counterbalancing terms.
13-680	13	20		21		Both tables need to be revised and cross-checked with Chapter 4! [European Union]	Accepted
13-681	13	20				Table 13.1: The last column of Table 13.1 is confusing. The subdivision of glaciers due to the GRACE-method into two groups: (1) those being peripheral to the big ice-sheets and (2) those defined as inland glaciers could lead to some confusion. Further the here clearly negativly labeled balance of land-water exchange (covering the positive balanced melting inland-glaciers and the nearly outweighted balance on reservoirs and groundwater exploitation) is not easily understandable and seems not to be consistent to the balance as mentioned in the text (13.3.2.1; 13.3.4). Finally, the specified timerange from 2005-2010 is part of the (clearly posititive labelled) timerange from 1993-2010. [Government of Germany]	The 2005-2010 period has been omitted because of the difficulty of getting meaningful trends over short periods.
13-682	13	20				tbl. 13.1 The reason for using an overlapping time period of 1971-2010 is not clearly explained in the text discussion leading up to this table. One also wonders how much uncertainty is added into understanding the reults from this table when there are significantly different time period lengths being compared. While some of this is linked to availability of observations and models, it should be acknowledged as contributing to the total uncertainty in drawing conclusions. [Government of United States of America]	Reasons for periods highlighted more clearly
13-683	13	20				tbl. 13.1 Observed GMSLR. Table 13.1 cites values of 1.5 mm/yr for 1901-1990 and 2.0 for 1971-2010. Where did these numbers come from? Shouldn't the 1901-1990 value at least be the consensus number of 1.7 mm/yr, as cited in Section 13.2.2? [Government of United States of America]	Accepted - text clarified. The 1.7 mm/yr is not for the 1900 to 1990 period.
13-684	13	20				Table 13,1: In this table for the period 2005-2010 you give a value of -0,33 mm/yr for the "GRACE land water" (which includes land water + glaciers except glaciers on Greenland and Antarctica) contribution to sea level. I could not find any reference to support that value. Can you give a reference here? [Benoit Meyssignac, France]	The 2005-2010 period has been omitted because of the difficulty of getting meaningful trends over short periods.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-685	13	20				Table 13,1: Another comment about the value of -0,33 mm/yr for the "GRACE land water" (including land water + glaciers except glaciers on Greenland and Antarctica) contribution to sea level. On page 17 line 29, you give an estimation of the global glacier contribution to sea level (including glaciers on Antartica and Greenland) over 2005-2009 of 1,03 mm/yr. If we consider this value as a rough estimation of the global glacier contribution to sea level over 2005-2010 (instead of 2005-2009) we can estimate the land water (only) contribution over 2005-2010 from the last column of table 13.1 as following: global glacier contribution (1,03 mm/yr, from page 17 line 29) - Greenland and Antarctica glaciers contribution (0,3 mm/yr, from table 13,1) - "GRACE land water" (-0,3 mm/yr, from table 13,1) = land water (+1 mm/yr). The result we get for land water (1 mm/yr over 2005-2010) is actually too high in my opinion to be realistic. I think that there is a probable inconsistency here between the "GRACE land water" estimate over 2005-2010 of table 13,1 and the global glaciers estimation given on page 17 line 29. [Benoit Meyssignac, France]	The 2005-2010 period has been omitted because of the difficulty of getting meaningful trends over short periods.
13-686	13	20				Table 13.1: The lack of modeled components needs to be explained, both in terms of why these are absent and also how this limits confidence in subsequent conclusions, possibly on p.23 lines 26-39. [Michael Oppenheimer, United States of America]	Accepted - text clarified
13-687	13	21	9	21	21	Table 13.2 provides a good SMB summary for the Greenland Ice Sheet. Why is there not a similar summary table for the Antarctica? [Phil Watson, Australia]	Noted. There is no need for a similar table because there are no significant trends. The relevant figures can be simply summarised instead in the text.
13-688	13	21	9			Table 13.2: we are wondering how some of these numbers in the table can be reconciled. For example the RACM2 numbers give a SMB trend from 1991-2010 of -12.7+-3.5 Gt yr-2. Over 20 year this would result in a reduction of ~250 Gt. However, the Mean SMB anomaly for the 1993-2010 period (relative to 1961-1990) is given as -82+-110. BTW, is "trend" the right word for the change in the rate of change in column 3 (in Gt yr-2)? [Thomas Stocker/ WGI TSU, Switzerland]	First remark referred to Michiel. Accepted second remark; changed to "rate of change of".
13-689	13	21	13	21	13	Table 13.2, caption states "positive in nearly all cases" - should this read "in all cases"? [European Union]	Taken into account.
13-690	13	21	24			Section 13.3.4 - The Aswan High Dam deserves a mention, since it dwarfs all others. Also, this section should mention the important implication of the 2nd-to-last sentence in the section: that because reservoir impoundment now lags behind groundwater depletion, a consequent acceleration in sea level, unrelated to climate change, should have been expected. The fact that no such acceleration has been detected is evidence that anthropogenic climate change is not accelerating sea level rise, and that fact should be mentioned in this section. [David Burton, United States of America]	Taken ito account - It is not approriateto focus on one single reservoir here. The implications for the rate of change has now been added and is also included in section 13.3.6.
13-691	13	21	30	21	30	“deemed inadequate”. Citation needed. [European Union]	We added a reference to Milly et al (2010) where this question is discussed
13-692	13	21	30			Is there a reference where one could find a discussion on the nature of the inadequate observations (changes in land-water storage)? What types of observations would be required to fill that gap? [Government of United States of America]	We added a reference to Milly et al (2010) where this question is discussed
13-693	13	21	36	31	36	SMB has been previously defined earlier in text. [Government of United Kingdom of Great Britain & Northern Ireland]	Should be for page 36
13-694	13	21		21		Table 13.2: it would be helpful to have a column which showed something that could be compared with observations -- either include the accumulation from the models, or the SMB from observations. [Christopher Little, United States of America]	Rejected. In the text, we already compare the model accumulation with obs accumulation.
13-695	13	22	12	22	15	It is not clear what "we" means here. Sounds like the authors present here own estimate which is not based on per-reviewed papers. [Andrey Ganopolski, Germany]	ok sentence corrected
13-696	13	22	22			Strange that the most recent estimate for groundwater depletion by Pokhrel et al. is not given. I understand that the authors do not believe in Pokhrel's 1mm/yr but is it enough to completely disregard results published in the high-profile journal? [Andrey Ganopolski, Germany]	Noted - Publicatoin supporting our decision included in the text
13-697	13	22	27	22	28	Don’t separate power in units “mm yr-1”. There seems several such cases in the text [Pavel Tkalich, Singapore]	ok corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-698	13	22	28	13	28	Why don't we have any estimation of the land water contribution to sea level over 2005-2010? Maybe it could be explained here. [Benoit Meyssignac, France]	Taken into account - this period has been removed from the budget table as it is to short to get robust trends.
13-699	13	22	30	22	33	The statements on self-amplifies must be backed up with references. [European Union]	References are given below , lines 34 and 37
13-700	13	22	35	22	35	“mostly” -> “most likely” – you don’t know for sure? [European Union]	Corrected
13-701	13	22	44	22	44	SLE - shor for what? [Charlotte Sparrenbom, Sweden]	now defined
13-702	13	22	57	23	7	There’s a discrepancy in the last sentence of this paragraph: "While the GRACE-based ocean mass is somewhat lower than observed estimates of the total land ice plus land water contributions over 2005–2010, the difference remains compatible with quoted uncertainties (see Section 13.4.6 and Table 13.1). The most recent GRACE-based ocean mass change for the 2005-2010 period is given as 1.2 mm/yr (Leuliette & Willis, 2011; vo Schuckmann and Le Traon, 2011)." From Table 13.1, the numbers for total land ice plus land water contributions are 0.30, 0.63, 0.31, -0.33 mm/yr (Glaciers, Greenland, Antarctica, Land Water, respectively). These add up to 0.91 mm/yr, which is LOWER than the GRACE results, not HIGHER. [Government of United States of America]	This was a mistake; The sentence has been corrected
13-703	13	23	2			Page 23, line 2. After “correcting", change “of" to “for". Alternatively, change “correcting of" entirely to “correction of". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Done
13-704	13	23	7	23	7	Section 13.4.6 is missing from this chapter. Should this text read "section 13.3.6"? [European Union]	Corrected.
13-705	13	23	7	23	7	Section 13.4.6, as referred here, doesn't exist [Government of Germany]	Corrected.
13-706	13	23	7			Page 23, line 7. Replace “with" to “within". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Done
13-707	13	23	10	23	12	"the sum of global ocean mass from GRACE and global thermosteric sea level change from Argo" may read "the sum of global ocean mass-equvalent sea level change from GRACE and global thermosteric sea level change from Argo" [Sok Kuh Kang, Republic of Korea]	Done
13-708	13	23	10			Perhaps "the sum of global ocean mass from GRACE" should be "The sum of global ocean mass change from GRACE" or possibly "change of global ocean mass measured from GRACE." [Government of United States of America]	Sentence modified
13-709	13	23	16			Section 13.3.6 - Again fails to distinguish between satellite (open ocean) and tide gauge (coastal) data, and fails to mention that thermal expansion increases the former with little effect on the later. [David Burton, United States of America]	Rejected. This statment is incorrect - see section 13.1.2 for new text addressing this comment.
13-710	13	23	18	23	18	Substitution is needed: 'Sections 13.1-13.5' to 'Sections 13.3.1-13.3.5'. [Mirko Orlic, Croatia]	Corrected
13-711	13	23	18	23	24	This paragraph is really hard to read with too many brackets and commas breaking the sentence up. A clear statement on the components that make up the global sea level budget would be helpful. [Government of Australia]	Text revised
13-712	13	23	18	23	24	It would be really useful if the information in this paragraph can be represented with a figure. The changing periods of reference (which are understandable) make it confusing to get the key messages from this info. A figure may be able to communicate this better than text. [Government of Australia]	In formation is in the table which has been simplified
13-713	13	23	21	13	21	satellite sea level altimetry began actually before 1993. Maybe you could say "precise satellite altimetry" began in 1993 [Benoit Meyssignac, France]	Accepted
13-714	13	23	23			Thawed permafrost (permafrost does not melt) [European Union]	Accepted.
13-715	13	23	26	23	39	see comment on Table 13.1 re absence of model estimates for 2005-10 [Michael Oppenheimer, United States of America]	Noted - the 2005-2010 period has been deleted becasuse reliable trends cannot be obtined over such a short period

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-716	13	23	33	23	35	Glacier Melting. There's a discrepancy in the percentage of observed GMSLR attributed to Glacier Melting in the 2005-2010 time period. "The two dominant terms are ocean thermal expansion (accounting for about 30% and 40% of the observed GMSL rise for the two periods, respectively, Figure 13.4a and 13.5) and glacier melting (accounting for a further 30% and 45%, Figure 13.4b and 13.5)." From Table 3.1, for 2005 to 2010, the Glacier number, 0.30 mm/yr, divided by the observed GMSLR number, 2.3 mm/yr, yields 13%, NOT 45%. (The 1993-2010 interval is also a little off at 33%, versus 30%.) [Government of United States of America]	Noted - the 2005-2010 period has been deleted becasuse reliable trends cannot be obtined over such a short period
13-717	13	23	35	23	35	One of the percentages (45) appears to be wrong. [Mirko Orlic, Croatia]	Corrected
13-718	13	23	35	23	39	In the view of serios discrepancies between modeling and obsetavtional estimates for the individual components of sea level rise, shown in the table 13.1, this statement sounds over-optimistic. [Andrey Ganopolski, Germany]	Wording revised - section rewritten
13-719	13	23	35			Earlier you distinguished between glacier melting and mass loss from Antarctica and Greenland. Are the latter now included in glaciers? or should you cite separate numbers for them as before and as in Table 13.1? [Terrence Joyce, United States of America]	Text clarified - separation now clearer
13-720	13	23	43	23	46	Model Contributions to GMSL Rise. There are discrepancies in the percentages explained for the 1971-2010 and 1993-2010 time periods. "The sum of these process-based model contributions, and the estimated change in land water storage (Figure 13.4c), which is relatively small, accounts for about 70% of the observed rate of GMSL rise for 1901–1990, and over 80% for 1971–2010 and 1993–2010 (Figure 13.4d and 13.5)." From Table 3.1, for 1971-2010, the sum of the model contributions and estimate land water storage, 1.8 mm/yr, divided by the observed GMSL rate, 2.0 mm/yr, yields 91% and for the 1993-2010 interval the percentage is 93%. With both of these numbers over 90%, why use the wording "over 80%"? [Government of United States of America]	text revised
13-721	13	23	47	23	50	Are there no results yet for coupled analysis (or reanalysis) for the data-rich 2005-2010 period using AOGCMs in data assimilation, which should ensure they have the right ENSO signal and Greenland warming? If not perhaps this could be flagged as a need and opportunity. [Adrian Simmons, United Kingdom]	Noted - the 2005-2010 period has been deleted becasuse reliable trends cannot be obtined over such a short period
13-722	13	23	47	23	51	Model Contribution to GMSL Rise. There's also a problem regarding why model-based estimates are not considered for the 2005-2010 interval. "We do not consider model-based estimates for 2005–2010, because it is a short period during which interannual climate variability has had a strong effect on GMSL change, notably through the influence of ENSO on ocean mass (Nerem et al., 2010) and heat content (Landerer et al., 2008) and regional variability affecting Greenland warming (Sections 10.5.2 and 13.3.3.2)." The references are inappropriate. The Nerem et al. paper doesn't deal with ocean mass change and the Landerer et al. paper is a simulation study that doesn't directly address the time period in question. The best reference for this statement is Boening, et al. 2012. [Government of United States of America]	Noted - the 2005-2010 period has been deleted becasuse reliable trends cannot be obtined over such a short period, but we have used this reference.
13-723	13	23	53			The meaning of "AOGCM-based models for the ice sheet contributions are not awailable for the past" is not clear to me. [Andrey Ganopolski, Germany]	text revised
13-724	13	23	55	23	55	"on the basis of" -> "directly forced by"? Present phrasing too obscure. [European Union]	Accepted - text revised
13-725	13	23	56	23	57	Inverting/calibrating to match a present day velocity snapshot does not ensure that the temporal response will be right. I would like a sentence to make that clear. [Aslak Grinsted, Denmark]	Accepted, qualifier added.
13-726	13	23	56	24	6	This paragraph, in my view is potentially misleading. It required an additional sentence or two that explains that, although the past contribution has been relatively small, this is no indication that it will be in the future. This is evidenced by the fact there is a non zero probability that the contribution could be as large as that proposed by Katsman or Pfeffer. [Jonathan Bamber, United Kingdom]	Accepted, qualifier added.
13-727	13	23	56			Page 23, line 56. Insert ";" after "outflow". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Editorial - done

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13-728	13	23	57	24	3	Ice-sheet Contribution. The paragraph beginning at the bottom of page 13-23 tries to make the argument that ice-sheet outflow is only a minor contributor to GMSL Rise in the 1993-2010 time period. (I'm ignoring the 1901-2010 results; they're not credible). "If increasing ice sheet outflow explains half the contribution from Greenland during 1993–2010 and all of that from Antarctica (Section 13.3.3.2), it would amount to about 6 mm, which is about 10% of the GMSL rise during that period, and about 3% of the GMSL rise during 1901–2010 (Chapter 4)." The assumption that ice sheet outflow explains HALF the contribution from Greenland isn't explained, and isn't particularly helpful to the argument. It would be more revealing to make the extreme assumption that ice sheet outflow explains ALL of the contribution from Greenland, in which case it would amount to only16% of the GMSL rise. The difference between 10% and 16% is unimportant; they're both small. [Government of United States of America]	Text revised
13-729	13	23		26		There are a lot of inconsistencies between the text and Table 13.1. [Government of United States of America]	Text and table revised
13-730	13	24	2			Change "of" to "for" [Robert Dean, United States of America]	of looks correct to me
13-731	13	24	8	24	16	Sometimes Greenland and Antarctica are included (observations) and sometimes not (models). It makes for a confusing discussion. Perhaps you could be clearer here. [Terrence Joyce, United States of America]	Accepted, text simplified and 2005-2010 period omitted.
13-732	13	24	9	24	9	Substitution is needed: 'Table 13.2' to 'Table 13.1'. [Mirko Orlic, Croatia]	Corrected
13-733	13	24	15	24	15	"relative" -> "with respect to" [European Union]	Accepted - used compared to
13-734	13	24	18	24	40	it is good to show that the residual could be closed with plausible guesses of these various components. However, I think that it is overly speculative when you quantify them to a degree that you can plot them in figure 13.5. [Aslak Grinsted, Denmark]	As now pointed out, all of these terms have been quantified in the literature.
13-735	13	24	18	24	40	This graph is used to argue that the budget is reasonably closed, and that we therefore should have confidence in process projections. However, the ice-sheet contribution is the key uncertainty, and they process models are not being validated at all here. So, this does *not* add confidence to the process projections. [Aslak Grinsted, Denmark]	Text revised and qulifiers added
13-736	13	24	18	24	40	In figure 13.4e there is a line for glacier_obs-models. This is then added to the model estimates and it is concluded that the budget is closed. That is like saying the budget is closed because observations=models+(observations-models) . There is clearly a problem with that. [Aslak Grinsted, Denmark]	Text revised
13-737	13	24	18	24	40	To add confidence you would have to have hind casts for all the model contributions using the exact same models as used for the projections. I would like to see hind-casts to atleast 1800 (to have any change in curvature). And ideally even further back (e.g. 2000 BP) so that we could compare to proxy records as well. [Aslak Grinsted, Denmark]	Noted - models hindcasts of all terms back to 1800 or 2000 BP would be desirable but results are not availablem to allow such a result
13-738	13	24	29			and not simulated by AOGCMs - This is an inaccurate statement. Some model ensemble members "could" simulate the 1930's warm period by chance. I have seen such a simulation at GFDL (unpublished). The statement is accruate for the ensemble mean. [Ronald Stouffer, United States of America]	text revised
13-739	13	24	38	24	40	incomprehensible [Michael Oppenheimer, United States of America]	text revised.
13-740	13	25	8	13	8	"result of an increase" instead of "result of a increase" [Benoit Meyssignac, France]	corrected
13-741	13	25	8	25	8	Concerning "has increased "; please indicate the time period this increase refers to. [Government of Germany]	text revised, qualifier inserted
13-742	13	25	8	25	9	Change text to "result of an increase in solar irradiance". Could also point out that solar output has been constant since the 1980s, and so any warming after this time is not due to increased solar output. [European Union]	Text revised.
13-743	13	25	8	25	11	Box 13.1: Since solar irradiance changes have a minor impact on observed climate changes, we were wondering why they are listed first here? We would suggest listing key factors according to the magnitude of	Agreed, text reordered

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						their influence [Thomas Stocker/ WGI TSU, Switzerland]	
13-744	13	25	8			AN increase… [Terrence Joyce, United States of America]	corrected
13-745	13	25	8			Page 25, line 8. Change “a" to “an" before “increase". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	corrected
13-746	13	25	17			The text says that tropospheric aerosols affect Earth's energy budget by reflecting sunlight and "enhance brightness of clouds." This doesn't really do justice to our current understanding of aerosol-cloud interactions, as discussed in Chapter 7. The view at the time of AR4 was that increasing aerosol primarily acts to increase cloud brightness by creating more but smaller droplets. Other aerosol-cloud interactions were too uncertain to estimate an effect. Today, it is recognized that the “cloud brightness” effect is modulated by other effects and is weaker than originally thought. Aerosol effects on clouds are know understood to be complicated and quite varied. It would be more appropriate to say here that aerosols “predominantly reflect sunlight and modify cloud properties and structure in ways that tend to reinforce the negative radiative forcing”. [Government of United States of America]	Accepted - text revised
13-747	13	25	21	26	39	Box 13.1 and its excellent Figure 1 (showing how small the changes in solar forcing are) provide an important and very well constructed discussion. However, the presentation neglects the contribution of the earlier Nimbus-7/ERB mission as well as the admittedly small contribution of the ScaRaB-Meteor mission (1994-95), with an instrument not designed at NASA LaRC (but an ERBE-like data processing system) that provided some independent radiation budget products from scanner data during the period from late ERBE to early CERES records (cf. Kandel & Viollier 2010). [Robert Kandel, France]	rejected - not suggiciently accurate to be relevant here
13-748	13	25	38			Page 25, line 38. Change “cloud" to “clouds", or to “cloud cover". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	text revised
13-749	13	25	48	25	48	Here delta T is used to represent the global mean temperature; presumably this should be temperature change, or else the symbol should be T, not delta T. [Government of United States of America]	Accepted - text revised
13-750	13	25	49	25	54	Page 25, lines 49–54. This is very repetitive relative to the figure caption of box 13.1, figure 1. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Agreed - text simplified
13-751	13	25		8	9	Why lead with solar? Lead with the large terms: ghg's [Stephen E Schwartz, United States of America]	Agreed - reordered
13-752	13	26	1			Hansen et al. 2005 - Could also reference the Stouffer and Manabe 1998 JoC paper (Vol 12, 2224-2237). [Ronald Stouffer, United States of America]	Agreed, reference added
13-753	13	26	3	26	4	This statement is untrue: "The associated thermal expansion of the ocean has contributed about 40% of the observed sea level rise since 1970." The only observed SLR from 1970 to 1993 was from coastal tide gauges, and they are not significantly affected by thermal expansion of the upper ocean. [David Burton, United States of America]	Rejected - Incorrect. This is expalined in section 13.1
13-754	13	26	6	26	6	Please add a sentence which explains that it only takes a fraction of the energy to raise sea level by ice melt compared to what it takes to raise it by steric expansion. [Aslak Grinsted, Denmark]	accepted text revised
13-755	13	26	8		13	The entire discussion of the residual will be seen to be moot once uncertainties are put on the forcings. Need to put uncertainty on ocean storage, too. [Stephen E Schwartz, United States of America]	Uncertainties now included.
13-756	13	26	11		13	See comment on figure 1, page 13-93. I think uncertainties are essential in this figure. My guess is that the combined uncertainty will greatly weaken the conclusion at page 13-26, lines 11-13: "Over the period from 1970 to 2012, this residual is small, less than 0.2 W m–2, and is consistent with a climate sensitivity well within the range of climate sensitivities of 2.0°C–4.5°C" but that remains to be seen. The implication of the 0.2 W m-2 would suggest that the forcing is known to that accuracy, which seems hard to support. [Stephen E Schwartz, United States of America]	Uncertainties added - they do not alter the conclusion
13-757	13	26	19	26	27	A cross-reference to Chapter 10 (un-numbered section starting on page 18, line 48 - though see comment 290) could be included here. [Adrian Simmons, United Kingdom]	Agreed - cross reference added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-758	13	26	41	26	51	Very important points for long-term sea level rise over next centuries, need to be emphasised as this chapter will be read by policy makers. [European Union]	Noted. (This comment probably applies to page 27 lines 41-51, not page 26.)
13-759	13	26	43			Section 13.4.1 - Again fails to distinguish between satellite (open ocean) and tide gauge (coastal) data, and fails to mention that thermal expansion increases the former with little effect on the later. [David Burton, United States of America]	Rejected. This comment is scientifically incorrect. Sea level at the coast is dynamically related to sea level in the open ocean.
13-760	13	26	46			Page 26, line 46. Here you say box 3.1. Is that correct? Or perhaps you mean books 13.1? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Rejected. Box 3.1 is intended, where the observational energy budget is assessed. Box 13.1 also refers to Box 3.1.
13-761	13	26	49	26	49	“among” -> “between” – former might imply within-model variability [European Union]	Taken into accout by rewording.
13-762	13	26	54			"approximately proportional". give value; don't just say approx proportional. I have estimated this quantity as 1.05 ± 0.06 W m-2 K-1, 1 sigma, where the kappa includes transfer of heat energy into atmosphere, land, and ice melt. Schwartz S. E. Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. 33, 745-777 (2012). DOI 10.1007/s10712-012-9180-4 [Stephen E Schwartz, United States of America]	Taken into account by adding a citation to Schwartz 2012.
13-763	13	26	54			"approximately proportional". Important to state implications. The implication is that the two heat transfer terms (to space and to deep ocean) are additive, leading to the transient sensitivity concept, namely that the change in global mean temperature is proportional to forcing by the transient sensitivity for a period of decades only slowly, increasing over centuries, to the so-called equiibrium sensitivity. See the Gregory 2000 and Held 2010 references and also my paper: Schwartz S. E. Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. 33, 745-777 (2012). DOI 10.1007/s10712-012-9180-4 [Stephen E Schwartz, United States of America]	Taken into account by mentioning ECS in the final paragraph of the section, which deals with the long term.
13-764	13	27	6		7	"Kappa is about half ... alpha". Given the uncertainty in alpha, range 0.82 to 1.85, more than a factor of 2, "about half" is pretty meaningless. Again better to give numbers. [Stephen E Schwartz, United States of America]	Noted. This text has been deleted, to save space.
13-765	13	27	8			The implication of the two heat flows being proportional to Delta T is much greater than for the transient response in climate models that are run at 1% per year increase in CO2. It has implications for the real world and any forcing scenario. [Stephen E Schwartz, United States of America]	Noted. The text does not appear to need modification. It does not mention the 1% CO2 scenario explicitly. The TCR is generally regarded as indicative of the response to increasing forcing.
13-766	13	27	9		10	Again give numbers; do not simply state spread is about twice as large, etc. This is one of the most important developments in understanding of climate change over the past several years and you are giving it much too short shrift. [Stephen E Schwartz, United States of America]	Rejected. Although the qualitative point is important and we agree that clarification of these ideas is important, we don't think the evidence available allows us to make more precise quantitative statements.
13-767	13	27	16		17	You might wish to add citation to my 2012 paper on same subject, especially as you have citations to submitted papers. Schwartz S. E. Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. 33, 745-777 (2012). DOI 10.1007/s10712-012-9180-4 [Stephen E Schwartz, United States of America]	Accepted.
13-768	13	27	20	27	25	The vertical structure of projected ocean heat uptake, and in particular the deep-ocean component can be a strong function of the formulation of the ocean model, even though total the 21st century steric sea-level rise varies by only modestly (by ~20%) between ocean model formulations (Hallberg et al., 2012). [Robert Hallberg, United States of America]	Accepted, but this point is already made in the first paragraph; we have added to the text there.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-769	13	27	21			In what sense is thermal diffusion "observed"? From the discussion to follow, it is clear that it is NOT observed, but that it is constrained by the observed changing stratification. [Terrence Joyce, United States of America]	Taken into account by shortening the text. We did not mean to imply here that diffusion is observed, but that a diffusive model can be tuned to reproduce observations.
13-770	13	27	23	27	24	The acronyms SRES and EMIC should be defines here or in the glossary. [Government of United States of America]	Accepted. They are in the list of acronyms.
13-771	13	27	24			I think it is important to acknowledge that modeled ocean heat uptake not only depends on the thermal diffusion parameter, but also on other uncertain model parameters such as climate sensitivity and aerosol scaling (which accounts for the uncertainty in radiative effects of aerosols). My group just published a paper attempting to quantify these uncertainties using a earth system model with a dynamic ocean component along with data-model calibration techniques based on observed trends in upper-ocean heat content, and we found the combined uncertainty in these parameters translates into a relatively large range of projected global mean SLR in 2100 for the RCP8.5 scenario, with a reliable upper bound around 0.6 meters (including deep ocean contribution), which is roughly double what is documented as the upper bound of the rcp8.5 cmip5 runs (arguably the cmip5 models are tightly tuned and reflect overconfidence). The finding is based on a climate sensitivity range consistent with the general consensus (CS<6). While it may be late to add new citations, nonetheless it is important to acknowledge that modeled SLR from thermal expansion is not solely affected by thermal diffusion, and the parameters are correlated in such ways that small differences in parameter values (within uncertain ranges) can combine to yield large ranges in projected SLR, even given good agreement with the observational record. The citation is here for reference: Sriver, R. L., Urban, N. M., Olson, R., and Keller, K. (2012), Toward a physically plausible upper bound of sea-level projections. Climatic Change, 115, 893-902, doi:10.1007/s10584-012-0610-6. [Ryan Sriver, United States of America]	Accepted.The important influence of climate sensitivity and forcing is already mentioned in a previous paragraph, and we have added a citation of Sriver et al.
13-772	13	27	32	27	36	Section 13.4.1 cites studies with highly idealized models extensively, but does little to draw from the literature relying more directly on analysis of AOGCM projections, which are far more useful for figuring out _why_ different projections differ. For instance, (Hallberg et al, 2012) show that two coupled AOGCMs (GFDL's ESM2M & ESM2G) that differ only in their ocean components differ by just 18% in projected 21st century global steric sea level rise (GSSLR) (and explores why they differ), indicating that most of the overall uncertainty in GSSLR projections comes from non-ocean components. [Robert Hallberg, United States of America]	Taken into account by adding a citation and drawing attention the uncertainty related to ocean modelling. In other sections, the spread in thermal expansion is put into the context of the overall uncertainty in GMSL rise.
13-773	13	27	32	27	36	The paper referred to in the previous comment has been accepted by J. Climate. The reference is Hallberg, R., A. Adcroft, J. P. Dunne, J. Krasting, and R. J. Stouffer, 2012: Sensitivity of 21st Century Global-mean Steric Sea Level Rise to Ocean Model Formulation. J. Climate, in press. [Robert Hallberg, United States of America]	See 13-772.
13-774	13	27	33	27	51	Important discussion - thermal expansion of oceans will continue long after greenhouse gas levels have stabilised. Important point for policy makers that should be emphasised. [European Union]	Noted. This point is made at the start of the final paragraph.
13-775	13	27	36	27	36	What is a "typical" non-mitigation scenario? Does such a thing really exist? It seems to be a reference to the SRES A1B scenario. [European Union]	Taken into account by deleting "typical". The papers use different scenarios.
13-776	13	27	43	27	43	Figure 13.6 does not show anything about ocean heat uptake, thermal expansion, and GHG concentration. [Sok Kuh Kang, Republic of Korea]	accepted. The reference was corrected to 13.10.
13-777	13	27	43	27	43	It seems that 'Figure 13.6' should be substituted by 'Figure 13.10'. [Mirko Orlic, Croatia]	accepted. The reference was corrected to 13.10.
13-778	13	27	43	27	44	why not give some broad estimates of these two time scales (eg. 50: 500 yrs) [Terrence Joyce, United States of America]	accepted. "centennial time scale" has been added to the text.
13-779	13	27	43			Is Figure 13.6 the right figure? [Government of United States of America]	accepted. The reference was corrected to 13.10.
13-780	13	27	43			Page 27, line 43. Here you refer to figure 13.6. However, figure 13.6 does not show the information referred to, but instead is about Greenland SMB. Please check and correct as necessary. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. The reference was corrected to 13.10.

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13-781	13	27	44		45	You should give numbers for the time constants; dont just say much longer than shallow ocean. They are about 5-10 years for upper ocean and about 500 years for deep ocean. This is quantitative science here. Addl reference: Schwartz S. E. Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. 33, 745-777 (2012). DOI 10.1007/s10712-012-9180-4 [Stephen E Schwartz, United States of America]	accepted. "centennial time scale" has been added to the text together with the reference.
13-782	13	27	49			"thermal rise can reach up to 2 m in the year 2500." relative to what? [Government of United States of America]	accepted. "above the pre-industrial level" was added to the text.
13-783	13	27	49			Page 27, line 49. Here you mention the thermosteric sealevel rise in the highest scenarios, but I cannot find where those are shown. Please insert a reference to the appropriate figure. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. "(Figure 13.11a)" was added.
13-784	13	27	55	27	57	13.4.2. Text uses the acronym SLE. Table 13.3 uses SLR. In neither case are these defined. In both cases a standard expression should be spelled out, presumably “Sea level rise.” [James Gower, Canada]	Taken into Account. See repsonse to 13-593.
13-785	13	27	57	27	57	please add references [Aslak Grinsted, Denmark]	Taken Into Account. These are actually given in the next sentence (pg 28 line 5), but there is a typo there that needs to be corrected. This will be edited to put the refs up front.
13-786	13	28	1		15	Is the term glacier still representing glaciers and ice caps? Line 1-15: The authors should mention that the ice dynamics might have a crucial effect on the retreat rate / volume losses for large parts of the glaciers - before the estimates are given. Later on the terminology 'process-based' is used before it is explicitly defined what processes are included. Please reorder the section and begin with explaining after the benefit of the new RGI the basis for the projections and give subsequently the numbers of the projection with the assessment. [European Union]	Agreed! ('glaciers' does refer to what used to be called 'glaciers and ice caps;' this is defined earlier (pg 17, line 14), although the term is used before this point. This definition will given at its first use, and possibly repeated if necessary. As to the potential for a dynamic response fro glaciers, this author is entirely in agreement. This potential is mentioned (page 29, line 27-33), but the discussion there is brief. Since that was written, more information has become available about the fraction of global glacier area draining to calving outlets, so some more specific estimates can be considered. This will be done, and the possibility mentioned up front, as suggested.
13-787	13	28	4	28	4	Word "Hypsometric" is uncommon and should be explained. [European Union]	Taken Into Account. This will be defined or replaced by simpler language.
13-788	13	28	7	28	7	Explain what AAR is. Non-experts will not know what this acronym stands for. [European Union]	text completely rewritten
13-789	13	28	14	28	15	Not clear what is meant by "still used to advantage". Please clarify wording here. [Thomas Stocker/ WGI TSU, Switzerland]	text completely rewritten
13-790	13	28	17	28	28	There is also a study by Giesen and Orlemans (Clim Dyn, submitted) that uses a suite of CMIP3 models with A1B and the Randolf database. Elsewhere you mix CMIP3 and CMIP5 results so why not here. [Jonathan Bamber, United Kingdom]	Taken Into Account. This will be brought into the assessment.
13-791	13	28	24	28	26	Here and in what follows, most citations are "submitted" and not suitable for the IPCC report. [Terrence Joyce, United States of America]	Noted. The 'Submitted' papers are actually allowed, per TSU. They must be "accepted" by March 15th, however.
13-792	13	28	24		28	Table 13.3: I would prefer to use the numbers as in the publications: 148 ± 35 mm SLE (scenario RCP26), 166 ± 42 mm SLE (scenario RCP45), 175 ± 40 mm SLE (scenario RCP60), or 217 ± 47 mm SLE (scenario RCP85). The numbers should be rounded according to the typical standard. Please note, that Marzeiron et al., give additional amounts during the 21st century. In case these numbers are taken up in other	Agreed. The [low range, high range] convention was decided on for Chap 13, but it is different than the convention for Chap 4, where ± is used, and this creates some confusion. This will be sorted out.

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						chapters, the amount from 2000-2009 has to be taken into account. [European Union]	
13-793	13	28	25	28	28	A systematic effect of ice which is below sea level and does not contribute to sea level rise must be mentionned (cf. Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion); this effect may be especially important during the near future. [Wilfried Haeberli, Switzerland]	Agreed. The paper cited was not yet available when this draft was written, but it will be included now. Unfortunately it will be hard to make a very robust estimate since there is almost no information on how much area in the 'glaciers' category is grounded below sea level.
13-794	13	28	31	28	32	font different to other tables [Mark Siddall, United Kingdom]	Taken Into Account. This will be corrected.
13-795	13	28	31	28	32	Table 13.3 indicates projected sea level rise to 2100, but, there is no indication of what the baseline year (or time period) is? [Phil Watson, Australia]	Taken Into Account. These will be specified.
13-796	13	28	31			Table 13.3: Please indicate what the reference period/ base year is used for the projected SLR given in the Tables. [Thomas Stocker/ WGI TSU, Switzerland]	Taken Into Account. These will be specified.
13-797	13	28	32			Table 13.3 Range of SLR by 2100 for four scenarios, inc. RCP2.6. Shows lower sea level rise under RCP2.6 than other scenarios with higher amounts of global warming. Important point for policy makers that should be emphasised. [European Union]	Noted.
13-798	13	28		28		13.4.2. Text uses the acronym SLE. Table 13.3 uses SLR. In neither case are these defined. In both cases a standard expression should be spelled out, presumably “Sea level rise.” [James Gower, Canada]	Taken Into Account. These will be defined. See response to 13-593.
13-799	13	28				Table 13.3: The term process-based is not suitable here, as ice dynamics was not included (see page 29). [European Union]	Rejected. These models did include surface mass balance, and they are based on that process.The label "Process-based" does not demand that every process operating be included (if it did we would have no processed-based models!). The absence of dynamics in these models will be noted here, however.
13-800	13	28				Page 28, table 13 3. In the row of Bahr et al., the cells needs to be vertically centred, as in their alignment is confusing as it is. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken Into Account. This will be reformated.
13-801	13	29	5			Change"model" to "models" [Robert Dean, United States of America]	Taken Into Account. This will be corrected.
13-802	13	29	10			AAR values for zero mass balance have a very large scatter of about plus/minus 30%. Systematic and continuously updated data material on this is available in the Glacier Mass Balance Bulletins of the World Glacier Moniroring Service (WGMS). Reference should correctly be made to this unique data source. A related recent analysis of modelled mass balance data is given by Machguth, H., Haeberli, W. and Paul, F. (2012): Mass-balance parameters derived from a synthetic network of mass-balance glaciers. Journal of Glaciology 58 (211), 965-979. Doi:10.3189/2012JoG11J223. It is shown that using mid-range elevation for ELA estimates is much simpler and (at least) as safe as AAR values. [Wilfried Haeberli, Switzerland]	Noted. This does not have much bearing on the text in question, where a paper using AAR is under discussion.
13-803	13	29	19	29	19	A mean and range of non-processed based models SLE is quoted here, but, whilst this presumably (?) refers to 2100 horizon, what is the baseline year (or time period) of relevance? [Phil Watson, Australia]	Taken Into Account. This will be clarified.
13-804	13	29	20	29	20	I realise that Tad Pfeffer is on the author list and so I assume that what is written here is correct. However, the Pfeffer et al 2008 result is very commonly miscited. Because of common miscitation, many may not understand why the range cited here is low. It is commonly cited that the upper limit from Pfeffer et al 2008 is 2m. I understand this is incorrect but some explanation explaining why is needed. [Mark Siddall, United Kingdom]	Noted. The range given in Table 13.3 for the Pfeffer et al (2008) projection is their value for *glaciers only*. The 2.0 m upper bound (misinterpreted discouragingly frequently, as the reviewer notes) is for all sources of SLR, including ice sheets and thermal expansion as well as glaciers. This should be made clearer in the text, however. This will be done.
13-805	13	29	22			Figure 13.7 is not referenced in the text. [Government of United States of America]	Noted. This will be corrected.
13-806	13	29	23			Figure 13.7: please provide more information about how these modelled ranges have been derived. Are they	Accepted. The ranges shown in the figure are the

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						representing a multi-model range or an uncertainty estimate (how estimated?) from the studies references? [Thomas Stocker/ WGI TSU, Switzerland]	uncertainties published for each of the models depicted. The accompanying publications are mentioned - very briefly - in the paragraph preceding Table 13.3 (where the same uncertainties are shown for the 2100 end point), but the discussion suggested here is an improvement and will be added.
13-807	13	29	27	29	28	Need consistent teminology for marine-ending glaciers between first mention here and redefining as marine-terminating on page 32 line 39. [Jeff Ridley, United Kingdom]	Accepted. Terminology at this level has been rather scattered, but is converging as the drafts progress. This will be made uniform.
13-808	13	29	35			With a view to the very large uncertainties and even systematic errors of area-related ice thickness/volume estimates, the relevance of the "size cutoff" mentionned appears rather unrealistic if not absurd (cf. comment 8 on Chapter 4, page 18, lines 4-5). Better eliminate the corresponding statement and reference. [Wilfried Haeberli, Switzerland]	Rejected. The potential significance of the lower size limit is clearly analyzed and evaluated in the reference cited, and the analysis is based on widely validated and accepted methods.
13-809	13	29	38	29	38	Typo. Suggest replacing the word “losses” with "loss". [Phil Watson, Australia]	Accpeted. This will be changed.
13-810	13	29	38	29	43	This is an important statement appearing - as far as I remember - for the first time in an IPCC report. Probably the primary phenomenon of interception is the formation of many – often small but also very large – new lakes in overdeepend parts of glacier beds becoming exposed through glacier vanishing. This is discussed in Haeberli, W. and Linsbauer, A. 2012: Global glacier volumes and sea level: effects of ice below the surface of the ocean and of new local lakes on land. The Cryosphere Discussion. The order of magnitude is most likely millimeters sea level equivalent. [Wilfried Haeberli, Switzerland]	Noted. Terrestrial interception is an important consideration that needs more attention.
13-811	13	29	40	29	43	This is(/should be) accounted for in observationally based estimates of reservoir storage and changes to ground water tables. So I believe there is no double accounting here. [Aslak Grinsted, Denmark]	Noted; In principle, the main effects of terrestrial storage should already be accounted for as the reviewer describes; the constructoin of reservoirs and depletion of ground water by irrigation, for example, can be calculated fairly confidently. However, there are a number of other situations where this correction is not so easily done. The potential acquifer interception of glacier runoff from the Himalayas, for example, is almost completely unknown. GRACE measurements are the obvious way to do this, but there's a lot going on in the Himalayas that will effect gravitational signals (to say the least), and this number has not been extracted in any robust fashion. Beyond these considerations, however, is the simple fact that while this correction is *possible,* it is general not done. In the vast majority of cases, those who are making projections of land ice contribution to sea level simply take whatever water-equivalent volume loss comes out of an analysis, divide by the ocean area, and call it a day. This topic was mentioned here partly as a consciousness-raising exercise.
13-812	13	29	40			ice sheets were excluded - which ice sheets peripheral glaciers are included? [European Union]	The precise separation of peripheral glaciers and ice caps from the ice sheet proper in both Greenland and Antarctica is an evolving issue, and decisions on where exactly the line is to be drawn was still being discussed at the time of this draft. The separation between peripheral ice and ice sheets proper will be clarified in the Final Draft, with the primary discussion in Chap. 4, but can be reiterated in Chap. 13.

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13-813	13	29	41			remove 'but' [Kathleen McInnes, Australia]	Accpeted. This will be corrected.
13-814	13	29	45	35	40	Page 29, and subsequently. In this section about Greenland, there is excessive use of the words “however", whether it suits or not. This will need to be made grammatically correct. In my comments, I will highlight the instances that I spotted. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Noted.
13-815	13	29	45			Section 13.4.3 - No discussion of Greenland's ice sheet can be complete without mention of the MWP, during which Greenland, especially, was clearly warmer than it is now, for several hundred years, yet apparently without resulting in substantial global sea level rise. [David Burton, United States of America]	Rejected. There is insufficient evidence to support such an assessment.
13-816	13	29	49	29	50	Which parts of this are supported by models, and which by observations? [Christopher Little, United States of America]	We don't think this needs to be discussed here in more detail because it is fully described in section 13.3.3.2.
13-817	13	29	50	29	53	Does this contradict the paragraph beginning on p 18, line 50? [Christopher Little, United States of America]	No. That paragraph also gives evidence that warming of regional climate leads to increasingly negative SMB.
13-818	13	29				Page 29, caption figure 13.7. The portrayal of lines needs to be made consistent in this figure, given that not all lines are shown as black. Also this figure seems unnecessarily “heavy" compared with other figures. A more subtle redrafting may be in order. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Figure redrawn
13-819	13	29				Figure 13.7. See comments made earlier. Not all lines are black, yet the caption does not explain any differences. Also the figure is “heavy" compared with other figures and may require more subtle redrafting. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Figure redrawn
13-820	13	30	10	30	12	This collection of acronyms will leave the uninitiated reader far behind - try to be more explanatory! [Terrence Joyce, United States of America]	Rejected. AR4, CMIP3, CMIP5, SRES and RCP are all in the list of acronyms, and this is not the first time they are used in the chapter.
13-821	13	30	23			Define 'SAT' [Kathleen McInnes, Australia]	Accepted.
13-822	13	30	30	30	30	Cite Vernon et al: Surface Mass Balance Model Intercomparison for the Greenland Ice Sheet, TC, sent to Gregory, which compares refreeze, mlet and runoff for four of the five models considered here and shows the large difference in refreeze between ECMWFd and others. [Jonathan Bamber, United Kingdom]	[It is not published yet - we will cite it if it meets the deadline]
13-823	13	30	30	30	30	….define acronym PDD ' here orin the glossary [Government of United States of America]	Rejected. It is defined on page 30 line 2.
13-824	13	30	33	30	33	Text states that four different RCMs were used by Rae et al., but only three are listed in the caption of Table 13.4, Note (g) [European Union]	Taken into account by deleting "four" to avoid confusion. Rae use four RCMs but only three of them are used for projections, so the fourth does not appear in the table.
13-825	13	30	35		40	Since meltwater retention in firn has a large impact on SMB model results, this chapter (and perhaps chapter 4 too) needs better discussion this process. Very recent studies on Greenland Ice Sheet have shown that a substantial amount of surface melt penetrates deeply (>10 meters) into firn (Humphrey et al., 2012; Forester et al., 2011). Inhomogeneous and deep infiltration of melt water is not well represented in SMB models. One study concluded that all pore space is ultimately filled by repeated inhomogeneous infiltration events, and that the present day storage capacity of firn in the percolation zone is such that fully filling all existing pore space requires decades of melt (Harper et al., 2012). However, once the firn pore space is filled, full runoff conditions will exist over a much larger area of the ice sheet. Refs: Humphrey, Neil F., Joel T. Harper, and W. Tad Pfeffer. "Thermal tracking of meltwater retention in Greenland's accumulation area." Journal of Geophysical Research 117.F1 (2012): F01010.	Agreed. This point will be elaborated slightly and the new references will be included.

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						R. Forster, AGU abstract, 2011 fall meeting, perhaps he has a paper in the works. Harper, J., et al. "Greenland ice-sheet contribution to sea-level rise buffered by meltwater storage in firn." Nature 491.7423 (2012): 240-243. [Joel Harper, United States of America]	
13-826	13	31	7	31	9	The projected AR5 glacier contribution is higher than the AR4 projection because the peripheral glaciers around the ice sheets are included. However, most studies listed in Table 13.4 do not distinguish between the peripheral glaciers around Greenland. This should at least be mentioned in the text or in the tabel and perhaps be quantified to get an idea about the double counting with the glacier numbers. [Philippe Huybrechts, Belgium]	Taken into Account. The irregularities in the handling of the peripheral glaciers is a significant problem, with complications arising in both Chapters 4 and 13 and in glacier as well as ice sheet balance estimates. The authors are aware of this important issue and are working on its resolution.
13-827	13	31	7	31	9	Table 13.4 could include the numbers from Fürst et al. (submitted). For scenario RCP4.5 and a selection of 15 CMIP AOGCMs the contribution to GMSLR is 0.01-0.08 m between 2000 and 2010, including a modest contribution from ice dynamics. This is from a 5 km PDD model. [Philippe Huybrechts, Belgium]	Accepted
13-828	13	31	7	31	10	The estimates in this Table don't give me any confidence in the models used to derive them, and we still have to add dynamic contributions. Surely, this has to yield very large uncertainty estimates?? [Robert Thomas, United States of America]	Rejected. The dynamical contribution is the subject of the next section. The SMB models all agree in projecting increasingly negative SMB for well-understood physical reasons and their results are consistent with observations (as discussed in 13.3.3.2). The spread of model results is a measure of uncertainty.
13-829	13	31	9	31	10	Please double check the Mernild 2010 numbers. [Aslak Grinsted, Denmark]	Accepted and checked. They are correct. [But cf 13-826]
13-830	13	31	31			Page 31, line 31. Insert a comma after “scales". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-831	13	31				Table 13.4 - although the SMB goes directly into the input-output method estimates of total mass balance, it is still the balance SMB-discharge that contributes to sea level change and not the SMB itself. Contribution to the total ice sheet mass balance in sea level equivalent would hence be a better terminology. last line: 0.0-1.8 from ???? to ???? [European Union]	Rejected. This would be confusing because we are distinguishing contributions from SMB (13.4.3.1) and discharge (13.4.3.2). This approach is explained in 13.1.5.1.
13-832	13	32	1	32	1	"nonlinear" is a rather vague word; it would be more useful to specify more clearly [Richard B. Alley, United States of America]	The next sentence describes the nonlinearity - the section has been revised also.
13-833	13	32	1	32	3	Please add a qualitative description of the nature of the non-linearity here. [Aslak Grinsted, Denmark]	The next sentence describes the nonlinearity - the section has been revised also.
13-834	13	32	8	32	9	This section is about the Greenland ice sheet. Although the reference of Antarctic results from Singedouw is correct ("They found...") it is not the right context to cite Antarctic results. Move to appropriate section or remove. [Olaf Eisen, Germany]	accepted. The sentence is omitted and shifted to the Antarctica section
13-835	13	32	8	32	9	We don't understand this sentence, and what is meant by 'interactive changes'. As a result, it is unclear to us why this sentence referring to changes in the Antarctic Ice Sheet is coming here in a section on Greenland mass change. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. The sentence was rephrased and transferred to the Antarctic section.
13-836	13	32	9	32	11	Is the "opposite effect" for Antarctica only, for Greenland only, or for both? Specify [Richard B. Alley, United States of America]	corrected. The sentence referrs to Greenland only. The problem was addressed by omitting the Antarctica sentence here.
13-837	13	32	20	32	21	The exact numbers are: 0.36 m (corresponding to an average rate of 0.36 mm yr-1) for 560 ppm and 2.59 m (2.6 mm yr-1) for 1120 ppm. In Huybrechts et al. (2011) the mean warmings are only +2.4°C (2xCO2) and +6.3°C (4xCO2) after 1000 years with respect to pre-industrial which explains the low sea-level response. [Philippe Huybrechts, Belgium]	accepted. The numbers and the reference to the warming sensitivity was added to the texts. Thanks!

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13-838	13	32	24	32	24	Using the same model as Huybrechts et al. (2011), Goelzer et al. ' should be 'Using the same model as Huybrechts et al. (2011) albeit with a slightly higher polar warming, Goelzer et al. ' (both model versions use a different parameter set for the climatic component making the polar warming somewhat larger in Goelzer et al; hence both models are 'similar' rather than 'the same' [Philippe Huybrechts, Belgium]	accepted. Text was added.
13-839	13	32	24	32	24	Goelzer et al (submitted): this paper does not appear in the reference list and has moreover now been published as: H Goelzer, P Huybrechts, S C B Raper, M-F Loutre, H Goosse and T Fichefet 2012 Millennial total sea-level commitments projected with the Earth system model of intermediate LOVECLIM, Environmental Research Letters, 7, 045401 doi:10.1088/1748-9326/7/4/045401; This paper should not be confused with the second Goelzer et al (submitted) mentioned elsewhere in the chapter that does figure in the reference list (initially submitted to The Cryosphere, but was redrawn and resubmitted to Journal of Glaciology because of a potential conflict of interest in the open review process of The Cryosphere). [Philippe Huybrechts, Belgium]	accepted and corrected.
13-840	13	32	27			Year 3000? Even though models can produce results, does not mean that these should be reported, especially in a document such as this where numbers can be plucked out and used incorrectly. I am not sure what the value is of sea level rise projections beyond 50-100 years and these numbers do not have error bars and have two significant figures... [Government of United States of America]	rejected. The physical representation in these models captures long-term inert processes better than the fast ice evolution. They are thereby potentially more reliable on longer time scales.
13-841	13	32	27			Page 32, line 27. The last sentence in this paragraph is not so clear to me. Maybe it can be elaborated a little bit to make the meaning unambiguous. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. The sentence was eliminated and ist contents transferred to the section on sea-level projetions beyond 2100 where the commitment figure is discussed.
13-842	13	32	33			Section referenced here doesn't exist. [Government of United States of America]	accepted and corrected.
13-843	13	32	35	34	48	The discussion on the dynamical change of the Greenland ice sheet is quite well argued and is indeed a very significant step forward compared to AR4. Still, I believe one ought to have much more confidence in the lower limit of 16 mm than in the higher limit of 68 mm for the likely range. That is because the lower limit (by itself a higher limit from the cited studies) is based on model experiments with realistic forcing (Nick) combined with a plausible generalisation (Goelzer). The calculation of the higher limit still suffers from having to make assumptions on repeat intervals of retreat (following Price) or having to make assumptions on the amount of ice shelf melting (Bindschadler and Nowicki approach) and therefore still plays in the same league as 'upscaling current increased discharge' by some arbitrary amount as done previously. After all, a 68 mm from ice dynamics is enormous as it assumes an almost doubled discharge sustained over the full 100 years which is way above what ice-dynamic models with realistic forcing can generate. The likely upper range should be lower than 68 mm: based on the material presented in section 13.4.3.2 a likely upper bound of around 30 mm can be defended equally well. The suspicion should not arise that arguments have been collected to come up with numbers for ice dynamics that are essentially the same as the scaled-up ice sheet discharge from the AR4. [Philippe Huybrechts, Belgium]	noted can we apply different confidence to upper and lower limits of range - interesting idea. Thinks the numbers are higher than process based models but then the process based model he is talking about do not have full physics.
13-844	13	32	35	34	48	The discussion in section 13.4.3.2 hinges crucially on only a few papers that seem to be all under review (except for the Price et al 2011 paper). What is the exit strategy in case some of these papers don't make it in time and either an upper or lower limit can not be backed up with a publication? [Philippe Huybrechts, Belgium]	noted - can not do much about this
13-845	13	32	35			Section 13.4.3.2 It should be made very clear that there is no true process-based modelling of the fast moving glaciers and ice streams included in the assessment. In general the ice dynamics are accessed from the literature. This should reflect on the level of confidence. (as stated on page 13-45, line 18-19) [European Union]	noted - i think we do this. The nick work is process based but the generarlization is not.
13-846	13	32	37	32	41	para indicates two mechanisms and then describes three. Also what is the nature of the interaction between SMB and ice flow? [Kathleen McInnes, Australia]	noted - needs to be reworded. Detail on link between SMB and dynmaics comes later.
13-847	13	32	37	34	48	Pages 33-34, throughout. I find this section overall to be written by and for specialists. To me it seems rather inaccessible to nonspecialists. This may need some attention, given the nature of the readership. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted - this need to be refined into an assessment.

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13-848	13	32	38			"icebergs" instead of "ice bergs" [Government of Brazil]	editorial
13-849	13	32	38			… icebergs (typo) [Government of France]	editorial
13-850	13	32	40	32	40	Cryptic sentence. Needs rewriting. First reading I thought this referred to impact on driving stress as slopes changed due to SMB but in fact it is something different. [Jonathan Bamber, United Kingdom]	accepted see 846
13-851	13	32	43	32	43	13.4.3.2. The acronym SLR is undefined and unnecessary. [James Gower, Canada]	noted
13-852	13	32	43	32	43	Typo: dynamical [Aslak Grinsted, Denmark]	editorial
13-853	13	32	43	32	43	dynamical [Frank Pattyn, Belgium]	editorial
13-854	13	32	43	32	43	typo error “dynmical change” [Pavel Tkalich, Singapore]	editorial
13-855	13	32	43	32	51	This paragraph gives a similarly gloomy picture of how well we are likely to be able to predict SLR from dynamic changes on Greenland. The rest of Section 13.4.3.2 fails to lighten the gloom; indeed it increases the murk, although the final paragraph at least admits a large uncertainty for Antarctica which, however, is probably still too small. There is plenty of room for surprises from West Antarctica. [Robert Thomas, United States of America]	noted - need a bit more than 'plenty of surprises' on which to base a statement of likelihood
13-856	13	32	43			"dynamical" instead of "dynmical" [Government of Brazil]	editorial
13-857	13	32	43			… dynamical (typo) [Government of France]	editorial
13-858	13	32	44	32	44	I don't think 'physical intuition' expresses this. How about 'qualitative physical arguments' [Mark Siddall, United Kingdom]	accepted - need to ask Tad for a better description "calculation aimed at rejecting implausible mass loss scenarios based on physical intuition/insight (constrained by physical understanding)" limit seeking model
13-859	13	32	44	32	45	What is meant by "low scenario" and "high scenario"? Does the text mean scenarios which produce low and high amounts of sea level rise? [European Union]	accepted needs to be reworded
13-860	13	32	44			why not share this physical intuition, or delete the discussion? As written, it is not satisfactory. [Terrence Joyce, United States of America]	rejected this is supposed to be an assessment - can not reporduce the whole methdology here
13-861	13	32	49	32	39	ther final estimate is 100mm' - between when and when? [Mark Siddall, United Kingdom]	noted - the paragraph gives the time frame at start but could be made clearer
13-862	13	32	50	32	50	What is meant by the phrase "extreme limit"? Is this limit really an extreme upper limit? Elsewhere in this chapter sea level rise of 1 - 6 m are quoted. [European Union]	accepted need to use phrasing other than limit
13-863	13	32	51	32	51	More plausible but not likely (i.e. why incorporated in some way into projections?) Does plausible imply possible but <17% exceedance? [Christopher Little, United States of America]	accepted see 862
13-864	13	32	53	14	4	The recent Bjork et al paper in Nature Geoscience needs citing here, as do potentially Kjær KH et al. (2012) Aerial Photographs Reveal Late−20th-Century Dynamic Ice Loss in Northwestern Greenland, Science 337: 569-573. DOI: 10.1126/science.1220614 AND Moon T, Joughin I, Smith B, Howat I (2012) 21st-Century Evolution of Greenland Outlet-glacier Velocities. Science 576-578. doi:10.1126/science.1219985 [Mark Siddall, United Kingdom]	editorial - supporting refs for process might drop and leave to chpt 4
13-865	13	32	53	32	55	Add discussion of Harig and Simons, PNAS, 2012 GRACE study showing more detailed spatial patterns of change. Harig, C. and F.J. Simons 2012. Mapping Greenland’s mass loss in space and time. Proceedings of the National Academy of Sciences, doi:10.1073/pnas.1206785109. [Matt King, Australia]	editorial - supporting refs for process mat drop and leave to chpt 4
13-866	13	32	53	32	55	Semething is missing in the sentence. [Mirko Orlic, Croatia]	editorial - might run into next sentence
13-867	13	32	53		57	These are observations and should be reported elsewhere. [European Union]	noted see 862

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-868	13	32	53			Page 32, line 53. The sentence that starts with “between 2005 in 2010" needs a verb. I suggest “occurred" after “ice sheet". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial see 866
13-869	13	32	54	32	54	Insert “there was” at before “widespread”. [European Union]	editorial see 866
13-870	13	32	54			insert 'occurred' after 'dynamic thinning' [Kathleen McInnes, Australia]	editorial see 866
13-871	13	33	1	33	4	Perhaps add a clarification that the water mass are deep rather than surface. [Jeff Ridley, United Kingdom]	accepted - add text
13-872	13	33	1		2	These are observations and should be reported elsewhere. [European Union]	agreed but need to provide some sort of context to projections. Will discuss how much to leave and how to link to chpt 4
13-873	13	33	2		4	Move this sentence further below the introduction of the projections. [European Union]	agreed
13-874	13	33	4	33	4	Changes to fjord circulation are also important, and the loss of arctic sea ice will probably have a huge influence on that through changes in wind-stress. [Aslak Grinsted, Denmark]	agreed will add clause
13-875	13	33	6	33	12	projections of outflow is also addressed in Gillet-Chaulet et al., TCD 2012, at the global scale but also for the major contributors of the GIS. [Olivier Gagliardini, France]	noted but this paper does not explicitly look at the effect of altered cacing - it fits more closely with the 3rd SMB coupling term
13-876	13	33	6	33	22	Are these regional models incorporated into the estimates of dynamic ice loss in the SLR projections? Or only the study where the Nick model is embedded in the larger-scale model (Goelzer)? [Christopher Little, United States of America]	noted - reviewer needs to read on to summary where these various lines of evidence are brought together - hopefully will be clearer once section recast as assessment
13-877	13	33	6	33	50	The entire section is highly speculative and relies heavily on several papers that have only been submitted. The text should be condensed and focused only on material in the peer-reviewed literature. [Government of United States of America]	rejected - SOD should use latest available literature unfortunately mostly only submitted. TOD will clarify this
13-878	13	33	6			earlier -> early [Kathleen McInnes, Australia]	editorial
13-879	13	33	6			Page 33, line 6. Change “earlier" to “early". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-880	13	33	6			Page 33, line 6. Insert a comma after “However". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-881	13	33	8	33	8	What does MTOG stand for? Non-experts will not know what this acronym stands for. [European Union]	noted MTOG defined 32 39
13-882	13	33	9	33	9	1 or 1.0? [Matt King, Australia]	accepted
13-883	13	33	9	33	9	Substitution is needed: 'associate' to 'associated'. [Mirko Orlic, Croatia]	editorial
13-884	13	33	9			… generalize (typo or English spelling??) [Government of France]	editorial
13-885	13	33	10			Page 33, line 10. Insert “and" before “the resultant". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-886	13	33	12			Page 33, line 12. Replace “however" by “although". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-887	13	33	14	33	22	This paragraph was hard to follow and dense. Would benefit from rewording. [Jonathan Bamber, United Kingdom]	accepted - need to set up why scaling is needed more clearly
13-888	13	33	16	17		The same model has been used to project the mass loss of the same glaciers? [European Union]	noted yes
13-889	13	33	18	33	19	Why not scale it to match the observed ~500Gt/yr discharge estimate from Rignot et al. 2011? [Aslak Grinsted, Denmark]	noted - the rignot number is likely high and indeed this is what price did (vd broeke uses rignot numbers)
13-890	13	33	24	24	35	Ditto [Jonathan Bamber, United Kingdom]	accepted this is complicated and will need to be

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							simplified (ie detail omitted) in next draft
13-891	13	33	25	33	35	Not necessary to go in to details that are so early in their development. Save for AR6 [Terrence Joyce, United States of America]	accepted - too much review and not enough assessment
13-892	13	33	26			Page 33, line 26. The sentence that starts with “they introduce" is rather vague. Can this be made more specific, please. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted reword
13-893	13	33	27	33	27	A bracket is missing. [Mirko Orlic, Croatia]	editorial
13-894	13	33	27			missing closing parentheses [Government of Brazil]	editorial
13-895	13	33	38	33	38	My understanding is that the result from Graversen et al 2011 is due mostly to changes in SMB [Mark Siddall, United Kingdom]	noted will need to check but i think this is just the calving related contribution
13-896	13	33	39	33	40	The somewhat low bias in Fürst et al. (submitted) is no longer present in the revised version of the manuscript owing to a more stringent selection of CMIP5 models and a recalibrated sliding enhancement function. The total projected contribution of increased ice discharge by 2100 is however hardly affected. [Philippe Huybrechts, Belgium]	noted await revised MS
13-897	13	33	40	33	40	low bias' - this needs brief explanation, even if just to refer to Ch4 [Mark Siddall, United Kingdom]	accepted this comes from the paper which notes the bias
13-898	13	33	42		50	This model intercomparison shall given some more space. The range and not only the mean of the projections of the ensemble shall be discussed. [European Union]	accepted should give range - not convinced that this needs much more space though the expt itself is unrealistic
13-899	13	33	42			Page 33, line 42. Nowicki et al. (submitted) is given in the reference list as a single authored paper. This needs to be checked for consistency. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-900	13	33	45	33	46	The AR5SOD estimate of 16-68 mm/century corresponds to 57-245 Gt/yr. Please compare this to present observationally based estimates of total greenland discharge. E.g. Rignot et al. GRL 2011 has ~500Gt/yr. So your discharge projection is essentially predicting a dramatic slow down over the 21st century under rcp6. Please point that out in the text. [Aslak Grinsted, Denmark]	accepted - good idea to compare to obs however will only compare to assessment of chpt not to individual papers
13-901	13	33	49	33	49	What is function of word “interestingly”? [European Union]	editorial
13-902	13	33	49	33	50	Belongs elsewhere (13.4.3.3?) [Christopher Little, United States of America]	accepted
13-903	13	33	52	33	52	"basal sliding": chapter 4 often uses the termin "lubrication", which is not used here. I would use it once so that it is clear the same effect is meant. [Olaf Eisen, Germany]	accepted
13-904	13	33	54			Page 33, line 54. Here “however"is used as a connecting word in a gramatically confused manner. The grammar improves if the sentence is broken with a full stop before “however", and that the next sentence then starts anew with “However,”, [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted
13-905	13	33	55	33	55	Provide reference to chapter 4, section 4.4.3.2.1 "Lubrication". [Olaf Eisen, Germany]	accepted
13-906	13	33	56	33	56	“shows great potential” is a subjective statement; the reason why these process based models are not used is because they are not calibrated, which in turn is due to the absence of data. [European Union]	accepted - should reword
13-907	13	33	56			insert 'a' between 'at' and 'stage' [Kathleen McInnes, Australia]	editorial
13-908	13	33	56			Page 33, line 56. Same as above and easily corrected by replacing “however" by “but". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-909	13	33	57	34	3	Given the finding that lubrication is not important (chapter 4, p.35, l55-57), the motivation for the parameterisation in models and referencing results of Bindschadler and Nowicki has to be better motivated. [Olaf Eisen, Germany]	noted while lubrication is shown in chpt 4 to be presently unimportant, this section needs to assess effect in future hence need to assess lit rather than dismiss a priori

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13-910	13	33	57			Is "have to be" a prescription for future modeling or a report of what is currently done? [Michael Oppenheimer, United States of America]	accepted the latter needs rewording
13-911	13	33	57			Page 33, line 57. When you say “these effects", please specify which effects exactly you are referring to. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-912	13	34	2	34	2	Should explain the motivation behind these numerical experiments. [European Union]	noted - not certain why this is necessary - isnt the motivation self evident?
13-913	13	34	2	34	4	What is the relation between uniformly increasing basal lsiding by factors 2, 2.5 and 3 and RCPs or other scenario's? Is threefold basal sliding an endmember calculation? [Frank Pattyn, Belgium]	noted there is not relation to scenario these are just sensitivity expts
13-914	13	34	2	34	43	ditto my comments above [Terrence Joyce, United States of America]	accepted see 891 will redraft as an assessment
13-915	13	34	2			Nowicki et al. (submitted) not included at references [Government of Brazil]	editorial
13-916	13	34	3			Again, the range of the ensemble and not only the mean shall be discussed here. [European Union]	accepted
13-917	13	34	6	34	6	“melt” -> “surface melt”? [European Union]	editorial
13-918	13	34	16			"...the work of Shannon et al. (submitted) appears to employ the more realistic forcing" -- is there a way to resolve whether it does or does not employ the more realistic forcing before this document is finalized? [Government of United States of America]	noted unclear what is required - the shannon paramaterization is based on obs the searise is simply a number
13-919	13	34	21	34	45	13.4.3.2. Text on page 13-34 uses “we” (line 21) and “our” (lines 28 and 34), a style different from the rest of the document. Many references are to papers that are only “submitted.” [James Gower, Canada]	accepted need to reword
13-920	13	34	23			Page 34, line 23. Same mis-use of “however” as above and easily corrected by replacing “however" by “but". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-921	13	34	25	34	26	This last sentence is superfluous - does not contribute to the discussion - delete [Jeff Ridley, United Kingdom]	rejected useful as context
13-922	13	34	28	34	29	Is it the forcing or the ice dynamics which limit the response? I think this question must be addressed more clearly so that readers can assess whether it is the climate models or the ice models which govern the uncertainty. [Christopher Little, United States of America]	noted most studies use A1B so that uncertainty here is due to dyanmics model not scenario - wider issue of sceanrio dependence.
13-923	13	34	33			Nowicki et al. (submitted) not included at references [Government of Brazil]	editorial
13-924	13	34	33			Page 34, line 33. When you say “this number", please specify which number exactly you are referring to (60 mm?) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-925	13	34	34	34	35	"iceberg" instead of "ice berg" (twice) [Government of Brazil]	editorial
13-926	13	34	34		42	Too many "likely range" and "very likely range" make the text ackward and difficult to read. [Government of France]	accepted - poorly worded
13-927	13	34	35	34	35	iceberg is generally one word in the literature and in Ch 4 [Jonathan Bamber, United Kingdom]	editorial
13-928	13	34	39	34	39	12 mm should be 4 mm since this is the low estimate of Nick et al combined with the generalization of Goelzer et al. Likewise, the lower limit of the likely range should be 8 mm, not 16 mm. [Philippe Huybrechts, Belgium]	accepted will recalculate
13-929	13	34	46		47	Sentence is too defensive. Try: "The projected SLR is expected to be scenario-sensitive but explicit model simulations are not available at this time." [Government of France]	accepted useful wording thanks
13-930	13	34	46			Page 34, line 46. I cannot see the reason to include “also" in this sentence. I think it can be deleted. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-931	13	34	49	34	49	The comments on meltwater lubrication miss the physical process identified by Parizek and Alley (2004, Quaternary Science Reviews) that inland migration of lakes that break through to the glacier bed in a warming world can thaw previously frozen regions and speed sliding. While not a large effect in their simulations,	rejected - this effect should be in the parameterization of shannon and does not affect the basic finding that changes in flow do not directly affect the mass budget

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						omitting it underestimates the role of lake drainages. The paper has been extensively cited, but with little progress on quantifying the effect. Some comment really should be added here or above. [Richard B. Alley, United States of America]	(only through SMB or calving coupling)
13-932	13	34	50	35	40	Important discussion on long-term effects of melting of Greenland ice sheet and dependence on future CO2 levels. Needs to be emphasised as is very important for policy makers. I.e page 35 lines 5 to 8 need to enter the SPM. [European Union]	Agreed - discussion strengthened
13-933	13	34	50	35	40	Section 13.4.3.3: This important section could benefit from the addition of a clear, short concluding paragraph. Several studies and various thresholds are discussed in the preceding paragraphs, but the reader is left unclear as to what the expert assessment of the chapter is. From the Executive summary (page 4, lines 39-41), it is clear that the chapter consensus is based on the threshold coming from the Gregory and Huybrechts study, but this is not really made clear in this section. Consider also if a confidence/likelihood language can be added to the threshold statement that is bolded in the Executive Summary. [Thomas Stocker/ WGI TSU, Switzerland]	Agreed - discusscussion strenethened
13-934	13	34	52	34	52	Section 12.6.4.4 does not exist. Wrong reference. [Olaf Eisen, Germany]	accepted. Text changed.
13-935	13	34	52	35	40	For consistency with Section 13.4.4.3, the sea level rise implied by a loss of the Greenland ice sheet should be given in this section. [Government of United States of America]	accepted. Text changed.
13-936	13	34	56			No need to specify "a multi-millenial period" since the time span is spelled out in the next line. DELETE. [Government of France]	rejected. It is important to emphasize that the warming needs to be sustained for several millennia.
13-937	13	34	56			"greater than" what? [Michael Oppenheimer, United States of America]	accepted. Text changed.
13-938	13	35	5		7	This important sentence should be made easier to read. E.g. try "Gregory and… estimated the overall SMB over Greenland would become negative above a global mean warming of 3.1°C [ ] above pre-industrial temperture." Or make two shorter sentences. [Government of France]	accepted. Text has been added to explain the two different approaches in estimating the threshold.
13-939	13	35	6			Page 35, line 6. Insert a comma after “negative". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-940	13	35	23	35	24	Please rephrase this sentence. It is not clear, what is meant. [Government of Germany]	accepted. The explanation has been rephrased.
13-941	13	35	23			There is no argument given here why the conclusion of Robinson et al might be wrong, that the ice sheet becomes unstable well before the point of overall negative SMB is reached. As an assessment, I think the chapter should actually provide a conclusion here and not just report side by side without comment the Robinson result and the assumption of zero SMB used in the other papers. In my view, it should conclude that Robinson et al. are probably right, and that the previous estimates of the critical threshold are therefore overestimates, or should be treated as upper limits. At the very least the Robinson et al result should be treated as equally valid as the others and included in the uncertainty range that makes it into the SPM. [Stefan Rahmstorf, Germany]	accepted. The text was changed, discussing that there are two criteria according to which the threshold is determined. The one requiring the total SMB of Greenland to become negative has been investigated in more studies and partially with more complex models, while the other used by Robinson but also other studies accounts for ice motion and is therefore more elaborated. All available studies are consistent with the range of 0.8-3.2K given by Robinson et al but not with their best estimate. Due to the limited confidence that can be derived from just one model study with one atmospheric representation the range is provided but no best estimate.
13-942	13	35	30	35	40	In this context, Matsuno et al. (2012a, b) should be cited. Matsuno, T., K. Maruyama, and J. Tsutsui, 2012a: Stabilization of atmospheric carbon dioxide via zero emissions - an alternative way to a stable global environment. Part 1: Examination of the traditional stabilization concept, Proc. Jpn. Acad., Ser. B, 88, 368-384. https://www.jstage.jst.go.jp/article/pjab/88/7/88_PJA8807B-05/_article Matsuno, T., K. Maruyama, and J. Tsutsui, 2012b: Stabilization of atmospheric carbon dioxide via zero emissions - an alternative way to a stable global environment. Part 2: A practical zero-emissions scenario,	rejected. The discussion of specific temporal emission pathways is beyond the scope of this section which is dealing specifically with the reponse of the Greenland ice sheet. Perhaps the papers are more relevant for WG-3 or other chapters of WG-1.

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						Proc. Jpn. Acad., Ser. B, 88, 385-395. https://www.jstage.jst.go.jp/article/pjab/88/7/88_PJA8807B-06/_article [Junichi Tsutsui, Japan]	
13-943	13	35	30	35	40	(continued from the previous row) This two-part paper clarifies the concept of stabilization of the atmospheric CO2 concentration and proposes a new "zero-emissions stabilization" concept, which allows relatively large CO2 emissions in the near future and yet avoids long-term (centuries to millennial) risks in the climate system, in particular, sea level rise. Here "zero emissions" mean emissions sufficiently below the natural uptake level, and the authors suppose an emissions pathway that peaks around the year 2020 and declines to the zero level in the middle of the next century. In such a pathway, the CO2 concentration and temperature rise decrease on a long time scale to approach their equilibrated stable states. These are contrastingly different from climate changes supposed under the traditional stabilization concept, which implies misunderstanding "inevitable" higher levels of the CO2 concentration and temperature rise. [Junichi Tsutsui, Japan]	rejected. The discussion of specific temporal emission pathways is beyond the scope of this section which is dealing specifically with the reponse of the Greenland ice sheet. Perhaps the papers are more relevant for WG-3 or other chapters of WG-1.
13-944	13	35	31	35	40	regrow' this is contradicted by the discussion of Ridley et al 2010. You may want to note that simulations and data from the LIG support the idea of multiple stable states for the GLIS [Mark Siddall, United Kingdom]	accepted. The paper by Ridley is discussed in the next sentence and it is noted in the summary sentence of the section that the specific conditions under which the loss is inevitable cannot be determined to high precision based on currently available information.
13-945	13	35	35	35	35	GtC should be deinfed here or in the glossary [Government of United States of America]	accepted.
13-946	13	35	42			I won't repeat the comments of the type I made for Greenland here, but consider them to apply here as well: don't present lots of discussion that hinges on submitted papers, especially in areas of recent development. Save for a future AR6. [Terrence Joyce, United States of America]	Rejected. Accourding to the IPCC rule, only accepted papers will be used in the final version.
13-947	13	35	46	42	27	13.4.4.1, 13.4.4.2, 13.4.4.3. These sections use far too many acronyms. Abbreviation of names (PIG, TG, WAIS) and mechanisms (MISI) saves very little room and makes the document much harder to read. [James Gower, Canada]	Rejected as regards 13.4.4.1, which uses none of these abbreviations. I cannot speak for 13.4.4.2-3.
13-948	13	35	54	35	54	Should not the unit be %°C^-1? [Government of Germany]	accepted. % added.
13-949	13	35	54	35	54	5.1 ± 1.5°C-1' should be : '5.1 ± 1.5°%C-1' [Philippe Huybrechts, Belgium]	accepted. % added.
13-950	13	35	54			units are right? It should be % / °C. [Government of Brazil]	accepted. % added.
13-951	13	36	4	36	5	I am surprised because Bengtsson et al. (2011) gets 3.7% per degC whereas Krinner et al. (2007) gets ~7% per degC. This difference is the same order of magnitude as the change itself. But according to this statement, then circulation differences can be ruled out as the source of the difference. [Aslak Grinsted, Denmark]	Agreed
13-952	13	36	16	36	46	This is a long, difficult paragraph…at least break in two for readability; it would be better to rethink an rewrite it. [Government of United States of America]	accepted. Text revised.
13-953	13	36	17	36	17	“done” -> “currently done” [European Union]	accepted. Text changed.
13-954	13	36	17	36	17	Reverse the order of 'ice sheet' and 'climate'. It is the climate model that is of intermediate complexity, not the ice-sheet model. [Philippe Huybrechts, Belgium]	accepted. Text changed even further to avoid this misunderstanding.
13-955	13	36	19			Page 36, line 19. When you say CO2equ, this deserves a little bit of explanation when it appears for the 1st time. This may be explained elsewhere, but for the general readability it is important to explain it briefly here again. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. The term CO2equ has been eliminated from the text, since it is simply the CO2 concentration in the model that is changed. The concept of CO2-equivalent in contradistinction to other greenhouse gases is not relevant here.
13-956	13	36	23	36	25	"mass gain in the equivalent of xxm of sea level fall …. negative sea level contribution" -- it might be confusing to the reader when you combine positive / negative mass changes / sea level changes in some of the paragraphs. We suggest to carefully review such cases and revise them to avoid ambiguities. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.

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13-957	13	36	25	36	27	"Huybrechts et al. (2011)" instead of "huybrechts et al." (twice) [Government of Brazil]	accepted. Text changed.
13-958	13	36	27			Page 36, line 27. Insert “a" before “long-term". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-959	13	36	33	36	36	The reference to Goelzer is not in the reference list. The one in the reference list is about Greenland and not about Antarctic modelling. [Frank Pattyn, Belgium]	accepted. Text changed.
13-960	13	36	33			Page 36, line 33. Please explain SRES when used for the 1st time. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-961	13	36	35	36	36	I'm missing a judgement about whether these models can capably represent basal melting and the resulting loss of grounded ice. My personal take is no. I think the reliability of multi-century simulations of Antarctica that project an SMB dominated response deserves discussion (see also my comment on ES) [Christopher Little, United States of America]	partially accepted. This text is provided in the 13.4.4.2 "Dynamical changes" section and will be further elaborated in section 13.5.2 for the coarse resolution coupled models. Text on this has also been added in section 13.4.4.2.
13-962	13	36	41	36	41	summary of explanation of negative feedback required here (similar to the earlier AMOC explanation for Greenland) "Negative feedback here stems from the formation of a cold halocline in the Southern Ocean, which limits sea-ice cover retreat under global warming and increases surface albedo, reducing local surface warming." [Jeff Ridley, United Kingdom]	rejected. As explained above the confidence in the coupled models ability to properly capture these processes are low.
13-963	13	36	46	36	46	Please add a reference for the E1 scenario. [Government of Germany]	accepted. Text changed.
13-964	13	36	48	36	48	“overcompensates for” -> “exceeds”. [European Union]	accepted
13-965	13	36	48		49	… ice sheet-climate simulations show consistently that surface melt overcompensates for increased snowfall, thus yielding a net ice-loss for higher GHG… [Government of France]	reworded
13-966	13	36	50			… "over which the AIS continues to loose mass indicates the importance of the climate-ice sheet feedback." [Government of France]	reworded
13-967	13	36	50			define 'AIS' on p35 line 56 or wherever 1st used [Kathleen McInnes, Australia]	accepted
13-968	13	36	53	36	53	“that” -> “then”. [European Union]	editorial
13-969	13	36	55	36	55	change “elevation …. loss" -> “elevation leads to increased surface temperature and further ice loss”. (The change in elevation does not directly cause surface temperature increase). [European Union]	accepted
13-970	13	36	55	36	56	I disagree with the last sentence: it is not necessarily the temperature-elevation feedback that inhibits a positive SL contribution from Antarctic SMB; the general warming has a more important effect than the feedback with elevation. [Philippe Huybrechts, Belgium]	True, but the feedback locks in the change
13-971	13	37	1	40	53	The discussion on dynamical change from Antarctica rests on much weaker foundations than Greenland. The section is long and detailed but the impression remains that it is not yet possible to quantify the likely range. Except for the Payne et al. study there seem to be no model studies using more or less realistic forcing; arguments are largely based on scaling arguments and ad-hoc assumptions. From my own modeling I believe that the likely range is too high but there may well be too little material available for a better assessment. The chapter should be clear what a range of 33-132 mm implies in terms of average increased discharge over the 21st century (132 mm must be close to a sustained average increase of 30% of outflow all around Antarctica which seems a lot in view of current observations). [Philippe Huybrechts, Belgium]	noted although it would be a mistake to ignore studies that are not entirely process model
13-972	13	37	3	37	4	Clarify that the marine sectors of West Antarctica can raise sea level 3.3 m. Additional ice on the higher grounds is not expected to contribute, but is there. [Richard B. Alley, United States of America]	accepted will use chapt 4 numbers
13-973	13	37	3	37	4	This is incorrect. The WAIS has the potential to raise SL by 5 m in total. The marine, "unstable" sectors of the WAIS have the potential to rasie SL by 3.3 m [Jonathan Bamber, United Kingdom]	accepted will use chapt 4 numbers
13-974	13	37	4	37	4	The West Antarctic ice sheet has not THE potential to raise SL by 3.3m (Bamber et al). The potential should be more, because in this model only a particular aspect has been examined (downward sloping bedrock) as a	accepted will use chapt 4 numbers

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						criterion of ice loss, without any feedback mechanism related to mass balance, buttressing, etc. It is a non-trivial scenario based on shifting grounding line position which is not even in full accord with theory (Schoof, 2007). [Frank Pattyn, Belgium]	
13-975	13	37	6	37	6	I suggest replacing "some of which" with "most of which". [Hilkka Pellikka, Finland]	editorial
13-976	13	37	14			Box 13.2 should appear earlier than page 41. [Government of United States of America]	editorial
13-977	13	37	15	37	18	within two sentences you seem to say that you will not and will estimate SLR beyond 2100. [Michael Oppenheimer, United States of America]	accepted needs rewording
13-978	13	37	21			The word "extrapolation" is misleading for two reasons: first, the Little et al method draws in part on process-based models to make its probabilistic assessment. Secondly, it's approach is entirely distinct from semi-empirical methods, which truly depend on extrapolation. "probabilistic estimation" or "probabilistic assessment" would be more accurate. [Michael Oppenheimer, United States of America]	accepted
13-979	13	37	33			Page 37, line 33. Change “the collapse" to “a collapse". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-980	13	37	37	37	44	In the cited Little et al. paper, in addition to providing a consistent probabilistic framework for embedding kinematic scenarios, we use an observational constraint and a PIG model, and we attempt to account for ice shelf breakup in a highly abstracted way. The method is not "process-based", but it does attempt to account for the physics discussed later in this section (some of which is not included in process based approaches), and assimilates the results of the Joughin et al (2010) analysis. I thus suggest that this paragraph be moved later -- next to and contrasting with the Ritz submitted paper (p 40 line 2) -- so the the discussion of other models (used in PIG only), kinematics, and the uncertainty associated with collapse can be referenced as being included in the approach. [Christopher Little, United States of America]	noted - not a bad idea and move to very likely range for ritz/levermann may well make easier to consider all numbers togther
13-981	13	37	37	37	44	The Little et al numbers have been updated per a new observational constraint (Shepherd et al submitted). The 5/17/50/83/95 percentile values for the Antarctic SLR contribution (with SMB uncertainty) are as follows: -8.6/-4.0/2.4/8.8/13.3 (all in cm). Without SMB uncertainty (i.e. no change in SMB over time), the values are: -3.4/0.9/6.8/12.5/16.7. I have sent the newest version to the CLA's. [Christopher Little, United States of America]	accepted will use accepted MS
13-982	13	37	38	37	39	Little et al incoporate process-based model output as well as observations. Description of observations used should be revised - refer to final version of the manuscript for this, to be provided. [Michael Oppenheimer, United States of America]	accepted will use accepted MS
13-983	13	37	39		43	"Results are then adjusted to match the observed mass-balance of Antarctica (...) yielding an upper bound ~ 170 mm for net SLR. This is equivalent to a contribution ~ 180 to 240mm from dynamics alone, taking into account the likely impact of SMB change (- 10 to - 70mm)." [Government of France]	editorial
13-984	13	37	44			Page 37, line 44. Change “the collapse" to “a collapse". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-985	13	37	46	37	53	What about fracturing and/or breakup driven by melting from below (Vaughan et al JGR 2012, Gladish 2012 J Glaciology, Rignot/Steffen 2008 GRL) [Christopher Little, United States of America]	noted while this is true i do not think that status of this work appraches the widely accepted story developed for larsen b and surface melt
13-986	13	37	51			Page 37, line 51. Change “the increased" to “an increased". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-987	13	37	55			Page 37, line 55. Remove “the" before “fracture-based”. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-988	13	37	57	37	57	A bracket is superfluous. [Mirko Orlic, Croatia]	editorial
13-989	13	37	57			Page 37, line 57. Change “the acceleration" to “accelerations". [Eelco Johan Rohling, United Kingdom of Great	editorial

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						Britain & Northern Ireland]	
13-990	13	37		40		The Section 13.4.4.2 is rather heavy in accounting the numerous papers. Perhaps some contraction/selection is feasible? [Government of France]	accepted - extensively revised
13-991	13	38	2			Do you mean B1 instead of E1? [Kathleen McInnes, Australia]	noted no E1 is a SRES scenario
13-992	13	38	2			E1 scenario -- as this is not a very commonly referred to scenario in the IPCC context, we suggest to refer to Section 12.3.1.4 where it is mentioned along with other SRES scenarios. [Thomas Stocker/ WGI TSU, Switzerland]	accepted
13-993	13	38	16	38	16	I feel like there's an assessment missing here of whether or not models can represent the two cited means for climate to influence ice sheets (surface melt driven collapse and basal melt buttressing reductions). There is far more discussion of whether or not the grounding line responds correctly than the implications of whether (both climate and coupled) models can represent these effects. If not, what can they do to bound them so that we believe they don't affect the likely range? [Christopher Little, United States of America]	accepted very good point - should indicate what the effect of GL modelling error is on these w studies. For levermann the hope is that using pollard and other model brackets the range, for ritz we indicate that must be biased high because assuming GL will retreat on inclined bed
13-994	13	38	17	38	25	This para in redundant with lines 44-51 of page 53 of Chapter 12. [Thierry Fichefet, Belgium]	noted - Yin is of relevance here will need to check consient use with 12
13-995	13	38	27	38	28	Again, is it the forcing or the dynamics which truly govern the uncertainty? It is not so clear to me as it is stated here. [Christopher Little, United States of America]	accepted need to make it clear we are talking about dynmaics only. Relates to scenario issue
13-996	13	38	33	38	33	The reference should be Pattyn (2012) and not Pattyn (submitted) [Pattyn et al. (2012) Results of the Marine Ice Sheet Model Intercomparison Project, MISMIP. The Cryosphere 6: 573-588] [Frank Pattyn, Belgium]	accepted
13-997	13	38	41	38	41	Note the very primitive ocean model used in these experiments [Christopher Little, United States of America]	noted will need to check this is not my impression of the hellmer work
13-998	13	38	45	38	45	Have you used the word “channels” before? I think more conventional “shelf lateral margins” is more appropriate. [European Union]	accepted better wording
13-999	13	38	50	38	53	Parizek et al, might qualify the "not in the next century" to "not projected in the next century" to avoid the implication of an impossibility. [Richard B. Alley, United States of America]	accepted
13-1000	13	38	51	38	53	This discussion, raising as it does the prospect of catastrophic retreat, calls for a bit more detail as to how much ice would be involved, when, etc. [Michael Oppenheimer, United States of America]	noted it would be a good idea BUT very hard to use flowline models to get SLR estimates (comments on use of Gladstone model results in FOD)
13-1001	13	38	55	38	55	Remove "subtle". For GL migration, two boundary conditions are need and one of them is floatation. The other is to evaluate longitudinal (membrane) stresses at both sides of the grounding line (Schoof, 2007). [Frank Pattyn, Belgium]	accepted
13-1002	13	38	57	39	52	This whole discussion misses the assumptions made about ice shelf behaviour changes in all the different models; in my view this is the key to understanding the differences between the models. If you agree, expand the discussion to include this; if you disagree, explain why. [European Union]	noted not really more to do with assumption about ice rheology and basal traction. The final sentence hints at this already. Exaplin why ice shlves not important.
13-1003	13	39	1	39	9	The paragraph is too speculative for this document. [Government of United States of America]	rejected this is based on accepted papers would be very problematic to ignore this effect
13-1004	13	39	7	39	8	or how grounding lines are treated in numerical ice sheet models [Frank Pattyn, Belgium]	accepted willadd
13-1005	13	39	18	39	18	“success” to a construction involving the word “comparability”? [European Union]	accpted need to make this clearer
13-1006	13	39	29	39	29	“Fully” -> “Entirely” – “fully” might be read as including all processes. [European Union]	accepted reword
13-1007	13	39	29	39	31	A little more detail is needed: explain to what degree the Payne et al model is "fully processed-based" compared to other models so the reader can make a judgment as to reliability. [Michael Oppenheimer, United	accepted reword

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						States of America]	
13-1008	13	39	38	39	40	Something is missing in the sentence. [Mirko Orlic, Croatia]	accepted need to remove 'that'
13-1009	13	39	49	39	49	There is no such thing as a fully process based model. This is a false dichotemy that is explained elswhere in the Ch (see P10, l25-30) which explains quite effectively why empiricism is empolyed in all numerical models of the Earth System. But it should be stressed somewhere that there are many processes which are not explicitly included in "process based models" which may or may not be important. Even the most sophsticated EBMs include empirical parameterizations etc. etc. [Jonathan Bamber, United Kingdom]	accepted reword - meant to say that GL is modelled without paramtereization
13-1010	13	39	49	39	54	Since the Ritz et al and Little et al approaches bear certain resemblences and also some key differences, perhaps these should be noted in this paragraph. In any event, the discussion here and estimates which follow should refer back to the Little et al discussion on p. 37. [Michael Oppenheimer, United States of America]	accepted - extensively revised - Ritz paper not yet published
13-1011	13	39	53	39	53	Although I didn't have access to the supplemental material which contains the majority of the details of the retreat rate and/or climate forcing parameterization used in the Ritz et al paper, the "high bias" must also take into account whether these sufficiently capture the range of possible responses. [Christopher Little, United States of America]	noted the high bias comes from the fact the GL is assumed to retreat on inclined bed and ignores possibility that GL will stabilise
13-1012	13	39	55	39	55	Should this be 83rd percentile if it is used to denote the top of the likely range? [Christopher Little, United States of America]	accepted will be redoing this and moving to very likley range from these models
13-1013	13	39		40		It is very difficult to match up the values from the Ritz et al paper quoted in the Chapter to the version of the paper which I was sent by the TSU [Christopher Little, United States of America]	accepted see 1012
13-1014	13	40	2	40	2	This tail broadening can be seen in Little et al as well if a likelihood of collapse is specified -- it is arguable whether the collapse associated with a specified rate of grounding line retreat is any more physical than an expertly-assigned likelihood of increased flux (see Bob Thomas comments on the FOD). [Christopher Little, United States of America]	noted the GL retreat rate has been selected to cover reasonable bounds so should cover likelihood range. The advantage is that only have to sample GL retreat rate and model works out masses involved (so there is some idea of potential reservoirs involve) which is not true of assigning flux
13-1015	13	40	4	40	21	Are these models able to be rationalized with the more detailed regional models in p 39, lines 38-39? i.e. where does the ice loss come from (PIG/THW)? If not, why? Do we believe one more than the other? The point I'm getting at is that the models should be able to match up at a regional level, rather than simply the total ice loss. [Christopher Little, United States of America]	noted very hard to get SLR out of flowline models. The ritz and levermann include cases with loss of PIG and TG - we should point this out
13-1016	13	40	4	40	21	The Little et al numbers have been updated per a new observational constraint (Shepherd et al submitted). The 5/17/50/83/95 percentile values for the Antarctic SLR contribution (with SMB uncertainty) are as follows: -8.6/-4.0/2.4/8.8/13.3 (all in cm). Without SMB uncertainty (i.e. no change in SMB over time), the values are: -3.4/0.9/6.8/12.5/16.7. I have sent the newest version to the CLA's. [Christopher Little, United States of America]	noted will revise in next draft
13-1017	13	40	4	40	21	This is a key paragraph but it is currently written in a confusing manner. Why is Little et al not mentioned on line 4 as a third study? Each of these three studies projects 21st century sea level rise, and each has strengths and weaknesses. If Ritz and Leverman are singled out for some reason, the rationale ought to be made clear. Is it their use of emissions scenarios perhaps? Is it because they explicitly offer a "likely" as opposed to a "very likely" range (a "likely" range also can be provided by Little et al). Was some expert judgment invoked as to which studies are to be preferred (if so, explain the judgment). One of the problems in trying to understand this paragraph is that it is difficult to ascertain whether "likely" and "very likely" are judgments by the respective studies cited, by Chapter 13 team, or both; and if by the cited authors, did they use the same criteria as IPCC? Perhaps chapter assessments of likelihood should be italicized. Also, the Payne et al study is at various times suggested to be both consistent and inconsistent with Ritz et al and Leverman at al. [Michael Oppenheimer, United States of America]	accepted will be redoing this assessment at very likley range. Certainly scope for moving little into this paragraph. See 993
13-1018	13	40	4			Page 40, line 4. Change “principle” to “principal". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial

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13-1019	13	40	11	40	13	This is not consistent with the very likely range given in Levermann et al. 2012 where he says as the final sentence: "The 90 %-range which is denoted by the IPCC as the “very likely”-range reaches up to 0.45 m for all models including the time-delay and even up to 0.53 m without time delay." [Aslak Grinsted, Denmark]	accepted see 1010
13-1020	13	40	11	40	13	Make sure that these numbers correpond to the likelihoods in the papers. Projections are highly sensitive to the tails and the choice of this upper bound (Little et al., submitted to PNAS, also the Ritz et al submitted manuscript). [Christopher Little, United States of America]	accepted - will check these numbers agreed that they are very sensitive to details which is why we are forced to consider only likley range
13-1021	13	40	14	40	16	All this is true, and implies that we simply have no idea as to what this ice will do. [Robert Thomas, United States of America]	noted but there is now a literature that must be assessed saying that we have some idea (not necessarily with complete confidence but better than no confidence)
13-1022	13	40	17	40	17	It would be nice if you explicitly stated whether Payne results in greater or smaller numbers. Please specify. [Aslak Grinsted, Denmark]	acepted need to improve clarity
13-1023	13	40	23			Page 40, line 23. This 1st sentence is clunky. I suggest rephrasing. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1024	13	40	29			Page 40, line 29. Remove “the" before “fracture-based". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1025	13	40	30			Page 40, line 30. Change “scenario" to “scenarios". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1026	13	40	30			Page 40, line 30. Change “intrusions" to “intrusion". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1027	13	40	32	40	33	The RCP8.5 estimates from Levermann et al. (submitted-b), Binschadler et al. (submitted); and Nick (submitted) papers are incompatible with the ranges you give. Your ice dynamical uncertainties are simply too optimisically tight and biased low. [Aslak Grinsted, Denmark]	noted - this comment links to scenario issue and use of likley (as oppsed to very likely) range. Both these decision have been discussed at length and decsions support by chapter team.
13-1028	13	40	32	40	33	It is still simply more correct to use the scenario dependent estimates even if there still are important model deficiencies. Applying your RCP6 projection to RCP2.6 and RCP8.5 does not solve this problem! Making it scenario independent has serious drawbacks: * It communicates that policy does not matter as much. * It looks as if there has been no progress since AR4 (and there clearly has been). [Aslak Grinsted, Denmark]	noted - this is a difficult story to sell but continued discussion with chpater authors supports the decision to keep dyanmics independent of scenario. Needs more work to emphasis why this is done. Certainly untrue to say no progress from AR4 (eg we now have a likley range)
13-1029	13	40	35	40	39	Is this SMB compensation effect as robust as the other number it's added to? Earlier in the chapter it's effect was judged to be less than 15%. And if it is, is a 42mm SMB compensation internally consistent with the SMB that is added to the projections? i.e. are we subtracting more mass than is being added? [Christopher Little, United States of America]	noted - we are taking the upper fraction from winkelmann not 15%. Not clear where 15% comes from. Discussion of this issue revamped
13-1030	13	40	39			It is entirely unclear why only the "likely" range is developed from the preceding estimates (Ritz al, Leverman et al) when Ritz and Leverman as well as Little all present the "very likely" cases according to the earlier paragraph, lines 4-21. This is again a very important aspect of the chapter but the rationale is obscure. Please make it transparent. IF the chapter authors made some expert judgments in the process, that is fine but you need to say so and provide an account of the basis for those judgments insofar as is possible. [Michael Oppenheimer, United States of America]	accepted will be revising numbers to give very likley range
13-1031	13	40	41	40	47	Please include some numbers to give the reader an idea of how long tails the distributions have. E.g. You could quote the levermann et al (Submitted-b) very likely ranges. [Aslak Grinsted, Denmark]	accepted will be revising numbers to give very likley range
13-1032	13	40	41			Page 40, line 41. Change “however" to “but". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1033	13	40	49	40	50	Using "very Likely" and "likely" in the same sentence is inconsistent with IPCC uncertainty language (and	accepted will edit

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						probably with nornmal logic, too) and is utterly confusing. [Michael Oppenheimer, United States of America]	
13-1034	13	40	50	40	51	It would still be good to have a very likely upper bound even if there is low confidence in that number. [Aslak Grinsted, Denmark]	noted - ongoing discussion about this
13-1035	13	40	50			Page 40, line 50. Change “very much larger" to “much larger". There is no need here for superlatives. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial
13-1036	13	40				I fully agree with the authors that the relation between SLR by ice sheet dynamics due to emission scenarios is very limited and taken into account that the model intercomparison discussed above showed the limited ability of the models of both publications cited, I cannot understand, why the details of these simulations are discussed over more than two pages. The summary would be that there low agreement and limited robustness. The state of the art in projecting mass loss of the ice sheets by dynamics could be summarized much shorter and the space rather used to explain the requirements for models to be able to give robust results. [European Union]	accepted - need to sharpen text into an assessment
13-1037	13	41	1	41	1	Section 12.6.4.4 does not exist. Wrong reference. [Olaf Eisen, Germany]	accepted. Text changed.
13-1038	13	41	2	41	2	Wrong Weertman reference (it is 1974). [European Union]	accepted. Text changed.
13-1039	13	41	3	41	4	Please rephrase, because it is unclear what "ice" is meant here. [Frank Pattyn, Belgium]	accepted. Text changed.
13-1040	13	41	10	41	10	local, global mean temperature? [Mark Siddall, United Kingdom]	accepted. Text changed.
13-1041	13	41	10	41	12	Both these statements require citations. [Jonathan Bamber, United Kingdom]	accepted. Text changed.
13-1042	13	41	10	41	12	I suppose, these two sentences should be included in the previous paragraph. [Andrey Ganopolski, Germany]	accepted. The sentences are put into the appropriate context within this and the previous paragraph.
13-1043	13	41	10	41	16	These two paragraphs seem out of place in this section and the first is not well-explained. Context and citations lacking. What studies are these from? [Michael Oppenheimer, United States of America]	accepted. The sentences are put into the appropriate context within this and the previous paragraph.
13-1044	13	41	11			Page 41, line 11. When you say “these simulations", please repeat the relevant reference for clarity. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1045	13	41	13	42		Box 13.2 is a very good addition, as is FAQ 13.2 [Michael Oppenheimer, United States of America]	accepted.
13-1046	13	41	14	41	16	see Pierce EL, Williams T, van de Flierdt T, et al, Characterizing the sediment provenance of East Antarctica's weak underbelly: The Aurora and Wilkes sub-glacial basins, PALEOCEANOGRAPHY, 2011, Vol:26, ISSN:0883-8305 [Mark Siddall, United Kingdom]	accepted. Text changed.
13-1047	13	41	14			Is it SLE or SLR, to be consistent with other text? [Government of United States of America]	accepted. It is SLR. Text changed.
13-1048	13	41	20	42	27	It is unclear whether Box 13.2. is an account of observed phenomena (observed by ?) or a theoretical concept of the putative MISI mechanism from current observations or past events. The writing-style makes it even difficult to embrace. [Government of France]	noted not clear what is being requested here. The box attempts to combine theory and observations
13-1049	13	41	20	42	27	This section is well-written, with summary statements about future challenges, but not exhaustive discussion about how it is being addressed. [Terrence Joyce, United States of America]	noted - this can not be an exhaustive discussion. Could add paragraph about future outlook although this will be vague?
13-1050	13	41	22	41	41	This paragraph provides a useful overview of ice sheets and grounding lines, plus the factors which can impact upon flow. However, it unnecessarily repeats information about grounding lines and flow that is imparted in earlier paragraphs of 13.4.4.2. [Government of United Kingdom of Great Britain & Northern Ireland]	noted some minor repetition but difficult to avoid as needed in 13.4.4.2 and would look odd to remove material here and use pointer to main text
13-1051	13	41	31	41	33	It seems that 'and' is missing before 'the ice outflow'. [Mirko Orlic, Croatia]	editorial
13-1052	13	41	31			Page 41, line 31. Following “Accordingly," please insert “when" or “because" to make the sentence work. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	editorial

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13-1053	13	41	47		48	Try: "These findings highlighted the importance of better understanding MISI for projecting future ice sheet contributions to SLR". [Government of France]	editorial
13-1054	13	41	51		56	Try: "Early MISI studies were not based on a formal understanding of ice-sheet dynamics and the robustness of the results could not be assessed. An open question was the expected impact of changes at the grounding line (GL) on ice-sheet flow (Hindmarsh …). A more complete analysis (…) recently indicated a power law (exponent ~ 5) betweek ice thickness and the ice flux at the GL" [Government of France]	editorial
13-1055	13	41	55	41	55	“the all-important” -> “a fundamental” [European Union]	editorial
13-1056	13	41	56	41	56	“flow” -> “ice flux” – “flow” is ambiguous [European Union]	accepted
13-1057	13	41	56	41	56	the 61% is overly accurate based on the use of the approximate index of 5 – say around 60%. [European Union]	accepted
13-1058	13	41	56			This sentence is unclear; suggest that it be rewritten. [Government of United States of America]	noted but this reads failrly well assuming understand what powers are
13-1059	13	42	7			Already discussed before. [European Union]	unclear what is being refered to here
13-1060	13	42	12			FAQ 13.2: Figure 1: We unfortunately do not find this a compelling figure, and it seems rather focussed on a very specific aspect of this FAQ. Especially in relation to Greenland, the relevance of the information in this figure is not clear from the text as it gets only a passing mention. It seems crucial that this FAQ should rather provide a figure or table quantifying the contribution of Greenland and Antarctica ice sheets to projected sea level rise. How will this contribution compare to contributions from other sources? This quantified information is lacking from the FAQ and would be expected by the reader. [Thomas Stocker/ WGI TSU, Switzerland]	noted - need to discuss with TSU what they think is required here. Note issues related to atand alone nature of FAQ and FOD figure that was similar to what is being requested but attracted criticism and was omitted in favour of current
13-1061	13	42	14	42	16	The values reported by Rignot et al., 2004, are much higher than the ones reported by Rott et al., The Cryosphere, 5, 125–134, 2011. The reference to Rott et al. has to be included here as well and the results adequtely incorporated. [Olaf Eisen, Germany]	accepted
13-1062	13	42	20	42	27	very nice to see this appraisal of future progress [Mark Siddall, United Kingdom]	noted
13-1063	13	42	20	42	27	I am not sure how "sound" the footing is, but the "steady-state" bit is true. We are certainly a long, long way from being able to model possible acceleration of glaciers draining marine ice sheets as ice shelves thin or break up. [Robert Thomas, United States of America]	rejected - this comments ignores several recent papers cited in the main body
13-1064	13	42	20		27	The last paragraph of Box 13.2 is an essay on the desirability of robust models and future expectations. Does not belong in an assessment of current scientific knowledge. Delete [Government of France]	noted but 1049 requests just such an outlook
13-1065	13	42	22			"There are major challenges in designing models whose results are not controlled by the details of their numerical design." -- this is a vague and troubling statement. Since no clarification or examples are given, it tends to cast doubt on the entire ice-sheet modeling enterprise. This might be a good location for one thing that is missing from the rest of the section -- a discussion/mention of or reference to the different flow approximations in use by the modeling community (SIA, SSA, L1L2, Blatter-Pattyn, full-Stokes) and their effects on the results. For example, the Pattyn et al (submitted) intercomparison also suggests that SSA and full-Stokes give noticeably different steady-state grounding-line positions.Perhaps this is what is being obliquely referenced by "more complete modelling of the GL stress regime" a few lines later in the paragraph. [Government of United States of America]	accepted - the following 2 sentences were intended to lead on from these and explain the assertion. Needs to be made clearer. Will add reference to MISMIP
13-1066	13	42	25	42	26	The reference Morlighem et al., 2010 is not adapted for this point as there are not solving for the GL dynamics in this paper. Regarding the study of the state of stress at the GL and its impact on the GL dynamics, Favier et al., TC 2012 or Gudmundsson et al., TC 2012 are more pertinent. [Olivier Gagliardini, France]	accepted
13-1067	13	42	30	42	30	Please give more information in the caption on what the different panels show. [Government of Germany]	accepted caption should have more detail
13-1068	13	42	34	44	16	FAQ 13.2 (Contribution of ice-sheets to sea level change): Would the lead Authors be comfortable with making this FAQ a little more quantitative by adding best estimates and uncertainty ranges for both current and future contributions of Greenland and Antarctic ice sheets to global sea level change ? [David Wratt, New Zealand]	noted not really as hard to see how this could be done and keep FAQ as stand alone

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13-1069	13	42	35	42	35	The Radic and Hock estimate is probably too high. Compare to more recent estimates from Grinsted 2012 in the cryosphere discussions, Huss and Farinotti 2012. Synchronize this estimate with the Cryosphere chapter. [Aslak Grinsted, Denmark]	Noted. This is the reviewer's opinion; the case may also be that the Huss and Farinotti estimate is too low. In any case, both estimates will be discussed and the issue will be coordinated with Chap 4. (note that comment is misplaced)
13-1070	13	42	36	42	46	Why include this FAQ? The reply says very little. [Robert Thomas, United States of America]	noted this FAQ was requested by IPCC leadership
13-1071	13	42	36			FAQ 13.2: It seems crucial that this FAQ should provide either a figure or table quantifying the contribution of Greenland and Antarctica ice sheets to projected sea level rise. How will this contribution compare to contributions from other sources? This quantified information is lacking from the FAQ and would be expected by the reader. [Thomas Stocker/ WGI TSU, Switzerland]	noted see 1068
13-1072	13	42	39	42	40	"As such, they have contributed to sea level..." . Here the reader would expect to see some numbers coming out of the underlying chapter assessment - how much have they contributed over geological and recent times? [Thomas Stocker/ WGI TSU, Switzerland]	noted see 1068
13-1073	13	42	42	42	42	Replace "boundary" with "boundaries". [Francis Zwiers, Canada]	editorial
13-1074	13	42	45	42	46	"...so that a continuing positive contribution to global sea level can be expected'. Here again the reader would expect to see the latest numbers coming out of the chapter assessment. How large will this positive contribution from the ice sheets be? how does the size of this contribution compare to the contribution from other sources? [Thomas Stocker/ WGI TSU, Switzerland]	noted see 1068
13-1075	13	42	50	42	51	It looks as though you are drawing a distinction between calving in Greenland and flow into ice shelves in Antarctica which I suspect you aren't really. [European Union]	noted - not trying to create disinction and i think next sentence "in turn .." clarifies this
13-1076	13	42	52	42	52	Suggest replacing "submarine melt" with "melting from below". [Francis Zwiers, Canada]	noted - submarine melt is more informative but does sound awkward
13-1077	13	42	52			Last paragraph: The whole discussion about the SMB of the Antarctic and Greenland ice sheet shall repeatedly remark, that the recent mass loss of the Antarctic ice sheet is driven by changes in ice discharge. As long as changes in the ice discharge cannot be estimated by process-based models, an assessment of the SMB in terms of contribution of the entire ice sheet to SLR is not possible. [European Union]	noted - not certain what is required here
13-1078	13	42	56	42	57	Throughout, it would be helpful to use lay reader friendly language as much as possible. For example, replace "satellite radar altimetry" with "satellite measurements of surface height", and replace satellite gravimetric observations" with "satellite measurements of gravity changes". [Francis Zwiers, Canada]	accepted although should ideally be consistent with chpt 4 usage
13-1079	13	43	3	43	3	Delete "polar" and insert "into polar regions" to give some sense that the increase in saturation vapour pressure enables greater transport of moisture. [Francis Zwiers, Canada]	accepted
13-1080	13	43	6	43	12	Page 43, paragraph from line 6 to line12. The 1st sentence in this paragraph talks about the future, but the rest of the paragraph talks about present. This is confusing and may need some elaboration to be clear. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted - first line needs to be reworded
13-1081	13	43	25	43	26	I don't know if the FAQ editors have a view on the use of the first person ("we"), but it would probably be good style to avoids its use. [Francis Zwiers, Canada]	editorial agreed scientific community etc
13-1082	13	43	29	3	29	This sentence seems unclear - is something missing? Should "thicker" be replaced with "thinner"? [Francis Zwiers, Canada]	noted no thcier is correct but sentence does have too much in it
13-1083	13	43	29			Is this the right figure? [Government of United States of America]	noted yes it is
13-1084	13	43	32	43	32	Would it be useful to include a figure illustrating this feedback processes schematically? [Francis Zwiers, Canada]	noted the figure for box 13.2 shows this but can not reference here?
13-1085	13	43	35	43	40	This section is too vague and could be read to be inconsistent. If enough basins collapse on a multi-century timescale then the multi-millennial timescale for WAIS is irrelevant. I suggest just note that a signifcant fraction	accepted

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						of WAIS ice could be lost on multi-century timescales. [Michael Oppenheimer, United States of America]	
13-1086	13	43	42	43	42	It is not clear, why the figure call is inserted in this sentence. [Government of Germany]	accepted remove figure will likley change anyway
13-1087	13	43	55	43	55	Perhaps insert "associated with greenhouse gas induced warming" or something to that effect, to suggest a possible cause for a long term trend. [Francis Zwiers, Canada]	accepted
13-1088	13	44	1	44	1	Suggest adding a few words to explain "basal lubrication" - lay readers might not be aware of what this refers to. [Francis Zwiers, Canada]	accepted
13-1089	13	44	25	44	34	Be aware that there are many groundwater system over exploited today and the systems are not sustainable in a longer perspective! Point out the need for more research within this area! [Charlotte Sparrenbom, Sweden]	Rejected. The purpose of the AR5 is to assess existing scientific literature, not to suggest a research agenda.
13-1090	13	44	28	44	39	13.4.5. The acronym SLE occurs four times in this section and should be omitted or replaced by the words “sea level rise.”. [James Gower, Canada]	Accepted.
13-1091	13	44	28			SLE or SLR? [Government of United States of America]	Accepted.
13-1092	13	44	41			The reference to Section 13.4.4 must be wrong. Change to 13.3.4 [Government of France]	Accepted.
13-1093	13	44	51	44	53	Please consider making the ground-water contribution scenario dependent. even if you have to leave the reservoir scenario-independent. Doing that is clearly closer to our best estimates at the moment, and will help underline that scenario pathway actually matters. [Aslak Grinsted, Denmark]	Rejected. As stated, we do not think there is a sufficient literature basis to do this robustly. See also 13-1094.
13-1094	13	44	53	44	53	Why would these processes show any relationship to the RCPs? They can probably be treated as decoupled. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted. No change is needed to the text.
13-1095	13	44	55	44	55	In Section 13.5 it is very important that the upper bounds of sea-level rise are defined as was not really done well in AR4. Policymakers focus extensively on this upper bound and this makes sense from a risk and decision perspective. If upper bounds cannot be determined with any confidence with our current understanding then this should be stated explictly -- including in the Executive Summary. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account by including discussion of the issue in 13.5.3 and the Exec Summ. We agree that this matter is important for policy and must be clearly described.
13-1096	13	45	1			Section 13.5.1 - Forecasts of sea level rise that ignore the scientific discipline of Forecasting, and use modeled data in preference to measurements, are inherently weak, but the "semi-empirical" approach is even weaker than the process-based approach. See: http://tinyurl.com/rahmstuff The fact that > 2/3 century of intense GHG emissions have not resulted in any measurable acceleration in rate of sea level rise is powerful evidence that GHG emissions are not strong drivers of sea level rise. Yet the SOD persists in treating GHG levels and emission scenarios as the ONLY drivers of sea level rise. The fact is that we've done the experiment, and we know the result: since WWII mankind has put enormous quanties of GHGs into the atmosphere, yet the rate of sea level rise has not increased at all. It defys reason to project that repeating the experiment in the 21st century will have a wildly different result. [David Burton, United States of America]	Rejected. The observed rate of GMSL rise can be accounted for in various budget periods as a sum of contributions derived from observations and models of the contributions. As described in section 13.3, the models are consistent with observations, as far as they can be evaluated. This agreement gives confidence in using these physically based models to make projections. There is a great deal of evidence to support the expectation of an increasingly large effect of GHG warming.
13-1097	13	45	13	45	16	Page 45, lines 13 to 16. I think the percentiles given are correct, but please check that you indeed wanted to give the percentiles for the 90% confidence interval. It is important that this is absolutely correct in this paragraph. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted. The percentiles are correct. Further explanation has been added.
13-1098	13	45	16			SAT should be defined at line 12 [Kathleen McInnes, Australia]	Accepted.
13-1099	13	45	23			Page 45, line 23. Insert “that" after “assume". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-1100	13	45	26			Why has AR5 shifted to "likely" ranges from AR4 which was 5-95% (very likely)? The Ritz, Leverman, and Little studies all seem to present 5-95% as well. This will continue to generate confusion and misinterpretation. I strongly urge that we either present the VERY LIKELY range or both, or have a very clear rationale for not doing so indicated in the text. The change in year-range will also add to misinterpretation. [Michael Oppenheimer, United States of America]	Taken into account by inserting further explanation. The AR4 did not give likely or very likely ranges for sea level; it simply reported the model range with no assessment of likelihood. In the AR5 we take the same approach for GMSL rise as the AR4 and the

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							AR5 do for global mean surface air temperature change, namely to regard the model 5-95% range as "likely" (not "very likely").
13-1101	13	45	32	45	40	To avoid mis-quotation, please include a clear up-front statement here that says that there is only medium confidence in these likelihood ranges, and cross-link to section 13.5.1.3. Note that Mastrandrea et al (2010) say that confidence is assumed to be high or very high unless stated to the contrary, so not giving this information as part of this paragraph would make it misleading and inconsistent with the IPCC uncertainty guidance note. More broadly, the authors should reflect whether it really is appropriate to call this a 'likely' range if they do not have at least high confidence in the range. Likelihood statements are intended to refer to probability of outcomes in the real world, not just a statistical description of available model results. Mastrandrea et al (2010) don't rule out giving and explicitly stating a medium confidence level, but I'm worried that the medium confidence will be easily lost in practical applications of this information. Not calling this the 'likely' range and more clearly signalling the limited confidence in the process-model based range may be more supportive of a risk-based approach to planning for sea level rise. [Andy Reisinger, New Zealand]	Accepted. By giving a likely range, although with medium confidence, we provide information that could be used in a risk-based assessment. This is an advance wrt the AR4.
13-1102	13	45	33	45	33	Relative to the mean of 1986-2005 has a midpoint 17 years ago! Report should project from the present, not from the distant past. [David Burton, United States of America]	Taken into account by puttng "Likely ranges" first. Also, "central" has been replaced by "median" or removed in a couple of places. It is not our intention to focus on the median.
13-1103	13	45	36	45	36	poorly worded -- perhaps "lagged response" [Christopher Little, United States of America]	Taken into account by deleting the sentence for the sake of simplicity. The response is not time-lagged, but time-integrated. This characteristic of thermal expansion and land ice SMB has already been discussed in 13.4.
13-1104	13	45	36	45	37	Sentence ends "time-integrating characteristic of sea level". Should this refer to the slow rate of thermal expansion of the oceans instead? Sentence is unclear as it stands. [European Union]	Taken into account by deleting the sentence for the sake of simplicity. The point applies to land ice SMB as well as thermal expansion, but has already been discussed in 13.4.
13-1105	13	45	42	45	44	Does a starting rate of rise of 4 mm yr-1 mean that projections overestimate sea level rise into the future? How is this accounted for? [Government of Australia]	Method improved
13-1106	13	45	42	45	57	It is suggested in these paragraphs that the AOGCMs have not reproduced the reduced rate of thermal expansion measured over the past decade. With thermal expansion attributing the largest contribution to SLR, what actual confidence do we have in the longer-term projections? This also begs the question, why haven't the AOGCMs been able to adequately predict such influences over the observational record where data quality is high and coverage extensive? If the AOGCMs are starting from a higher base rate of SLR (at 4mm/yr rather than 3mm/yr) obviously this will affect the projection estimates over the longer-term? [Phil Watson, Australia]	Rejected. Confidence in models for thermal expansion is assessed in Section 13.3.1.2. Method improved
13-1107	13	45	54			Don't understand what 'central' means - please define. [Terrence Joyce, United States of America]	Accepted. Replaced with "median".
13-1108	13	45				As noted in general comments, suggest that Appendix A be included as there are substantial repeats of information and Appendix A is of primary importance. [Christopher Little, United States of America]	Taken into account by moving material from the Appendix into subsections of 13.4.
13-1109	13	46	15	46	23	Please write out SMB in full in the caption. [Timothy Carter, Finland]	Accepted.
13-1110	13	46	15	46	23	Table 13.5: please clarify units in the table, e.g. by adding a column before the numbers to indicate the units. It is too easy to get confused between m en mm/yr in the current situation. [Philippe Marbaix, Belgium]	Taken into account by explicitly indicating the units for the rate.
13-1111	13	46	15	46	24	Table 13.5. Entry for Antarctic ice sheet under RCP2.6 has upper range of -0.00, i.e. minus zero. Should be zero? Table caption (line 15) does not state that these results have come from process-based models. Caption needs amending so readers are clear [European Union]	Accepted.
13-1112	13	46	15	46	24	I have a hard time reconciling the Greenland SMB estimate with the numbers you give in table 13.4.	Taken into account by moving material from the

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						In table 13.4 you show that the 90% range is something like 0-10cm for RCP4.5. In table 13.5 you give a 66% range of 0-7 cm for RCP4.5 I would expect the 66% range to be tighter than the 90% range on both sides (not just the high side as you have here). That cannot be correct. [Aslak Grinsted, Denmark]	Appendix into subsections of 13.4.
13-1113	13	46	15	46	24	The total greenland contribution from RCP8.5 is incompatible with Bindschadler et al.s estimate of 22cm even when choosing the highest likely value for both SMB and DYN. (Bindschadler et al. 2012 "Ice-Sheet Model Sensitivities to Environmental Forcing and Their Use in Projecting Future SeaLevel (The SeaRISE Project)") - Further, I have seen Faezeh Nick's new calving estimates for RCP8.5, and they are reasonably consistent with Bindschadler (Nick et al. submitted). [Aslak Grinsted, Denmark]	Now using Nick results
13-1114	13	46	15	46	24	The total AIS contribution for RCP8.5 is lower than Bindschadler et al.'s ~7cm estimate. Further, the AIS-RCP8.5 uncertainty range is overly tight when comparing to SeaRISE papers (e.g. Levermann et al. TCD 2012 & Binschadler et al. 2012). [Aslak Grinsted, Denmark]	Results used now more consistend with some of the Sea rise results
13-1115	13	46	15	46	24	Table 13.5: don't use square brackets, use round ones. Square brackets indicate a 90% confidence interval in the WGI report, whereas here you are giving likely ranges. Avoid confusion and misinterpretation. [Andy Reisinger, New Zealand]	TSU advised us to proceed
13-1116	13	46	15	46	24	Table 13.5 indicates likely ranges for relevant projections. Are these 5%, 95% confidence intervals? Should advise accordingly. [Phil Watson, Australia]	Taken into account by inserting further explanation in the text.
13-1117	13	46	15			I found it strange that the " future projections" are given relative to the 1986-2005 interval. This would be natural for SAR but now this "future" include almost 20 years from the past. [Andrey Ganopolski, Germany]	Rejected. It would not be statistically robust to make projections relative to a short period, because of the influence of unforced variability. The AR4 has adopted this 20-year reference period consistently for projections.
13-1118	13	46	15			ditto above [Terrence Joyce, United States of America]	Accepted. See 13-1107, presuming that's what "above" refers to.
13-1119	13	46	15			The scenario independent treatment of ice sheet dynamics used in the summary of projections for SLR in Table 13.5 is understandable given the limited amount of understanding for how that will evolve. However, the emphasis that is currently given to using central estimates implies that ice sheet dynamics would play a relatively smaller role for the greater warming in RCP8.5 than it does for RCP2.6. In conjunction with the summary of SLR committment per degree of warming shown in Figure 13.11, this implies that the ice sheet response has considerable inertia and so would only be seen over time scales larger than 100 years. This is at odds with the large amount of data which has emerged over the last two decades showing that the major ice sheets have some very dynamic features. Too much emphasis is being given to the central estimates and it would be better just to show the ranges that come from current models. [Martin Manning, New Zealand]	Taken into account by puttng "Likely ranges" first. Also, "central" has been replaced by "median" or removed in a couple of places. It is not our intention to focus on the median.
13-1120	13	46	15			Also the estimates of SLR that have been derived by climate models based on RCPs as given here are only just becoming open to wider review in the science community. To assign a medium confidence for likely ranges at this stage seems rather presumptuous and a lack of recognition of epistemic uncertainty in science. [Martin Manning, New Zealand]	We disagree. We have extensive discussion of the reasons for our medium confidence in 13.5.3. We judge that our confidence is sufficient to give a likely range, on account of the evidence and agreement, but the uncertainty shown by that range is still large.
13-1121	13	46	20	46	22	this is fine in the table, but the figure 13.7 makes it look small and inconsequential. It should be plotted independent of scenario on every entry as in Fig. 13.9. [Terrence Joyce, United States of America]	Rejected. We plot it separately to draw attention to the different treatment.
13-1122	13	46	22	46	22	Wrong. The scenario dependence of the dynamical contribution has been evaluated in the litterature: Levermann et al, submitted.b & Nick et al. Submitted. [Aslak Grinsted, Denmark]	Taken into account by rewording. Although there is some published material on this, we think it is so far insufficient for assessing the scenario dependence.
13-1123	13	46	23	46	24	Table 13.5 (also fig 13.8). The IMBIE study was published yesterday and reading the numerous commentaries by scientists such as Bindschalder and Alley and the lead author, it is repeatedly stated that: i) rates have been accelerating and ii) in a warming world rates will increase. In Ch4, for Greenland for last epoch (07-11) their central rate is 0.7 mm/yr with an accel of about 0.05 mm/yr-2. They also state that this is roughly equally	Estimates updated

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						split between SMB and D. For RCP4.5, which involves ~3.5 deg warming above PI, the contribution from Greenland is 4 +4 cm. This is about the rate for 07-11 (0.7 vs 0.8) despite a warming climate. There is no evidence that the acceleration in Greenland has stopped (in fact 2012 was an extreme loss year, ~500 Gt according to GRACE) and so the fact that the contribution from Greenland is predicted not to increase above ~present-day will be surprising to many given the rather large dT over Greenland for, say RCP4.5, which will be ~1.5 to 2 times global. This counter-intuitive result will need some explanation here. For example, it could be that the recent warming (and trends) in Greenland are due to internal variability. But Fig 4 from Bamber and Aspinall suggests that there is no certainty about this conclusion and a secular trend is equally likely. If this is the case, it is hard to see how the 07-11 rate would be the ~central estimate for the integral from 86-05 to 2081-2100. It is possible to see how it could be within the pdf but, one would assume, below the mean. [Jonathan Bamber, United Kingdom]	
13-1124	13	46	23	46	24	Related point. The assumption of uniform PDF and uncorrelated with magnitude of global change for D seems poor. The former is unlikely and not supported by process based or other approaches. The pdf likely has a long upper tail which is not captured by the range of uncertainties in Table 13.5. The assumption that Greenland discharge is uncorrelated to scenario also seems poor for a range of reasons. For these reasons, it seems that the likely range is underestimated in Table 13.5 and associated Fig 13.8. The message of the table and Fig is that for Antarctic and Greenland dynamics and SMB their uncertainty is smaller than that of, say, glaciers or steric, which have been modelled more extensively and, which, after reading the chapter, are apparently much better understood and modelled than the ice sheets. It is, therefore, again counter-intuitive to see that the uncertainties in ice sheet SMB and D are smaller. This cannot be correct. If, as is discussed at length in the Ch, these systems are incompletely constrained by models one would expect their uncertainty range to be larger than systems which are well constrained. I consider this a serious issue. [Jonathan Bamber, United Kingdom]	Taken into account by changing the estimate of the likely dynamical contributions to cover the 5-95% range of model results, as for other contributions, and using a non-uniform pdf with an upper tail.
13-1125	13	46	23	46	24	Table 13.5: It would be clearer if the Rate of SLR units were indicated in the penultimate row (not only in the caption), as these are different from the units used elsewhere in the table(metres). [Timothy Carter, Finland]	Accepted.
13-1126	13	46	23	46	24	Is it possible to make the land-water storage scenario dependent? There will almost certainly be a greater irrigation demand in RCP8.5 than in RCP2.6. [Aslak Grinsted, Denmark]	Rejected. We agree that there could be a dependence, but we do not think the literature gives a sufficient basis for making a confident estimate of it.
13-1127	13	46	23			What is the difference between "Sea Level Rise" and Sea Level rise at 2100"? Is the first the 2080-2100 average? If so, I would put the rate of SLR on the bottom line (flipping the last 2 columns). Furthermore, I think the units are m for the whole table but the rate. Correct? What time period is the rate computed over? 2080-2100? [Ronald Stouffer, United States of America]	The reviewer has made the correct interpretation but we accept the point that the table should be clarified.
13-1128	13	46	26	49	21	This new section on semiempirical models of sea level rise is important and I commend the authors for adding it. Table 13.6 presents semiempirical results under SRES A1B and RCP 4.5. I suggest that you add results to this table for SRES A2 or A1Fi and RCP 6.0 and 8.5, otherwise policy makers will tend to assume that the table contains the high end limits of sea level rise under this type of modeling, in part, because Table 13.5 with the process-based model resutls includes all RCPs. The addition of at least a few results under the higher emission scenarios and RCPs are important to policymakers who are operating in a risk-based context -- and policymakers do use these tables. In the planning of new bridges and nuclear power plants in the coastal zone, for example, policy makers should be aware that semiempirical model results under RCP 8.5 and SRES A2 or A1Fi are higher than the rates of sea level rise under A2 and RCP 4.5 that are shown in the current Table 13.6. As a former Director of a state coastal zone management program I would look for the full range from the semiempirical modeling literature. [Virginia Burkett, United States of America]	Taken into account by adding a new figure which shows semi-empirical results for all scenarios.
13-1129	13	46	26	49	21	Also please consider the more recent literature on both the process-based and semiempirical modeling of future sea level rise, such as Jevrejeva, S. J. C. Moore and A. Grinsted, 2012, Sea level projections with new generation of scenarios for climate change. Global and Planetary Change, doi.org/10.1016/j.gloplacha.2011.09.006. Also this one, if accepted before your cutoff date: Rahmstorf, S., G. Foster, and A. Cazenave, 2012: Comparing climate projections to observations up to 2011. Environ. Res. Lett., submitted. [Virginia Burkett, United States of America]	Accepted. The table of semi-empirical model projections has been extended and a new figure has been added. Rahmstorf et al. Is assessed in 13.3.6.

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13-1130	13	46	26	49	21	This section could be greatly reduced in length. It is a repetition of the same handful of references over 4 pages of text. Moreover, the results of this type of modelling are not taken into account in the final evaluation of future SLR. The most important statement regarding semi-empirical models is page 48 line 10-13, which says it all. [Frank Pattyn, Belgium]	We disagree. We think it is important to treat this approach thoroughly, because it is of interest to decision-makers and scientifically controversial, as the comments show.
13-1131	13	46	26	49	23	Section 13.5.1.2 I am afraid that this section may not include a sufficiently balanced summary of scientific views. It looks like an investigation prepared against semi-empirical (SE) models. It is nice to document their limitations, and I may agree that there is somewhat less confidence in these models than in complex process-based models, but the picture is not clearly black and white - complex model also have limitations, even if we can trust them more due to their physical basis. Please check that you provide all arguments against AND in favour of these models. [Philippe Marbaix, Belgium]	Taken into account by some rearrangement of 13.5.3. That section now assesses confidence in both process-based and semi-empirical models. We agree that process-based models have limitations. The capabilities and limitations of process-based models are discussed in several places (and overall at much greater length) in this chapter (especially 13.3) and other chapters. The presentation may seem unbalanced because the treatment of process-based model+H35s is spread over many sections while the treatment of semi-empirical models is a single section.
13-1132	13	46	28			This is a reasonable discussion. The discussion of the semi-empirical models seems overly deferential to that approach, but being so is reasonable given that--in the end--those models play no direct role in the projections. In effect, the discussion of semi-empirical models is an implicit caveat to the projections. [James G Titus, United States of America]	Noted. We agree. See also reply to comment 13-1131. As stated in 13.5.3, the disagreement between process-based and semi-empirical models is a reason for having only medium confidence in the projections.
13-1133	13	46	30	46	30	I suggest 'does not explicitly' it does implicitly because it is tuned to the dominant components in its tuning window and some versions have two response times (infinitely long and instantaneous - see V&R 2009) [Mark Siddall, United Kingdom]	Accepted.
13-1134	13	46	34	46	35	I do not understand the meaning of this sentence but, in any case, semi-empirical models cannot be applied to "glacial/interglacial time scales" [Andrey Ganopolski, Germany]	Taken into account by deleting the sentence.
13-1135	13	46	37	46	37	"multidecadal time scales" should be replaced by "multidecadal and longer time scales". [Stefan Rahmstorf, Germany]	Taken into account by inserting "21st century and beyond".
13-1136	13	46	39	46	39	motivated by two problems' reference needed [Mark Siddall, United Kingdom]	Rejected. The two problems are described, with references, in the following sentences.
13-1137	13	46	39	47	9	the case for why semi-implicit models are used here needs to be made more strongly. By stating why they were first developed leaves the reader thinking that they have served their purpose. If the assessment is that they are too uncertain, then this needs to be made more clearly. and the subsequent discussion on the differing assumptions and time periods they use leaves the reader wondering why they are being considered at all. Nor do they seem to have a role in the longer term (beyond 2100) projections. [Kathleen McInnes, Australia]	We disagree. H39 but it is not the role of this section to advocate or argue against it. In this next section, we compare and assess confidence in various approaches to making projections.
13-1138	13	46				Table 13.5 - How were the 5% and 95% summations calculated? If statistical independence of the components being added together was assumed, that's a clear error. It presumes that any source of error in one component (e.g., Greenland SMB) is just as likely to be reduced as increased by an error in another component (e.g., Antarctic SMB). In other words, it assumes that there are no systematic errors at all in the sources of the data, in the model-derived corrections, in the projection methods, etc., that would affect both components. That would obviously be a ridiculous assumption, since very similar models, assumptions, and data sources are used for the Greenland SMB and Antarctic SMB numbers. But if statistical independence was not assumed, I'd like to know how the confidence interval for the sum was calculated. To do so properly would require having a handle on how much correlation there is between the errors in the various components, and I can't imagine how you can get that. [David Burton, United States of America]	Taken into account by adding a comment to the table caption on combination of uncertainties. This was already described in the text. We assume that uncertainty related to the magnitude of climate change for a given scenario is correlated, but that methodological uncertainty in the contributions is not correlated. We disagree with the reviewer and consider, on the contrary, that the data sources and models for Greenland and Antarctica are generally independent.
13-1139	13	46				Figures 13.8 and 13.9 and Table 13.5: The inclusion of SRES A1B in the figures will greatly assist many readers, but people will also be interested in the A1B numbers. Please include in Table 13.5. Also it would greatly assist future users if the data sets for Fig. 13.9 can be posted on an accessible site after publication.	Accepted.

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						[Donald Forbes, Canada]	
13-1140	13	46				tbl. 13.5 The table does not clearly indicate the units. Specifically, line 16, "GMSL rise at 2100", there are no units specified. May want to put the units in the table as well as the text for the table. [Government of United States of America]	Accepted.
13-1141	13	46				Table 13.5. Add a column for confidence [Christopher Little, United States of America]	Takeninto account by stating the general level of confidence in the caption and referring to 13.5.3.
13-1142	13	46				Table 13.5 Note top Antarctic ice dynamics bound is different from text (13.3 cm, I think) [Christopher Little, United States of America]	Estimates updated
13-1143	13	46				In Table 13.5, the entries for the range of Antarctic ice sheet dynamics (0.07 [0.03 to 0.11]) bear no obvious relation to the 33-132mm in the text. Please explain. [Michael Oppenheimer, United States of America]	Estimates updated
13-1144	13	46				Page 46, caption of figure 13.8. The caption would be more instructive if it stated the level of confidence portrayed. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-1145	13	46				Page 46, caption of figure 13.9. The caption does not match the figure. First, it mentions dashed lines as showing likely ranges, but instead they dashed lines portray different processes, and instead there is hatching to portray the likely ranges. Second, the caption mentions panels a and b, but these letters are not given in the figure. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted.
13-1146	13	46				Page 46, caption of table 13.5. When you mention “likely ranges" then the caption would be more instructive in a stand-alone sense if you were to add the percentile values here. I know that the IPCC report has fixed definitions for likely, but it does no harm to repeat it here just for clarity. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Taken into account by referring to 13.5.1, where quantification of uncertainties is discussed.
13-1147	13	46				In the caption of figure 13.8, as explained before, I think that there needs to be mention of the numerical value of the confidence level portrayed, to ensure that the figure becomes a proper standalone item. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted (as 13-1144).
13-1148	13	46				Caption of figure 13.9. As mentioned before, the caption does not match the figure. First, it mentions dashed lines as showing likely ranges, but instead they dashed lines portray different processes, and instead there is hatching to portray the likely ranges. Second, the caption mentions panels a and b, but these letters are not given in the figure. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted (as 13-1145).
13-1149	13	47	4	47	4	replace 'while' with 'although' [Mark Siddall, United Kingdom]	Taken into account by rewording.
13-1150	13	47	4	47	5	I would replace “While semi-empirical models do not solve the two problems that motivated their development” with “While there is no obvious reason why semi-empirical models should solve the two problems that motivated their development”. [John Hunter, Australia]	Taken into account by rewording.
13-1151	13	47	4	47	5	"While semi-empirical models do not solve the two problems that motivated their development" is a strange way of describing things (it may appear loaded). It would seems more neutral to say that SE models are "not aimed at solving the problems that motivated their development, but to go around them by providing (...) ". (maybe such wording is not necessary anyway, as it is not difficult to understand that a SE model is not a full physical complex model, by definition). [Philippe Marbaix, Belgium]	Taken into account by rewording.
13-1152	13	47	4	47	6	Suggested change "do not solve the two problems" -> "do not solve, but bypass the two problems" [Aslak Grinsted, Denmark]	Taken into account by rewording.
13-1153	13	47	6	47	6	To my knowledge only Jevrejeva's work does this - most only consider temperature [Mark Siddall, United Kingdom]	Accepted. We have deleted the phrase. It is a possible application of the models, but not relevant to remainder of this section.
13-1154	13	47	6	47	9	While this sentence is undoubtedly true, I don't see it's relevance to this stage of the discussion of semi-empirical models. I'd remove it and possibly insert it somewhere more appropriate. [John Hunter, Australia]	Partly accepted, partly not. A statement about observations is relevant here, to avoid giving the

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							impression that the observational budget has the same deficiency as at the time of the AR4, but we have shortened it. We have deleted the model statement, which we agree is not relevant in this section.
13-1155	13	47	10			Please add here something like: "In the meantime, process-based models not only reproduce past sea-level rise better but consequently also project a greater future sea-level rise; almost twice the rise over the 21st century than the AR4 range for a given emissions scenario (see section 13.5.1.3). This has greatly diminished the difference between process-based and semi-empirical projections. However, semi-empirical projections are still higher, and process-based models still only reproduce 70% of the observed 20th C sea-level rise." I think this is a very important part of the story; I think that semi-empirical models made a valid contribution by pointing to the fact that process-based models at the time of the AR4 were probably underestimating sea-level rise, and this is now acknowledged in form of the new, much higher projections of the AR5. Although this point may be hard to admit for some, it is no reason to be ashamed of and should be made clearly in this chapter and the SPM. And I would not rule out that at the time of the AR6, process-based models project yet higher SLR and will be even closer to the semi-empirical models. [Stefan Rahmstorf, Germany]	Rejected. The main change in process-based modelling is the inclusion of rapid ice sheet dynamics. That contribution was extensively discussed in the AR4 but not quantified, but can now be quantified because of the development of land ice dynamical models. Other differences arise from improvements in observational datasets. The observational budget can now be closed. See section 13.3.6.
13-1156	13	47	20	47	22	This statement is statistically misleading. In both cases, Jevrejeva et al., and Bitterman et al., sea-level estimations in a validation interval are incompatible with observations. The believe that it suffices that the estimations are within the uncertainty ranges is statistically misleading, since the residuals (differences between estimations and observations) display a linear trend in the case of Jevrejeva et al, or all all positive in the case of Bittermann et al. A non random behaviour of those residuals invalidates either statistical model or the choice of predictors or both. The probability that all residuals of a regression model are by chance all positive, as in the case of Bittermann et al., or are all negative in the first half of the validation period and all positive in the second half is vanishingly small. The open question is then not why the semi-empirical methods provide higher estimates than process based methods. As Smith et al.('Statistical analysis of global surface temperature and sea level using co-integration methods'), the semi-empirical methods applied so far are intrinsically wrong because they applied usual regression methods to trendy time series. Econometrics, where these type of series appear very often, has recognized this and this is why co-integration methods have been developed in the first place. [Eduardo Zorita, Germany]	Discussion expanded
13-1157	13	47	27	47	32	I'm not sure whether these sentences mean anything to anyone but those involved in this little exchange. They are too cryptic to understand what this was about, or what it means. Also, just reporting "there has been debate" is not really an assessment. Ultimately, this debate was entirely inconsequential, and I wonder whether it is worth discussing here. I can live with this description, though - just don't think it will enlighten any reader. [Stefan Rahmstorf, Germany]	Taken into account by slight shortening. In view of the controversial nature of the subject, we think it is useful to
13-1158	13	47	30	47	32	Please clarify the conclusion regarding this first issue : 0.04 m is rather small, so this may imply that issue 1 is no longer substantial? [Philippe Marbaix, Belgium]	Taken into account by inserting a ref to Table 13.6 so the reader can compare it with the central values and the full uncertainty range.
13-1159	13	47	34	47	34	Suggested change to "Second, there is some sensitivity to the choice of dataset used for calibration, and this should be included in the full uncertainty budget." [Aslak Grinsted, Denmark]	Taken into account by inserting "there is some uncertainty in projections".
13-1160	13	47	34	47	46	The key is that there is allowance for the true level of uncertainty in the calibration target (the GMSL reconstruction). This is not only the standard error, but also the uncertainty covariance. It is clear that there must be a very large degree of uncertainty covariance as the different GMSL reconstructions have different century scale trends. If the uncertainty estimate is too optimistic, then the semi-empirical calibration will be too strongly constrained to the data, and the consequent projected uncertainties will be too narrow, and senstive to minor changes in the calibration data. This is what I perceive to be the problem with Rahmstorf's difference for the two church and white reconstructions. The specific inversion method (Markov Chain Monte Carlo / Monte Carlo Inversion) is not what solves the problem for our semi empirical calibrations. It is that we have a conservative uncertainty covariance matrix for the sea level reconstruction. [Aslak Grinsted, Denmark]	Accepted. This comment has helped us to simplify the text.

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13-1161	13	47	34	47	49	You are basically saying "Look, the confidence intervals don't overlap". This type of criticism can also be levelled at almost every single process estimate. The list of semi-empirical critique points is very one-sided. [Aslak Grinsted, Denmark]	We disagree. The aim is to discuss the source of uncertainties. Process-based models also have uncertainties.
13-1162	13	47	35	47	40	Can this be made more compact and readable? [Aslak Grinsted, Denmark]	Accepted. The text has been shortened.
13-1163	13	47	40	47	40	Suggested additional sentence: "This suggests that the calibration process does not fully account for the temporal covariance of uncertainties in the Church and White 2011 sea level reconstruction." [Aslak Grinsted, Denmark]	Accepted.
13-1164	13	47	41	47	42	sentence totally obscure [Michael Oppenheimer, United States of America]	Accepted. The sentence has been deleted.
13-1165	13	47	42	47	46	This sentence is too difficult for me. The “observational space” is larger than what? [European Union]	Accepted. The sentence has been deleted.
13-1166	13	47	45	47	46	I don't understand what is meant by “ but it is unknown whether this procedure fully samples the observational space of other GMSL estimates” - it needs a bit of clarification. [John Hunter, Australia]	Accepted. The sentence has been deleted.
13-1167	13	47	51	47	55	Rahmstorf demonstrate that their model behaves like that. I think their inversions are so fickle to these changes because they basically have over constrained their model to the data. The jevrejeva et al. [Aslak Grinsted, Denmark]	Noted. We do not have published evidence for this.
13-1168	13	47	53	47	54	“implicitly scaled … change”. I think I know what this means but I think it can be said more elegantly. I guess you are saying that there is a background change not related to temperature which the semi-empirical method fails to pick up? Sounds a bit strange, but I suppose it’s possible. If that’s not what is meant, what is meant here? [European Union]	Taken into account by rewording.
13-1169	13	47	54	47	55	I found the part of the sentence "particularly…..change" unclear. Can you explain what "the semi-empirical predictors of sea-level change" are? [Roland Gehrels, United Kingdom]	Taken into account by rewording.
13-1170	13	48	6	48	8	Correct, but did you calculate the spread in the process based estimates? [Aslak Grinsted, Denmark]	Taken into account by deleting the paragraph.
13-1171	13	48	6	48	8	Such arguments would also cast doubt on many projections from process-based models. I think that you should improve the sentence to put things into context. SE models produce a *relatively* wide range of results, but this does not undermine their credibility. It may also be that older results (2007) should not be included in current estimates, as approaches simply have improved. [Philippe Marbaix, Belgium]	Taken into account by deleting the paragraph.
13-1172	13	48	15	48	21	If you want to keep this discussion (again, I think the enlightenment value is near-zero - Von Storch misapplied our model to a situation where the approximation I proposed in 2007 does not hold), then you should add that Vermeer and Rahmstorf (2009) added a rapid-response term to the semi-empirical model to capture the oceanic mixed-layer response, with which the ECHO-G model results of Von Storch were very nicely reproduced. [Stefan Rahmstorf, Germany]	Accepted.
13-1173	13	48	15	48	26	Von Storch shows that radiative forcing (as used by Jevrejeva) is a near perfect predictor of steric expansion. This is also demonstrated by Jevrejeva et al 2012 where a semi-empirical model is trained on historical expansion from process models, and very accurately reproduces their projections as well. Please add sentence to that effect. [Aslak Grinsted, Denmark]	Rejected. Von Storch showed that the rate of ocean heat uptake is a near-perfect predictor of the rate of thermal expansion (their Fig 1d). They did not consider radiative forcing as a predictor.
13-1174	13	48	17	48	18	Von Storch showed that neither T or dT/dt correlated consistently with dS/dt. However, he did not test a Grinsted type formulation which is somewhere in between, and judging from Von Storchs graphs then when T stops working then dT/dt seems to take over. Exactly as you would expect from the Grinsted model. [Aslak Grinsted, Denmark]	Noted.
13-1175	13	48	20	48	21	It would probably be most accurate to change "the semi-empirical model of Rahmstorf (2007b) ... was 21 intended to simulate the response to sustained forcing on multidecadal timescales" to "the semi-empirical model of Rahmstorf (2007b) ...was intended to simulate the response to the sustained forcing that took place during the 20th century." That is not the argument that Vermeer and Rahmstorf make, but putting it that way makes it clear why their model should project future sea level rise better than the impact of volcanies--but also why one can not have much confidence if the forcing accelerates or decelerates. It would not hurt to bring this	Rejected. While it is true that Rahmstorf (2007b) calibrated using the 20th century only, the model is not specific to that particular timeseries of sustained forcing.

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						out a little more. [James G Titus, United States of America]	
13-1176	13	48	21	48	26	Ocean heat content is unaffected by radiative forcing? Sounds strange, but I have no time to check the paper. I note the paper is not assessed in any way here, and as a reader who does not know this paper, I have no clue what this all means from the little that is reported here. I wonder whether this is useful for the intended audience of IPCC reports. [Stefan Rahmstorf, Germany]	Taken into account by deleting the sentence.
13-1177	13	48	22	48	26	This suggests that radiative forcing may be a more appropriate predictor than temperature in semi-empirical models. Please add a sentence to that effect. [Aslak Grinsted, Denmark]	Taken into account by deleting the sentence.
13-1178	13	48	24	48	25	I really don't know what is meant by “because of the latter’s role as a heat capacitor in internally generated climate variability, whereas ocean heat content is unaffected by radiative forcing”. How on Earth is “ocean heat content is unaffected by radiative forcing”? - the ocean is where most of the “greenhouse” heat goes! - I must be missing something here. [John Hunter, Australia]	Taken into account by deleting the sentence.
13-1179	13	48	24	48	25	Is oceanic heat content really unaffected by radiative forcing ? This seems very odd. How could it be that ocean heat content is unaffected by forcing ? What does it mean ? I suspect that there is a problem in this sentence. Please check the reliability of the source, and verify that it contains valid arguments against the semi-empirical models. [Philippe Marbaix, Belgium]	Taken into account by deleting the sentence.
13-1180	13	48	25			phrase on ocean heat content confusing. [Michael Oppenheimer, United States of America]	Taken into account by deleting the sentence.
13-1181	13	48	28	48	34	I think you are trying to say that for small forcing, variability in OHC controls SAT. It does not come across clearly so this whole argument needs a rewrite. [Michael Oppenheimer, United States of America]	Taken into account by rewording.
13-1182	13	48	33	48	33	Remark the forcing series used by Jevrejeva includes natural contributions as well. [Aslak Grinsted, Denmark]	Rejected. This point is concerned with internally generated (unforced) variability.
13-1183	13	48	33	48	34	I do not see how they can say more than this null-hypothesis cannot be rejected. So i suspect this statement to be too categorical. I am looking forwards to reading this paper to see what they actually say. [Aslak Grinsted, Denmark]	Taken into account by removing the citation, which is not essential here because the issue is discussed in 13.3.2.2 and 13.3.3.2.
13-1184	13	48	34			Insert "to the" between "due" and "internal" [Robert Dean, United States of America]	Taken into account by removing the sentence.
13-1185	13	48	52	49	3	Jevrejeva et al. JGR 2012 tunes a semi-empirical model to the estimated historical glacier contribution and forces it with RCP4.5. This results in 26cm of sea level rise over the 21st century which we can compare with the AR5 estimate of 16cm. So, an estimate of the J11 bias from this effect would be ~10cm (although as we mention in the JGR paper the glacier projections are not perfect - glacier calving is completely missing). [Aslak Grinsted, Denmark]	Noted.
13-1186	13	48	57	48	57	“an” should be “a”. [John Hunter, Australia]	Accepted.
13-1187	13	49	1	49	3	The near linear contribution with little acceleration through the 20th century is reproduced by semi-empirical models. So it is not evidence for a detectably reduced sensitivity yet (Although i think it will be important for the 21st century in line with what e.g. Marzeion et al. shows). The simple explanation for why it can be linear is the same as for the near linear behaviour of modelled steric expansion: The timing of the major volcanic eruptions has taken a chunk out of the acceleration, (See Church et al 2005 and Gleckler et al.). [Aslak Grinsted, Denmark]	Taken into account by replacing "is evidence" with "suggests", to avoid implying that it is conclusive. Volcanic eruptions are probably not the explanation; they may have reduced the increase in global mean temperature, but not reversed it, and therefore a constant global glacier sensitivity should still imply an increasing rate of glacier mass loss.
13-1188	13	49	3	49	3	The deceleration of small glaciers will depend on present day glacier volume. If the 35cm glacier estimate by Grinsted TCD, 2012 is correct rather than Radic and Hock's higher estimate, then the average glacier is much thinner, and consequently easier to melt away so that it stops contributing. This would affect both Marzeions, and Radic's projections (especially for high forcing scenarios). [see also Huss and Farinotti's glacier volume estimate] [Aslak Grinsted, Denmark]	Accepted, certainly need to allow for decreaseing area in projections
13-1189	13	49	5	49	6	Please add: "However, recent modelling studies indicate that the ice sheet models responds near-linearly with environmental forcing (Bindschadler et al. submitted), and semi-empirical models can accurately capture the	Rejected. Bindschadler means that time-dependent responses to individual forcings can be added to

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						total ice sheet volume response." Quote from Binschadler et al. abstract 2012: "In most cases, dependence of the ice volume lost on the strength of the forcing is close to linear. Combinations of forcings can be closely approximated by summing the contributions from single forcing experiments." This essentially says semi-empirical models are consistent with our process model knowledge, and should work as the ice sheet forcing terms are strongly correlated with globalT or Radiative Forcing. [Aslak Grinsted, Denmark]	obtain the time-dependent response to the sum of the forcings. That is not the same as the response being linearly related to the forcing at any time.
13-1190	13	49	5	49	6	However, the methods used in process based estimates will also be unable to capture such strongly non-linear dynamical responses. E.g. The Levermann et al. Submitted-b estimates of AIS-DYN are based on a linear response assumption. Please add a sentence making that point clear, because at the moment the list of perceived problems with semi-empirical models is quite frankly extremely one-sided. [Aslak Grinsted, Denmark]	Taken into account by moving this paragraph to 13.5.3. Its main relevance is to the question of whether rapid ice sheet dynamics could explain the difference between process-based and semi-empirical models.
13-1191	13	49	6	49	6	Is really Section 13.5.4 meant here? [Mirko Orlic, Croatia]	Accepted and corrected.
13-1192	13	49	13	49	14	Table 13.6: Caption should outline what the '5%, 50%, 95%' indicate. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-1193	13	49	13			tbl. 13.6 To be clear, the table caption or the table itself should explain (label) what 5%, 50% and 95% are representative of. Previous text discussion describes them a the "spread of their central values"? Perhaps in the figure notes "refer to Appendix 13-A" (three times) [Government of United States of America]	Accepted.
13-1194	13	49	13			Table 13.6: the table currently compares the AR4 projections with projections from semi-empirical models. We suggest to also include the AR5 projections for the RCPs reported in Table 13.5 as well as the results for SRES A1B discussed on p51, l39-41 of Chapter 13. As these results are available in the chapter, it's not clear to us why they are not included in this table. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
13-1195	13	49	14			Table 13-6: Should include Schaeffer et al. (2012) [Robert Kopp, United States]	Accepted.
13-1196	13	49	18	49	18	I think “2000-2099” should read “2090-2099”. [John Hunter, Australia]	Accepted.
13-1197	13	49	19	49	20	From the values given in Table 10.7 in Meehl et al. (2007) for scaled-up ice sheet discharge for A1B -0.01-0.13m it is not clear how the difference between the line including this effect and the line above have been calculated. [Government of Germany]	Taken into account by noting that the scaled-up ice-sheet discharge has also been extrapolated to 2100.
13-1198	13	49	24	49	24	This section is where you could assess UKCP09 and also Nicholls et al (http://www.ipcc-data.org/docs/Sea_Level_Scenario_Guidance_Oct2011.pdf). Are the suggested bounds reasonable or not? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	We disagree. Our role as IPCC lead authors is to make an assessment of the peer-reviewed scientific literature, not to assess other assessments. However, bounds suggested in the literature are discussed in this section.
13-1199	13	49	26	49	23	Section 13.5.1.2 I agree that semi-empirical models results may be given less confidence than process-based ones for a range of reasons, but still it appears that even process-based models have limitations, so that it is not possible to provide a range of results with high confidence. Therefore, I think that results should be provided for each RCP on the basis of published estimates from semi-empirical models. Such estimates are available e.g. in Jevrejeva et al., 2012, Glob and Planetary Change. I think that it is important for the IPCC to fulfill its mandate of providing the "complete picture" of scientifically acceptable views, and that this is also important to inform policy makers : they need to know about the "worst case scenario", with an uncertainty / confidence assessment, even if it indicates that the confidence is limited. [Philippe Marbaix, Belgium]	We note the comment that there is less confidence in semi-empirical models than process-based models: we assess our confidence as low in semi-empirical model projections and medium in process-based model projections. We accept the suggestion to provide results for all RCPs from semi-empirical models and have done so in a new figure. We agree that it is our role to inform policy-makers of the range of views, but In view of the low confidence in these models, following the uncertainty guidelines, we cannot assess the likelihood of their projections.
13-1200	13	49		51		Section 13.5.1.3: Several references to other sections have incorrect section numbers. Some of the sections referenced don’t exist. Needs to be checked. [Government of United States of America]	Accepted.
13-1201	13	49				Table 13.6: it would be helpful to make clear that the first two rows of this Table are the only ones that come	Accepted.

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						from GCMs - this is because the note against Horton et al (2008) refers to "the range across the 11 GCMs" - although I realise that Horton et al (2008) used a semi-empirical model, the reader may be confused into believing that this was another projection based directly on AOGCMs. [John Hunter, Australia]	
13-1202	13	50	7	50	10	There are actually 4 approaches. The fourth, which is not discussed here at all, but which was included in the TAR and for which there is a citable publication (in press and to be published as an AOP in Nature CC in December) is via a structured expert judgement elicitation. Quoting directly from this paper: It is"an approach that is useful for determining the degree of consensus within a scientific community and for exploring collective views on ranges of uncertainties.... This type of approach is valuable when there is a pressing need to confront scientific issues and to focus future work but with incomplete data or understanding. It is not a substitute for improved process understanding; nor is it intended to remove uncertainty but rather to quantify it, given limitations in available information. EE and judgement pooling is used in a wide range of applications from medicine to engineering and natural sciences". One criticism laid at this sort of approach is its perceived lack of repeatability. In this study, the exercise was repeated over two years and showed excellent repeatability for key questions with robust outcomes. In addition, it is the only study that provides complete pdfs of the SLR contributions, individually and aggregated, for the ice sheets (Little considers only Antarctica). The approach is less subjective than, for example, Katsman and Pfeffer. I would assert, therefore, that it provides valuable, unique additional information on the range of uncertainty in the future response of the ice sheets that should be included here. A preprint was sent to Gregory, Clark and Church on 30/7/12. The AOP should be available online in Dec 2012: : Bamber and Aspinall, An expert judgement assessment of future sea level rise from the ice sheets [Jonathan Bamber, United Kingdom]	We disagree that expert elicitation provides an approach comparable to the others we use, because it is not directly based on physical understanding. Presumably the experts' views that are its input are based at least partly on physical understanding, but the process is not transparent and we cannot know how and why various processes have been weighted. Our role is to assess the state of scientific knowledge represented by what is published in the peer-reviewed literature, rather than to assess others' assessments, of which an expert elicitation is a particular type. Expert elicitation does however give a quantitative indication of the degree of agreement in the field.
13-1203	13	50	7	51	9	This section seems to be out of place. Parts of it read as if it was meant to be the introduction to section 13.5. Some re-structuring required. [Roland Gehrels, United Kingdom]	Rejected. This section is placed here, following the sections on process-based and semi-empirical models, because its purpose is to compare the degree of confidence which can be placed in these and other approaches.
13-1204	13	50	8	50	8	"three other approaches" is followed by (line 19) "The first approach is process-based projections….", but the "three other approaches" refer to the second, third and fourth approaches discussed in the next three paragraph. Again, some restructuring required. [Roland Gehrels, United Kingdom]	Accepted. "Three" deleted.
13-1205	13	50	8	51	9	The "three" approaches on page 50, line 8 is followed in later paragraphs by four approaches -- review carefully and correct. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Accepted. "Three" deleted.
13-1206	13	50	9	50	9	Substitution is needed: 'Section 13.6.1.2' to 'Section 13.5.1.2'. [Mirko Orlic, Croatia]	Accepted.
13-1207	13	50	9	50	9	replace 'analogues' with 'indicators'. Few looking at past climates are arguing for them as analogues BUT they can be used against arguments based on existing, still limited, physical understanding whcih suggest maximum possible rates limited by ice dynamics. Assuming paleo data is reliable (which it may not be) any model of ice sheets/dynamics would need to be able to reproduce those rates under those circumstances. [Mark Siddall, United Kingdom]	Accepted.
13-1208	13	50	10	50	10	Is really Section 13.5.4 meant here? [Mirko Orlic, Croatia]	Accepted.
13-1209	13	50	11	50	14	Unclear to me. Perhaps some useful support for this could come from: Little, C.M., N. Urban, and M. Oppenheimer. A comprehensive, probabilistic framework for assessing the ice sheet contribution to sea level change. In revision for PNAS. [Christopher Little, United States of America]	Accepted.
13-1210	13	50	11	50	14	While I agree that the criteria for determining upper bounds using the various estimation methods are often rather limited and unspecified, your explanation of this issue doesn't help the average reader. Any set of probabilities is only objective within a given set of assumptions. That's not the problem with upper bounds; rather, it's that the assumptions were not quantified in order to convert to probabilities (see Little et al). Subjectivity isn't the issue; lack of transparency is. After all, so-called best estimates are also subjective. [Michael Oppenheimer, United States of America]	Accepted.
13-1211	13	50	11	50	18	Seems like this is part of the evaluation of these approaches. I would rather hear about the approaches first, then understand why they can/can't be used. [Christopher Little, United States of America]	Rejected. In these paragraphs we are not evaluating the approaches, but stating the grounds on which they

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							are to be evaluated.
13-1212	13	50	16	50	17	match sentences to read ..based on.. [Government of Kenya]	Rejected. This sentence is not particularly associated with the next paragraph, but with all the approaches.
13-1213	13	50	19	50	23	This paragraph does not refer to Table 13.5 which compares sea level projections from process-based models. Table 13.5 should be referenced here since it contains key results. [European Union]	Accepted.
13-1214	13	50	19	50	23	The problem is that the process models used in the projections, are not those which agree with the observational budget.E.g. the projected 21st century calving discharge for GrIS is less than the present day calving discharge (see Rignot et al 2011, and Bigg et al. 1999). [Aslak Grinsted, Denmark]	Rejected. The process-based models are the same or similar as those used for projections, as described in Section 13.3. The contribution from Greenland ice sheet dynamical acceleration in recent years is about 0.3 mm yr-1 SLE, which is half of the rate of ice-sheet mass loss, and consistent with Rignot et al., who estimate that discharge has increased by about 100 Gt yr-1 (0.3 mm yr-1) since a state of balance in the early 1990s. By contrast, the central projection of the dynamical contribution in the 21st century is 40 mm (0.4 mm yr-1).
13-1215	13	50	19	50	23	Process based is not part of the above list of upper-bound generating approaches mentioned in the previous paragraph, and described in the three subsequent paragraphs. [Christopher Little, United States of America]	Accepted. We have repeated "of GMSL rise" in the previous paragraph to make clear that we are not talking only about upper bounds.
13-1216	13	50	20	50	20	Substitution is needed: 'Section 13.6.1.2' to 'Section 13.5.1.2'. [Mirko Orlic, Croatia]	Accepted.
13-1217	13	50	25	50	25	Substitution is needed: 'Section 13.6.1.2' to 'Section 13.5.1.2'. [Mirko Orlic, Croatia]	Accepted.
13-1218	13	50	25	50	31	This is repetition from section 13.5.1.2. [Roland Gehrels, United Kingdom]	Noted. However, we think that a small summary is necessary here in order to make the comparison, as for the process-based approach in the previous paragraph.
13-1219	13	50	25	50	31	This paragraph does not refer to Table 13.6 which compares sea level projections from semi-empirical models. Table 13.5 should be referenced here since it contains key results. [European Union]	Accepted.
13-1220	13	50	29	50	30	Note that extrapolation beyond calibration is a common feature in climate modelling, and is not unique to SEM's. Much has been written about this from both a practical and philosophical angle. [Christopher Little, United States of America]	Noted.
13-1221	13	50	31	50	31	I don't see the relevance of 'successful calibration'. Perhaps 'successful calibration and testing'? [Mark Siddall, United Kingdom]	Accepted. Insert "and evaluation".
13-1222	13	50	33	50	34	Please modify/add here: "… they still project significantly higher GSML rise than process-based models, although process-based models now project almost twice the GSML rise that they projected at the time of the AR4." [Stefan Rahmstorf, Germany]	Rejected. The main change in process-based modelling is the inclusion of rapid ice sheet dynamics. That contribution was extensively discussed in the AR4 but not quantified, so such a remark would not be a comparison of like with like. The comparison is discussed in detail at the end of this section.
13-1223	13	50	33	50	43	The observation that semi-empirical model projections all project higher GMSL rise than process-based models, but that the proffered explanation for this difference seems "unlikely" to the authors (incidentally, is this formal uncertainty language?), begs the question - why the difference? [Timothy Carter, Finland]	Taken into account by rewording. The new text does not say "unlikely". The previous para, summarising 13.5.2, suggests that sea level in the future might not bear the same relationship to the predictors used by the semi-empirical model as it did in the past. In general terms, this could explain the difference, but because the semi-empirical model results do not give a breakdown into contributions, it is not possible to be

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							specific.
13-1224	13	50	35	50	37	Antarctic Dyn is the most obvious candidate as it has a very long tail on the uncertainty distributions. The very likely upper limit is ~0.5 m in Levermann et al. 2012. Quote: "The 90 %-range which is denoted by the IPCC as the “very likely”-range reaches up to 0.45 m for all models including the time-delay and even up to 0.53 m without time delay.". [Aslak Grinsted, Denmark]	Noted. The text discusses the evidence for this explanation.
13-1225	13	50	35	50	37	Any process with a long tail in the uncertainty distribution is a potential candidate for bridging the gap between semi-empirical models and process models. It could also be explained by a combination of different uncertainties. Especially as the uncertainties in the different contributions are likely covarying through their common dependence on the uncertain climate sensitivity. [Aslak Grinsted, Denmark]	Noted, but we are assessing the literature, and ice-sheet dynamics is the only suggestion that has been proposed. Climate sensitivity does not explain the difference because it affects process-based and semi-empirical model projections in the same way.
13-1226	13	50	35	50	40	The disagreement between semi-empirical models and the process budget is overstated considering that their very likely ranges overlap by atleast 30cm. That is compare the process based upper limit with the semi-empirical lower limit for e.g. RCP8.5. Consider this high-end process budget estimate constructed for RCP8.5: GrIS = 22 cm (Binschadler et al. 2012 - consistent with Nick et al. submitted) Glaciers = 21 cm (Marzeion et al. 2012 - similar to other published estimates) LandWaterStorage = 8 cm (water demand will be greater in a warmer world) AIS_SMB = -5 cm AIS_DYN = 35 cm (below the very likely upper range given by Levermann et al. TCD 2012) Steric = 27 cm (AR5-SOD table) Total = 108 cm. In this budget I use many best estimates for all contributions except for AIS where I choose a high-end estimate, but one that cannot be ruled out by process models (see Levermann et al. TCD 2012). The 108 cm is IMHO clearly below the very likely upper limit as I have not yet allowed for uncertainties in any of the other components. The uncertainty from other components is likely covarying (through climate sensitivity uncertainty), and this adds up to a substantial uncertainty. With these numbers the sum of 108cm is the same as the central estimate from Jevrejeva et al. 2011 which has a lower likely limit of 79cm. [Aslak Grinsted, Denmark]	We have taken into account the point about overlap by including a new figure of semi-empirical model projections, in which the limited overlap can be seen. We note that in the budget presented by this comment the reason for the larger projection than the central value of 13.5.1 for this scenario is the larger contributions from ice-sheet dynamics. As discussed extensively in the chapter, this is the dominant uncertainty in projections.
13-1227	13	50	35	50	42	Another reason for advocating the semi-empirical models might be called the "comedy of errors theory". We know that semi-empirical models provide higher estimates, and we know that it is possible that dynamic ice sheet response will be greater than the process models project. So to achieve some sort of "rough justice" we use a model that gets the higher answer so that the subjective probability distribution is covered by the range of models, though perhaps for the wrong reasons. While you probably realize that this is going on to some extent, addressing this directly can be confusing and possibly not seem scientific. Nevertheless, I think that you could add a statement similar to the following, somewhere in the section: "Although we have been unable to find a scientific basis for using the semi-empirical models in this assessment, we do not mean to suggest that their scenarios ought not be considered by coastal decisionmakers, especially when considering matters that are sensitive to low-probability/high impact events. The semi-empirical model scenarios may contain useful information; we simply have found no scientifically valid way to incorporate them here." [James G Titus, United States of America]	Taken into account by our conclusion regarding upper limits. There we list values from various methods, including semi-empirical models, but our assessment of the state of science is that we cannot say how likely any of them is. This means we are not able to advise decision-makers on whether they should use them.
13-1228	13	50	37	50	43	For this argument to make sense then you have to argue that there is zero correlation between regional ocean warming and surface warming. I find that implausible. [Aslak Grinsted, Denmark]	Taken into account by clarifying the point, which is that there is not a demonstrated relationship in recent decades to *global* surface temperature change.
13-1229	13	50	38			Change "unlikely" to "very unlikely". You have provided no evidence of a significant dynamical contribution from ice sheets during the calibration for the semi-empirical models; nor is there even a reasonable hypothesic for how that could have occurred. So "extremely unlikely" or "impossible" are probably accurate but the point is made with "very unlikely". [James G Titus, United States of America]	Taken into account by rewording to avoid saying "likely", since we cannot make a confident judgement.
13-1230	13	50	40	50	42	Bigg, Wei, Wilton, Zhao, Billings, Hanna, and Kadirkamanathan finds evidence to the contrary for GrIS in a paper in review for PNAS. [Aslak Grinsted, Denmark]	Noted. We have not considered this paper. (The deadline for papers to be considered is that they should be accepted for publication by mid-March

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							2013.)
13-1231	13	50	40	50	42	The ice sheets have had calving discharge for the entire calibration period. Semi-empirical models may not be able to capture the inter-annual variability in discharge, but neither does process models. [Aslak Grinsted, Denmark]	Taken into account by clarifying the point (see Section 13.5.2), which is that there is not a demonstrated relationship in recent decades to *global* surface temperature change, and therefore that using such a relationship to make projections is not robust.
13-1232	13	50	40	50	43	This "the only suggested explanation" is a bit unfair. Indeed, it is not logical that semi-empirical models project higher future rise because of recent dynamical changes in Greenland and Antarctica, because these changes are too recent to have played any significant role in the calibration of semi-empirical models. These models give a similar calibration also if the last two decades, where Greenland and Antarctica started to move, are excluded. But because semi-empirical models by construction do not separate their response into individual processes, it is misguided to look for individual processes that explain the difference. Maybe the difference simply is that semi-empirical models are calibrated to correctly reproduce the total 20th C sea-level rise, while process-based models reproduce only 70% of it? [Stefan Rahmstorf, Germany]	We disagree. We do not think it is misguided to require a physical explanation for a projected change; we cannot have confidence in projections without such an understanding. In fact it is possible to account for 20th century sea level rise in terms of contributions (see section 13.3).
13-1233	13	50	40	50	43	Cite Bjork et al, Kjaer et al, Moon et al (references either in the existing Ch13 list or in my earlier comment [Mark Siddall, United Kingdom]	Bjork reference added.
13-1234	13	50	48			"have" should be "had". [Adrian Simmons, United Kingdom]	Accepted.
13-1235	13	50	49	50	49	interglaciations' should be 'interglacials'. [Philippe Huybrechts, Belgium]	Editorial
13-1236	13	50	51	50	52	I am not sure what to do with units here. It is clearly a little odd to use different units (m kyr^-1) compared to elsewhere in the text. On the other hand these units express the fact that this is the millennial average rate. I suggest that you write 'a millennial average rate of xx common units' to get around that. That would, in one phrase also express that this is the millennial average. [Mark Siddall, United Kingdom]	Accepted. We have changed the units to mm yr-1. The text already said it refers to 1000-year means.
13-1237	13	50	51	50	53	But do they provide any information for the likely ranges? [Christopher Little, United States of America]	Taken into account by a small rephrasing to express the two limits in the same way.
13-1238	13	50	52	50	54	analogy should be analogue. BUT this is besides the point. The LIG is useful as an indication of physically plausible/possible rates during interglacials under whatever forcing. Analogy is NOT the only use of paleo data [Mark Siddall, United Kingdom]	Taken into account by replacing "analogy" and spelling out that the different forcing is the reason for the analogue being limited, as the reviewer appreciated but may not have been obvious. The different forcing means that the relationship to global mean temperature change could be different, for instance.
13-1239	13	50	54	50	54	Is there any data that can be used to for century rates of rise in periods like the LIG? See e.g. Section 5.6.2.3 & 5.6.2.4 [Aslak Grinsted, Denmark]	We note the question. The information provided here is the most specific that can be given on the basis of existing evidence.
13-1240	13	50	56	50	56	Seems like the first sentence needs to describe what kinematics do for the other terms of the SL budget, to be comparable to the other approaches, which are "top-down". [Christopher Little, United States of America]	Taken into account.
13-1241	13	51	1			giving 5 m -- by when? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account by repeating "by 2100" for clarity.
13-1242	13	51	2	51	9	It's not clear to me how to develop an assumption-free projection, so this doesn't separate kinematics from any other approach. The Little et al. paper cited later in the paragraph tries to examine a more complete set of different regional discharge scenarios by which the Antarctic SLR can reach those high values (50-60 cm). But in the end we have to make a judgement about what we think is plausible. This chapter is making a statement about why these studies are not included in the quantitative estimate. It seems to me this can be done in two ways: these constraints are not applicable, or the scenarios represent an outcome with an extremely low likelihood of occurence. It seems to me the second approach is more consistent with the rest of the chapter. [Christopher Little, United States of America]	Taken into account by rewording, with thanks for this helpful comment, which provides useful clarity. We cannot give a likelihood because our level of confidence would not justify it.
13-1243	13	51	2			of 1.1 m -- by when? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account by rearranging the text.

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13-1244	13	51	6	51	9	Leverman et al. TCD, 2012 has a process model based estimate of AIS dynamical loss based on searise. He has a 90% range of 2-53cm for RCP8.5. He demonstrates that the process-based uncertainties has a long tail and that even half-a-metre contributions cannot be ruled out yet. The probability model by Little et al. is based on statistical extrapolation and qualified guesses on the basin-covariance structure. I do not believe that such an approach can be used to rule out what is simulated by Levermann (which is one step closer to the actual ice sheet models). [Aslak Grinsted, Denmark]	Levermann et al. is not published.
13-1245	13	51	6	52	36	From the impacts community point of view, it would be useful to be able to state if possible an upper threshold that GMSL by 2100 is very unlikely to exceed. Often it is low probability but high impact scenario that may be highly relevant for impact assessments within some sectors. [Kathleen McInnes, Australia]	Noted. We have considered this issue very carefully and concluded that our level of confidence does not permit us to state an upper bound by 2100. We recognise that it would be useful, but the state of science is insufficient.
13-1246	13	51	11	51	23	This paragraph addresses directly the issue of greatest concern to coastal planners, namely how to set an upper limit on GMSL rise. The authors have assembled the available evidence, including more components than were available for the AR4, and I commend them on their explanation for choosing the upper bound as 0.96 m by 2100, even though they recognise that a definitive uper bound cannot be given. I think that this section of the chapter offers a more persuasive case for the numbers presented than could be offered in the AR4. [Timothy Carter, Finland]	Noted with thanks.
13-1247	13	51	11	51	50	This text should be sharpened as it is of little use to users of sea-level science which seems to be the target audience. Here some clear judgements need to be stated even if the judgement is that we do not know. The 0.84 m value is offered as the top of the likely range and yet in the next paragraph you "cannot give a definite upper bound". Please be clear about what is known and what is not known and guide endusers in the choices they make. There is a danger here that the endusers are left to interpret material that is better interpreted by this author team. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Taken into account by clarifying that higher values are unlikely.
13-1248	13	51	11	51	50	Statements concerning the need for confidence levels are made and the importance of physical achieveability are made -- so what are your judgements in this regard? H++ was an attempt -- what is your assessment of its value in this regard? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	See response to comment 13-1198.
13-1249	13	51	14	51	15	I disagree with the sentence as it stands. Mastrandrea et al (2010) clearly say that likelihood statements are assumed to be based on high or very high confidence assessments; only if you have lesser confidence than that do you need to spell it out, and rather prominently because it deviates from that explicit assumption. [Andy Reisinger, New Zealand]	Taken into account by deleting the statement.
13-1250	13	51	14			A good place to introduce UKCP09 and the H++ concept. It has been influential in the UK and applied in guidance of the Environment Agency and in documents like Wilby et al (2011) (see earlier comment. Further included in Nicholls et al (http://www.ipcc-data.org/docs/Sea_Level_Scenario_Guidance_Oct2011.pdf). [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	See response to comment 13-1198.
13-1251	13	51	19	51	23	I am extremely concerned to read this type of argument. We have little or no way yet to pin down the long tail probabilities of SLR from the ice sheets. This is partly because we do not have large enough ensembles to get at low probability events - numerics will always struggle to capture long tail probability, catastrophic change. This is not unique to ice sheets but is a general truth in these types of systems. Furthermore, there are still 'unknown unkowns' in the ice sheet response. Although, we rightly have low confidence in them, paleo data do exist with SLR of more than 1 m per century. My concern is the statement 'physically unacheivable makes them of little value'. Many a seismologist or vulcanologist has made such a claim only to be tragically surprised - as the Japanese were with the tsunami. We are crossing over to WG2 territory where this type of statement simply would not be allowed. It is not just the physics but the statistics which are poorly understood in this text. Just in the paragraph below we read 'we have only medium confidence in our ranges, and cannot give a definite upper bound' [Mark Siddall, United Kingdom]	Taken into account by deleting the statement.
13-1252	13	51	22	51	23	Not clear where this number of 0.84 m is coming from. According to Table 13.5 the central estimate and likely range for RCP 8.5 by 2100 will be 0.76 [0.56 - 0.96], and this is also consistent with the values reported in the Executive summary. '0.84' is perhaps a leftover from the First Order Draft? Please check carefully. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The number should have been 0.96 m, as in Table 13.5 of the SOD.

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13-1253	13	51	22			0.84 does not appear in Table 13.5 - where does it come from? Explain how you derived it? [Michael Oppenheimer, United States of America]	Accepted. The number should have been 0.96 m, as in Table 13.5 of the SOD.
13-1254	13	51	23	51	23	"similar to the lower estimate of Pfeffer" is misleading. It suggest agreement between Pfeffer and the process based estimate which is not the case. In Pfeffer, Gris=16.5 cm and AIS 15 cm. In table 13.5 the numbers are 11 and 2 cm so they agree for the wrong reasons. [Jonathan Bamber, United Kingdom]	Accepted. We have deleted the comparison.
13-1255	13	51	25	51	36	This is a good summary of the issues, but I find far too much discussion of the semi-empirical AND process models in this chapter. [Terrence Joyce, United States of America]	Noted. We consider the extensive discussion to be appropriate because GMSL rise by 2100 is a subject of great interest to policy-makers, to whom it is important to explain the basis for projections and the level of confidence.
13-1256	13	51	27	51	28	this is poor language. Little et al and Ch 13 both give plenty of reasons for not using "upper bound" in any circumstance without a probability attached so why return to it now? it's a straw man. [Michael Oppenheimer, United States of America]	Taken into account by expanding the discussion of "upper bound". We think that it is necessary to discuss this concept because policy-makers often ask for an upper bound.
13-1257	13	51	29	51	29	“poorly quantified”. The discussion so far has implied that the uncertainty is due to model inadequacies, but there is another aspect, which is the inherent unpredictability of the system. Some comment should be made around here, if not before. [European Union]	The sentence in question has been deleted.
13-1258	13	51	30	51	32	Process models demonstrate that under certain given scenarios grounding line retreat is increasing linearly or exponentially, but may show a more complex picture depending on the geometry of the drainage basin. For instance, stabilization of grounding lines on bedrock bumps (Nick et al., 2009) and the effect of buttressing (Gudmundsson et al, submitted) show a more complex, but also more complete picture. Semi-empirical models clearly overestimate GL, based on an increased mass loss from glaciers (while their relative contribution may most likely decrease since their volume decreases). The feedback mechanisms are not included (see also page 48 lines 10-13). So, it is not correct to state that we don't know why there is such a large difference between the process-based and the semi-empirical method. [Frank Pattyn, Belgium]	Concerning the semi-empirical models, we agree with the reviewer that there are possible explanations for why semi-empirical models may give larger projections. However, we cannot be certain, because it is not possible to break down semi-empirical model projections into components.
13-1259	13	51	30	51	36	These lines, particularly 35/36, create a logical inconsistency that must be resolved. The authors say that scientific understanding is insufficient to evaluate probability of higher values than process-based model results. That may well be so, but in that case, by definition, you cannot give a likely range for future SLR. On the other hand, if the authors insist that they have assessed the evidence and based on this can give a likely range, then by definition this means that the probability of values higher than this is less than 33%, i.e. higher values are "unlikely". If that's the case, it is a very important conclusion that must be stated explicitly. The authors could then still say that scientific evidence is too limited to give a more precise evaluation of this likelihood (i.e. is it less than 1, 5, 10 or 30%), but the overall bound on probability that a "likely" SLR range implies would be very important. You can't shy away from this: either it's a likely range and SLR is 'unlikely' to be above it, or you can't say that it is unlikely, in which case you can't call the range from process-based models 'likely' either. The authors really must make a choice here. [Andy Reisinger, New Zealand]	Accepted. Values lying outside the range are unlikely. We are making another point too, that we cannot give a probability for SLR exceeding any particular value outside the likely range.
13-1260	13	51	32	51	32	Suggest inserting "or both" after "overestimates" since the two hypotheses are not mutually exclusive. [James G Titus, United States of America]	Accepted. Deleted "either" in order not to imply they are incompatible possibilities.
13-1261	13	51	33	51	34	Change "if this increase were entirely … would be" to ""if this increase proves to be entirely…will be". Your use of the subjunctive (also known as secnd conditional) is only appropriate for something that is very unlikely or impossible. The 1st conditional tense is correct grammar for this situation, since a continuation of current trends is not impossible. [James G Titus, United States of America]	Taken into account by rephrasing the sentence. The conditional really refers to what is included in the projections, rather than what will happen in the future.
13-1262	13	51	35	51	36	can we instead of these double negatives point a way forward? why not just leave this out and end with the next paragraph which, if clarified, would say all that needs to be said rather nicely. [Michael Oppenheimer, United States of America]	Accepted.
13-1263	13	51	36	51	36	I think it would be useful if you had a sentence which stated how much of an overlap you estimate there is between the very_likely ranges from SE and process-based models. [Aslak Grinsted, Denmark]	Taken into account by adding a figure to compare process-based and semi-empirical model projections for a particular scenario. This gives an impression of

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							the overlap. Because of our low confidence, we cannot give a likely range for semi-empirical model projections.
13-1264	13	51	38	51	44	This paragraph provides a very valuable comparision but a column for A1B should also be placed in table 13.5 with the endpoint periods lined up and the relation of SRES to RCPs explained in text a bit. This would allow the reader who just looks at the table to understand what has really changed and by how much, between the two assessments. Also, on line 43, give the full range for dynamical contribution. [Michael Oppenheimer, United States of America]	Find out where RCPs are explained in the report - ch12? Ask TSU if we can rotate the table. Last bit done.
13-1265	13	51	46	51	47	Here it is stated that "The contribution from glaciers is larger than in the AR4 primarily because of the greater estimate of the present glacier volume in new inventories". SOD Chapter 4 (p18, lines 16-17) however states that "The values used in AR4 (area: 795,000 km2, volume 260,000 km3) were higher than the new numbers". Is there a contradiction or do I miss some crucial information here? [Hilkka Pellikka, Finland]	Taken into account by noting in the text that the area is unchanged. Despite this, the estimated volume is greater.
13-1266	13	51	50			Would a new box be useful here illustrating future judgements for one location -- New York post Hurricane Sandy? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Not pursuing this option
13-1267	13	52	7	52	15	suggest to refer to Section 12.3.1.3 where the RCP extensions are explained. We note that neither Ch1 nor Ch12 seem to be referring to the RCP extensions as ECPs but simply stick with RCPs even beyond 2100. A common naming convention is clearly preferable from our perspective. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.
13-1268	13	52	7			Page 52, line 7. Change “apply" to “applies". (Number is singular; see the different sense that has been chosen in line 17 “synthesis…. is". Consistency is needed.) [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	partially accepted. The beginning of the sentence has been changed into "Some model…apply..."
13-1269	13	52	9			"GHG concentrations" -- the numbers are probably CO2 equivalent concentrations including more than just GHG, i.e., plus other forcing components. If so, please change text to "CO2 equivalent concentrations" [Thomas Stocker/ WGI TSU, Switzerland]	rejected. In most models there is no distinction made between CO2 and other GHG and only CO2 is changed. While it is correct that in reality a mixure of GHG will be emitted, this does not reflect the model reality which is reported here. For consistency the term CO2 equivalent has been eliminated completely from the section.
13-1270	13	52	21	52	21	References should include Goelzer et al. (2012), ERL, also to be included in Figure 13.10. [Philippe Huybrechts, Belgium]	accepted. Text changed. Numbers added to Figure.
13-1271	13	52	27	52	33	When the model spread is low, the black lines dominate the figure. Thus, the individual contributions are hard to distinguish. [Government of Germany]	accepted. A table was added which provides the number of models for each component and each century.
13-1272	13	52	27	52	33	Due to the different scales, the three panels are hard to compare. [Government of Germany]	Rejected. In order for the reader to see most of the content of the panels we decide to keep different scales. This way the reader has both information the shape of the curves and details and the scale as given on the axes.
13-1273	13	52	27	52	33	Please indicate in the legend or figure caption, what the light blue crosses show (not only in the text below). [Government of Germany]	accepted. Text changed.
13-1274	13	52	27			Figure 13.10: are these numbers for the year 2100, 2200, etc. or for decadal means around these years? [Thomas Stocker/ WGI TSU, Switzerland]	Since the models do not resolve internal variability it was possible to provide numbers for the specific years.
13-1275	13	52	37			Page 52, line 37. Here you say that the ranges cannot be interpreted as uncertainty ranges, yet in line 41 you then yourself confuse the issue by referring to them as uncertainty ranges. This needs to be rectified. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. This was an inconsistency which has been removed.

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13-1276	13	52	48			Page 52, line 48. Insert a comma after “same". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1277	13	52	54			Page 52, line 54. Remove hyphen in “low-spatial". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1278	13	52				Figure 13.10 needs more detailed tick marks the Y axes because the current lack of detail means that no proper values can be read off the axes. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Figure changed.
13-1279	13	53	6			Page 53, line 6. “increasingly less" is pretty poor English. Might this be replaced by “a reduction in". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1280	13	53	10			Figure 13.11: we assume that these numbers for the sea level commitment per degree warming are for an equilibrium situation. This should be clearly stated here and in the text discussing the figure (page 53, l53-55) [Thomas Stocker/ WGI TSU, Switzerland]	accepted. The figure has been changed to show the contribution after 2000years which is now discussed in detail.
13-1281	13	53	11	53	11	Sea level commitment per degree of warming for the Greenland needs to be explained in some details especially about response time, since Figure 13.11 shows an abrupt GMSL increase, which is possible due to the complete melting, when about 1.6°C warming occurs (Robinson et al, 2012). For 2.0°warming, it takes about 50,000 year for the total melting of the Greenland ice sheet to occur (Fig.3b of Robinson et al., 2012). [Sok Kuh Kang, Republic of Korea]	accepted. The figure has been changed to show the contribution after 2000years which is now discussed in detail.
13-1282	13	53	18		26	Given that volcanoes cause a temporary reduction in the rate of SLR and there is the likelihood of future volcanic eruptions over the next few hundred years, should this be acknowledged as another uncertainty? [Kathleen McInnes, Australia]	Rejected. The volcanic eruption do matter on short time scales, but their influence is very small compared the values discussed here. Such a discussion would suggest a higher level of detail than justified by the accuracy of the model results.
13-1283	13	53	20	53	22	Ice model can capture the instability? Really? Does is relieve the shallow-ice approximation or incorporate a Schoof-like grounding line description? Or is the Anarctic contribution from Pollard and De Conto used here, as implied on p. 54 lines 2-3? Please clarify the discussion: which model is used where and is valid for what is very unclear. [Michael Oppenheimer, United States of America]	accepted. While the model is indeed capable of the instability. This was not referenced in the text, which as been corrected. Furthermore it was not clear that a different model was used for the projections until 2500 than for the multi-millennial commitment figure 13.11. This has now been clarified and is discussed in detail.
13-1284	13	53	22	53	22	What is the reason for giving Huybrecht’s model only medium confidence for capturing the timescale? [European Union]	As stated in their publications the model´s representation of the dynamics near the grounding line is not designed for short time scales. This is also reflected in the MISMIP intercomparison projects (Pattyn et al. 2012; 2013). This is now cited in the report.
13-1285	13	53	22			Page 53, line 22. Change “models" to “model's". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1286	13	53	28	53	41	Throughout this paragraph, ECP is used. Should it be RCP? [Government of United States of America]	accepted. Text changed.
13-1287	13	53	28	53	41	IMPORTANT: Page 53, paragraph from line 28 to line 41. Here the quoted ranges do not match up with those in table 13.8. This paragraph needs to be checked very carefully and needs to be made completely consistent with the information in table 13.8. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Table and text have been synchronized.
13-1288	13	53	28	53	41	The basis for several numbers in this paragraph is unclear, i.e., they don't seem to match the numbers reported in Table13.8. 1) Line 28 - '0.56 to 0.74 m', 2) line 34 - '0.35 to 1.71 m', and 3) line 35 - '1.72 to 5.82 m'. Please check carefully, and clarify. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Table and text have been synchronized.
13-1289	13	53	28		41	Since the results of the semi-empirical models are discussed it seems odd that their results are not listed in table 13.9? Their inclusion seems to be an after thought. If it is because their results are too uncertain, then it should be stated that they are not considered to give credible projections? [Kathleen McInnes, Australia]	accepted. A detailed discussion of semi-empirical methods is provided in section 13.5.1.2 for the 21st century. This discussion is now referred to and a

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							comment is added that the confidence in these methods cannot be expected to increase for longer time periods.
13-1290	13	53	28			the range quoted in the text of '0.56 to 0.74 m' differs from that in the table 13.9 for the low scenario by 2300 of 0.59–0.67 [Kathleen McInnes, Australia]	accepted. Table and text have been synchronized.
13-1291	13	53	38	53	38	Please add a statement comparing medium scenarios with the projections for 2100. The numbers in table 13.8 are so low that they seem to be inconsistent with RCP4.5/6 projections for 2100. [Aslak Grinsted, Denmark]	rejected. The figure 13.10 already shows the ranges from Section 13.5.1 for the 21st century and the results are similar. The fact that they are systematically smaller to a certain extent is already discussed.
13-1292	13	53	41	53	41	Further, the confidence in the process models decrease in long-term projections. [Aslak Grinsted, Denmark]	rejected. This statement cannot be made here because there is no evidence for this. The physics captrued in ice sheet models might make them more accurate in predicting long-term changes. As a consequence their ability to project short-term behaviour might be lower than for the longer time scales. As a consequence this statement cannot be supported here.
13-1293	13	53	43	53	48	All models will pretty much agree on the shape of the long term response. Especially i believe that virtually all models will have that the rise in the 22nd century being greater than the rise in the 21st century (even in rcp2.6pd). I think such a statement would be useful if it can be agreed upon. [Aslak Grinsted, Denmark]	rejected. While this would be a useful statement indeed, it is not generally true but depends on the scenario.
13-1294	13	53	43	53	56	I think it would be valuable to also add other types of hi-res paleo records. E.g. Toker et al. 2012, http://dx.doi.org/10.1016/j.epsl.2011.07.019 Jens Morten Hansen et al., Boreas 2011, doi:10.1111/j.1502-3885.2011.00229.x [Aslak Grinsted, Denmark]	Rejected, this is a specific combination of data and simulation that reproduces it.
13-1295	13	53	43			Define GMT [Government of France]	Accepted. The term was been exchanged by the gerneral term SAT.
13-1296	13	53	43			Page 53, line 43. Please write out GMT when used for the 1st time. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Accepted. The term was been exchanged by the gerneral term SAT.
13-1297	13	53	48	53	48	Add references. [Aslak Grinsted, Denmark]	rejected. The text was removed because of comment 13-1298 which correctly stated that this is an assumption made in the semi-empirical approach and not an outcome of the simulations.
13-1298	13	53	48	53	48	I don't think you can cite SE models here - positive SLR due to warming is built into the approach. There is no allowance for growth of Antarctica or reaching thresholds between relatively stable ice sheet configurations for example [Mark Siddall, United Kingdom]	accepted. This is now omitted.
13-1299	13	53	52			Page 53, line 52. Insert “been" before “shown". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1300	13	53	55			reference to figure 10.43 from Solomon et al. 2007: there is no figure 10.43 in AR4, Chapter 10. Probably this is referring to figure 10.34. However, figure 10.34 does include 8 coupled climate models, not 6. So it's unclear if the six should be corrected to 8 or if the estimated thermal expansion in Figure 13.11 has indeed been calculated from a subset of the models presented in AR4 Figure 10.34. If the latter, how the models been selected/excluded? [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.
13-1301	13	53				In figure 13.11, the uncertainties given for MIS 11 do not match those discussed in the text, which instead mentioned 5 to 15 m. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1302	13	54	1	54	1	Typo: "contraint" -> "constrained" [Aslak Grinsted, Denmark]	accepted. Text changed.

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13-1303	13	54	1	54	1	constrained' [Mark Siddall, United Kingdom]	accepted. Text changed.
13-1304	13	54	1	54	9	make sure that this is covered in the paleo chapter in more detail and refer to the paleo chapter [Mark Siddall, United Kingdom]	accepted.
13-1305	13	54	1			where contraint > were constrained [Kathleen McInnes, Australia]	accepted. Text changed.
13-1306	13	54	1			Page 54, line 1. I think “where constraint" needs to be “were constrained". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1307	13	54	3			Page 54, line 3. A “C” is needed in ”Deconto”. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	accepted. Text changed.
13-1308	13	54	11			Units are not given for tables. [Government of United States of America]	accepted. Text changed.
13-1309	13	54	11			Table 13.7: please add unit for the thermal expansion numbers [m] in the header as well as in the table; suggest to refer to Ch12 for the scenarios used (ECP/RCP) [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.
13-1310	13	54	13	54	15	This seems to be rather part of the section body instead of a table caption, since it synthesizes Table 13.7 and Table 13.8. [Government of Germany]	rejected. This brief comparision between the different approaches is considered a useful summary for the table caption.
13-1311	13	54	18	54	22	In contrast with Table 13.5 of P46 (2081-2100 projections), Table 13.8 (2191-2500) projections do not report on the projected contribution of land water storage. The reason for that should be clarified [European Union]	accepted. The reason is that projections do not exist which is now stated in the text.
13-1312	13	54	18	54	22	Show also for 2100, so that we can compare with preceding sections. (It looks unrealistic that the medium rise in 2200 is less than the RCP4.5 at 2100.) [Aslak Grinsted, Denmark]	accepted. Text changed.
13-1313	13	54	18			Table 13.8: Should reiterate the caveat in the text that these models do not attempt to include ice sheet dynamics, only SMB. [Robert Kopp, United States]	accepted. Text changed.
13-1314	13	54	18			Would adding a last column which shows the estimated equilibrium changes be helpful? As Solomon et al. have shown, CO2 has a very long residence time in the atmosphere. [Ronald Stouffer, United States of America]	partically accepted. An additional figure for the sea-level commitment after 2000 years is added with a quasi-linear regression which makes the comparison easy for the reader. Adding it in the table is difficult, because the scenario definition used here would require very large uncertainty ranges.
13-1315	13	54	18			Table 13.8: the table is not referred to anywhere in the text except for the header of Table 13.7; add unit for the thermal expansion numbers [m] in the header as well as in the table; suggest to refer to Ch12 for the scenarios used (ECP/RCP) [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.
13-1316	13	54	18			Table 13.8: Regarding lower end of LOW scenario: The numbers provided here in this table could be confusing as the lower end of the total sea level rise in the LOW scenario is substantially larger than in the MEDIUM scenario. Please consider to expand footnote a) or alternatively expand the discussion in the text in order to clearly identify that only having one simulation for the two ice sheets impacts on the lower bound of the total sea level change for the LOW scenario. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text changed.
13-1317	13	54	19	54	19	Is really Figure 13.11a meant here? [Mirko Orlic, Croatia]	accepted. No it is figure 13.10 which is now corrected.
13-1318	13	54	22	54	22	Please explain, why this approach preferred over showing no results. [Government of Germany]	While we do not have projections for glaciers beyond 2300 the time scale of glacier growth and the fact that temperatures are not assumed to decline significantly in the scenarios make it unlikely that the sea-level contribution decrease beyond 2300. Not showing any contribution would thus lead to a larger error than estimating it to be the value for 2300. This is now explained in the text.
13-1319	13	54				tbl. 13.7 Is it ECP or RCP? [Government of United States of America]	accepted. Text changed to RCP

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13-1320	13	54				tbl. 13.8 The table would benefit by including units. [Government of United States of America]	accepted. Table changed.
13-1321	13	55	11	55	14	terms such as steric, thermosteric and holosteric could be defined in glossary somewhere for clarity. [Government of Kenya]	Noted - they are in Glossary for AR5.
13-1322	13	55	13	56	52	II. Although Chapter 3 can benefit from the additions, in Chapter 13 (&&13.6.1-2) the regional variations of sea-level associated with the halosteric compensation of thermosteric changes based on modelling result are given in details. [Dmitry Aleynik, United Kingdom of Great Britain & Northern Ireland]	Not clear to me what is suggested here.
13-1323	13	55	22			NAO cannot substantially change GMSL can it? Aren't you trying to say here that LOCALLY, these natural modes can cause large deviations from the GMSL change. [Terrence Joyce, United States of America]	This is what we say; the sentence was strengthed.
13-1324	13	55	23			Becker 2012 (the date is incorrect, also in the list of references) [Belén Martín Míguez, Spain]	OK
13-1325	13	55	32	55	32	The name "Sea of Japan" or "Japan Sea" must be used instead of "the East/Japan Sea," because "Sea of Japan (or Japan Sea)" is the only internationally established name for the sea area concerned. [Government of Japan]	OK
13-1326	13	55	32	55	32	5.4±10.3 should read 5.4±0.3. [Sok Kuh Kang, Republic of Korea]	OK
13-1327	13	55	32	55	32	Uncertainty range seems too large. Don't you mean '5.4 +/- 0.3'? [Thomas Stocker/ WGI TSU, Switzerland]	OK
13-1328	13	55	32			Page 55, line 32. There is a very large uncertainty given year, of the order of ± 200%. Is this correct? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Uncertainty range was corrected.
13-1329	13	55	41			Explain figure 13.12a. [Government of United States of America]	Explanation is given in the caption.
13-1330	13	55	42	55	42	"…(up to 8 cm, RMS)" should read "…(up to 80 cm or 0.8m, RMS)", which is seen in Figure 13.12a. [Sok Kuh Kang, Republic of Korea]	Needs correction in the figure.
13-1331	13	55	42	55	42	This section advises regional sea level changes "up to 8cm, RMS" whereas the scale on the associated Figure 13.12 has a scale in metres. This needs to be corrected. I presume the correct scale in the figure should also be cm. [Phil Watson, Australia]	Needs correction in the figure.
13-1332	13	55	47	55	50	This should now be available for a 21-model ensemble (new data will be provided by Mark Carson <mark.carson@zmaw.de>, same for comments nrs 2, 4, 9,10,11). [Aimee Slangen, Netherlands]	Correct, was updated accordingly.
13-1333	13	55	47			Figure 13.12: units displayed are [m] but last sentence in caption indicates (mm). Please correct. [Thomas Stocker/ WGI TSU, Switzerland]	Was corrected.
13-1334	13	55	52			Page 55, line 52. Insert “that" after “indicate". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1335	13	56	10	56	10	Some comment on spatial scales of these structures needed. [European Union]	Not clear if this is requried.
13-1336	13	56	15	56	16	Be careful here - there is no AMOC in the N. Pacific. But in both cases there is a northward shift in the seperation points of the Gulf Stream and Kuroshio which will produce a SL rise. [Terrence Joyce, United States of America]	Not clear what is meant; but we the text was checked for clarity.
13-1337	13	56	19			Page 56, line 19. Change “oceans" to “ocean's". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1338	13	56	25	56	28	21-model ensemble available, with probably slightly changed values in line 28 [Aimee Slangen, Netherlands]	Results were updated.
13-1339	13	56	27	27		Following with CMIP5, in certain cases there is a misspelling and it reads CMPI5 [Belén Martín Míguez, Spain]	OK, was corrected throughout.
13-1340	13	56	27	56	27	change "SREX" to "SRES" [Xuebin Zhang, Australia]	OK
13-1341	13	56	27			Following with CMIP5, in certain cases there is a misspelling and it reads CMPI5 [Belén Martín Míguez, Spain]	OK
13-1342	13	56	27			SREX-> SRES [Kathleen McInnes, Australia]	OK

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13-1343	13	56	27			CMPI = CMIP [Aimee Slangen, Netherlands]	OK
13-1344	13	56	28	56	29	Page 56, lines 28 and 29. “which at least partly, is possible to results from". This is really poor English. Do you mean something like “which may at least partly result from"? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	YES
13-1345	13	56	28	56	30	This sentence is poorly written and contains a number of grammatical errors. Needs rephrasing. [European Union]	Sentence was rephrased.
13-1346	13	56	28			sentence needs rewording [Kathleen McInnes, Australia]	Sentence was rephrased.
13-1347	13	56	29	56	30	Please give a reference for the second half of the sentence. [Government of Germany]	OK
13-1348	13	56	32			Figure 13.13: We note that the abbreviation (RSL) is never actually introduced in the text. Other abbreviations (e.g., GMSL etc) are clearly introduced early in the chapter. Suggest RSL is also introduced, and/or spelt out in the caption. [Thomas Stocker/ WGI TSU, Switzerland]	Correct, was corrected.
13-1349	13	56	34	56	35	21-model ensemble available, likely changing values in l 35 [Aimee Slangen, Netherlands]	Results were updated.
13-1350	13	56	47			I don't recall any discussion of this (evidence for halosteric SL change) in the ocean chapter. Why not? [Terrence Joyce, United States of America]	Probably does not belong there.
13-1351	13	56	54	57	7	Where possible, please indicate the order of magnitude of these effects for the different areas. [Government of Germany]	OK, was done as suggested.
13-1352	13	57	1	57	10	No numbers for air pressure effects? [European Union]	A figure was included.
13-1353	13	57	13			Again, it reads CMPI5 instead of CMIP5 [Belén Martín Míguez, Spain]	OK, corrected.
13-1354	13	57	13			CMPI = CMIP [Aimee Slangen, Netherlands]	OK, corrected.
13-1355	13	57	23	57	23	13.6.4.1. I strongly recommend removing references to Stammer (2008), since this paper comes to erroneous conclusions on timing of sea level change in response to melting Greenland and Antarctic ice. The paper suggests time delays of many decades. Paragraph 47 of this paper concludes: “The corollary of our findings is that melt water dumped into the North Atlantic from Greenland will reside first of all in the Atlantic and will only slowly propagate into the other basins. In particular, it will take a significant length of time until the Pacific Ocean will ‘‘feel’’ this extra volume, for example, in form of sea level rise. This is an important result since it implies that melting of Greenland’s ice cap is much less of a threat to tropical islands in the Pacific than it is for the coasts of North America and Europe.” This erroneous conclusion was reported in the press. The error is due to modeling only the baroclinic changes in water structure, and omitting the actual addition of fresh water. The error has been pointed out (Gower 2010, Lorbacher et al., 2012), but the paper remains part of the public record. By referencing this paper, the IPCC is giving it at least implied endorsement, and associating itself quite unnecessarily with a false conclusion. [James Gower, Canada]	We disagree with this statement. It is entirely clear here that the reference to the paper concerns the steric response of the ocean to Greenland ice melting. This is clearly stated in the text and the paper is relevat for that matter.
13-1356	13	57	25	57	28	Text in paragraph 13.6.4.1 also contributes to the confusion by stating “The adjustment of the ocean to high-latitude meltwater input also involves atmospheric teleconnections which, such as in response to Greenland meltwater pulses, could lead to sea level changes in the Pacific within months (e.g., Stammer et al., 2011).” This again gives the impression that delays of several months will occur before the Pacific feels the extra volume of water. Better wording would be “The adjustment of the ocean to high-latitude meltwater input also involves atmospheric teleconnections which could lead to continuing small sea level changes in the Pacific, months after the occurrence of Greenland meltwater pulses for example (e.g., Stammer et al., 2011).” [James Gower, Canada]	The reference to Stammer et al., 2011 is correct and the paper clearly explaines what it is.
13-1357	13	57	28	57	29	The reverse of the Bering Strait throughflow only occurs when the AMOC is strongly reduced. [Andrey Ganopolski, Germany]	Wording was quecked.
13-1358	13	57	29			Reversal of the Bering Strait throughflow is another ocean effect I don't recall seeing in the oceans chapter!	OK

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						[Terrence Joyce, United States of America]	
13-1359	13	57	33	57	33	Make reference to Chapter 12 here. [Thierry Fichefet, Belgium]	Yes, needs to be double checked.
13-1360	13	57	35	57	35	At line 35 in this paragraph, the word “rise” needs to be inserted after “global mean sea level” to say “The combination of this dynamic sea level rise and the global mean sea level rise makes the northeastern North American coast vulnerable…” [James Gower, Canada]	Was corrected.
13-1361	13	57	39			As mass is transfered from high to low latitudes by melting, what will be the expected change in the LOD? [Terrence Joyce, United States of America]	Don't know.
13-1362	13	57	44	57	44	I don't think this is really true/accurate: "Most studies....". There aren't that many and both Bamber and Riva and Spada et al do not use end members. [Jonathan Bamber, United Kingdom]	Text was revised.
13-1363	13	57	49	57	49	Spada et al (emailed to Gregory, Clark Church) took the analysis further and examined the RSL pattern from future ice melt using scenarios discussed elsewhere in the Chapter. Spada et al, The gravitationally consistent sea–level fingerprint of future terrestrial ice loss, GRL (in press). In addition, they investigated the relative importance of ice melt vs. steric effects spatially using the ECHAM5 and AIB steric data. [Jonathan Bamber, United Kingdom]	Reference was worked in.
13-1364	13	58	2	58	2	Gehrels and Woodworth (2013) suggest that the larger magnitude of the early 20th century sea-level acceleration observed in Australia and New Zealand, as compared with the North Atlantic, may represent a fingerprint of the increased melt contributions of Greenland and Arctic glaciers in the 1930s. [Roland Gehrels, United Kingdom]	Reference was worked in.
13-1365	13	58	2	58	4	Kopp et al. (2010) studied a freshwater hosing experiment, not RCP 8.5; they concluded that "static equilibrium effects [i.e., ice sheet fingerprints] will dominate in most of the ocean when melt exceeds ~20 cm equivalent sea level." [Robert Kopp, United States]	Wording was adjusted.
13-1366	13	58	13	58	13	this is ambiguous and potentially contradictory. You need to include the exceptional sites near the former ice sheets explicitly here to avoid the impression of contradiction with discussion elswhere in the text [Mark Siddall, United Kingdom]	Text was revised.
13-1367	13	58	17			Official publication year for this Slangen et al paper is 2012 [Aimee Slangen, Netherlands]	OK, corrected.
13-1368	13	58	20			Ice sheet mass loss - the ice sheets are not melting away [European Union]	OK
13-1369	13	58	23			This does not make sense: "to mid latitudes due ice-melt fingerprints." [Government of United States of America]	Text revised.
13-1370	13	58	29			for clarity, maybe add ice sheet SMB to explanation of figure 13.14c [Aimee Slangen, Netherlands]	OK
13-1371	13	58	34	58	35	My comments earlier about natural modes affecting local sea level are still true, but here you show how there are local modes to global sea level change - interesting figure! [Terrence Joyce, United States of America]	OK
13-1372	13	58	36			45% and 12% should be changed to: 30% and 9% [Aimee Slangen, Netherlands]	OK, corrected.
13-1373	13	58	42	58	45	Concerning part b); in the graph the colorbar should be labeled % instead of m [Government of Germany]	Correct, was changed.
13-1374	13	58	42	58	45	21-model ensemble available, change values in l.43 [Aimee Slangen, Netherlands]	Corrected.
13-1375	13	58	42			As has been done for the SPM, we suggest to add a map of total sea level rise by 2100, combining all the components contributing to total sea level, i.e., figures 13.12-13.14. [Thomas Stocker/ WGI TSU, Switzerland]	This exactly is Fig. 15a
13-1376	13	58	42			Figure 13.15: the lower panel's units are indicated as meters ("RSL change (m)"), but should be "RSL change (%)" [Thomas Stocker/ WGI TSU, Switzerland]	Was corrected in the figure.
13-1377	13	58	47	58	47	function or distribution? [Mark Siddall, United Kingdom]	Was corrected.
13-1378	13	58	47	58	49	It is not clear, how this figure has been prepared. Especially concerning impacts this figure may be often used.	Explanation was added to the supplementary material.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						A recipe as supplementary material and more explanation in the text and/or caption may be helpful. [Government of Germany]	
13-1379	13	58	47	58	54	It would be helpful for the reader to understand how these model estimates have been applied to coastlines. Do the results refer to all grid boxes that coincide with coasts, or are some other criteria used? Are all AOGCMs transformed to the same grid? [Timothy Carter, Finland]	Explanation was added.
13-1380	13	58	47	58	54	Page 58, lines 47 to 54 and including the caption of figure 13.16. Here we see a range of different abbreviations including RCP, RPC, and ROC. This is confusing and/or incorrect and needs to be fixed. I think all of these should be RCP, as introduced on page 51, line 54. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Was corrected.
13-1381	13	58	47			Page 58, line 47. Remove the apostrophe in PDF's. This needs simply to be PDFs. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1382	13	58	49			Change "The figure shows" to "The figures show". [Robert Dean, United States of America]	OK
13-1383	13	58	52			Page 58, line 52. Change “PDF" to “PDFs". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1384	13	58				Page 58, caption of figure 13.14. Firstly, the caption needs to use letters a, b, and c to point at the various panels. Secondly, I think another panel is needed to account for uncertainties, which at the moment are completely ignored here. There are considerable uncertainties to accounts for; for example due to uncertainties in the different Earth models that can be/have been used, or in the choice of Earth model that was selected. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Respective results were added.
13-1385	13	58				Page 58, caption of figure 13.15. There is a problem in this figure with the legend. In panel b, the legend says RSL change (m), but I think this needs to be a percentage? [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Corrected.
13-1386	13	58				As mentioned before: Page 58, caption of figure 13.14. Firstly, the caption needs to use letters a, b, and c to point at the various panels. Secondly, I think another panel is needed to account for uncertainties, which at the moment are completely ignored here. There are considerable uncertainties to accounts for; for example due to uncertainties in the different Earth models that can be/have been used, or in the choice of Earth model that was selected. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	See 65 above.
13-1387	13	58				In figure 13.15 the legend seems to be incorrect as mentioned before. In panel b, the legend should say percent, I think. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1388	13	59	7	59	16	Page 59, lines 7-16. Firstly, the locations discussed should be shown on a map. Secondly you show 12 records in the figure, yet you discuss only one of those, and then start discussing the record for Palermo (note the absence of an “n” at the end), which is not shown in the figure. The next paragraph, in lines 25 to 29, again discusses records that are not shown in the figure. This presentation makes no sense to me. Show the records that you discuss, and do not show records that you don't discuss. [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	Was changed.
13-1389	13	59	10	59	10	Palermon --> Palermo (?); It is surprising to us that an example is given (Palermon) which is not one of the locations shown in Figure 13.17. [Thomas Stocker/ WGI TSU, Switzerland]	Text was changed.
13-1390	13	59	12	59	12	Does this statement refer to Figure 13.12b instead? [Government of Germany]	Yes.
13-1391	13	59	13			change value of 35 cm to new value based on 21-model ensemble [Aimee Slangen, Netherlands]	OK
13-1392	13	59	16	59	16	Reference to section 13.8 does not seem to be correct - perhaps referring to "Sea level impacts and extreme event" [Michael Davies, Canada]	Was corrected.
13-1393	13	59	16	59	16	but 13.8 refers to variability. Perhaps reference should be to section 13.7 [Michael Davies, Canada]	Corrected.
13-1394	13	59	16	59	16	Substitution is needed: 'Section 13.8' to 'Section 13.7'. [Mirko Orlic, Croatia]	OK

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-1395	13	59	21			21-model ensemble available [Aimee Slangen, Netherlands]	OK
13-1396	13	59	25	59	26	A regional sea level change estimate by Grinsted et al. is cited here: +50 cm in the southern Baltic Sea and -30 cm in the Bay of Bothnia. Similar results are also obtained by Johansson et al. (accepted): +29 cm in Gulf of Finland and -27 cm in the Bay of Bothnia. Reference: Johansson et al.: Global sea level rise scenarios adapted to the Finnish coast. Journal of Marine Systems, http://dx.doi.org/10.1016/j.jmarsys.2012.08.007 [Hilkka Pellikka, Finland]	Was worked into the text.
13-1397	13	59	26	69	26	Isn't it usually the "Gulf of Bothnia", not the Bay of Bothnia? [Neil White, Australia]	OK
13-1398	13	59	26			"Sallenger" is misspelled. [Robert Dean, United States of America]	OK
13-1399	13	59	27	59	28	two times 'increase' [Aimee Slangen, Netherlands]	Corrected.
13-1400	13	59	31			Section 13.6.6: suggest to refer to the relevant Ch9 section (9.4.2.2) with a model evaluation of sea level and ocean heat content. [Thomas Stocker/ WGI TSU, Switzerland]	Yes, good suggestion.
13-1401	13	59	36			The steric uncertainties also appear to be a major problem for global mean sea-levels, not just in the regional patterns, shown by the large model spread in hindcasted SLR in Figure 13.4 A. [Ryan Sriver, United States of America]	OK
13-1402	13	59	38	59	39	There is no reference given for the assertion that "the dynamical response of the ocean to melt-water imput" … is not properly simulated. Does this refer to archaic rigid lid ocean models or models that use a virtual salt flux, or something else. In my experience, if modern ocean models are given a melt-water input, they respond appropriately. This phrase should be clarified, omitted, or a recent and specific reference provided. [Robert Hallberg, United States of America]	Text was expanded accordingly.
13-1403	13	59	42	59	42	Typo. Word “form” should be "from". [Phil Watson, Australia]	Corrected.
13-1404	13	59	42			form = from [Aimee Slangen, Netherlands]	OK
13-1405	13	59	50	59	54	The representation of dense overflows is particularly problematic in many ocean models used for climate studies, with direct impacts on the simulated vertical patterns of ocean heat uptake (Legg et al., 2009, Improving oceanic overflow representation in climate models: The gravity current entrainment climate process team. Bull. Amer. Met. Soc., 90, 657-670, doi:10.1175/2008BAMS2667.1). [Robert Hallberg, United States of America]	Accepted - text revised
13-1406	13	59	50	60	2	suggest to provide some more explanation on the meaning of technical terms such as "rigid lid", "virtual tracer", or "boussinesq approximation" to help the non-expert readers. [Thomas Stocker/ WGI TSU, Switzerland]	Where should this be included?
13-1407	13	59	50	60	24	Also accompanying Fig.13.18 of p107. This figure shows large geographical differences between the 12 AOGCMs (Or ESMs) models in use. Looking at Table 9.1 (p138-156) which describes each model component shows a great variability, with some including e.g. a biogeochemical module and other not. The text commenting the model skills refers to unresolved physical processes, grid resolution, initilizarion and freshwater forcing without referring to the individual model structure per se. For instances it should be interesting to know what explains the difference between the projected geographical sea level. [European Union]	We agree that this would be of interest. The respective information is not available at this point.
13-1408	13	59				In figure 13.16, the Y axes titles should be given (frequency). [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	OK
13-1409	13	60	1	60	1	I recommend adding to the Greatbatch (1994) citation the following paper: Griffies, S.M., and R.J. Greatbatch, 2012: Physical processes that impact the evolution of global mean sea level in ocean climate models, Ocean Modelling, vol. 51, pages 37-72, doi:10.1016/j.ocemod.2012.04.003. This newer paper provides far more details of the adjusment required to diagnose global mean sea level in a Boussinesq model. [Stephen Griffies, United States of America]	OK
13-1410	13	60	1			this is the first mention of the standard a posteriori adjustment of Greatbatch (1994). It needs more elaboration	OK

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						[Kathleen McInnes, Australia]	
13-1411	13	60	6	60	6	The result attributed to Farneti et al. (2010) was originally due by Hallberg and Gnanadesikan (JPO, 2006) using realistic ocean models. Farneti confirmed that this result pertains with coupled models as well. This should be rephrased as "As shown by Hallberg and Gnanadesikan (2006) and Farneti et al. (2010), ...". This result is already correctly attributed in Chapter 3, p. 26, l. 51. The Hallberg & Gnanadesikan 2006 citation can be copied from chapters 3 or 9. [Robert Hallberg, United States of America]	OK
13-1412	13	60	13	60	15	One might add here, "For instance, Hallberg et al. (2012) show that two well-spun-up coupled models that are identical apart from their ocean components differ by only 18% in projections of global steric sea level rise, which is much smaller than the range of variability in the CMIP5 ensemble as a whole." [Robert Hallberg, United States of America]	OK
13-1413	13	60	17	62	47	This section should include the analysis of the storm surge conducted for the Thames Estuary 2100 project which was a fairly comprehensive analysis of surges to support the upgrade of London's tidal defences. Some of this work is in UKCP09 and Jason Lowe can be contacted to find out more -- there are certianly reports, but I cannot guide you to papers off the top of my head. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	OK
13-1414	13	60	20			Official publication year for this Slangen et al paper is 2012 [Aimee Slangen, Netherlands]	OK
13-1415	13	60	24	60	24	Did the study of Martin and Adcroft suggest that locating iceberg melt appropriately made a significant difference? [European Union]	Not sure.
13-1416	13	60	26	60	50	the Guidance note on treatment of uncertainty suggests not to write out the "high confidence" in the assessment if it's supporting a very likely statement. Assigning a likelihood to a statement, in principle, implies high confidence at least [Thomas Stocker/ WGI TSU, Switzerland]	Text was revised.
13-1417	13	60	26			A better section title might be "Summary Assessment of Regional Sea Level Changes" [Government of United States of America]	OK
13-1418	13	60	36			Does not make sense: "level change will remain to have.." [Government of United States of America]	OK, text revised.
13-1419	13	60	36			rephrase 'will remain to have a strong regional pattern' [Kathleen McInnes, Australia]	OK
13-1420	13	60	38	60	38	Ditto [Michael Davies, Canada]	OK
13-1421	13	60	38	60	38	Substitution is needed: 'Section 13.8' to 'Section 13.7'. [Mirko Orlic, Croatia]	OK
13-1422	13	60	38			Add "a" between "be" and "critical" [Robert Dean, United States of America]	OK
13-1423	13	60	44	60	50	Please include time-frames for these summary projection statements. We presume these statements apply for the end of the 21st century, but this is not indicated. [Thomas Stocker/ WGI TSU, Switzerland]	OK
13-1424	13	60	44		50	this sentence is poorly phrased. By implication, sea level rise is positive. Regarding sea level fall near glaciers and ice sheets, what is the time frame over which this result is likely to persist? Presumably net mass addition will eventually lead to increases everywhere even though increases may be smaller in some regions compared to others. [Kathleen McInnes, Australia]	Sentence was rephrased.
13-1425	13	60	44			Change "sea level rise will be positive" to "sea level will rise". [Robert Dean, United States of America]	OK
13-1426	13	60	47	60	49	I don't follow this statistical result - is it because the pdf is skewed? [Terrence Joyce, United States of America]	Text rephrased.
13-1427	13	60	49	60	50	What is this sentence trying to say -- as writen seeems a pointless truism? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Text rephrased.
13-1428	13	60	52	61	5	The Pickering et al (2012) paper belongs here (see earlier comment) [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Not considered. This is a regional/local study. We have included some other studies on changes in tides (mainly based on observations). In fact, there are hardly any studies that relate changes in tides to sea

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							level extremes
13-1429	13	61	1		5	MSL change may also affect the tidal characteristics [Kathleen McInnes, Australia]	will be mentioned
13-1430	13	61	21	61	21	It seems that 'are' is missing before 'based'. [Mirko Orlic, Croatia]	will be included
13-1431	13	61	26			Page 61, line 26. Insert a comma after “century". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	will be incorporated
13-1432	13	61	27			Page 61, line 27. Insert a comma after “MSL". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	will be incorporated
13-1433	13	61	33	61	36	It would be preferable to replace this SREX-based statement on tropical cyclone global frequency and frequency of the most intense storms, with the latest assessment coming out of Chapter 14. [Thomas Stocker/ WGI TSU, Switzerland]	will be modified
13-1434	13	61	42	62	47	Section 13.7.2.2 does not mention sea level rise or storm surge projections from the UKCP09 climate projections (see: http://ukclimateprojections.defra.gov.uk) which were generated using a probabilistic approach. This is a major omission. [European Union]	Noted and included
13-1435	13	61	55			Page 61, line 55. Insert “that" after “found". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	will be incorprated
13-1436	13	62	1			Page 62, line 1. Insert a comma before “although". [Eelco Johan Rohling, United Kingdom of Great Britain & Northern Ireland]	will be incorporated
13-1437	13	62	14		16	suggest the following rephrasing of sentence to 'Similarly, for the tropical east coast of Australia,Harper et al. (2009) found that a 10% increase in tropical cyclone intensity for 2050 led to increases in the 1-in-100 year total sea level (including tides) that at most locations were smaller than 0.1 m. [Kathleen McInnes, Australia]	will be modified
13-1438	13	62	29	62	36	The point about the increase in frequency of currently rare but damaging events is extremely important and deserves emphasis. However, the explanation in the text will not make the figure intelligible to most readers. The nature of the 0.5 m sea level rise also needs to be clarified - global mean or local - as some will say that a place such as Churchill will not see a rise in local relative sea level. [Donald Forbes, Canada]	will be clarified
13-1439	13	62	29	62	36	Can the latest paper (Hunter et al. in review) be included or did it miss the cutoff? [Donald Forbes, Canada]	This paper discusses "Sea level allowance", which is not discussed in the present chapter. Sea level allowance is more appropriate for impact studies, the present chapter deals with the science
13-1440	13	62	35	62	36	Although, I wrote most of this small section myself, I'm not that happy with the phrase “a 0.5 m MSL rise would likely result in the 100-year return period event shifting to a 10-year or even 1-year return period”. Hunter (2012) gave a range of the multiplication factor as 16 to 1600 (over the 198 locations, and based on the +/- one standard deviation range of the inverse of the scale parameter). However, a 100-year return period event becoming a 1-year event represents a multiplication factor of only 100. I suggest changing the phrase slightly to read “a 0.5 m MSL rise would likely result in the 100-year return period event shifting to a 10-year or possibly even less than a 1-year return period”. [John Hunter, Australia]	noted
13-1441	13	62	45			Figure 13.19: Please expand the caption in order to include details about the exponential Gumbel factor. Otherwise the importance of including this specific term is not clear. [Thomas Stocker/ WGI TSU, Switzerland]	will be modified
13-1442	13	62	49	62	57	What about the indirect effect of rising temperature of wave climate at high latitudes -- loss of sea ice which is transforming wave climates by increasing wave seasons and the fetch. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	This point will be mentioned.
13-1443	13	62	49	64	38	Sea state (waves), driven by wind and propagation has very little to do with mean sea level and should be addressed in chapter 3 as regards observations, or even better with surface parameters in chapter 2 (with surface wind), and , as regards projections, with climate projections. [Government of France]	Current sea state is addressed in chapter 3. Sea state projections (whch are considered here) could be included in Chapter 14, however, the decision has been made to link waves and extreme sea levels, as

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							both include a wind-driven component.
13-1444	13	62	49	64	38	Are the number of simulations of wave climate sufficient to understand climagte change -- my experience is that many of these papers are written about limited numbers of ensembles and it is doubtful that they are a menaingful sample of possible future conditions? Do the team agree? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	This point will be addressed.
13-1445	13	62	54			NAO and SAM not defined; define here or add to glossary. [Government of United States of America]	these acronyms will be defined in the TSU compiled acronym list
13-1446	13	62				Figure 13.19: See comment on text above. [Donald Forbes, Canada]	to be included
13-1447	13	63	2			remove the term 'storm projections' from the title because the previous section on storm surges is also implicitly incorporating storm projections. Perhaps alter the title of 13.7.2 to 'Projections of Extreme Sea Levels due to changes in storms and MSL' to indicate the scope of the section as a whole [Kathleen McInnes, Australia]	will be modified
13-1448	13	63	27	63	27	Please introduce the 'SWH' here where it is first used. [Thomas Stocker/ WGI TSU, Switzerland]	these acronyms will be defined in the TSU compiled acronym list
13-1449	13	63	27			SWHs not defined. Define here or add to glossary. [Government of United States of America]	these acronyms will be defined in the TSU compiled acronym list
13-1450	13	63	42	63	42	If the caption is correct, Figure 13.20b shows austral summer (Jan-Mar) instead of winter. [Government of Germany]	corrected
13-1451	13	64	4	64	11	This section is sloppy and needs major rewriting. For details please see the following comments No. 9-14. [Ralf Weisse, Germany]	This section will be modified.
13-1452	13	64	4	64	11	Metrics in the cited studies are not directly comparable as is done here. For example, Leake et al. use annual maxima while Debernhard & Roed and Grabemann & Weisse use annual 99-percentiles. Wheter or not the 99-percentiles are directly comparable depends on the sample from which percentiles have been estimated (e.g. hourly, half-hourly or six-hourly sampling) which is not made clear in all studies. [Ralf Weisse, Germany]	Metric comparisons will be removed.
13-1453	13	64	4	64	11	While all studies mentioned discuss changes in the North Sea the numbers (statements) compared here were partly made for different regions (e.g. off UK coast, German Bight, eastern North Sea). [Ralf Weisse, Germany]	Will be modified.
13-1454	13	64	4	64	11	Some of the statements are nowhere made in the cited studies such as the 21 cm decrease in Leake et al. Please use correct quotation and references. [Ralf Weisse, Germany]	Will be modified.
13-1455	13	64	4	64	11	The statements here suggest major differences between some of the studies although they partly rely on same models and/or scenarios. While this is partly true, the detailed statements here are incorrect. For example, the text reads an insignifcant change in Debernhard and Roed in contrast to a 5-8% increas in Grabemann and Weisse. Grabemann and Weisse do not made statements about statistical significance and the percentage changes provided in Debernhard and Roed appear to be rather similar to those in Grabemann and Weisse. Please correct. [Ralf Weisse, Germany]	Will be modified.
13-1456	13	64	4	64	11	The study of Leake et al. appeared in a hardly accessible conference proceeding. Please replace with a corresponding peer-reviewed reference. [Ralf Weisse, Germany]	Reference will be replaced.
13-1457	13	64	4	64	11	The statements here related to Leake et al. are hard to derive from the material provided there. For example, there are only grey-shading plots which do not allow to retrieve any numbers. Moreover, the only time series provided suggests an increase in wave height rather than a decrease as described here. Please revise accordingly. [Ralf Weisse, Germany]	Reference will be replaced.
13-1458	13	64	40			A better section title might be "Summary Assessment of Sea Level Extremes and Waves." [Government of United States of America]	Noted. This Chapter discusses projections of Extreme Sea level, whereas Chapter 3 discusses observed changes in extreme sea level

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13-1459	13	64	42			Should "very likely" be in italics? [Government of United States of America]	considered
13-1460	13	64	43	64	45	This is oversimplified in this general form. Depending on the distribution the impact of a given sea level rise on return periods may vary considerably. See for example Nicholls et al. 1999, Figure 3, doi:10.1016/S0959-3780(99)00019-9 [Ralf Weisse, Germany]	considered
13-1461	13	64	45	64	47	Extreme sea level at the coast (in relation to risks of flooding) is a more complex issue than implicitly assumed here. [Government of France]	this chapter considers science, but impacts will be dealt with in Working Group II
13-1462	13	64	46	64	46	It's better to replace the word "decide" with some other word such as "affect",because there are also other factors such as the topography or astronomical tides can affect the extremes.. [Ke Xiu LIU, China]	considered
13-1463	13	64	55	65	57	This section is missing any summary practical guidance for endusers. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	This chapter has a focus on sea level and not adaptation issues
13-1464	13	64	55			The synthesis and uncertainties discussion seems incomplete. It is too focused on the contribution of polar ice, and it reads as if this the only remaining uncertainty. I think there should be at least some discussion on uncertainties surrounding changes in future storminess, which is the main driver of flooding events and hence damages due to sea-level rise. Section 13.7 provided a nice summary of the recent work in this area, and it would be good to see some summary in the final section (and its relation to uncertainty). [Ryan Sriver, United States of America]	accepted - section completely rewritten
13-1465	13	65	1	65	1	It seems that 'it' is missing before 'is'. [Mirko Orlic, Croatia]	accepted - section completely rewritten
13-1466	13	65	1	65	8	I consider this a false argument. Few looking at past climates are arguing for them as analogues BUT they can be used against arguments based on existing, still limited, physical understanding which suggest maximum possible rates limited by ice dynamics. Assuming paleo data is reliable (which it may not be) any model of ice sheets/dynamics would need to be able to reproduce those rates under those circumstances. Questions like what drives ice sheet change? is temperature more or less important than insolation? Are there multiple stable states of the ice sheets? What is the background, gross sensitivity of the ice sheets to temperature change? Did the WAIS EVER collapse? Are all appropriate questions for paleo data. WHY state something that paleo cannot do and then say it can't do it? You are creating a straw-man argument and dissassembling it and I can't see the point. If you feel the need to say what it can't do then also show what it can - ongoing GIA calculations require paleo data for callibration for example... the very next paragraph discusses the natural background SLR from paleo data. [Mark Siddall, United Kingdom]	accepted - section completely rewritten
13-1467	13	65	2	65	2	"virtually certain that higher sea level will be experienced during the 21st century...." -> higher than what? Higher than current levels? Higher than the paleo levels mentioned in the previous sentence? Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	accepted - section completely rewritten
13-1468	13	65	2			higher sea level - I assume global. If so, add "global" before "sea level". [Ronald Stouffer, United States of America]	accepted - section completely rewritten
13-1469	13	65	3			Paleodata, of course, are usefull but it very unlikely that they provide any direct analogue for any future [Andrey Ganopolski, Germany]	accepted - section completely rewritten
13-1470	13	65	7	65	8	there is very little understanding …. - This seems way too broad and sweeping. You assigned a high confisdence for the SLR during this period in section 13.2.1.3. These are inconsistent. [Ronald Stouffer, United States of America]	accepted - section completely rewritten
13-1471	13	65	10	65	11	This relates to my comment 4. The single palaeo-record in Fig. 13.21 cannot "clearly" indicate that the rate of global sea-level rise has increased. [Roland Gehrels, United Kingdom]	accepted - section completely rewritten
13-1472	13	65	10	65	18	Need to caution about inferring comparability of sea level rise rates over periods that that are not directly comparable. Paleo and 20th century rates are likely comparable. But century timescale rates and altimetry era rates are not directly comparable as the latter is highly likely to contain bias due to inter-decadal variability. [Phil Watson, Australia]	accepted - section completely rewritten

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13-1473	13	65	12	65	18	It is a rather risky to base future sea-level predictions on models based on observations alone. Why do you bring in such models. As there are no processes incorporated, the do nothing in explaining why and how sea levels change in the future!!! This does not belong in an IPCC report! [Charlotte Sparrenbom, Sweden]	This comment looks either misplaced or a misinterpretation - text revised.
13-1474	13	65	21			Figure 13.21: suggest to add the number of models used to calculate the multi-model CMIP5 mean and range. This does substantially effect the results for different scenarios and thus should probably be added to all figures (or captions) showing CMIP5 based results [Thomas Stocker/ WGI TSU, Switzerland]	section completely rewritten
13-1475	13	65	25	65	25	Except possibly in the 1930s? [Roland Gehrels, United Kingdom]	section completely rewritten
13-1476	13	65	25	65	37	This is almost a distorted discussion here on the ice sheets because the consensus of SMB assessments indicate AIS will contribute to sea level fall over the course of the 21st century? [Phil Watson, Australia]	section completely rewritten
13-1477	13	65	27	65	27	Attributing acceleration to all of Antarctica is far too general. At the very least it is West Antarctica as in the SOD Ch 4. King et al. 2012 limit statistically significant results from GRACE to just the basin containing Pine Is Glacier. Flament and Remi, 2012 show the front of Thwaites glacier to be accelerating. Flament, T. and F. Remy 2012. Dynamic thinning of Antarctic glaciers from along-track repeat radar altimetry. Journal of Glaciology, 58(211): 830-840 doi:10.3189/2012JoG11J118. [Matt King, Australia]	section completely rewritten
13-1478	13	65	30	65	34	Indicate where the lack of understanding is and how it might impact results. How might poorly represented processes affect ice-shelf viability, and thereby computed rates of grounding-line motion? [European Union]	section completely rewritten
13-1479	13	65	34	65	36	See comment above …… why semi-emprical models? Unscientific in a climate perspective!!!! [Charlotte Sparrenbom, Sweden]	section completely rewritten
13-1480	13	65	39	65	40	sentence not clear in meaning [Government of Kenya]	section completely rewritten
13-1481	13	65	39	65	45	The text should be more specific regarding the timeframe for a "sea level rise of metres" [Government of United States of America]	section completely rewritten
13-1482	13	65	40			should this say "with the amount of sea level rise dependent on the total amount of future greenhouse gas emissions", to reflect the key dependence on the cumulated emissions rather than any particular temporal evolution? [Thomas Stocker/ WGI TSU, Switzerland]	section completely rewritten
13-1483	13	65	43	65	45	"While the loss of the Greenland Ice Sheet is not inevitable, a significant decay of the ice sheet may be irreversible on millennial time scales." If this is the case, then the IGM and the ice decay thereafter, might not be a bad comaprison after all?! [Charlotte Sparrenbom, Sweden]	section completely rewritten
13-1484	13	65	51	65	52	It is not fair to say that there is "little understanding of the regional sea level rise projection patterns" - because you have been through quite alot of processes - you just have not gone into detail in each region , which would not be an impossible task to perform with todays knowledge! [Charlotte Sparrenbom, Sweden]	section completely rewritten
13-1485	13	65	55			What does intensity mean in this context? Suggest delete as increased frequency is enough. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	section completely rewritten
13-1486	13	66	50			The reference needs editing (correct year of publication, 2012) [Belén Martín Míguez, Spain]	Accepted.
13-1487	13	67				FAQ 13.2 Figure 1: The caption refers to West Antartica and some of East Antarctica. General reader smight not know which part of Antarctica is West Antarctica and which is East. I suggest adding some labels to the map of Antarctica indicating West Antarctica, East Antarctica, and also the Antarctic Penisular (which is refered to in the FAQ text). [David Wratt, New Zealand]	Noted. This figure will likely no longer be used.
13-1488	13	70	3	70	4	Gehrels and Woodworth is now published: Gehrels, W.R. and Woodworth, P.L. (2013). When did modern rates of sea-level rise start? Global and Planetary Change 100, 263-277, doi:10.1016/j.gloplacha.2012.10.020. [Roland Gehrels, United Kingdom]	Noted.
13-1489	13	70	31	70	32	Goelzer et al (submitted) has been resubmitted to Journal of Glaciology because of a potential conflict of interest in the open review process of The Cryosphere [Philippe Huybrechts, Belgium]	Taken into account.
13-1490	13	70	33	70	33	Reference list should include: Goelzer, H., Huybrechts, P., Raper, S. C. B., Loutre, M. F., Goosse, H. and	Accepted.

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						Fichefet, T. (2012). Millennial total sea level commitments projected with the Earth system model of intermediate complexity LOVECLIM. Environmental Research Letters, 7, 045401, doi:10.1088/1748-9326/7/4/045401 [Philippe Huybrechts, Belgium]	
13-1491	13	72	20	72	20	natural' [Mark Siddall, United Kingdom]	Accepted
13-1492	13	73	60	73	61	Typo in Author names: Langen, Solgaard, and Hvidberg. [Aslak Grinsted, Denmark]	Accepted
13-1493	13	81	50			pages of the reference must be corrected [Government of Brazil]	Accepted
13-1494	13	83	8	83	8	I'm not sure that “spin-up drift” is the correct term here - “spin up” and “drift” are normally considered to be different things - “spin up” ultimately dies out, while “drift” can carry on for ever. I'd omit the word “spin-up”, or alternatively replace “spin-up drift” with “spin up and drift”. [John Hunter, Australia]	Accepted.
13-1495	13	83	8	83	9	The phrase "the effect on thermal expansion of climate model spin-up drift in deep-ocean temperatures" is unclear. Does this mean "the thermal expansion due to spin-up related drift in the climate model's deep-ocean temperatures" or is this a statement about the drift (and bias) in the temperature-dependent thermal expansion coefficients? [Robert Hallberg, United States of America]	Accepted.
13-1496	13	83	9	83	9	...temperatures.' reference needed [Mark Siddall, United Kingdom]	Accepted. [Not really needed, I think, because it is standard procedure, but I will add some]
13-1497	13	83	14	83	15	I don't understand the meaning of “with high confidence” and why it is included. Either the “5–95% interval” is defined as the “likely range” or it isn't - “with high confidence” has nothing to do with it. [John Hunter, Australia]	Taken into account by deleting the phrase. Confidence is treated elsewhere. In fact the statement follows the uncertainty guidance but if confidence is not stated, "high" or "very high" is assumed, so it is OK to omit it.
13-1498	13	83		83		What goes into the likely ranges for each of the individual SLR components (fig 13.8 and Appendix A)? It looks like thermosteric is a 5-95% model-uncertainty range. Glaciers and SMB are similar, but with far fewer models (which might suggest a wider range of uncertainty). It is hard to see how these map onto the official IPCC uncertainty guidance and what the "sum" range indicates. As indicated in my comment on the ES, can pdf's with different confidence be combined? If so, what confidence is given to the cumulative pdf? [Christopher Little, United States of America]	Taken into account by presenting an explicit assessment of the confidence for each component in the relevant subsections and of the sum in 13.5.3.
13-1499	13	84	1	84	2	I don't understand the meaning of the phrase “using a random and uniform linear weight”. Does this mean that the PDF used was a uniform (or “boxcar”) distribution bounded by the minimum and maximum at each time during the projection? (I assume it does). This needs to be explained more clearly. [John Hunter, Australia]	Taken into account by rephrasing. The reviewer's interpretation is correct.
13-1500	13	85	1	110	8	In many cases the figure captions do not have sources or a statement of how developed so they do not work as standalone. I have not checked, but some of the figures appear to come from published papers like Figure 13.19 by Hunter. This needs to be checked and I think that all figures should "work" in themselves. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Sources need to added where appropriate and we believe it is correct to use published figures where appropriate
13-1501	13	86	1	86	10	I suggest to separate ocean properties and circulation in the figure [Mark Siddall, United Kingdom]	done
13-1502	13	86	6	86	6	It looks as though this figure is for global sea level and not regional sea-level. Is this correct? Make clear in caption, or consider elaborating figure. [European Union]	added wording to include regiona sea level
13-1503	13	86	6	86	8	trim figure caption [Government of Kenya]	Noted - although unclear what "trim" refers to (shorten?).
13-1504	13	86				Figure 13.1: The chapter's text refers to water impoundment on land. Hence I suggest depicting a hydroelectric dam and reservoir in the mountainous region of the continent (left part of the diagram). [Denis Gilbert, Canada]	Rejected - that is not the focus of the figure
13-1505	13	86				In Figure 13.1, not all processes are included.For example, isostatic deformation is not showed. It should be include all the processes and differentiate the effect on local-regional or on global; on the relative sea level,	We chose to keep the figure simple to display the most important issues

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						or on global sea level, or on geocentric sea level [José Daniel Pabon-Caicedo, Colombia]	
13-1506	13	87	1	87	11	I like Figure 13.2 defining an explicit road map and with the link to WG II explictly defined. However, it is an incomplete accounting of the relative sea-level change processes that are needed for impact assessment. I have raised comments about this omission earlier. Here either the caption needs to be modified to mention that these elements are missing, or the figure should be modified to be comprehensive, and acknowledge that some processes are not considered in this chapter. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Rejected This chapter is about sea level change and not impacts
13-1507	13	87	4	87	9	This figure is a great figure in terms of processes and as a pointer to sections in the various chapters. However it has no pointer to detection and attribution, and the forcings that drive the components/processes in this figure. It would seem to me to that it should point also to parts of Chapter 10 (Section 10.4.1 and 10.4.3, and parts of Section 10.5.2) and perhaps parts of chapter 8. [Nathaniel Lee Bindoff, Australia]	Rejected- the figure is already complex and this would add to the complexity
13-1508	13	87	4			Figure 13.2: "Gravity and solid earth effects" should have "land ice" as an input. [Robert Kopp, United States]	Accepted - added
13-1509	13	87				Figure 13.2: In the middle box of the top row, we read O: 4.2 whereas this should be O: 4.4. It turns out that section 4.2 deals with sea ice which is not part of land ice. [Denis Gilbert, Canada]	Corrected, Thank you
13-1510	13	88	1	88	16	are the purple and magenta a little too close on colour. Could the colour and symbol be varied (i.e. Diamonds for Tump point, circles for the other)? [Mark Siddall, United Kingdom]	Taken into account - colors will be changed.
13-1511	13	88	4	88	14	This figure needs some more clarification and modification. The North Carolina record (a) is a local proxy-based record, one of many that are available from around the world. There is no reason why this record is 'special'. In (d) the record is plotted with global data sets, implying some global 'validity'. However, it remains a local relative sea-level record, so is of no global sigficance whatsoever and plotting the record with the altimeter and tide-gauge datasets is misleading and incorrect. [Roland Gehrels, United Kingdom]	Taken into account - the figure is being extensively revised.
13-1512	13	88	7	88	8	this is all very poetic but I am not sure that 'twighlight blue', 'autumn orange' and 'light green line' are better than plain old 'blue', 'orange' and 'green' [Mark Siddall, United Kingdom]	Taken into account by referring to better names for colors.
13-1513	13	88	13	88	14	It is not at all clear why the paleo data are not zeroed in the same way as the other data. This would be more objective and consistent [Mark Siddall, United Kingdom]	Taken into account - the figure is being extensively revised.
13-1514	13	88				Figure13.3 (a) and (d) The color difference between the following items should be much clearer. "Tump Point" and "Sand Point" [Government of Japan]	Taken into account - colors will be changed.
13-1515	13	88				fig. 13.3 Spell out "GIA" in (a) [Government of United States of America]	Accepted.
13-1516	13	88				The very recent downturn in mean sea level from altimetry - do we understand where that comes from? I couldn't find a lot of discussion in the text (did I miss it?) is it credible? Is it consistent with earlier bumps eg the slowdown in the late 90s? [Gabriele Hegerl, United Kingdom]	The 2010-11 dip now explained in the text.
13-1517	13	89	1	89	1	This Report desperately needs REAL graphs of >100 year sea level measurements from high-quality tide gauges, not useless thumbnails! [David Burton, United States of America]	These are discussed inChapter 3
13-1518	13	89	1	89	1	I think it would be useful to include a vertical marker on each of the line graphs in the upper panel indicating the start of the period that is covered in the central panel. Otherwise readers might spend time wondering why the long term tide gauge trend at a place like San Francisco is positive, while the trend shown in the map is negative. It is only after one realizes that the periods of record are different that it becomes apparent that there is no inconsistency. [Francis Zwiers, Canada]	Not changed
13-1519	13	89	1	89	12	FAQ Figure is good, but in a black and white copy the satellite data is not apparent -- please check. [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	OK
13-1520	13	89	2	89	2	Fig 1b. Why show RSL patterns from arbitrary and essentially meaningless rates and spatial distributions of mass loss. It would be far more instructive and useful to show the RSL for realistic patterns of present-day mass loss. [Jonathan Bamber, United Kingdom]	The figure is to communicate the idea.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-1521	13	89	5	89	5	It took me too long to work out that the sentence refers to the map. I suggest 'Map of the mean rates of change...' [Mark Siddall, United Kingdom]	OK
13-1522	13	89	8	89	8	Citation needed for model output, and some indication of what processes were included in the model [European Union]	OK
13-1523	13	89	9	89	9	The latest measurements indicate that the West Antarctic Ice Sheet is not losing 0.5 mm/yr SLE, but is, in fact, gaining ice mass; net mass gain for all of Antarctica is measured at about 0.14 SLE. See Zwally et al: http://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/20120013495_2012013235.pdf [David Burton, United States of America]	The most autoritative paper is Shepherd et al. 2012
13-1524	13	89		89		If FAQ13.1 Fig 1b were to show the total sea level rise predicted by models, instead of just effects of ice sheet melting, we would have an image to compare with Figure 1a, showing that indeed almost all the structure shown in Figure 1a is bogus (as Figure 1b already suggests). [James Gower, Canada]	Figure 1a is dominated by natural variability, as discussed int he text.
13-1525	13	89		89		If FAQ13.1 Fig 1b were to show the total sea level rise predicted by models, instead of just effects of ice sheet melting, we would have an image to compare with Figure 1a, showing that indeed almost all the structure shown in Figure 1a is bogus (as Figure 1b already suggests). [James Gower, Canada]	Figure 1a is dominated by natural variability, as discussed int he text.
13-1526	13	89				fig. faq 13.1 Data from 1993-2010 not shown in graphs, confusing. What year is (b)? No tick marks in (a) graphs for year. [Government of United States of America]	OK
13-1527	13	90	1	90	23	the text is too small. I suggest that this figure takes the whole page with the caption on the following or preceeding page. [Mark Siddall, United Kingdom]	Accepted
13-1528	13	90	1	90	23	Figure 13.4 would also benefit from a comparative plot of a semi-empirical model in addition to the outputs from process based models only. [Phil Watson, Australia]	Noted - a new figure comparing SEM with process-based results is being prepared for elsewhere in the report
13-1529	13	90	18	90	18	Figure 13.4, "(g) Trends in sea level calculated over 18 year periods…" is likely to be replaced by "Trends of 18 year moving averaged sea level". [Sok Kuh Kang, Republic of Korea]	Rejected - figure redrawn
13-1530	13	90	19	90	19	The red dot remarked in "altimetry (red dot)" is not seen or not clearly shown, in Figure 13.4(g). [Sok Kuh Kang, Republic of Korea]	Noted figure redrawn
13-1531	13	90				Figure 13.4: At the very top of the diagram, I suggest adding the same year labels as those that appear on the bottom axis of the diagram. This will improve readability. [Denis Gilbert, Canada]	accepted - year added
13-1532	13	90				Figure 13.4: We are missing a text label for the solid black curve in panel d) [Denis Gilbert, Canada]	Accepted figure redrawn
13-1533	13	90				fig. 13.4 The background grey lines in panels, especially a) and f) are too light to be of visual practical use in interpeting the graphs. In addtion , some of the graphs (d and f) have lines that are not labeled (although they are explained in the caption) [Government of United States of America]	accepted - figure redrawn
13-1534	13	90				Fig. 13.4 Watch the tick marks, they don't align with the numbers. [Stefan Rahmstorf, Germany]	corrrected - figure redrawn
13-1535	13	91	1	91	2	odd choice to change the aspect ratio in this way. These clearly need to be a column of figures (1 across, 3 down) to maintain equivalent aspect ratios and scales [Mark Siddall, United Kingdom]	Agreed - redraawn
13-1536	13	91	1	91	13	Figure 13.5, panel (c) would also benefit from a comparative plot of a semi-empirical model in addition to the outputs from process based models and altimetry. Similarly this panel has a "historical" in the key that makes little sense with no further clarification in the associated notes. I suggest this requires further clarification. Does the shading indicate the 95%CI if so, advise accordingly. Further, if I am interpreting this figure correctly, both the satellite altimetry and average of the global tide gauge network indicate SLR over the period between 1993-2010 is currently above the 4 RCP projections? This is a key point that should be highlighted with greater clarity in the summary of the chapter and key points. [Phil Watson, Australia]	Noted - a new figure on SEM is being prepared but not a comparison to the results here. Figure and caption revised.
13-1537	13	91	1			Figure 13.5 C. Why show all the different RCP scenarios for the hindcast? They all use the same historic forcings. [Ryan Sriver, United States of America]	figure redrawn

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13-1538	13	91	5	91	5	It seems that '1993' should be substituted by '1985'. [Mirko Orlic, Croatia]	figure redrawn
13-1539	13	91				fig. 13.5 Spell out SLE in graph and use larger fonts. Define "historical." [Government of United States of America]	figure redrawn
13-1540	13	91				fig. 13.5 The figure caption says the data cover from 1993 - 2010, however many of the time series plotted, and the x-axis covers, a time period starting prior to 1993 (a bit confusing to the reader) [Government of United States of America]	figure redrawn
13-1541	13	91				Figure 13.5 (c): I assume that the continuous black line are the model projections prior to 2005, when they are forced by observed gas concentrations – the caption needs to say this (just adding the word “forcing” after “historical” in the key would probably do this adequately). [John Hunter, Australia]	figure redrawn
13-1542	13	91				Figure 13.5 (c): It is worth stating in the caption that the dotted model projections almost exactly overlay each other (as one would expect early on in the projections) in order to save the reader having to search for them. [John Hunter, Australia]	figure redrawn
13-1543	13	91				Figure 13.5 (c): I assume that “tide gauges” means “tide-gauge reconstruction” (i.e. using altimeters in order to derive appropriate basis functions for the “interpolation” of tide-gauge data) – I assume that “tide gauges” is used with this meaning throughout the Chapter, in which case the issue needs to be addressed early on in the Chapter – solutions would be to redefine “tide gauges” (in this sense) to “tide-gauge reconstruction” (or similar) throughout, or to define “tide gauges” to mean “tide-gauge reconstruction” throughout – I prefer the former solution.. [John Hunter, Australia]	figure redrawn
13-1544	13	92				fig. 13.6 Define y-axis labels [Government of United States of America]	Accepted
13-1545	13	93				Box 13.1, Fig 1 The five colored captions in the upper left of Fig 1b of Box 13.1 appear to refer to two sets of lines and three colored regions. This is potentially confusing. Further, the "land + atmos + ice" region is too small to be legible. This figure might be clearer if only lines were used, and no colored regions. This seems to work well for other figures in this section. [Government of United States of America]	Figure revised
13-1546	13	93				Fig 1. Suggest bands around each line to represent uncertainty in each of the cumulative forcings, and the total forcing. This seems essential in part b of the figure in which the cumulative forcing is used to infer something about planetary energy balance. I would t think this would require three versions of figure b, one with the central value of forcing as shown, one with the high limit, one with the low limit. Some care would need to be given to retaining the same sense of uncertainty in forcing as in alpha; the outrigger values of alpha correspond to the "likely" range of climate sensitivity (essentially ± 1 sigma, if gaussian distributed) so would want to do the same for forcings. My guess is that the combined uncertainty will greatly weaken the conclusion at page 13-26, lines 11-13: "Over the period from 1970 to 2012, this residual is small, less than 0.2 W m–2, and is consistent with a climate sensitivity well within the range of climate sensitivities of 2.0°C–4.5°C" but that remains to be seen. [Stephen E Schwartz, United States of America]	Agreed - uncertainties added.
13-1547	13	93				Fig 1. The text refers to heating rate in W m-2. This, together with the abscissa of the figure being years, calls for the unit of the energy to be in W yr m-2, not Joules per planet as given; that way the slope corresponds to W m-2. Why make matters difficult for the reader? Suggest put units in W yr m-2 (and have an auxiliary axis in J, if you wish). Suggest as well to draw some lines of constant slope on the figure corresponding to -0.2 +0.2m +0.5, +1 W m-2. [Stephen E Schwartz, United States of America]	Not done - but significnt changes to the figure completed.
13-1548	13	94	1	94	2	I find it hard to attibute the various approaches to the authors using these figures [Mark Siddall, United Kingdom]	Taken into Account. This figure is being revised.
13-1549	13	94	4	94	4	remove 'from glaciers' [Mark Siddall, United Kingdom]	Accepted. Will correct.
13-1550	13	94				fig. 13.7 Is there a reference to figure 13.7 in the nearby text discussion? [Government of United States of America]	Accepted. Will insert Figure reference.
13-1551	13	95	4	95	4	Caption needs much more explanation, in particular the fact that flux increases with thickness to some high power. It also skips round the issue of buttressing; this is pretty important, as the melt-ponds and ocean melt only play a modulating role when buttressing is present. [European Union]	Processes described in text

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13-1552	13	95				Box 13.2, Fig.1, No figure caption - what is Q? presumably an ice flux. [European Union]	Processes described in text
13-1553	13	95				fig. box 13.2 No explanation of Q in text or figure. No explanation of a, b, c or d. [Government of United States of America]	Processes described in text
13-1554	13	96	4	96	4	Citation? – is there a policy not to put them in captions? [European Union]	Not aware of it
13-1555	13	96				fig. FAQ 13.2 What does zero elevation mean? No reference. What time frame? [Government of United States of America]	Figure redrawn
13-1556	13	97	1	97	1	Fig 13.8. This relates primarily to comment number 15 and 16. This figure will be reproduced many times in many places by many authors and it is important, therefore, that the fundamental messages portrayed in the figure are correct and not misleading. As presented, for reasons explained in comment 16, I believe this plot is misleading. It implies that the likely uncertainty range for some ice sheet SMB and D are smaller than either glacier or steric uncertainty ranges for of all scenarios. Given the much larger signals per unit warming shown in Fig 13.11 this makes no sense. For the ice sheets the commitment is an order magnitude larger than glaciers. [Jonathan Bamber, United Kingdom]	Rejected - our judgment is that for the 21st century the glaciers are *likely* to make a larger contribution. This may be differnt if a *very likely* range was specified
13-1557	13	97				Figure 13.8: What is the reason for showing the A1B scenario? Certainly it was most often used. Alternatively one could show the spread from all AR4 scenarios derived with CMIP5. I understand that such data might not be available. In this case the figure caption should highlight that A1B was randomly selected and that the projection was derived with CMIP5. [Thomas Wahl, Germany]	For comparison
13-1558	13	97				Figure 13.9: Labels (a) and (b) are missing and I can't find A1B as mentioned in the caption. [Thomas Wahl, Germany]	a and b added
13-1559	13	97				Figures 13.12 to 13.15:What happened to the Mediterranean, Baltic and Black Sea? From the Slangen et al. paper I understood that the model resolution was to coarse and that not all dnyamical processes in these basins were accurately repoduced by the models. This should be mentioned somewhere in the text and/or figure captions. [Thomas Wahl, Germany]	Mediterranean now included
13-1560	13	98	1	98	9	"(a)" and "(b)" are not marked in Figure 13.9. [Ke Xiu LIU, China]	a and b added
13-1561	13	98	2	98	9	Figure 13.9 indicates in the associated notes panels (a) and (b), but they are denoted as (1) and (2). The associated notes indicate also that the four RCP scenarios and scenario SRES A1B are depicted, but there is no charts of scenario SRES A1B? [Phil Watson, Australia]	a and b added
13-1562	13	98	5	98	7	Figure 13.9 does not have (a) and (b) characters inside the figure. [Sok Kuh Kang, Republic of Korea]	A and B added
13-1563	13	98	6	98	6	should delete "scenario SRES A1B", which is not shown in this figure. [Xuebin Zhang, Australia]	deleted
13-1564	13	98				Figure 13.9. This is the basis for my main concern with the report. As noted, the trends start at 4 mm/yr, greater than the 3.2 mm/yr from the satellites. Yet earlier in the report (Chapter 3), there is reservation expressed regarding whether the 3.2 mm/yr will be sustained or is part of an oscillation. Chapter 3, Page 32, line 49 "It is likely that a rate comparable to that since 1993 occurred between 1930 and 1950, possibly due to a multi-decadal climate oscillation, as individual tide gauges around the world and all reconstructions of GMSL show increased rates of sea level rise during this period.". Also, Chapter 13, Page 14, Line 23 "Chapter 3 discusses the significance of this higher GMSL trend since 1993 compared to mean rates over previous decades. It concludes that there is high confidence that this higher rate, which is also seen in tide gauge data over the same period, is real but does not necessarily reflect a recent acceleration, considering the previously reported multi-decadal oscillations of the mean sea level." Although it is probably too late in the process to address this concern (if you believe it valid), In my view, the report would be much more solid if this apparent mismatch were not present. I have read the explanation presented in Chapter 13, Page 45, Line 42. [Robert Dean, United States of America]	This is now discussed in the text. The recent increase seems to be ar esponse to radiative forcing and increased ice sheet flos and not part of a natural cycle. There is better agreement between the projections and the observations now.
13-1565	13	98				fig. 13.9 a and b not defined in figure. [Government of United States of America]	a and b added
13-1566	13	98				fig. 13.9 It is not readily clear that the top panels refer to a) and the bottom panels refer to b) - can the graphic be amended to label the top 4 and the bottom 4? [Government of United States of America]	a dn b added

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13-1567	13	99	1	99	12	Figure 13.10 is a good figure . However, I believe that the recent Meehl et al (2012) Nature Climate Change paper has bigger changes? [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	accepted. It is now included.
13-1568	13	99	1			Fig.13.10 Very useful figure. Coloured bars and caption are not in the same order. Bar for glaciers hard to distinguish as colour is hidden by thick black bars indicating individual model simulations. Grey shaded line in caption appears to be light blue on the figure. It would be possible to add something on 400 PPMV scenarios in line with RCP2.6. Preferably in the figure otherwise in the accompanying text. [European Union]	Partially accepted. The figure has been adjusted to be clearer. Due to the lack of available scenarios especially for the ice sheets, no additional scenario below 400ppmv can be added.
13-1569	13	99				fig. 13.10 This figure isery confusing and hard to read figure; suggest that it be reformatted. [Government of United States of America]	Partially accepted. The figure has been adjusted to be clearer.
13-1570	13	99				Fig. 13.10: this should also include multi-century projections of semi-empirical models, e.g. Schaeffer et al. (Nature Climate Change 2012) [Stefan Rahmstorf, Germany]	Rejected. It is explained in the text (in accordance with statements by the authors of the paper in the respective papers) that the confidence semi-empirical projections for multi-centennial time scales is low.
13-1571	13	100	1	100	2	Fig 13.11b is inconsistent with the estimated total volume stored in glaciers. At T=4 the minimum of the plotted range is 0.4m. This is a loss of more ice than the lower uncertainty bound of the total volume stored in glaciers and ice caps. This makes me believe that the initial volume used in the models for this subplot is based 100% on Radic and Hocks volume estimate. However, Grinsted 2012 (the cryosphere discussions) and Huss and Farinotti (2012) both estimate a lower volume stored in glaciers. [Aslak Grinsted, Denmark]	rejected. The current estimate of total glacier volume is above 0.4m as stated in the chapter.
13-1572	13	100	1	100	10	Again Figure 13.11 is excellent. But is the caption complete -- should it say the "Total" commitment "at equilibrium". While this is important to know, from a practical impact and adaptation viewpoint the transient informtation in Figure 13.10 is more important. (I am not suggesting to remove the figure). [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	accepted. The caption has been expanded.
13-1573	13	100	1	100	12	this is a nice figure to include BUT it contains no WAIS threshold and no scope for multiple stable states for Greenland - e.g. The reasonable results which suggest two domes for Greenland during the LIG. Furthermore, RCP8.5 goes well beyond this level of warming [Mark Siddall, United Kingdom]	acceptec. This is now better explained in the text. The commitment was derived from transient simulations and thus the commitment needs to be understood as a multi-millennial commitment. On these time scales the WAIS contribution including the uncertainty in the forcing threshold is included in the continuous curve shown. Another figure for the commitment after 2000years is added.
13-1574	13	100	1			Fig.13.11, Which simulations were used to make this figure? The sea level change as a function of temperature change for Greenland shown in panel (c) has a step change around 1.5 C, yet section 13.4.3.3, page 13-35, lines 5-14 suggests that a temperature of 2.5 - 3. [European Union]	accecpted. This is now explained in more detail in the text.
13-1575	13	100	2	100	2	Interesting that there is a possibility that Antarctica eventually will contribute with more than 2 metres of sea level rise with no further warming. [Aslak Grinsted, Denmark]	accepted. This is interesting.
13-1576	13	100	2	100	2	Please add errorbars on the slope estimates in fig 13.11d and 13.11e [Aslak Grinsted, Denmark]	rejected. The uncertainty is clearly show in the figure.
13-1577	13	100	2	100	2	Fig 13.11d: Would it be possible to put an LIG paleo estimate on this plot. [Aslak Grinsted, Denmark]	rejected. The estimates for Antarctica alone are highly uncertain.
13-1578	13	100	5	100	5	There is greater uncertainty in the threshold location than what is indicated in fig13.11c). [Aslak Grinsted, Denmark]	accepted. The full range of model spread is now shown.
13-1579	13	100	5	100	5	You could add an observational contraints from LIG to figure 13.11c - Make sure it is consistent with the paleo chapter. [Aslak Grinsted, Denmark]	rejected. The estimates for Greenland alone are highly uncertain.
13-1580	13	100	5	100	5	From 13.11c it looks as if the threshold os crossed then you will certainly get atleast 6m. I believe there is more uncertainty in this number. Please double-check the 6m estimate against published commitment experiments. Atleast from our unpublished ensemble then we have a few models where we cross the threshold but where only ~60% of the volume is lost after 20k years. [Aslak Grinsted, Denmark]	accepted. This will be dealt with.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-1581	13	100	5	100	5	How certain is the pre-threshold slope in figure 13.11c [Aslak Grinsted, Denmark]	The uncertainty is reflected by the model spread.
13-1582	13	100				This is a fascinating figure. However, i was wondering (particularly when showing to and discussing with our palaeo expert in my research group) to what extent the uncertainties were accounted for. For example, do we really have such a narrow uncertainty around the greenland melting as shown in the green panel, and in Palaeo as shown in the black panel? This figure is from a paper, maybe it could be redrafted attempting to account for uncertainties a bit more completely? [Gabriele Hegerl, United Kingdom]	accepted. This will be dealt with.
13-1583	13	101	1	101	9	Figure 13.12 appears to have incorrect scales. I suggest the scale perhaps should be cm instead of metres? These should be carefully checked. [Phil Watson, Australia]	figure redrawn
13-1584	13	101	1	104	10	Suggest to use the same format as Fig. 13.18 or Fig. 13.20 so that the Pacific is not split around the Date Line. It's hard to "link" the whole Pacific together by eye. [Xuebin Zhang, Australia]	figures redrawn
13-1585	13	101	4			Figure 13.12: In the caption, the units should be (cm), not (m). [Denis Gilbert, Canada]	rejected
13-1586	13	101	6			Figure 13.12: In the caption, the units for the standard deviation should also be (cm), not (m). [Denis Gilbert, Canada]	rejected
13-1587	13	101		101		Figure 13.12. Units of (a) are millimetres not metres [James Gower, Canada]	corrrected - figure redrawn
13-1588	13	101				fig. 13.12 The caption says the units on the color bars of the two figures are meters, but this seems unlikely. centimeters? [Government of United States of America]	figure redrawn
13-1589	13	104	1	104	1	Fig 13.15. I don't understand what is plotted in Fig a and the caption doesn't help. It is unclear what components of SLR are included and what not. The global mean is 35 cm which implies it is steric plus something else but it lies outside the grey box for RCP4.5 in Fig 13.8 so it's pretty confusing. It could be steric plus glacier but then what is the point of it if it doesn't include ice sheets and if it does then how....? [Jonathan Bamber, United Kingdom]	figure redrawn
13-1590	13	104	1	104	10	Figure 13.15 panel (b) appears to have an incorrect scale. I suggest the scale should be "%" not "RSL change (m)"? These should be carefully checked. [Phil Watson, Australia]	figure redrawn
13-1591	13	104		104		Figure 13.15. Units of (b) are % not metres [James Gower, Canada]	figure redrawn
13-1592	13	104				Figure13.15 (b) legend: RSL change (m) should be RSL change (%). [Government of Japan]	figure redrawn
13-1593	13	104				fig.13.15 Figure b is percentage so remove "RSL change (m)". [Government of United States of America]	figure redrawn
13-1594	13	104				fig. 13.15b Caption on the color bar of 13.15b should be "%",apprently not "m". [Government of United States of America]	figure redrawn
13-1595	13	104				fig. 13.15 It seems that the legend on Fig 13.15 (b) is incorrect and should be "deviation from mean change (percent)" instead of "RSL Change (m)" [Government of United States of America]	figure redrawn
13-1596	13	105	1	105	9	the dashed lines are not defined [Mark Siddall, United Kingdom]	figure redrawn
13-1597	13	105	1	105	9	this plot and the related text place high confidence in the ability of models to capture the translation of deep-ocean changes in dynamic height to the coastal margin. The models used to generate this plot capture no coastal oceanographic processes (tides, coastal currents, estuaries etc.) and it is by no means clear that they are relevant at the coast. This is an under-investigated issue but I suspect that coastal processes will dramatically dampen open ocean changes. Tidal dissipation and dissipation in the form or energised coastal currents will absorb a signicant amount of changing gravitational potential energy from the open ocean and coastal winds will modify any changes. see e.g. Hong et al, Journal of Physical Oceanography, V30, 2088-2098 [Mark Siddall, United Kingdom]	This is a research question
13-1598	13	105	1			Fig.13-16, Caption does not explain what the vertical dashed lines on each panel are. There appears to be a third set of data shown in dark red at the bottom of each panel - what is it? [European Union]	description added
13-1599	13	105				fig. 13.16 What are vertical dashed lines? [Government of United States of America]	description added

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13-1600	13	105				Figures 13.16, 13.17, and 13.18: Should cite the source of the underlying analysis. Do these all include gravitational-elastic effects as well as the effects modeled with AOGCMs? [Robert Kopp, United States]	More details are provided in supplemenatary material
13-1601	13	106	1	106	10	On Figure 13.17, can the 95 percentile value for New York in 2100 be added to the caption as it is not shown with this scale. (I am not suggesting replotting -- selected scale works for most sites well). [Robert Nicholls, United Kingdom of Great Britain & Northern Ireland]	Figures redrawn
13-1602	13	106	1	106	10	Figure 13.17 advises sea level change for 9 representative stations though there are 12 station records shown? [Phil Watson, Australia]	corrected
13-1603	13	106				fig. 13.17 Distorted x axis. In line 7 should it be 2010? [Government of United States of America]	figure redrawn
13-1604	13	107	2			Change "result" to "results" [Robert Dean, United States of America]	corrected
13-1605	13	108	1			Figure 13.19: The different sizes of the red circles are difficult to distinguish in the map. The figure may be easier to interpret if the circles were color-coded. [Ryan Sriver, United States of America]	figure redrawn
13-1606	13	108	4	108	4	Might be helpful to put in brackets that this is the same for any height increase because it's a power-law scaling. [European Union]	Additional material added to the caption
13-1607	13	110	1	110	6	Fig 13.21. This relates to comment 1. Plotting a random local proxy record with global datasets has little value. The flat appearance of the record prior to the 20th century is dependent on the selected method of detrending for millennial-scale process like GIA. It is not (yet) possible to make a useful global average proxy curve. This diagram only goes back to 1700, so perhaps the Jevrejeva tide-gauge compilation could be shown instead. However, that compilation also suffers from geographical bias in the 18th and 19th century. This diagram is really only useful from 1870 onwards and the proxy data should be excluded. If it is useful to show proxy and instrumental data together, then Figure 4 of Gehrels and Woodworth (2013) would be a possibility. That diagram shows seven sites with proxy and tide-gauge records, since 1650. [Roland Gehrels, United Kingdom]	Taken into account - the figure is being extensively revised.
13-1608	13	110	1	110	6	It is in my opinion quite to make this hockeystick type stiching of paleo+intrumental+projection considering the present status of the paleo records. At the very least specify in the caption exactly what the data is, and add a caveat concerning that it is not global sea level. [Aslak Grinsted, Denmark]	Taken into account - the figure is being extensively revised.
13-1609	13	110	1	110	6	We actually have proper instrumental data reaching back to 1700 (Jevrejeva et al. 2008) and do not have to use a single paleo record which clearly has a much greater 20th Century slope than the tide gauge record. Please add Jevrejeva et al. 2008. [Aslak Grinsted, Denmark]	Taken into account - the figure is being extensively revised.
13-1610	13	110	1	110	6	Why the preference for the North Carolina salt marsh record over other paleo records? See chapter 5 figure 5.17 for other records. I have argued that the subsidence correction applied to the NC record is too small in a comment to Kemp et al. and in http://www.clim-past-discuss.net/8/C1408/2012/cpd-8-C1408-2012.pdf (see figure 2). [Aslak Grinsted, Denmark]	Taken into account - the figure is being extensively revised.
13-1611	13	110	1	110	6	If you include paleo data, then please ensure that it matches what the paleo-chapter uses. I do not see the subsidence corrected proxy record in chapter 3. [Aslak Grinsted, Denmark]	Taken into account - the figure is being extensively revised.
13-1612	13	110				fig. 13.21 Define points on graph. [Government of United States of America]	Accepted