

Expert Review Comments on the IPCC WGI AR5 First Order Draft -- Chapter 13

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|------------|---------|-----------|-----------|---------|---------|---|---|
| 13-1 | 13 | 0 | 0 | | | There are several glaring problems with Chapter 13: Sea Level Change: [David Burton, USA] | Specific criticisms are responded to below |
| 13-2 | 13 | 0 | 1 | 0 | 10 | I am not sure what the path forward is: involve more authors, take a broader view at the recent literature, put larger error bars on predictions (who is afraid of doing that?), improve cross referencing of other chapters, reduce the length of the text, polish the executive summary to reduce the number of bullets, focus on an assesment rather than a literature review. [Eric Rignot, USA] | Specific criticisms are responded to below. We are attempting to take a broader view, improve cross referencing (this is a two way exercise), reduce the length, rewrite the executive summary and focus on a tighter assessment. |
| 13-3 | 13 | 0 | 1 | 0 | 10 | [Eric Rignot, USA] | There is no comment associated with this reviewer's name. It appears the reviewer's name was carried over from the immediately preceding comment. No action needed. |
| 13-4 | 13 | 0 | | | | In all the chapter, submitted papers should not be cited, for noyt yet accepted. [Michel Boko, Benin] | Submitted papers are allowed in FOD |
| 13-5 | 13 | 0 | | | | 1. There are no graphs of representative tide gauges. Such graphs are essential for "grounding" the reader's understanding of the issue, in particular the (lack of) response (thus far) in rate of SLR to GHG forcings, and the amounts and timescales of typical fluctuation in LMSL, and the variation in LMSL trends between locations. Here's a good example: http://tidesandcurrents.noaa.gov/sltrends/sltrends_global_station.shtml?stnid=120-012 [David Burton, USA] | Rejected - Assessment of observations is primarily the responsibility of Chapter 3 and there are some graphs of tide-gauge records in FAQ13.2. |
| 13-6 | 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 goss error, which confuses the reader and calls into question all associated conclusions. [David Burton, USA] | Rejected. We clearly make the distinction between these two measurements at several places in the chapter (e.g., p. 13-16, line 53-56). |
| 13-7 | 13 | 0 | | | | 3. There is no mention of the fact that, over the period during which there have been large GHG emissions, the rate of coastal sea level rise, as measured by tide gauges, has not accelerated. [David Burton, USA] | Rejected. We discuss the evidence from tide gauges for an acceleration in sea level on p. 13-16. |
| 13-8 | 13 | 0 | | | | 4. There is no mention of the fact that, over the 19 years for which we have data, satellite measurements of GMSL have exhibited a striking deceleration in rate of sea level rise; nor is it mentioned that the various satellites differ strikingly form one another in the rates of SLR they are measuring; nor is it mentioned that the satellite which has the broadest coverage of the world's oceans, Envisat, has measured only about 0.5 mm/yr SLR since 2004. Instead, it is claimed that the satellites (as if they were unanimous!) have been measuring about 3.2 mm/yr SLR. That is highly misleading. [David Burton, USA] | Rejected. The most high quality satellite altimeters are the Topex/Poseidon/Jason series. There is evidence of fluctuations in the rate of rise but we are aware of no clear indication of striking deceleration. |
| 13-9 | 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, USA] | Rejected - This is a miunderstanding of the science. Thermal expansion of the ocean results in a dynamic response of the ocean and the flow of additional water onto the shelves immediately adjacent to the coast, thus affecting coastal sea level. |
| 13-10 | 13 | 0 | | | | 6. There is pervasive understatement of the uncertainties regarding sea level projections. [David Burton, USA] | Taken into account. Specific Criticisms are responded to below and we have revised the statement of uncertantiny. |
| 13-11 | 13 | 0 | | | | 7. There is extensive discussion of, and credence given to, Rahmstorf's discredited "semi-emperical models." That material should be deleted. [David Burton, USA] | Rejected - The SEMs are part of the literature and must be discussed. We are not aware of additional specific papers in the peer reviewed literature beyond those referred to that discredit SEMs |
| 13-12 | 13 | 0 | | | | 8. There is excessive reliance on modeling (which suggest dramatic accelerations in SLR should be happening), and far too little reliance on measurements (which show no such accelerations in SLR), throughout. [David Burton, USA] | Rejected - Models and observations must be compared over the same period. We do this to the extent that this is covered in the literature or is an application of known methods. Observations offer no way of projecting future change. |

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| 13-13 | 13 | 0 | | | | 9. GMSL is properly defined, for the context of satellite measurements, as being sea level relative to the center of the Earth's mass (p.13-3), or to "a geocentric reference such as the reference ellipsoid" (p. 13-9). However, there's no explanation of the fact that, for the context of coastal tide-gauge measurements, GMSL is something quite different, being calculated from weighted averages of corrected LMSLs. Really, to avoid confusion by the readers (and, probably the authors!), two different terms should be used throughout for these two different quantities: perhaps OGMSL (Open-ocean Global Mean Sea Level) and CAMSL (Coastal Average Mean Sea Level). [David Burton, USA] | Rejected. IT is important to recognise the differences between the two data sets. There are a a number of analysis of tide gauge data, some of which are averages of costal sea level and others which use information on the spatial distribution of sea level to estimate GMSL. |
| 13-14 | 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. http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1099-131X [David Burton, USA] | Noted. However, there is extensive reference to papers on projecting climate and sea level. |
| 13-15 | 13 | 0 | | | | This chapter is a great advance on several fronts and will be extremely useful to the impacts and adaptation community, providing considerable guidance for the generation of regional and local projections, despite the numerous remaining uncertainties. [Donald Forbes, Canada] | Thank you |
| 13-16 | 13 | 0 | | | | The clear presentation of projected contributions from specific sources (particularly from land ice sources in the Greenland and Antarctic ice sheets and other glaciers and icecaps) is valuable for computing appropriate gravitational fingerprinting factors for individual sites. However, the discussion of this effect in Section 17.3.7.2 is not particularly clear and does not clearly indicate the global impact of this effect. [Donald Forbes, Canada] | This will be addressed in responses in Section 13.7. We have attempted to make the global impact of the fingerprints clearer, including in FAQ13.1 |
| 13-17 | 13 | 0 | | | | I am very impressed with the comprehensive review. It really is a very good chapter. I have a few comments that I hope will improve the chapter. I am a review editor in Chapter 3 and so permitted to comment on Chapter 13. [Howard J. Freeland, Canada] | Thank you |
| 13-18 | 13 | 0 | | | | Again the projections which are calculated from exaggerated CO2 forcing do not agree with the most recent observed tendency reproduced in Fig. 13.3, less than 2 mm per year for the past 8 years, as a likely effect of the 60 years-period oscillation addressed throughout this reviewer's report but not taken into account in the models. Consider that the altimeter of the most recent, big and modern satellite launched in 2002, ENVISAT, measures even less : 0.6 mm per year. [François GERVAIS, France] | Rejected. The most high quality satellite altimeters are the Topex/Poseidon/Jason series. There is evidence of fluctuations in the rate of rise. ENVISAT may be the biggest (non-optimum for precise sea level measurements) but it is not the optimum for measuring sea level. |
| 13-19 | 13 | 0 | | | | In general I feel that the chapter paints an overly rosy picture of progress since AR4. My own sense is that we are still some way from convincing closing the sea level budget even in the well observed present and we are much further away from achieving this over the longer term (since 1960 or 1970). I will attempt to make clear where I believe the problems are in the text below. [Simon Holgate, UK] | Accepted. We feel that much progress has been made but there are remaining uncertainties. We have carefully revised our statements. Specific Criticisms are responded to below |
| 13-20 | 13 | 0 | | | | There needs to be a glossary for everything from GMSL to RCP. [Simon Holgate, UK] | Noted. There will be a glossary for the entire report and we are attempting to ensure all terms are defined. |
| 13-21 | 13 | 0 | | | | Many errors in the formatting of citations. [Simon Holgate, UK] | Noted. Many of these are a function of ENDNOTE issues and these are being addressed |
| 13-22 | 13 | 0 | | | | (1) I think it would be helpful for the reader to find some explicit reference to the sections where he can find more information about each of the items, since the executive summary seems to follow the structure and sections of the chapter. In this respect I suggest that the titles are the same (for instance. "Contributions to Global Mean Sea Level Rise During the Instrumental Period (Section 13.4)" instead of "Contributions fo Sea Level Change"). (2) I miss the summary of section 13.2 ("Components ad Models of Sea Level and Land-Ice Change") [Belén Martín Míguez, Spain] | Noted. (1) Specific reference to sections in chapter has been added to the Executive Summary. (2) Section 13.2 is a review of models, and thus provides no discussion of the science for the Executive Summary. Material has been added to this section to describe glacier and ice sheet models. |
| 13-23 | 13 | 0 | | | | I would like to compliment the authors on the comprehensive approach taken in this draft, and on its even-handedness. Nevertheless, there are places where inconsistencies arise and the overall message gets twisted or lost. To rectify this problem, additional space should be devoted in 13.9 to discussing more fully the weaknesses in our ability to project for the 21st century and beyond, bringing together the arguments on the limitations of the projections of Tables 13.6 and 13.8. [Michael Oppenheimer, USA] | Accepted. Thank you. Much appreciated. Section 13.9 has been expanded to address these comments. |
| 13-24 | 13 | 0 | | | | The entire Ch. 13 is very well reflected and comprehensive. But at the other hand I have the impression that it is too detailed and lengthy, partly it appears even narrative. For example, I found numerous individual papers | Accepted. Thank you. Efforts have been taken to address these comments regarding shortening of text |

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| | | | | | | that were discussed in full, partly over 1/2 a page or more (page 30). I simply got lost in all the details. Given the rapid changes in these fields of research, I recommend to shorten these 'paper contents descriptions' substantially. [Frank PAUL, Switzerland] | and assessment (rather than review) of the literature. |
| 13-25 | 13 | 0 | | | | I focus my comments on statements related to glaciers as I am not an expert for the other domains. [Frank PAUL, Switzerland] | thank you. No response necessary |
| 13-26 | 13 | 0 | | | | <p>There is considerable overlap of content between chapter 13 (sea level) and chapter 5 (palaeoclimate). This is mainly because chapter 5 also tries to summarise sea-level evidence, which in my view is superfluous because chapter 13 is specifically meant for that, and does a better job at it. More worrying, the messages are not consistent. It would be better if chapter 5 would just refer for sea level to the more authoritative and balanced assessment of chapter 13.</p> <p>In both chapters 5 and 13, there is – for undisclosed reasons – great reliance on a single, yet unpublished, and actually not even accepted, study (Dutton and Lambeck, submitted). Especially in chapter 5, other studies are (very) critically assessed, but the same is not done for the D&L paper, which is presented as a sort of 'end all' statement. This may be a reflection of the authorships of the chapter, where personal preferences and opinions have obscured the scientific assessment processes by too much.</p> <p>It is a specific worry to me that the arguments in both chapters completely bypass the critical importance in coral studies (or any other sea-level study) of not just having well-dated masses of loose datapoints, but to also consider the stratigraphic context. Only strict stratigraphy can truly constrain relative age relationships and so irrefutably portrays developments through time, including rates of change. A key study for this appeared recently in Nature Geoscience (Thompson et al., 2011), yet it is completely missing from both chapters 5 and 13. Possibly this omission resulted from a chapter-author-based bias in favour of strict closed system ages. However, any field geologist knows that – for reconstruction of temporal developments in a relative sense, and ultimately rates of change – less perfect datings within a tightly constrained stratigraphic framework are more valuable than perfect datings on a random collection of samples from settings that lack clear documentation of stratigraphic relationships relative to each other. More balance is needed.</p> <p>--- continued below ---- [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland]</p> | <p>Rejected. The primary goal of Chapter 13 is to make projections of sea level, but it also serves as a "synthesis" chapter that covers aspects of sea level that inevitably occur in other chapters. In this context, paleo-sea level information is only needed to provide additional information on possible rates of sea-level rise. Chapters 5 and 13 agreed during the second Lead Author meeting that Chapter 5 would provide the assessment of paleo-sea level, and they would then provide Chapter 13 with the essential information on paleo rates of sea level rise needed from that assessment. Based on conversations with Chapter 5, we address the remaining comments on the data that were used for the paleo sea level assessment here. First, the FOD concluded that there is "moderate confidence that local LIG sea levels experienced a meter-scale fluctuation sometime between 126 ka and 120 ka" and referred to several studies in support of this, including the Kopp study that the reviewer refers to. It was an oversight not to include the Thompson paper, and this is now included, as are other additional references. While we did not provide a comprehensive literature review, this point was clearly stated, while the reviewer seems to indicate that we did not address it. On the other hand, we continue to have low confidence in the Red Sea 18O record, but limit our reasons to (1) similar variability in this record during the Holocene, when such sea level fluctuations did not exist (thus the general statement that some other factor may contribute to interglacial variability in this record, without implying what that may be), (2) a lack of replication between the cores (we do not see any discussion of sedimentation low sedimentation rates in KL09 in the Rohling 2008 supplementary information that the reviewer refers to), and (3) the chronology for the LIG used in the study, which is shorter than the longer duration of Chapter 5, reduces the rates.</p> |
| 13-27 | 13 | 0 | | | | <p>continued- This importance of stratigraphic control is exemplified by the fact that Thompson et al. (2011) observed in their Bahamas study region clear evidence for a millennial-scale oscillation within the last interglacial, where highstands are separated by erosional surfaces (lowstands) in a sequence that is highly reminiscent of similar successions observed within other last interglacial fossil reefs (e.g., Florida, Yucatan – see Thompson et al., 2011; Red Sea – see Bruggeman et al., 2004) and in coastal morphological developments (Orszag-Sperber et al., 2001) over tracts of hundreds of kilometres of length around the Red Sea (Plaziat et al., 1995, 1998; Orszag-Sperber et al., 2001; Bruggemann et al., 2004; see also further summaries in the supplement of Rohling et al., 2008). These clear variations are of an up-down nature and of a millennial timescale that is not compatible with any isostatic variability, and also the reproducibility of these</p> | See above |

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| | | | | | | <p>variations is incompatible with any spurious tectonic explanations. Given the length of coastline over which these fluctuations have been documented in the Red Sea alone, I have calculated with my geophysical colleague Nick Harmon that a millennial-scale series of M>8 earthquakes would be needed with displacements that were first down, then up, then down, then up, then down, and then up again. That is an entirely unrealistic tectonic scenario. So if isostasy and tectonics cannot reasonably explain what is found, then the strong suspicion has to be that sea level oscillated. That then is confirmed by studies with tight stratigraphic control such as Rohling et al., (2008), which further extends the spatial evidence of oscillation in Red Sea sea-level markers, and such as Thompson et al. (2011) and those they refer to, from a completely different region. Moreover, the variability within the last interglacial is preserved also in the statistical compilation of Kopp et al. (2009), where it is of specific interest that (again stratigraphically well-constrained) deep-sea benthic oxygen isotope records also preserve evidence of a considerable oscillation (e.g., Lisiecki and Raymo, 2005). Hence, a compelling case is emerging in favour of a significant oscillation within the last interglacial, given that it is revealed in stratigraphically coherent records of: (1) fossil corals/reefs; (3) coastal sediment sequences; (3) Red Sea residence-time based sea-level reconstruction; (4) deep-sea benthic isotope records. Although each individual record might have its own sources of bias, these sources of bias are not the same/common between the various methods, so that the overall picture across different methods becomes hard to fault.</p> <p>I am puzzled that studies with the essential stratigraphic coherence are so close to being dismissed in the current write-up of chapters 5 and 13. This is not a balanced representation of the geological understanding of variability. It is a personally motivated/subjective choice, which is out of place in an impartial assessment report such as the IPCC.</p> <p>--- continued below ---- [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland]</p> | |
| 13-28 | 13 | 0 | | | | <p>continued - As an aside, blowing our own trumpet a bit, I note that the Red Sea record of Rohling et al. (2008) is dismissed on vague grounds in chapter 5 (but not in the more authoritative chapter 13), without any real substantiation. In fact, the lack of fine-scale reproducibility of the variations using the record of core KL09, which is mentioned in chapter 5, was explained already in the study of Rohling et al (2008, see the supplement) as a result of the too-low sedimentation rate in KL09 to pick up such signals. The cause for the anomaly in KL09 was also further explained in Trommer et al. (2011). That latter study in addition shows that the last interglacial highstand corresponds to a relatively arid episode (evidence for humidity appearing only after the highstand had peaked), which would counter any suggestion that freshwater addition to the Red Sea might have caused the light isotope values that underlie the highstand sequence reported in Rohling et al. (2008), which I think the authors may be alluding to in their non-specified statement of possible 'additional controls'. So the arguments given in chapter 5 to reject the Red Sea results do not hold water because they infer (but fail to specify) spurious additional controls that have been debunked already. Moreover, the arguments completely ignore the substantiating evidence for a substantial sea-level oscillation within the last interglacial from many other stratigraphically well-constrained studies (see above). I find it poor form that such stratigraphically careful studies as Bruggemann et al. (2004), Orszag-Sperber et al. (2001), and Thompson et al. (2011) have been systematically omitted from the sea-level compilations of both chapters 5 and 13.</p> <p>In short, chapter 5's sea-level summary to me seems rather poorly thought through and incomplete/unbalanced. More importantly – it is not useful because there is a more authoritative special chapter on this subject (chapter 13). In addition, it is imperative that the various stratigraphically well-constrained studies that document a substantial oscillation within the target period are carefully included (both in chapter 5, if sea level is kept in there, and in chapter 13).</p> <p>--- continued below ---- [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland]</p> | See above |
| 13-29 | 13 | 0 | | | | <p>continued - New references:</p> <p>Bruggemann, J. H. et al. Stratigraphy, palaeoenvironments and model for the deposition of the Abdur Reef Limestone: context for an important archaeological site from the last interglacial on the Red Sea coast of Eritrea. <i>Palaeogeogr. Palaeoclimatol. Palaeoecol.</i> 203, 179-206 (2004).</p> <p>Orszag-Sperber, F., Plaziat, J. C., Baltzer, F. & Purser, B. H. Gypsum salina-coral reef relationships during the</p> | See above |

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| | | | | | | <p>Last Interglacial (Marine Isotopic Stage 5e) on the Egyptian Red Sea coast: a Quaternary analogue for Neogene marginal evaporites? <i>Sed. Geol.</i> 140, 61–85 (2001).</p> <p>Plaziat, J. C. et al. Mise en évidence, sur la côte récifale d'Egypte, d'une régression interrompant le plus haut niveau du Dernier Interglaciaire (5e): un nouvel indice de variations glacio-eustatiques haute fréquence au Pléistocène? <i>Bull. Soc. Géol. Fr.</i> 169, 115–125 (1998).</p> <p>Plaziat, J. C. et al. Quaternary changes in the Egyptian shoreline of the northwestern Red Sea and Gulf of Suez, <i>Quat. Internat.</i> 29/30, 11–22 (1995).</p> <p>Siddall, M., Bard, E., Rohling, E.J. & Hemleben, Ch., Sea-level reversal during Termination II. <i>Geology</i> 34, 817–820 (2006).</p> <p>Thompson, W.G., Curran, H.A., Wilson, M.A. & White, B., Sea-level oscillations during the last interglacial highstand recorded by Bahamas corals, <i>Nature Geosci.</i> 4, 684–687 (2011).</p> <p>Trommer, G., Siccha, M., Rohling, E.J., Grant, K., van der Meer, M.T.J., Schouten, S., Baranowski, U. & Kucera, M., Sensitivity of Red Sea circulation to sea level and insolation forcing during the last interglacial. <i>Clim. Past</i> 7, 941–955 (2011). [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland]</p> | |
| 13-30 | 13 | 0 | | | | <p>Why are section 13.5 and 13.6 separate sections? Wouldn't it be more logical to keep them together, since 13.6 basically summarizes and concludes 13.5? [Aimee Slangen, Netherlands]</p> | <p>Rejected. Section 13.5 provides an assessment of the literature on projections of components of sea level, whereas Section 13.6 brings this all together and succinctly provides projections of global mean sea level based on the methods described in Appendix 13.1 and developed based on the assessment in Section 13.5.</p> |
| 13-31 | 13 | 0 | | | | <p>Overall the chapter reads better than I had expected. While there are still issues to tackle, the authors have done a good job of pulling together the important topics and developing a useful framework for presenting the results as they continue to evolve prior to the final publication. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland]</p> | <p>Noted. Thank you</p> |
| 13-32 | 13 | 0 | | | | <p>My comments are more focused on the presentation of topics rather than carefully checking values, which are bound to change with new publications prior to the publication deadline. I apologise; my comments are not nearly as detailed or as numerous as I would have liked. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland]</p> | <p>Noted. No response necessary</p> |
| 13-33 | 13 | 0 | | | | <p>My largest concern for the chapter is focus on the closure of the sea level budget, and by extension the increased confidence in modeling. While our understanding has greatly improved since the last assessment, there are still differences in the values of the components between analyses. Some of this is due to different time spans and model corrections. However, it does point to the fact while the budget may close for a particular study, our understanding is probably still not complete. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland]</p> | <p>Accepted. This issue will be addresses in responses to Section 13.4.</p> |
| 13-34 | 13 | 0 | | | | <p>I assume IPCC will review this for trivial errors such as repeated word on line 27. [Robert Thomas, USA]</p> | <p>Editorial. The chapter authors will also be looking to correct these errors.</p> |
| 13-35 | 13 | 0 | | | | <p>This chapter tends to pre-suppose that readers are scientists with fairly specialist knowledge. [Robert Thomas, USA]</p> | <p>Accepted. Efforts have been made to present material at a level of general understanding.</p> |
| 13-36 | 13 | 0 | | | | <p>In parts, such as sections on ice loss from non-ice-sheet glaciers, it becomes overly detailed, to the extent of losing the forest by dissecting the trees. There is also quite a lot of repetition, which is probably OK, because most readers won't wade through the whole chapter. [Robert Thomas, USA]</p> | <p>Accepted. The material has been shortened considerably.</p> |
| 13-37 | 13 | 0 | | | | <p>I have focussed on the „ice” sections, but read much of the rest. Error estimates are often poorly explained,</p> | <p>Taken into account. These issues will be addressed in</p> |

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| | | | | | | and frequently appear not to be estimates of total uncertainty. Some of the ice-related errors seem to be ludicrously low. In view of the fairly rudimentary ability to model rapid changes in ice sheets, the possibility of more rapid SLR than „modeled" here appears to be under-stated [Robert Thomas, USA] | sections 13.5 and 13.6. |
| 13-38 | 13 | 0 | | | | This is the best sea level chapter ever, out of the five IPCC WG1 assessments. I won't let that stop me from complaining about things I do not like in the comments that follow, but it is the quality of this document that makes me view commenting as worthwhile [James G Titus, United States of America] | Noted! Thank you! |
| 13-39 | 13 | 0 | | | | The report uses both yr and a with an exponent of -1 to signify "per year". Except for when it is in an equation, using yr-1 is quite annoying to read in the middle of a paragraph, and a-1 is even worse. [James G Titus, United States of America] | Editorial. We are required to use approaches that are consistent throughout the entire report. |
| 13-40 | 13 | 0 | | | | The chapter seems to define "sea level" as including instantaneous local water levels (e.g. it uses the phrase "extreme sea level" to refer to storm surges). The more widely accepted definition of sea level is the tidal water level averaged over a reasonably long period of time (e.g. years or a tidal epoch). I encourage you to adhere to that standard definition and use the phrase "water level" when referring to storm surges. [James G Titus, United States of America] | We now have a section 13.1.2 on definition of terms. |
| 13-41 | 13 | 0 | | | | The chapter appears to use the term "glacier" in a common but nonstandard way. The most common definition of glacier is a persistent (i.e. multi year) body of ice resting on land, so that the term would include ice sheets, ice caps, and the small glaciers. The second most common definition seems to be to use the phrase "glacier" to refer to "small glaciers". This chapter, however, seems to use the term "glacier" as referring to "ice caps, ice fields, and small glaciers", in effect, everything but ice sheets. The term is eventually defined on page 18, but one has seen the word many times by that point. [James G Titus, United States of America] | Noted. There is a glossary for the definition of major terms, including "glaciers". |
| 13-42 | 13 | 0 | | | | Table 13.6: The 3 quantities that are scenario independent, should be graphically treated differently in the Table to avoid confusion. Currently they seem associated with RCP2.6 only. [Thomas Stocker/ WGI TSU, Switzerland] | Accepted. The table will need to be revised. |
| 13-43 | 13 | 0 | | | | Given the page constraints for all chapters, please consider where overlaps can be reduced. We note some potential overlap with the observational chapters (3,4), for example, within Sections 13.3 and 13.4. [Thomas Stocker/ WGI TSU, Switzerland] | Accepted. We have substantially reduced the length of those sections that have overlap with other chapters, particularly Chapters 3,4 and 5. |
| 13-44 | 13 | 0 | | | | The concept of 'Sea-level allowance' discussed in Chapter 12 is not very clear, and we had difficulties to see the value added by including this small sub-section which appears to be based on few papers from a single expert. We suggest to remove this subsection in the SOD. [Thomas Stocker/ WGI TSU, Switzerland] | Accepted. This will appear in WGII. |
| 13-45 | 13 | 0 | | | | The chapter on SL change is very well written, clear and thorough in treatment of the literature and modelling results. My only suggestion is to also include the Houston & Dean (2011) paper which reports minimal SLR or deceleration based on limited gauge data. The paper is published and discredited by several and that should be brought out in Ch 13. [s. jeffress (jeff) williams, usa] | Rejected. This material needs to appear in Chapter 3 who assess the observational record. |
| 13-46 | 13 | 1 | 1 | 1 | 1 | First off well done to authors and lead authors on tackling a very challenging subject and creating an overarching structure and content that covers what is needed. [Mark Siddall, UK] | Noted. Thank you |
| 13-47 | 13 | 1 | 1 | 1 | 1 | It is very good to see that spatial distributions of sea-level rise are discussed at length. However, the presentation is somewhat confusing. Not all aspects of local sea-level rise are attributable directly to climate change and so these are not discussed at length, which make sense. However, these issues pop up here and there and there is not overall consistency in whether these issues are discussed. In particular, it needs careful noting in the summary that the list of local sea-level effects given is not exhaustive. It should also be noted that some effects, though not directly linked to global warming, are still anthropogenic (i.e groundwater extraction). They also often have a common sign (land sinking) in key urban areas [Mark Siddall, UK] | Accepted. These effects are now mentioned in section 13.1 |
| 13-48 | 13 | 1 | 1 | 1 | 1 | While the structure covers all the bases needed here, it is also at times repetitive and confusing and I will try and note particular instances that I spotted below [Mark Siddall, UK] | Accepted. We have attempted to remove repetition and make clearer. |

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| 13-49 | 13 | 1 | 1 | 1 | 1 | Sections of this chapter linked to chapter 5 will need updating if changes are made there [Mark Siddall, UK] | Noted. We will revise all material that is revised in Chapter 5. |
| 13-50 | 13 | 1 | 1 | 1 | 1 | It should be made very clear that estimates of future relative sea-level rise from GIA models owe a lot to field and lab work gathering local sea-level samples and analysing them. Key references should be given [Mark Siddall, UK] | Noted. But this sort of reporting is not the central focus or purpose of the chapter. |
| 13-51 | 13 | 1 | 1 | 1 | 1 | many subsections have their own syntheses. It would be best to collate these at the end of each section. This would also help unify the themes and language of the different disciplines. Differences between the meanings between different disciplines are often confusing - for example, the effects of ocean dynamics vs GIA/rotation/gravitation in the regional sea level section. [Mark Siddall, UK] | Taken into account. Syntheses will be presented at the end of each section. |
| 13-52 | 13 | 1 | 1 | 1 | | Sea Level Change [Medani Bhandari, Nepal] | No response necessary |
| 13-53 | 13 | 1 | 2 | 1 | 2 | The Title is misleading. Just call it "Sea Level" Why are you only interested in "Change" and why are you only interested in linear, and upwards change? Is it because you want to distort the evidence to confirm your preconceived theories? [VINCENT GRAY, NEW ZEALAND] | Rejected. The Chapter is about change in sea level, both variability and positive and negative trends. The central issue is how and when sea level has and will change, so retaining change in the title is appropriate. Change does not by itself indicate the direction (up or down). |
| 13-54 | 13 | 1 | 16 | 1 | 16 | Jan Lanaerts should be Jan Lenaerts [Philippe Huybrechts, Belgium] | Editorial This has been corrected. |
| 13-55 | 13 | 1 | 16 | 1 | 16 | My name has a spelling error: Lenaerts instead of Lanaerts [Jan Lenaerts, The Netherlands] | Editorial. This has been corrected. |
| 13-56 | 13 | 1 | 49 | 1 | 49 | Spell out GMSL for the sake of consistency with titles of Sections 13.4 and 13.6 in the same Table of Contents [Belén Martín Míguez, Spain] | Editorial. GMSL has now been spelled out in the Table of Contents. |
| 13-57 | 13 | 1 | | | | Overall comment: The sea level chapter shows that sea level science associated with climate variability and change has significantly increased and evolved since the first IPCC report (with clear side-effects for the reviewers on the number of pages, from ~20 pages (FAR, 1991) to ~110 pages (AR5, 2012)). This significant progress is very much appreciated given the difficulties of investigating the complex nature of sea level and the important socio-economic-environmental implications of sea level change to our society. The text of this FOD chapter also shows that the team of authors is taking great care to provide a comprehensive and objective assessment of the relevant scientific literature. I would like to congratulate the authors for this effort [Catia Motta Domingues, Australia] | Noted. Thank you |
| 13-58 | 13 | 1 | | | | General comment: For future draft versions, I would like to suggest, whenever possible, if the authors could consider opportunities to soften up the technical language (and possibly technical details which could be a distraction in the main text) to facilitate reading/understanding for a broader audience. Section comments are found below followed by more specific comments. [Catia Motta Domingues, Australia] | Accepted. We have attempted to make the text more readable. We address specific comments below |
| 13-59 | 13 | 1 | | | | This chapter is a welcome addition to the IPCC table of contents and makes a valuable contribution. However, the glaring discrepancy between the simulated past and projected future Antarctic mass balance here (positive) and the observations of Antarctic mass balance in chapter 4 (negative) create a serious problem when it comes to projected sea level. Alternative methods of projecting Antarctic net mass balance should be explored, and the GCMs should be better validated here and/or in chapter 9 (where the issue is treated briefly). [Philip Mote, USA] | Rejected. Chapter 9 only briefly addresses the issue. The is sound theoretical reasoning why Antarctic precipitation should increase, paleo observations support this theory as do models. Observations are mostly inadequate to detect the magnitude of change expected to date. We continue to discuss this issue with Chapter 4. |
| 13-60 | 13 | 1 | | | | What are the units of sea level and sea level change? The chapter uses mm, cm, m, and also mm/yr, m/ka, and probably somewhere furlongs per fortnight. Unless there is a very clear rationale, why not simply pick cm and mm/yr? Putting the paleo obs in those units provides a direct comparison. [Philip Mote, USA] | Accepted. Efforts have been made to standardize these units that is also consistent with the entire report. |
| 13-61 | 13 | 3 | 1 | | | In general the AR5 FOD Sea Level Change chapter provides a very authoritative and up to date appraisal of the status of science concerning the principal elements of the global sea level rise budget and future projection modelling. | Taken into account. This is an important issue and requires careful wording. We have discussed the fact that tide gauge and altimetry records both identify |

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| | | | | | | <p>AR4 provided a cautious position on the difference between the global averaged rate of SLR measured during the altimetry period, post 1961 period and average for the 20th century. In particular, AR4 advised it was “unclear” whether the faster rate during the altimetry period was reflective of decadal variability or an increase in the longer term trend (Synthesis Report - Summary for Policymakers).</p> <p>The current AR5 FOD of the Sea Level Change chapter is less circumspect on these issues providing what appears a more consistent position of an assumed acceleration and increase in the rate of SLR, due to anthropogenic forcing. These are critical factors. Whilst the evidence of acceleration in global average SLR is between the 19th and 20th centuries is clear (Woodworth et al, 2009), the evidence for continued acceleration throughout the 20th century is less evident. I believe this was the point of the circumspection on the issue in AR4.</p> <p>The issue of acceleration in SLR records has become extremely contentious during 2011 particularly with published works relating to long tide gauge records in America (Houston and Dean, 2011) and Australasia (Watson, 2011). Although these published works are quite different, they point to a general trend of weak average deceleration in the longer-term records around America and throughout Australasia over the period post 1930. Trends of acceleration are particularly sensitive to the data period available and the curve fitting chosen to consider the acceleration component of non-linear time series. More work remains to be done at the international level on the use of innovative non linear time series analyses that provide a more authoritative position on “real-time” SLR and associated accelerations.</p> <p>Notwithstanding the acceleration analysis, Watson (2011) also looked at decadal rates of rise from the 4 longest tide gauge records available for the southern hemisphere (Australasia). Watson (2011) concluded that short period trends of acceleration in mean sea level post 1990 were evident at each site, although they were not yet abnormal or higher than other short term rates measured throughout the historical record. The latter conclusion accords with the findings of other researchers looking at international records, notably Haigh et al (2009), Hannah (1990, 2004), Holgate and Woodworth (2004), Holgate (2007) and Wahl et al (2011). These consistent, relevant broad scale findings are absent from any contextual discussion in the AR5 FOD.</p> <p>In particular, these various findings provide some robustness to the caution exercised in the AR4 findings relevant to the rates of SLR measured during the altimetry era. It is relevant that the next 10-20 years will provide the keenest evidence yet of whether the post altimetry era data start to indicate rates of rise in mean sea level and associated accelerations that are indeed higher or abnormal in the context of the historical records of key long-term tide gauge records. When these historical thresholds have been surpassed (on a consistent basis at key long-term gauge sites representative of the major ocean basins of the world), then it might be definitively concluded that we are moving into an era signalling the onset of high (anthropogenically forced) projected sea level rise for the 21st century. Work is urgently needed beyond AR5 to investigate analytical techniques to distinguish the “tipping points” in records where the anthropogenic forcing is clearly differentiated from the inter-decadal variability of long term sea level records. The AR5 FOD Sea Level Change chapter might benefit from some broader discussion around the above-mentioned issues.</p> <p>References</p> <p>Haigh, I.D., Nicholls, R.J., Wells, N.C (2009). Mean sea level trends around the English Channel over the 20th century and their wider context. <i>Continental Shelf Research</i>, 29 (17), 2083-2098.</p> <p>Hannah, J. (1990). Analysis of mean sea level data from New Zealand for the period 1899-1988. <i>Journal of Geophysical Research</i>, Vol 95(B6), pp 12,399-12,405.</p> <p>Hannah, J. (2004). An updated analysis of long-term sea level change in New Zealand. <i>Geophysical Research Letters</i>, 31:L03307, DOI:10.1029/2003GL019166.</p> <p>Holgate, S.J., and Woodworth, P.L. (2004). Evidence for enhanced coastal sea level rise during the 1990s.</p> | <p>differences in the rate of rise over the 20th century (p. 13-16 to 13-17), that there is uncertainty in the interannual to decadal variability inferred from individual tide gauges because of differences between curves (p. 13-17), that there are multiple controls on sea level that can give rise to regional and global differences in the rate of sea-level rise on interannual to multi-decadal timescales, and that these can strongly modulate a long-term secular signal (section 13.7; FAQ 13.1). We have now added a discussion of the Houston and Dean (2011) and Watson (2011) papers.</p> |

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| | | | | | | <p>Geophysical Research Letters, 31:L07305, DOI:10.1029/2004GL019926.</p> <p>Holgate, S.J. (2007). On the decadal rates of sea level change during the twentieth century. Geophysical Research Letters, 34:L01602, DOI:10.1029/2006GL028492.</p> <p>Houston, J.R., and Dean, R.G. (2011). Sea-Level Acceleration Based on U.S. Tide Gauges and Extensions of Previous Global Gauge Analyses. Journal of Coastal Research, Vol 27 (3), pp. 409-417, DOI: 10.2112/JCOASTRES-D-10-00157.1, May.</p> <p>Wahl, T., Jensen, J., Frank, T., Haigh, I.D. (2011). Improved estimates of mean sea level changes in the German Bight over the last 166 years. Ocean Dynamics, 61 (5), 701-705.</p> <p>Watson, P.J. (2011). Is There Evidence Yet of Acceleration in Mean Sea Level Rise Around Mainland Australia. Journal of Coastal Research, Vol 27 (2), pp. 368-377, DOI: 10.2112/JCOASTRES-D-10-00141.1, February.</p> <p>Woodworth, P.L.; White, N.J.; Jevrejeva, S.; Holgate, S.J.; Church, J.A., and Gehrels, W.R. (2009). Review – Evidence for the Accelerations of Sea Level on Multi-Decade and Century Timescales. International Journal of Climatology, Vol 29, pp 777-789, doi:10.1002/joc.1771. [Phil Watson, Australia]</p> | |
| 13-62 | 13 | 3 | 3 | 3 | 11 | <p>This paragraph applies only to the open oceans. Changes in sea level on coasts are determined by a number of other additional factors. These include isostasy (geological movement of the land), weight of buildings and removal of minerals and groundwater on the land, and changes in harbours to change level within them. It also depends on the reliability of measuring equipment to withstand the constant battering by the sea and particularly hurricanes and tsunamis. Many early readings are prone to a downwards bias from these influences which are now minimised by modern levelling equipment. Again why are you just interested in "changes" and only on influences which have an upwards direction? Sea level, like all climate information sometimes goes down as well as up. [VINCENT GRAY, NEW ZEALAND]</p> | <p>Rejected. We specifically refer to "regional and local phenomena that may strongly modulate the global rise at any given location." We are also interested in change because this is what impacts human society, and as is well documented here and in Chapter 3, global mean sea level has been rising over the last century. We also clearly illustrate that relative sea level may fall in some locations.</p> |
| 13-63 | 13 | 3 | 3 | 29 | 33 | <p>It does not make it easy for the average reader to have numbers quoted in m/kyr in one paragraph and in mm/yr in another of the executive summary. No foul intent expected, but I would suggest to write everything in mm/yr, so it is clearer how the paleo values are relevant to present day. [Eric Rignot, USA]</p> | <p>Accepted. We have changed these units so as to be consistent. Rates will be reported in mm/yr and sea level rise in m.</p> |
| 13-64 | 13 | 3 | 4 | 3 | 4 | <p>Possibly change "sea level change" to "sea level rise", since the text is explicitly referring to ice sheet melt and thermal expansion, which increase sea level. [Carmen Boening, USA]</p> | <p>Accepted. Rejected - sea level may fall locally when ice melts and runs off into the ocean.</p> |
| 13-65 | 13 | 3 | 4 | 3 | 5 | <p>The "expansion of the ocean as it warms" contributes to open-ocean sea level, but it does NOT significantly affect coastal sea levels. Since only coastal sea levels have practical consequences, it is misleading to say that "expansion of the ocean as it warms" is a "primary contributor" to sea level change. [David Burton, USA]</p> | <p>Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level.</p> |
| 13-66 | 13 | 3 | 4 | 3 | 5 | <p>What about the contribution from changes in the current system? Aren't they major contributors for regional sea level changes? [Uwe Stoeber, Germany]</p> | <p>Taken into account. Although the specific current system mentioned by the reviewer is not identified, section 13.7 discusses changes in regional sea level associated with changes in wind fields and resulting changes in ocean circulation as well as from changes in the AMOC.</p> |
| 13-67 | 13 | 3 | 7 | 3 | 9 | <p>I would put the sentence around the other way and indicate that one of the main ways in which we do (and will) experience "a rise in mean sea level" is through "changes in extreme sea level events" - it is very hard to experience a rise in mean sea level in isolation. In addition to this, coasts will also be impacted by changes in extreme sea level events relative to mean sea level and by changes in the wave climate. [John Hunter, Australia]</p> | <p>Rejected. We think it is important to make the point first that mean sea level will rise, and as a result of this, there will be an increase in sea level extremes. Not all coastal regions will be subject to extremes, particularly protected areas, and so the two are not always the same. Nevertheless, the sentence has been reworded.</p> |

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| 13-68 | 13 | 3 | 9 | 3 | 11 | This sentence seems to be too constraining because it suggests that the only types of "sea level" to be addressed in this chapter are relative sea level (i.e. with respect to the land) and geocentric sea level. This omits eustatic sea level, steric sea level, GIA-adjusted sea level etc. Incidentally, it is worth defining all these terms up front in this chapter, because there are many misconceptions (e.g. many web site conflate "global-average sea level", "eustatic sea level" and "geocentric sea level"). [John Hunter, Australia] | Rejected. Relative sea level and geocentric sea level are measures of changes in sea level due to mass changes (i.e., eustatic), steric effects, GIA effects, etc. Additional material has been included to make clear some of the definitions. |
| 13-69 | 13 | 3 | 10 | | | (a comma is needed after the parenthesis, before "and sea level relative to...") [David Burton, USA] | Editorial. |
| 13-70 | 13 | 3 | 13 | 3 | 41 | It should be stated more clearly to which variable these statements refer to. Some of them explicitly refer to global mean sea level (GMSL). Others do not even cite what they are referring to, for instance bullet #4 : regional rates? global rates? [Eduardo Zorita, Germany] | Accepted - section to be rewritten. |
| 13-71 | 13 | 3 | 13 | 3 | 43 | There are too many different units for sea level change in this short passage, ranging from m to m kyr-1 to mm a-1. That's confusing and hampers direct comparison. [Uwe Stoeber, Germany] | Accepted. We have changed these units so as to be consistent. Rates will be reported in mm/yr and sea level rise in m. |
| 13-72 | 13 | 3 | 15 | 3 | 18 | Tide gauge measurements are subject to influences that are additional to geological knowledge such as changes in local land from habitation and ability of measuring equipment to withstand the sea [VINCENT GRAY, NEW ZEALAND] | Noted. We make specific mention of these non-climatic effects on p. 13-8, line 34-39, and note on p. 13-16 (line 13-26) that tide gauges may be subject to geological effects. |
| 13-73 | 13 | 3 | 15 | 3 | 43 | Give time periods for palaeo eras [Ian Allison, Australia] | Accepted. We have added this information. |
| 13-74 | 13 | 3 | 15 | 3 | 43 | Confusion between a and kyr. Should be yr and kyr, or a and ka. The international stratigraphic convention is to use "a", "ka", and "Ma" for ages, and "y", "ky", and "My" for durations. Increasingly, publishers invent their own conventions, but the international stratigraphic agreement is as outlined here. In any case, consistency is key. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. We have changed these units so as to be consistent. |
| 13-75 | 13 | 3 | 19 | 3 | 19 | These CO2 concentrations mean little, unless authors also state current levels plus a range of those anticipated by 2100 [Robert Thomas, USA] | Taken into account. We have now added pre-Industrial and current levels. |
| 13-76 | 13 | 3 | 19 | 3 | 22 | The range of CO2 shown (350-415 ppm) is different from that shown in Chapter 5, page 3, lines 17-18 (330-420). [Henry Pollack, USA] | Taken into account. This has been corrected. |
| 13-77 | 13 | 3 | 19 | | | replace "about 3 million ago," by "which lasted from 5.3 to 2.6 million years ago," [Ernst Schrama, Netherlands] | Rejected. We are referring specifically to the middle Pliocene time when sea levels are thought to have been higher. |
| 13-78 | 13 | 3 | 20 | 3 | 28 | The text explains how much warmer the Pliocene was, so it should also state how much warmer the Eemian was, since it is far more recent and presumably easier to measure. The text seems to imply that the Pliocene had a stable climate without a glacial-interglacial cycle. If that is not the case, then the text needs some clarification [James G Titus, United States of America] | Taken into account. We have added how much warmer the last interglaciation was (from Chapter 5). |
| 13-79 | 13 | 3 | 23 | 3 | 23 | State when last interglacial was [Robert Thomas, USA] | Taken into account. We have added this. |
| 13-80 | 13 | 3 | 23 | 3 | 24 | This sentence is oddly phrased. A more appropriate phrasing would be that "There is high confidence that during the LIG, GMSL was more than 6 m higher than current values and less than 10 m above current values." [Robert Kopp, USA] | Accepted. |
| 13-81 | 13 | 3 | 23 | 3 | 25 | Chapter 5 highlights that last interglacial sea level was 4-6 m higher than today whereas Chapter 13 highlights that last interglacial sea level greater than 6 m higher than today and possibly 10m; these statements are inconsistent. Suggest that this inconsistency be resolved as well as confidence and likelihood statements (e.g. is the confidence lower than stated and uncertainty higher than implied by these ranges or is one of these statement incorrect?). [Haroon Khesghi, United States of America] | Taken into account. These changes will be made in consultation with Chapter 5. |
| 13-82 | 13 | 3 | 23 | 3 | 28 | I have questioned above the reliance on one, as yet unpublished study (Dutton and Lambeck, subm.) versus a published study (Kopp et al) to define these precise bounds, though I note that numbers are very similar in | Taken into account. The intent is to use the similarity of both studies (as noted by the reviewer) as |

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| | | | | | | both studies. [Mark Siddall, UK] | corroborating evidence for the higher values. Will be rewritten to reflect this. It is acceptable to place the same emphasis on submitted papers as compared to published papers at this point. |
| 13-83 | 13 | 3 | 23 | 25 | | It would be useful to state the approximate timing, CO2 concentration and global temperature for the last interglacial period, for reader information and consistency with the bullet point immediately above. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland] | Accepted. We have added this information. |
| 13-84 | 13 | 3 | 27 | 3 | 28 | What is the difference between solar changes and surface warming? More is needed. [Ronald Stouffer, USA] | Taken into account. We have added more information to clarify this. |
| 13-85 | 13 | 3 | 27 | | | delete 'about' [Elie Verleyen, Belgium] | Editorial. |
| 13-86 | 13 | 3 | 28 | 3 | 18 | "... half to surface warming" - ice sheet surface? [Donald Forbes, Canada] | Taken into account. We have added more information to clarify this. |
| 13-87 | 13 | 3 | 28 | 3 | 28 | Given that this is the Executive Summary, this phrase should be clarified - I would replace "surface warming" with "higher surface air temperatures" or similar. [John Hunter, Australia] | Accepted. |
| 13-88 | 13 | 3 | 29 | 3 | 29 | I assume that there is some perceived good reason for using (effectively identical) units of m kyr-1 for paleo data and mm a-1 for modern data., but I find it confusing. I don't think there is anything inherent in units to indicate the time scale over which the quantity has been averaged, so there should be no difference between the units used for paleo and modern data. Also, should it be "mm yr-1" or "mm a-1"? [John Hunter, Australia] | Accepted. Part of the issue is averaging low resolution data over very long periods versus high-resolution data over short periods. We have changed these units so as to be consistent. Rates will be reported in mm/yr and sea level rise in m, unless otherwise specified for these cases. |
| 13-89 | 13 | 3 | 29 | 3 | 29 | "interglaciation" or "interglacial"?? Also, the rate of sea-level rise quoted is only 1 to 2.5 mm/yr; hardly a big deal since present rates are already higher!!!! [Robert Thomas, USA] | Taken into account. "Interglaciation" is the noun, "interglacial" is an adjective. We are working with Chapter 5 to make this grammatically correct useage consistent in the report. The point about rates is well taken. |
| 13-90 | 13 | 3 | 29 | 3 | 33 | Again I have questioned the reliance of this number on only one study, though the numbers in the Dutton et al study are in agreement with Kopp et al [Mark Siddall, UK] | Taken into account. The intent is to use the similarity of both studies (as noted by the reviewer) as corroborating evidence for the higher values. Will be rewritten to reflect this. |
| 13-91 | 13 | 3 | 29 | 3 | 33 | This entire bullet is misleading. I assume the first rate is the average over some longer period. If so, what period? saying "interglacial" is not enough. And the final sentence is totally misleading. What are these upper limits? Surely not the 5.6 to 10 mm/yr quoted earlier. Indeed I doubt there is ANY evidence that would tell us what these upper limits are. But I note that these rates are considerably lower than ones quoted by the authors in second para of Section 13.3.1.2 on page 13.15. [Robert Thomas, USA] | Accepted - section to be rewritten. |
| 13-92 | 13 | 3 | 29 | | | sloppy typesetting at page-break kyr-1 [Ernst Schrama, Netherlands] | Editorial |
| 13-93 | 13 | 3 | 30 | 3 | 30 | I find "last two interglacial highstands" very confusing - it sounds as if it applies to the last two interglacials rather than to the last two highstands. I assume that this applies to the last interglacial highstand and to the holocene highstand. [John Hunter, Australia] | Taken into account. This has been reworded to clarify. |
| 13-94 | 13 | 3 | 31 | 3 | 31 | "shorter" than what? [James G Titus, United States of America] | Taken into account. This has been reworded to clarify. |
| 13-95 | 13 | 3 | 31 | 3 | 39 | there are various units used in this chapter for SLR - I suggest bringing them into line with each other to aid comparison [Mark Siddall, UK] | Accepted. Part of the issue is averaging low resolution data over very long periods versus high-resolution data over short periods. We have changed these units so as to be consistent. Rates will be reported in mm/yr and sea level rise in m, unless otherwise specified for |

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| | | | | | | | these cases. |
| 13-96 | 13 | 3 | 34 | 3 | 34 | There appears to be a conflation of different timescales here. Late Holocene to the 19th / 20th Centuries is a big difference. Comparing the mean of the last few thousand years with the last couple of hundred years is unreasonable since there is variation on many timescales. I'm aware of the Kemp et al (2011) paper but I find it hard to imagine that there can have been so little impact on sea level from the Little Ice Age or Medieval Warm Period. Recent evidence is suggesting that pre-industrial sea level was far from stable (e.g. Toker, E. et al., 2012. Evidence for centennial scale sea level variability during the Medieval Climate Optimum (Crusader Period) in Israel, eastern Mediterranean. Earth and Planetary Science Letters, 315-316, pp.51-61.; Long, A.J. et al., 2012. Relative sea-level change in Greenland during the last 700yrs and ice sheet response to the Little Ice Age. Earth and Planetary Science Letters, 315-316, pp.76-85.; Barlow, N.L.M., Shennan, I. & Long, A.J., 2012. Relative sea-level response to Little Ice Age ice mass change in south central Alaska: Reconciling model predictions and geological evidence. Earth and Planetary Science Letters, 315-316, pp.62-75.) The mean sea level change over the last 2000 years may have been close to zero but the variability certainly wasn't. [Simon Holgate, UK] | Taken into account. This comment has been shared with Chapter 5, and we will revise according to their revision. |
| 13-97 | 13 | 3 | 34 | 3 | 35 | late 19th to early 20th century (1840-1920) [Roland Gehrels, United Kingdom] | Accepted. This has been reworded as suggested. |
| 13-98 | 13 | 3 | 34 | 3 | 35 | 1840-1920 is not the "late 19th century" . If there is a difference of opinion on which of those time periods is accurate, the text should say so. Otherwise, it would be best to pick one or the other. [James G Titus, United States of America] | Accepted. This has been reworded as suggested. |
| 13-99 | 13 | 3 | 34 | 40 | | I would suggest using the units "mm yr ⁻¹ " rather than "mm a ⁻¹ " for consistency with previous units used in this section (i.e. "m kyr ⁻¹ "). [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland] | Accepted. Part of the issue is averaging low resolution data over very long periods versus high-resolution data over short periods. We have changed these units so as to be consistent. Rates will be reported in mm/yr and sea level rise in m, unless otherwise specified for these cases. |
| 13-100 | 13 | 3 | 35 | 3 | 36 | sloppy typesetting at page-break of a-1 [Ernst Schrama, Netherlands] | Editorial |
| 13-101 | 13 | 3 | 38 | 3 | 38 | This point again suggests a larger increase in rates in recent times than decadal variation in tide gauges suggests. The altimetry period may indeed be larger but the decadal rates from Holgate and Woodworth (2004), Church and White (2006) and Holgate (2007) all suggest that there were significant variations in the global rate over the past century. [Simon Holgate, UK] | Accepted - section to be rewritten. |
| 13-102 | 13 | 3 | 38 | 3 | 40 | This statement is misleading. Early tide-gauge measurements were subject to upwards bias from storm damage to equipment, depression of neighbouring land from weight of building and removal of ground water and minerals, and from harbour modifications to increase ship access. Recent measurements corrected using modern levelling equipment have shown little change for the last ten years. Also the satellite measurements have levelled off recently. The overall averaged figure ignores regional and local fluctuations whose details you do not supply. [VINCENT GRAY, NEW ZEALAND] | Rejected - comments on early measurements are irrelevant as all recent work has accounted for these issues. Taken into account - the comment on sea level over the last decade. Rejected - the comment about ignoring regional and local effects as these are accounted for in the global average. |
| 13-103 | 13 | 3 | 38 | 3 | 40 | This bullet will lead many people to conclude that therefore, we know that the rate of sea level rise has doubled (or increased by 1.5 mm/yr). Some portion of that acceleration may be decadal variation, and the Church and White analysis only supports a 1 mm/yr acceleration over the last century. This bullet should either specifically endorse the view that there has been an acceleration of 1.5 mm/yr, or perhaps consider saying that the most thorough time-series analysis find that there has been a sustained acceleration of 1 mm/yr, and we do not know whether the rest of the acceleration is decadal variation or something more permanent. [James G Titus, United States of America] | Accepted - section to be rewritten. |
| 13-104 | 13 | 3 | 38 | | 40 | The bullet is wrong, on four counts: [David Burton, USA] | The comment appears to be missing information (the specifics of the four counts). No action possible. |
| 13-105 | 13 | 3 | 39 | 3 | 40 | This is confusing because it appears to compare an average taken over ~100 years with an average taken over 19 years. It implies an acceleration which it should not. There needs to be an indication that the trend, when averaged over a period of about 2 decades (like the satellite data), varied over a range of ~0 to 2.4 | Accepted - section to be rewritten. |

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| | | | | | | mm/year over the 20th century, with an uncertainty of ~ +/- 1 mm/year (Church and White, 2011). In other words, the present satellite observations are not (statistically speaking) indicative of an acceleration over the 20th century. I think that this is a really important issue as it has lead to the common and oft-publicised perception that the satellite data indicates an acceleration in sea-level rise at the end of the 20th century - it doesn't. [John Hunter, Australia] | |
| 13-106 | 13 | 3 | 39 | | | If the 20th century rise rates appear to be so much smaller that what has happened before (p. 13 l. 31), why should we bother at all? [Uwe Stoeber, Germany] | Rejected. IPCC is charged with determining if sea level has changed, and why and how will it change in the future in response to changing greenhouse gas concentrations. |
| 13-107 | 13 | 3 | 41 | 3 | 41 | Previous bullet mentions tide gauge data while here we say in situ. Is there a reason to use the broader term? [James G Titus, United States of America] | Accepted - will be reworded for consistency. |
| 13-108 | 13 | 3 | 42 | 3 | 43 | I think the relationship between global mean sea level and ENSO deserves emphasising, given the present "skeptical" mantra that "sea-level rise has slowed". This isn't a criticism, but just a suggestion that this message shouldn't be lessened or lost as this chapter undergoes further development, and it should possibly be enhanced. [John Hunter, Australia] | Accepted - section to be rewritten. |
| 13-109 | 13 | 3 | 45 | 4 | 33 | I feel that since the AR4, there has been lot of advancement in understanding the link between geoidal changes and its reflection into the sealevel change. Therefore, it would be a good to see at least some introductory paragraphs about it under the "contribution" section. The Contributions of the Solid Earth (the effect of the viscosity of the Earth in response to movements of ocean or ice mass at its surface), Cryosphere (the best estimates that are available of the spatial distribution of changing ice sheet) and the Ocean (effects of thermal expansion, and of historical and present inputs of freshwater from the ice sheets) could be adequately emphasised. [Dhananjai Kumar Pandey, India] | Rejected. Each of the suggested components identified by the reviewer are discussed here, with the exception of GIA effects, which are removed as necessary. |
| 13-110 | 13 | 3 | 49 | 3 | 50 | What is 30%? Ocean warming or GMSL? [Ian Allison, Australia] | Taken into account. We have clarified this. |
| 13-111 | 13 | 3 | 49 | 3 | 50 | No period is given and this is just a speculative calculation. There is evidence that sea temperatures change periodically and that they are now beginning to fall [VINCENT GRAY, NEW ZEALAND] | Rejected. The start of the period is defined on line 48 ("Since the early 1970's"). Reviewer provides no publication in support of evidence for sea surface temperature change. |
| 13-112 | 13 | 3 | 49 | 3 | 50 | This is similar to the previous comment - this sounds like a comparison of an average over about 40 years with one over about 20 years. I don't know how one addresses this general problem, as this chapter must be full of estimates made over different time spans. Somehow you need to have a caveat (implicit or explicit) whenever estimates of averages taken over different time spans occur close to each other (e.g. in the same sentence). [John Hunter, Australia] | Accepted - section to be rewritten. |
| 13-113 | 13 | 3 | 51 | 3 | 51 | Glaciers contribution to GMSL=35% is not correct, see comment 1. [Vladimir Konovalov, Russian Federation] | Rejected. We do not see a comment 1, and do not see an explanation as to why this value for glacier contribution is incorrect. |
| 13-114 | 13 | 3 | 51 | 3 | 51 | How could anyone state that glaciers contributed 35% of the SLR? There is no error bars on that number, no attached time period. This is a red flag for a report with the quality standard of IPCC. I don't think Chapter 4 quoted a number like this. Moreover, whether it is 35% or 25% or 40% is not relevant for the take home message of this bullet. [Eric Rignot, USA] | Rejected - Values given in Chapters 4 (Table 4.4) and 13 (Table 13.1) show glaciers contributed 42% of GMSLR for 1971-2010 and 33% for 2005-2010. "About 35%" since the early 1970s, which is what this section states, is perfectly appropriate. Uncertainties aren't given for any other number in this section. And why is this number not relevant? This appears with approximate magnitudes of thermal expansion, ice sheet contributions, terrestrial storage, and the comparison of the sum with observations. Mentioning glacier contributions seems reasonable. |
| 13-115 | 13 | 3 | 51 | 3 | 52 | No period is given and this is just a speculative calculation. There is evidence that glaciers change periodically | Rejected. We have added the periods, but all studies |

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| | | | | | | and that they are now beginning to advance [VINCENT GRAY, NEW ZEALAND] | that we are aware of show no evidence for the statement that glaciers are beginning to advance, with a few exceptions which will be discussed in SOD. |
| 13-116 | 13 | 3 | 53 | 3 | 53 | What does it mean to say that the ice sheet contribution was small since the 1970s? Small relative to what? We don't have a whole lot of data prior to the 1990s in Antarctica, so I don't know the basis for this statement about Antarctica. Even so, what is the point of using a reference to the 1970s? [Eric Rignot, USA] | Accepted - section to be rewritten. |
| 13-117 | 13 | 3 | 53 | 3 | 55 | Sheer speculation, unconfirmed by actual measurements [VINCENT GRAY, NEW ZEALAND] | Rejected. We are not aware of any measurements that differ from this conclusion. |
| 13-118 | 13 | 3 | | 6 | | Chapter 13 is well written. The authors make some tough, but right decisions. [Terje Wahl, Norway] | Thank you. |
| 13-119 | 13 | 3 | | | | Density changes in the upper layers of the oceans do not affect average coastal sea levels. If an area of water becomes less dense, because it freezes, or because it warms and expands, the water doesn't run downhill and cause sea levels to rise at distant shores. Instead, it rises up, in place, and does not affect sea levels elsewhere at all, because its displacement (mass!) is unchanged. That means that a change in the density of the oceans (due to temperature change) will affect the depth of the deep oceans (though without affecting the pressure at the bottom!). But it cannot affect sea level at the shorelines. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-120 | 13 | 3 | | | | That has interesting implications w/r/t sea levels in the context of the broader debate about climate change. It means that if the oceans warm then ocean surface altimetry measurements, from satellites over the deep ocean, will show the water level rising, but sea level measurements at the coast will not be affected. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-121 | 13 | 3 | | | | If the oceans were to warm everywhere, from top to bottom, by enough to decrease their density by 0.01% (which would require an average increase of about 1 degree Celsius, but less in the tropics), then the depth of the ocean would increase everywhere by 0.01%. [David Burton, USA] | Noted. No response needed. |
| 13-122 | 13 | 3 | | | | Where the Pacific Ocean is 5000 meters deep, adding 0.01% would increase the depth by 1/2 meter. But at shorelines, 0.01% of zero is zero, so there would be no effect at all on sea level at the world's coasts. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-123 | 13 | 3 | | | | Since the only place that sea level has any practical importance is at shorelines, the temperature of the oceans (like the percentage of ocean water that is frozen into solid floating ice or slush) has no direct effect on sea levels. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-124 | 13 | 3 | | | | Gravity causes the mass of fluids to be balanced, not their height. That's why icebergs stick up above the level of the water around them. Except for objects resting on the ocean floor, that is true for anything floating around in the ocean -- including the water, itself, whether it is solid or liquid. [David Burton, USA] | Noted. No response needed. |
| 13-125 | 13 | 3 | | | | 1. The "1.7 mm/yr" figure for 20th century SLR includes an addition of 0.3 mm/yr GIA, to account for hypothetical sinking of the ocean floor. However, this document defines sea level as level of the SURFACE of the ocean relative to the Earth's center of mass, which, by definition, excludes Peltier's 0.3 mm/yr GIA. If 0.3 mm SLE of water is added to the oceans but is exactly compensated by a 0.3 mm SLE sinking of the ocean floor, then GMSL, defined as the level of the surface of the ocean relative to the Earth's center of mass, is unchanged. It is not +0.3 mm/yr. It is an error to include Peltier's 0.3 mm/yr GIA in the GMSL figure. It is potentially useful for water-mass balance calculations, and that sort of thing, but is an error to include it in GMSL. Thus, 1.4 mm/yr is the correct number to use (though the median and geographically-weighted average of the best GLOSS-LTT tide gauges yields only 1.1 mm/yr). [David Burton, USA] | Rejected. The 0.3 mm/yr is an adjustment to satellite altimetry data required to estimate ocean volume. It is not the adjustment that is allowed for vertical movement of tide gauges and changing ocean shape. For example, sea level is rising about twice the global average on some parts of the US east coast. There is a need to carefully describe sea level. |
| 13-126 | 13 | 3 | | | | 2. There's no confidence interval given. [David Burton, USA] | Accepted. Uncertainties will be added |
| 13-127 | 13 | 3 | | | | 3. The 3.2 mm/yr figure is NOT comparable to the 1.4 (or 1.7) mm/yr figure, since the former is satellite-measured deep ocean sea level, and the later is tide-gauge-measured coastal sea level. Conflating SLR measurements from different locations, to create the illusion of acceleration, is a severe error! [David Burton, USA] | Rejected. Where there is overlap, the tide gauge and satellite data agree. |

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| 13-128 | 13 | 3 | | | | 4. The 3.2 mm/yr figure for satellite-measured sea level is incorrect: it adds Peltier's 0.3 mm/yr GIA (which is part of ocean depth, but not part of sea level!), and it uses outdated figures for satellite measurements. The latest Jason and Envisat measurements have GMSL rising at a much lower rate; even the old Topex/Poseidon 1994-2006 data showed only 3.12 mm/yr: http://www.aviso.oceanobs.com/fileadmin/images/news/indic/msl/MSL_Serie_TP_Global_IB_RWT_NoGIA_Adjust.png (nearly linear, very slight deceleration apparent) Jason-1 2002-2012 shows only 2.26 mm/yr (and clearly decelerating): http://www.aviso.oceanobs.com/fileadmin/images/news/indic/msl/MSL_Serie_J1_Global_IB_RWT_NoGIA_Adjust.png Envisat 2004-2012 (which has the broadest ocean coverage) shows only 0.45 mm/yr: http://www.aviso.oceanobs.com/fileadmin/images/news/indic/msl/MSL_Serie_EN_Global_IB_RWT_NoGIA_Adjust.png Jason-2 2008-2012 (which is just 4 years of data, so trends are unclear) shows only 0.87 mm/yr: http://www.aviso.oceanobs.com/fileadmin/images/news/indic/msl/MSL_Serie_J2_Global_IB_RWT_NoGIA_Adjust.png [David Burton, USA] | Rejected. The Topex/Poseidon/Jason-1 and 2 series of satellite are the most accurate. Envisat has significant deficiencies in measuring GMSL. The 0.3 mm/yr is an adjustment to satellite altimeter data required to estimate ocean volume. It is important to compare different measurements over the same period because of natural variability. |
| 13-129 | 13 | 3 | | | | replace "preindustrial" by "pre-industrial" [Ernst Schrama, Netherlands] | Editorial. |
| 13-130 | 13 | 3 | | | | inconsistent typesetting of a-1 and year-1 throughout this chapter, also, ca-1 and century-1 etc [Ernst Schrama, Netherlands] | Taken into account. We have changed these units so as to be consistent. |
| 13-131 | 13 | 4 | 1 | 4 | 1 | 'Storage in dams' should read 'storage in dammed reservoirs'. Only in Australian does "dam" by itself stand for "reservoir of freshwater." [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. |
| 13-132 | 13 | 4 | 1 | 4 | 7 | These are only estimates and they do not entirely agree with the observations [VINCENT GRAY, NEW ZEALAND] | Rejected. We discuss at length the comparison of observations and estimated components, which leads us to our conclusion as stated here. |
| 13-133 | 13 | 4 | 3 | 4 | 3 | Add "contributing a net rise in sea level." [Ian Allison, Australia] | Accepted. |
| 13-134 | 13 | 4 | 3 | | | Add bullet: Peltier's GIA estimate that sinking of the ocean floor offsets 0.3 mm/yr SLR. But note that that figure, though very widely used, is highly uncertain. [David Burton, USA] | Rejected. Not information for an Executive Summary. |
| 13-135 | 13 | 4 | 4 | 4 | 4 | The 25% difference is a number thrown in with no error bar. I recommend using the IPCC language. [Eric Rignot, USA] | Taken into account. Error bar will be added. |
| 13-136 | 13 | 4 | 4 | 4 | 4 | This executive summary is too long and has too many points that dilute the overall message. I did not get to read this draft until now, which is unfortunate. The glacier part is not well written and not consistent with Chapter 4. It is not correct to state that the ice sheets are melting because of an ocean trigger. In Greenland, half of the mass loss is from increased runoff which is a direct consequence of warmer air temperature. Furthermore, it is absurd to say that the estimates for glaciers improved. Improved compared to what? Does it mean improved compared to AR4? In which case, this statement is really part of Chapter 4, not Chapter 13 [Eric Rignot, USA] | Taken into account. We have shortened the Executive Summary. We are working closely with Chapter 4 to make sure that all statements regarding observations are consistent with those made in Chapter 4. |
| 13-137 | 13 | 4 | 6 | 4 | 7 | it is "satellite altimetry", furthermore Argo measurements require a reference. [Ernst Schrama, Netherlands] | Taken into account. We have inserted "satellite" but do not add a reference since they do not belong in the Executive Summary. |
| 13-138 | 13 | 4 | 9 | 4 | 9 | improved since the AR4? [Mark Siddall, UK] | Taken into account. We have added this to the text. |
| 13-139 | 13 | 4 | 9 | 4 | 33 | You have ignored most of what is happening with sea levels in order to concoct a highly distorted overall supposedly "observed" figure with the sole object of trying to confirm your dubious models [VINCENT GRAY, NEW ZEALAND] | Rejected. Without providing details on what the reviewer states is happening with sea level, we are unable to respond to this. |
| 13-140 | 13 | 4 | 10 | 4 | 10 | Over what period are the models supposed to agree with observations? Which observations and which models? Since all the reconstructions differ to some extent (Domingues et al; Church et al; Ishii and | Accepted - section to be rewritten. |

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| | | | | | | Matsumoto; Levitus et al; Smith and Murphy; Carton et al) and the models all differ to some extent, how can we say that "the models" reproduce "the observed" variability? Is the sentence supposed to mean that the model RMS is similar to the RMS from different reconstructions? How similar? I assume this is temporal variability rather than spatial variability? I think some clarification is required here. [Simon Holgate, UK] | |
| 13-141 | 13 | 4 | 10 | 4 | 10 | [Simon Holgate, UK] | There is no comment associated with this reviewer's name. It appears the reviewer's name was carried over from the immediately preceding comment. No action needed. |
| 13-142 | 13 | 4 | 10 | 4 | 10 | I also don't believe that global sea level variability is principally due to large volcanic eruptions. I appreciate that this is supposed to be just the thermosteric sea level but since we don't know exactly what the thermosteric sea level change has been, attributing all the variability to large volcanic eruptions is too much of a stretch. There are clearly other factors affecting global temperatures than volcanic eruptions unless the whole cooling from the 1940s to the late 1960s/early 1970s can be attributed to volcanism. Volcanism may be a contributor but cannot be the "principal" driver of variability. [Simon Holgate, UK] | Taken into account. The available comparison from observations and models do confirm an important, and indeed dominant, role of volcanic forcing in variability in ocean heat content. Nevertheless the text will be reworded. |
| 13-143 | 13 | 4 | 10 | 4 | 13 | What about the 700-3000 m region? [Donald Forbes, Canada] | Taken into account. We have added this to the text. |
| 13-144 | 13 | 4 | 13 | 4 | 13 | warmed globally or in some places? [Mark Siddall, UK] | Taken into account. We have added this to the text. |
| 13-145 | 13 | 4 | 13 | | | This needs a note to the effect that abyssal ocean warming, if it is occurring, cannot be substantially from anthropogenic causes, due to the very slow turnover of the oceans (Atlantic conveyor est. 800 yrs). Otherwise, readers may erroneously presume that this is due to anthropogenic GHGs. [David Burton, USA] | Rejected. Our chapter is reporting observations - attribution is covered in Chapter 10. In addition, the time for surface signals to reach the bottom of the ocean is much faster than the overall overturning of the ocean. This is why CFCs are detected in some waters near the ocean floor. |
| 13-146 | 13 | 4 | 14 | 4 | 14 | The glacier inventory has indeed improved but it is still very incomplete especially when considering mass balance data sets. This should be made clear. [Simon Holgate, UK] | Taken into account. The inventory is now essentially complete, and this will be discussed in the SOD. |
| 13-147 | 13 | 4 | 17 | | 18 | This bullet (about Greenland and Antarctic SMB) needs to note that it is highly uncertain. The word "limited" helps to convey this, but I think it needs to be explicit. [David Burton, USA] | Accepted - will be rewritten |
| 13-148 | 13 | 4 | 20 | 4 | 22 | "Our current understanding of the causes of increased ice discharge in Greenland and Antarctica is that they have been largely triggered by local changes in ocean circulation and associated heat transport." However, in lines 28-29 on this page it is stated "sea level rise from ocean warming is a central part of the Earth's response to increasing greenhouse gas concentrations.". There is an inconsistency between these statements as, according to the 2nd sentence, increased ice discharge is related to ocean warming as a response to greenhouse gas concentrations. [Andrew Glikson, Australia] | Rejected. We think there is a clear and consistent distinction between the first sentence mentioned, which only refers to "local changes in ocean circulation and associated heat transport" as causing increased ice discharge, as opposed to the second sentence mentioned which refers specifically to sea level rise from ocean warming (thermosteric). |
| 13-149 | 13 | 4 | 23 | | | "has not been well-explored" is rather vague language. Be specific: insufficient models? Too few runs? Describe the limitation. [Michael Oppenheimer, USA] | Accepted - will be rewritten |
| 13-150 | 13 | 4 | 24 | 4 | 24 | The sum of contributions may explain the observed sea level rise but only within large error bars. Different authors claim to close the budget using different numbers, even for the highly observed last 5 years, so it is hard to see how we can be confident in closing the budget since 1970. [Simon Holgate, UK] | Noted. |
| 13-151 | 13 | 4 | 24 | | 26 | This bullet is erroneous. The so-called "faster rate of rise since the early 1990s" is really conflation of satellite measurements with tide-gauge measurements. Neither tide-gauges nor satellites are showing an increase in rate of SLR. Only by comparing measurements at DIFFERENT locations (apples-to-oranges) can an apparent (deceptive) increase be seen. [David Burton, USA] | Rejected. Even using only one methodology (tide gauges), there is a clear difference in the rate of sea level rise since 1993 compared with the rate since the 1970s. |
| 13-152 | 13 | 4 | 24 | | | "simulated contributions" is misleading. As noted at several places, the process-based models don't reproduce the ice sheet contribution. I assume you mean here that the components of the mass budget add up more or less to the observed sea level rise, but the implication of "simulation" is that all of the individual components can be satisfactorily modeled. [Michael Oppenheimer, USA] | Taken into account. Our assessment of published process-based models is that they do adequately simulate observed ice-sheet losses. |

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| 13-153 | 13 | 4 | 35 | 6 | 12 | These are all mere speculations which are completely out of touch with what is actually happening. You give no information at all about actual current observations, only your overall botched supposedly linear "change" [VINCENT GRAY, NEW ZEALAND] | Rejected. Without providing details on what the reviewer states is happening with sea level, we are unable to respond to this. |
| 13-154 | 13 | 4 | 37 | 4 | 37 | This paragraph gives the impression that the authors know, to the nearest centimetre, the limits on the range of sea-level rise in different scenarios. They don't. More honest would be, for instance: '...and about 0.4 to 0.7 m for RCP8.5'. Moreover, the tale is somewhat confused by the statement soon after that: 'Under RCP 8.5, the likely range reaches 0.84 m in 2100'. [Robert Thomas, USA] | Accepted - will be rewritten |
| 13-155 | 13 | 4 | 37 | 4 | 45 | This executive summary is below part compared to AR4 and does not reflect an assessment of the state of knowledge. If we do not have confidence in numerical models of ice sheet evolution, then the results of the models should NOT be presented as the basis of the summary assessment. The summary recognizes that model skills are low, that simpler models give higher rates; it does not mention the most important fact that ice sheets in the past 20 years have contributed more to SLR than any of these models can explain. This chapter team need to take the matter at heart and not limit the IPCC assessment to projections from faulty models. The criticism of semi-empirical models is harsh compared to ice sheet models - which for the most part are not evaluated - [Eric Rignot, USA] | Taken into account. Our assessment of models is that they do a better job at simulating recent ice-sheet behavior than the reviewer states. The reviewer does not provide and reference to literature that states otherwise. |
| 13-156 | 13 | 4 | 37 | | | "Very likely" is incorrect. "Unlikely" is correct. The error results from excessive reliance on modelling, and insufficient reliance on real, measured data. [David Burton, USA] | Rejected. The context of this statement is on the projections, when there are no data. |
| 13-157 | 13 | 4 | 37 | | | RCP has not been introduced. [Uwe Stoeber, Germany] | Editorial. |
| 13-158 | 13 | 4 | 38 | 4 | 38 | An advanced online publication in Nature on 8th Feb suggests that the GIC contribution to sea level during the GRACE epoch (and by implication for early periods) has been significantly over-estimated using extrapolation of in-situ data. The results raise questions over the accuracy of previous estimates of the past contribution of GIC to SLR and its rate of increase. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account - Jacob et al came out long after the First Order Draft was submitted, but can be included in the Second Order Draft. The results of that analysis, however, do not immediately invalidate all previous work. Note that the Jacob et al paper does not simply reveal overestimates by previous workers: their results are, in fact, lower than conventional estimates, but their the lower numbers also reflect an actual drop in glacier loss rates since 2007, and that drop was seen in conventional measurements as well (e.g. in Cogley, 2012). |
| 13-159 | 13 | 4 | 38 | 4 | 39 | Shouldn't the possibility that ice sheets might make the largest contribution to 21st century GMSL rise be acknowledged here? [Robert Kopp, USA] | Taken into account. "Might" is a vague word in uncertainty language. Material will be rewritten but our central estimates are not likely to have ice sheets make the largest contribution. |
| 13-160 | 13 | 4 | 38 | 4 | 39 | If possible, a judgment should be inserted with regard to how confident we are that it is "likely" that the ice sheet contribution will remain smaller than ocean thermal expansion and glacier melting. The recent acceleration of the ice sheet contribution and the large uncertainties argue against making an unqualified assertion. [Michael Oppenheimer, USA] | Taken into account. A statement to this effect will be added. |
| 13-161 | 13 | 4 | 38 | | | "Ocean thermal expansion" is likely to make a substantial contribution to satellite-measured open-ocean SLR (which is inconsequential for coastal planning), but it will not contribute to coastal SLR. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-162 | 13 | 4 | 39 | | 40 | "Surface melting from Greenland is very likely to make a positive contribution" is confusing on two levels: (1) "very likely" overstates the certainty, and suggests a very strong likelihood that such contribution will be significant; and (2) "positive contribution" could easily be misread to mean that it will be helpful, i.e., that it will reduce SLR. [David Burton, USA] | Rejected. Our assessment of the literature supports the "very likely" statement regarding contribution of Greenland surface melting to sea level rise, and that it will be a "positive contribution" is understood in the scientific literature. |
| 13-163 | 13 | 4 | 40 | | 43 | These model-derived predictions are wildly at variance with what the history of SL measurements tells us about the response of SL to GHG forcings. GHGs have been soaring for at least 2/3 century, and SLR hasn't | Rejected. We disagree with the statement that sea level rise up to now has not responded to greenhouse |

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| | | | | | | increased at all in response. It is highly unlikely that it will suddenly start responding dramatically to GHGs over the next 2/3 century. [David Burton, USA] | gas forcing, and have provided substantial information to demonstrate that this is the case. |
| 13-164 | 13 | 4 | 41 | 4 | 42 | Some assessment of confidence placed on these numbers should be given here. The discussion in 13.6.1 is muddy on this point. [Michael Oppenheimer, USA] | Accepted - will be rewritten |
| 13-165 | 13 | 4 | 41 | 4 | 43 | There ought to be a reference in this section to sea level rise estimates made Hansen and Sato 2011 (www.columbia.edu/~jeh1/mailings/.../20110118_MilankovicPaper.pdf), where they state "The 5 m estimate is what Hansen (2007) suggested was possible, given the assumption of a typical IPCC's BAU climate forcing scenario." (p.15) and "We suggest that a nonlinear process spurred by an increasing forcing and amplifying feedbacks is better characterized by the doubling time for the rate of mass disintegration, rather than a linear rate of mass change." (p. 19) [Andrew Glikson, Australia] | Rejected. The results of this paper are assessed elsewhere in the chapter to be unreasonable, but this is not of sufficient importance to be included in the Executive Summary. |
| 13-166 | 13 | 4 | 43 | 4 | 43 | "...the upper limit reaches 0.84 m...." [Ian Allison, Australia] | Accepted - will be rewritten |
| 13-167 | 13 | 4 | 43 | 4 | 43 | It is reasonable to provide the 0.84m for 2100, but I suggest providing the whole range as with the rest of the paragraph. [James G Titus, United States of America] | Accepted - will be rewritten |
| 13-168 | 13 | 4 | 47 | 4 | 48 | Likely ranges are reported using the probability scale in the uncertainty guidance. These are reported as having medium confidence using the evidence and agreement based scale, which is fine. However, the subsequent statements are less informative. How much confidence is attached to the agreement of process-based models with observations and physical understanding - be explicit. Similarly, if we do not have high confidence in modelling of ice-sheet dynamics does this mean that this is very low, low or medium confidence? [Timothy Carter, Finland] | Accepted - will be rewritten |
| 13-169 | 13 | 4 | 47 | 5 | 2 | This paragraph is important. Indeed there has been some improvement since AR4 in our ability to estimate possible "dynamic" losses from the ice sheets. I would claim that this is primarily because observations have given strong hints, which then forced the modelers to try to simulate them. I would also claim that "semi-empirical" models have led the way, with simulations closer to reality than the more elaborate "rigorous" models. This should not be surprising since we have advanced little since AR4 in developing the necessary "scientific basis", and semi-empirical models are driven to some extent at least by observed reality. [Robert Thomas, USA] | Noted. |
| 13-170 | 13 | 4 | 47 | | | Much as my comment on 4-24 indicated, this assertion about "process-based models" is confusing because it seems to imply that confidence extends to ice-sheet models. [Michael Oppenheimer, USA] | Taken into account. Our assessment of models will be more nuanced in the SOD. |
| 13-171 | 13 | 4 | 52 | 5 | 2 | Here you explain that semi-empirical models give higher projections than process-based models and then you basically dismiss the semi-empirical approach with the subsequent statements: "This might point to some presently unidentified or underestimated contribution." ... "It is not understood why semi-empirical models project a higher rate of rise than process-based models and there is no consensus about the reliability of semi empirical model projections." Since physics-based models are not capable of simulating the actual rise in sea level that has been observed (nor the rate of observed land ice decline) it seems very dangerous to dismiss the semi-empirical models. The semi-empirical approach has matured significantly since AR4, and the linear relationship between the rate of sea-level rise and temperature may prove to be just as valid, if not more so, than the process-based approach. This should be stated explicitly in the executive summary for Ch. 13. [Virginia Burkett, United States of America] | Rejected. Our assessment of process-based models is that they are capable of simulating processes sufficiently well to provide confidence, whereas a number of unknowns implicit in semi-empirical models leads us to assess them as not being physically plausible and subject to uncertainties that are difficult to quantify. We are not placing "all of our confidence in one approach". The higher projections of the semi-empirical models are an important reason why we have only medium confidence and give likely (rather than very likely) ranges. Another reason is our low confidence in projections of ice-sheet dynamical change on these timescales. In fact, as we state, there is not a consensus in the community about the reliability of semi-empirical models. There is much greater agreement about projections of the contributions, apart from ice-sheet dynamics, but even in that area it is now possible to give quantitative projections. Because this was not possible at the time of the AR4, they were omitted from the ranges given; the AR4 did not underestimate their importance, but |

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| | | | | | | | was not able to quantify them at all. We have also added some extra qualifiers about the potential for larger rises. |
| 13-172 | 13 | 4 | 53 | 5 | 2 | There surely must be some understanding of how semi-empirical models are producing the high estimates they do and process-based models lower estimates. This would require an appreciation of how each of these approaches is being applied, so I wonder if the inability to comprehend the differences reflects a real shortfall in evidence to explain the results obtained, or rather a methodological schism in the scientific community and lack of willingness to engage across the divide, or a combination of these. In any case, it is encouraging to see some attempt, at least, to quantify the components that were "missing" from the AR4. Finally in this section, is there an upper limit that can be defined for global SLR by 2100 (sometimes referred to as the H++ scenario)? It would be useful to bound this, from different theoretical perspectives, even if no likelihood can be attached to the specific values presented. At least this would show what global mean SLR can be ruled out, presumably with some "highish" level of confidence, by 2100. [Timothy Carter, Finland] | Noted. We have struggled hard to understand the differences between the approaches. Rejected. We do not provide an upper bound for which we can not provide a likelihood but we do discuss the possibility of higher rises. |
| 13-173 | 13 | 4 | 53 | | | Rahmstorf's so-called "semi-empirical models" are discredited, and do not belong in this report [David Burton, USA] | Rejected. We do not know of any peer-reviewed literature that has completely discredited semi-empirical models. |
| 13-174 | 13 | 4 | 54 | 4 | 54 | Could it not also point to a flaw in the use of semi empirical models to parts of the the system parameter space that have not be used in the calibration of the model (i.e. a GHG induced warmer future) [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Noted. Indeed this is one of the times discussed. |
| 13-175 | 13 | 4 | 54 | 4 | 54 | This might point to some presently unidentified or underestimated contribution OR TO PROBLEMS WITH THE SEMI EMPIRICAL APPROACH WHICH HAVE NOT YET BEEN FULLY IDENTIFIED.' [Mark Siddall, UK] | Accepted. |
| 13-176 | 13 | 5 | 6 | | | "critically" should be changed to "little" [David Burton, USA] | Rejected. The use of the word "little" here would not correctly convey our point. |
| 13-177 | 13 | 5 | 7 | 5 | 7 | My apologies if I am being pedantic here - but the word "global temperature" is used frequently - I assume it means "global-average surface air temperature" - is this defined anywhere? Would the general reader know what it means? Could the words "global temperature" be better replaced by "radiative forcing"? [John Hunter, Australia] | Taken into account. Text will be rewritten to clarify. |
| 13-178 | 13 | 5 | 9 | 5 | 21 | Careful thought should be given to the phrasing here, as at the moment it can leave the misleading initial impression that GIS is expected to grow until temperatures reach 3.1 C. [Robert Kopp, USA] | Taken into account. Text will be rewritten to clarify. |
| 13-179 | 13 | 5 | 10 | 5 | 10 | "... for global average temperature [increases] above ..." [Donald Forbes, Canada] | Accepted. We have added the word "increases" here. |
| 13-180 | 13 | 5 | 10 | 5 | 10 | Are you missing the text "above pre-industrial" here ?? (refer to page 35, line 11 in chapter 13) [Bogi Hansen, Faroe Islands] | Accepted. We have clarified this. |
| 13-181 | 13 | 5 | 10 | 5 | 10 | I can find no general explanation of the meaning of '±' either in Chapter 1 or here. Is it +/- one standard deviation or +/- some specific percentile? The TAR Chapter 11 (Sea Level) contained a nice little Box (11.1) entitled "Accuracy" which described the meaning and use of uncertainties. I would assume that the IPCC has moved on from 2001 and now has a consistent useage of such uncertainties across all Assessment Reports - but I can find no general discussion of it anywhere, although there are several statements like "the uncertainties quoted are one standard deviation" scattered through this chapter. There should be some general guidance on the meaning of '±' when it appears (unqualified) in sentences like this one. [John Hunter, Australia] | Accepted. The uncertainty will be defined. |
| 13-182 | 13 | 5 | 15 | 5 | 15 | value of what? [Mark Siddall, UK] | Taken into account. We have clarified this. |
| 13-183 | 13 | 5 | 16 | 5 | 16 | "models" should be "models'" (with an apostrophe) [John Hunter, Australia] | Editorial |
| 13-184 | 13 | 5 | 18 | 5 | 18 | typo: outlets -> outlet [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Editorial |
| 13-185 | 13 | 5 | 20 | 5 | 20 | shouldn't this be "West Antarctic Ice Sheet"? [Neil White, Australia] | Accepted. |

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| 13-186 | 13 | 5 | 22 | 5 | 22 | It is suggested to check the figure for the range of sea level rise by 2500 for the low-emission scenarios as the lower end of the figures (0.03 to 1.2) is significant lower compared to the figure estimated for 2100 (0.27 - 0.50 m see page 4, line 41). [Klaus Radunsky, Austria] | Accepted - will be rewritten |
| 13-187 | 13 | 5 | 22 | 5 | 24 | This bullet is both confusing and difficult to believe. First, does "For 2500" mean "by 2500" or "during the century 2500-2600", or during the year 2500? Second, what does "low-emission scenarios" mean? Depending on answers to these, the quoted numbers appear to be ridiculous, particularly a total 3 cm sea-level rise by 2500 (if that is what they mean??). Moreover the "high-emission scenario" seems to assume little or no increase in rates of SLR after 2100? [Robert Thomas, USA] | Accepted - will be rewritten |
| 13-188 | 13 | 5 | 22 | | 24 | There's no reasonable basis for projections to 2500. This should be deleted from the report. [David Burton, USA] | Rejected. There is literature available on this issue that is used for our assessment. |
| 13-189 | 13 | 5 | 24 | 5 | 24 | ".....trigger instabilities, and thus they may....." [Ian Allison, Australia] | Editorial |
| 13-190 | 13 | 5 | 26 | 5 | 45 | It is suggested to provide more specific information on regional sea level rise in relation to those regions that are specifically vulnerable to sea level rise (e.g. AOSIS countries) because such information would be very policy relevant. It would be helpful to learn also more about the skills of models to provide robust information on the regional distribution of sea level rise and how modelled data compare with experimental data. [Klaus Radunsky, Austria] | First comment - rejected. We need to give information for all of the globe. Second comment - accepted - will be rewritten. |
| 13-191 | 13 | 5 | 28 | 5 | 28 | I suggest: "Regional variability in sea-level..." [Robert Thomas, USA] | Editorial |
| 13-192 | 13 | 5 | 28 | 5 | 45 | This section reads as if we are presenting an exhaustive list of RSL effects, which we are not [Mark Siddall, UK] | Accepted - will be rewritten |
| 13-193 | 13 | 5 | 31 | | 33 | Regional sea level change differences are also due to local and regional subsidence. Some tide gauge graphs are needed here, illustrating rising and declining LMSLs and varying degrees of variability, at various GLOSS-LTT gauges. [David Burton, USA] | Rejected. Graphs are not included in the Executive Summary. |
| 13-194 | 13 | 5 | 34 | | 36 | The first sentence is inaccurate, because it assumes acceleration in GMSL which is not in evidence. [David Burton, USA] | Rejected. No mention or inference of acceleration is made here. |
| 13-195 | 13 | 5 | 43 | 5 | 43 | I think we can be stronger here with respect to saying that regional patterns will deviate from the global mean. I would say "virtually certain" rather than "very likely". Spatially varying patterns of heating and melt determine this even without dynamic effects. [Simon Holgate, UK] | Taken into account. Reworded. This will be true for some regions but it is unclear that this will be true for the majority of the oceans. |
| 13-196 | 13 | 5 | 44 | 5 | 45 | The sentence starting with "However..." is a bit meaningless unless you can quantify this statement more precisely. [Roland Gehrels, United Kingdom] | Rejected. We state that regional sea level rise will "very likely" be positive. |
| 13-197 | 13 | 5 | 44 | | | This section advises that it is very likely that a range of factors will result in regional patterns of SLR deviating significantly from the global mean. In making this statement, it should be qualified with some quantitative measure of what the likely extent of the deviation might be? [Phil Watson, Australia] | Accepted - will be rewritten |
| 13-198 | 13 | 5 | 49 | 5 | 52 | It seems unclear why uncertainties in the projected atmospheric forcing should make it more difficult to project extreme water levels compared to the projections of other extremes, e.g. those related to temperature. Some further explanation would be welcomed. [Klaus Radunsky, Austria] | Rejected. The context of this sentence is that uncertainties in projected atmospheric forcing make it difficult to specify regional changes. Changes in temperature are easy to project than changes in winds and thus extreme sea levels |
| 13-199 | 13 | 5 | 49 | 6 | 12 | I am surprised to read that these effects cannot even be resolved in a statistical sense using a multi-model comparison...? Statistical methods are mentioned in relevant section of the main text... [Mark Siddall, UK] | Rejected. There is no literature available on this subject. |
| 13-200 | 13 | 5 | 55 | | 57 | The 2nd and 3rd sentences in this paragraph are inconsistent with each other. [David Burton, USA] | Accepted - the material supporting the second sentence will move to WGII |
| 13-201 | 13 | 5 | 56 | 5 | 56 | I think that the two sentences need to be decoupled. Extreme sea levels may not be increased by atmospheric | Accepted - will be rewritten |

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| | | | | | | forcing alone but that is unrelated to the fact that increased flooding risk will arise from increases in MSL alone. This point needs clarification. Moreover, another reason that increases in coastal infrastructure need to differ from central GMSL projections is that there will be significant regional differences and the uncertainty in these is greater. [Simon Holgate, UK] | |
| 13-202 | 13 | 5 | 56 | 5 | 57 | I don't follow the final sentence. Rephrase and make it clearer. [Roland Gehrels, United Kingdom] | Accepted - will be rewritten |
| 13-203 | 13 | 5 | 56 | 5 | 57 | The sentence: If the expected frequency of flooding of coastal infrastructure is not to increase, Is difficult to understand. It is expected, that the frequency of flooding of coastal infrastructure is the result of modelling but not a fixed parameter. [Klaus Radunsky, Austria] | Accepted - - the material supporting the second sentence will move to WGII |
| 13-204 | 13 | 6 | 11 | 6 | 12 | "Over the next few decades, recovery of the ozone hole and increases in greenhouse gases are expected to have significant but opposing effects on the Southern Annular Mode and its attendant climate impacts during summer." This quote is from the abstract to the review article "Signatures of the Antarctic ozone hole in Southern Hemisphere surface climate change" by Thompson et al,2011, Nature Geosciences, page 741-749. Statements about the increase in the SAM may thus need some caveat to account for the changes due to ozone. [Stephen Griffies, USA] | Accepted. |
| 13-205 | 13 | 7 | 1 | 8 | 45 | I found this section under-referenced and is perhaps 'over-introduction' given that the summary already acts as an introduction. It may be that the chapter is repetitive because of the introduction. [Mark Siddall, UK] | Taken into account - We have revised the Introduction, but general format is not to heavily reference it. There will also be inevitable overlap with the Executive Summary is inevitable, since the E.S. should be a stand alone statement. We have also merged 13.1 and 13.2. |
| 13-206 | 13 | 7 | 1 | 13 | 48 | This section has no information whatsoever about actual observations of sea level. It is pure speculation based on doubtful theoretical calculations, which are completely at odds with what is currently observed [VINCENT GRAY, NEW ZEALAND] | Rejected - This is an introduction only. Without providing details on what the reviewer states is happening with sea level, we are unable to respond to this. |
| 13-207 | 13 | 7 | 9 | | 10 | Thermal expansion of upper ocean layers does not contribute to coastal SLR. [David Burton, USA] | Rejected - The ocean responds dynamically such that water flows onto the shelf and thus affects coastal sea level. |
| 13-208 | 13 | 7 | 11 | | 13 | This sentence states as fact that which is quite possible, but not confirmed by measurements. (That's a pervasive problem in this report: excessive reliance on plausible calculations which have not been confirmed by actual measurements.) [David Burton, USA] | Accepted - Reworded to "Observations indicate .." and a reference added. |
| 13-209 | 13 | 7 | 15 | 7 | 15 | "relatively" is superfluous here. Glaciers are simply more sensitive to climate change than ice sheets. [Simon Holgate, UK] | Accepted. This sentence has now been deleted. |
| 13-210 | 13 | 7 | 15 | 7 | 15 | relatively more sensitive' in proportion to their volume? [Mark Siddall, UK] | Accepted. This sentence has now been deleted. |
| 13-211 | 13 | 7 | 16 | 7 | 17 | In the sentence the mass rather than the volume of water stored in the ocean should be mentioned. [Mirko Orlic, Croatia] | Rejected - Volume is the correct word here. This sentence has now been deleted. |
| 13-212 | 13 | 7 | 21 | 7 | 21 | This may be the best place to discuss in detail which aspects of RSL are included and which are not [Mark Siddall, UK] | Rejected - We feel this is too much detail for this general Introduction. |
| 13-213 | 13 | 7 | 24 | 7 | 25 | See comment (1) to Chapter 13, page 3, lines 7-9. [John Hunter, Australia] | Rejected - We think it is important to make the point first that mean sea level will rise, and as a result of this, there will be an increase in sea level extremes. Not all coastal regions will be subject to extremes, particularly protected areas, and so the two are not always the same. This sentence has now been deleted |
| 13-214 | 13 | 7 | 25 | 7 | 25 | terms like 'extreme sea-level events' need defining the first time they are mentioned [Mark Siddall, UK] | Accepted - We have added more information. |

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| 13-215 | 13 | 7 | 29 | 7 | 29 | sorry if I am missing something, but is a date needed for Warrick or should it be Warrick and Oerlemans? [Mark Siddall, UK] | Accepted - The reference has been corrected. |
| 13-216 | 13 | 7 | 38 | | | Add: "The Third Assessment Report" noted the "observational finding of no acceleration in sea level rise during the 20th century." [David Burton, USA] | Rejected - The TAR recognised a change in the rate of rise between the 19th and 20th centuries but no 20th acceleration had been detected at that time. However, this is a level of detail below what is given here in a brief introduction. |
| 13-217 | 13 | 7 | 39 | 7 | 56 | I like this paragraph explaining the history of progress [Mark Siddall, UK] | thank you |
| 13-218 | 13 | 7 | 43 | 7 | 47 | I think this needs to be a bit clearer - "(rather than just 20th century trends)" should perhaps be "(rather than just mean trends over the 20th century)" [Neil White, Australia] | Accepted - We have revised this sentence as suggested. |
| 13-219 | 13 | 7 | 48 | 7 | 56 | The initial failure (and later only a partial consideration) of AR4 to recognize the potential for the Antarctic ice sheet to contribute to sea level rise over the next century even as the empirical evidence already showed it might already be contributing, was widely criticized in the literature. That part of the story needs to be told, especially given that the evolution of IPCC assessments is being told. The difficulties of the task are explained, and that is reasonable--but acknowledging that IPCC heard the message is just as important as folding its content into the revision--most especially when it was as big a story as it was in this case. [James G Titus, United States of America] | Rejected - we think the existing text clearly makes this point. |
| 13-220 | 13 | 7 | 52 | 7 | 53 | worth noting that the regional distribution also complicates the interpretation of past records (e.g., Bromirski), to point toward the discussion below (pp 8ff) [Philip Mote, USA] | Rejected. In the interest of keeping the revised text within length limits, we have not added this information. |
| 13-221 | 13 | 8 | 4 | 8 | 5 | Abbreviations FAR, SAR & TAR not defined despite being spelt in full on page 7 [Jeff Ridley, UK] | Accepted - We have made these changes as recommended. |
| 13-222 | 13 | 8 | 8 | 8 | 9 | <p>This section advises in part "Since the AR4, it is virtually certain that sea level has continued to rise at a rate faster than the 20th century average (Chapter 3)....". Such a statement would appear to have little scientific context. The rate measured over such a short time (ie. Since AR4) has little relevance to the average rate measured over a lengthy periods such as a century timescales. There are numerous published scientific papers (including Haigh et al (2009), Hannah (1990, 2004), Holgate and Woodworth (2004), Holgate (2007), Wahl et al (2011) and Watson (2011)) which show that recent rates of rise (post 1990) are not higher (or abnormal) than rates measured elsewhere in the historical records of long tide gauge records around the ocean basins of the world (refer also comment above at 3).</p> <p>References</p> <p>Haigh, I.D., Nicholls, R.J., Wells, N.C (2009). Mean sea level trends around the English Channel over the 20th century and their wider context. <i>Continental Shelf Research</i>, 29 (17), 2083-2098.</p> <p>Hannah, J. (1990). Analysis of mean sea level data from New Zealand for the period 1899-1988. <i>Journal of Geophysical Research</i>, Vol 95(B6), pp 12,399-12,405.</p> <p>Hannah, J. (2004). An updated analysis of long-term sea level change in New Zealand. <i>Geophysical Research Letters</i>, 31:L03307, DOI:10.1029/2003GL019166.</p> <p>Holgate, S.J., and Woodworth, P.L. (2004). Evidence for enhanced coastal sea level rise (during the 1990s). <i>Geophysical Research Letters</i>, 31:L07305, DOI:10.1029/2004GL019926.</p> <p>Holgate, S.J. (2007). On the decadal rates of sea level change during the twentieth century. <i>Geophysical Research Letters</i>, 34:L01602, DOI:10.1029/2006GL028492.</p> <p>Wahl, T., Jensen, J., Frank, T., Haigh, I.D. (2011). Improved estimates of mean sea level changes in the</p> | Accepted - the focus on the last few years has been deleted. |

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| | | | | | | German Bight over the last 166 years. Ocean Dynamics, 61 (5), 701-705. Watson, P.J. (2011). Is There Evidence Yet of Acceleration in Mean Sea Level Rise Around Mainland Australia. Journal of Coastal Research, Vol 27 (2), pp. 368-377, DOI: 10.2112/JCOASTRES-D-10-00141.1, February. [Phil Watson, Australia] | |
| 13-223 | 13 | 8 | 8 | | 9 | change "faster than the 20th century average" to "faster than the 20th century average, but no faster than the average over the last 3/4 of the 20th century" [David Burton, USA] | Accepted - the focus on the last few years has been deleted. |
| 13-224 | 13 | 8 | 23 | 8 | 29 | Is this really done (modeled) by the authors or do they review literature? [Uwe Stoeber, Germany] | Accepted - reworded to focus on "Assessment" |
| 13-225 | 13 | 8 | 26 | | 27 | Scrap the "semi-emperical model" junk [David Burton, USA] | Rejected. It is important for the complete literature to be assessed There is no basis for removing discussion and assessment of semi-empirical models. |
| 13-226 | 13 | 8 | 31 | 8 | 31 | here you go back to RSL discussion from paragraph 3 of the intro. Some rethinking about the structure would be useful. Also, please note at least the most common sign for these non-climate change effects [Mark Siddall, UK] | We do not see a need to restructure the section, but have clarified what the most common sign of non-climate effects is. |
| 13-227 | 13 | 8 | 34 | 8 | 34 | Repeated "do not" [Ian Allison, Australia] | We have deleted the extra "do not" |
| 13-228 | 13 | 8 | 34 | 8 | 34 | Delete "dot not" [Michel Boko, Benin] | We have deleted the extra "do not" |
| 13-229 | 13 | 8 | 34 | 8 | 34 | Repetition of "do not" [Simon Holgate, UK] | We have deleted the extra "do not" |
| 13-230 | 13 | 8 | 34 | | | delete 'do not' [Elie Verleyen, Belgium] | We have deleted the extra "do not" |
| 13-231 | 13 | 8 | 34 | | | Delete one of the "do not" (douple mentioned) [Thomas Voigt, Germany] | We have deleted the extra "do not" |
| 13-232 | 13 | 8 | 34 | | | Typo. Words "do not" duplicated. [Phil Watson, Australia] | We have deleted the extra "do not" |
| 13-233 | 13 | 8 | 35 | 8 | 35 | This is unclear. Suggest "...that are not climate related but which may be important for assessing its impacts." [Simon Holgate, UK] | Accepted - We have reworded the sentence as suggested. |
| 13-234 | 13 | 8 | 35 | | 37 | In "local relative sea level rise resulting from... compaction of sediments or the withdrawal of water or petroleum products is not considered." change the word "sediments" to "soil." Also, add to the end of the sentence: ", and because these processes cause LMSL increases, when the affected tide gauges are included in averages used to calculate GMSL, they inflate the calculated GMSL, unless GPS measurements are available to correct for the errors. [David Burton, USA] | Rejected - in geology, sediments is the correct term; soil is used in engineering language. Rejected - this is an introduction and not the place to make this type of assertions. |
| 13-235 | 13 | 8 | 52 | | | In my opinion the term 'contemporary' is very vague and needs precise definition. [Uwe Stoeber, Germany] | Rejected. This adjective has a specific meaning and its use here is consistent with this (i.e. sea level changes being produced by climate change happening at the same time). |
| 13-236 | 13 | 9 | 1 | 9 | 19 | I think these paragraphs need to be in the introduction and seem rushed and out of place here. They also need more references [Mark Siddall, UK] | Taken into account. Sections 13.1 and 13.2 are now combined into a single introductory section and more references have been added. |
| 13-237 | 13 | 9 | 2 | | | a comma is needed after the right-parenthesis, before "or the Earth's center..." [David Burton, USA] | Editorial. |
| 13-238 | 13 | 9 | 2 | | | "absolute sea level" is a poor term. Two different terms should be used throughout for the two different types of Global Mean Sea Level: perhaps OGMSL (Open-ocean Global Mean Sea Level) and CAMSL (Coastal Average Mean Sea Level). [David Burton, USA] | Taken into account - term removed. |
| 13-239 | 13 | 9 | 9 | 9 | 19 | I like this paragraph. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Noted. |

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| 13-240 | 13 | 9 | 23 | 9 | 23 | If the authors really want to stress the fact that sea level change is due to certain processes (a cause-effect), I would suggest to change the title to "Components Influenced by Contemporary Climate Change" [Belén Martín Míguez, Spain] | Accepted. |
| 13-241 | 13 | 9 | 30 | 9 | 33 | I am not clear why GIA and related effects are not on here. These seem just as 'direct' as FW influences on circulation [Mark Siddall, UK] | Rejected. As described in the text and figure caption, this figure only illustrates the components influenced by contemporary climate change. Glacial isostatic adjustment does not fall under this category (and so is described in 13.2.1.2.) |
| 13-242 | 13 | 9 | 37 | 9 | 37 | What is the "marine geoid"? I cannot explicitly find this term in Lambeck, 1988. I think more specific language should be used. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Taken into account - "marine" removed. |
| 13-243 | 13 | 9 | 40 | 9 | 41 | I believe the statement that salinity changes do not alter global mean sea level may need some qualifications. There is no general principle at work here. It is merely that the total salt in the ocean is nearly constant, and so the changes in salinity only project mostly on local sea level patterns but not global sea level. Is there any model study that supports this statement in a clear manner? [Stephen Griffies, USA] | Taken into account - text now reads "While both of these effects can contribute to regional sea level change (Church et al., 2010), only the thermosteric component produces a significant contribution to global average ocean volume" |
| 13-244 | 13 | 9 | 40 | 9 | 41 | "Changes in temperature affect global average ocean volume whereas changes in salinity do not". This disagrees slightly with Chapter 3, page 27, lines 26-28: "Although variations in the density related to upper-ocean salinity changes will cause regional changes in sea level, when globally averaged the effect on sea level rise is about an order of magnitude smaller than the thermal effects (Antonov et al., 2002)." [David Parker, United Kingdom of Great Britain & Northern Ireland] | Taken into account - see response to comment 13-243. |
| 13-245 | 13 | 9 | 40 | 9 | 42 | The relative effects of changes in temperature and salinity require more explanation, because this issue is frequently misunderstood. The important points are that: (a) temperatures changes naturally occur under conditions of constant ocean mass and so change the ocean volume through thermal expansion; (b) salinity changes naturally occur through the addition or removal of freshwater and so change the ocean volume directly. However, the removal or addition of freshwater is generally treated separately from salinity change and so the remaining effect of salinity is only to cause regional change and not to change the total ocean volume, I think this needs to be spelled out. [John Hunter, Australia] | Taken into account. This paragraph has been significantly revised and clarified. We note also that this paragraph only deals with freshwater transfer between the atmosphere and ocean. The following paragraph discusses freshwater flux between the land and oceans. |
| 13-246 | 13 | 9 | 40 | 9 | 42 | Saying that salinity has no effect is an overstatement, since salinity can contribute to ice melting which in turn can affect sea level. You probably mean no "direct effect." [James G Titus, United States of America] | Taken into account - see responses to comments 13-243 and 13-245. |
| 13-247 | 13 | 9 | 40 | 9 | 42 | Getting a bit pedantic here, but, given the non-linearity of the equation of state, is this strictly true? [Neil White, Australia] | Taken into account - see response to comment 13-243. |
| 13-248 | 13 | 9 | 40 | | 41 | The statement that "changes in temperature affect global average ocean volume whereas changes in salinity do not" is apt to confuse the reader. Changes in salinity result from processes that change the amount of water in the ocean, which do change sea level. But changes in temperature, which affect total (not "average!") ocean volume nevertheless do not affect coastal sea levels. [David Burton, USA] | Taken into account - see response to comment 13-245. |
| 13-249 | 13 | 9 | 41 | 9 | 41 | I thought that changes in salinity DO affect ocean volume?! [Roland Gehrels, United Kingdom] | Taken into account - see response to comment 13-243. |
| 13-250 | 13 | 9 | 43 | 9 | 43 | I have seen this point misunderstood in the past, I recommend emphasizing the word 'regional' perhaps put it in italics or boldface. [Howard J. Freeland, Canada] | Editorial. |
| 13-251 | 13 | 9 | 48 | 9 | 48 | Date of Miller publication? [Ian Allison, Australia] | Editorial. |
| 13-252 | 13 | 9 | 48 | 9 | 48 | Should be Miller and Douglas. [Simon Holgate, UK] | Editorial. |
| 13-253 | 13 | 9 | 48 | | | replace (Miller; Douglas, 2007) by (Miller and Douglas, 2007) [Ernst Schrama, Netherlands] | Editorial. |
| 13-254 | 13 | 9 | 49 | 9 | 49 | The reference "White et al., 2005a" has been duplicated in the references - i.e. it should only be "White et al., 2005". [John Hunter, Australia] | Editorial. |

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| 13-255 | 13 | 9 | 52 | 9 | 52 | This is the first time that GMSL is mentioned. I would find the chapter easier going if the definitions of such key concepts were brought in more systematically at the outset - i.e. juxtaposing GMSL with RSL... [Mark Siddall, UK] | Taken into account. This term is now defined in 13.1.2 (and is also in the Glossary). |
| 13-256 | 13 | 10 | 1 | | | "results in an isostatic adjustment of the ocean floor" is an example of a terminology problem. In this example, the word "movement" instead of "adjustment" would be better. But this is just one example of a terminology problem which is common in this report. GIA is sometimes used to refer to adjustments to data, but sometimes used to refer to movement of the land. The former is the better usage. The "A" in "GIA" stands for "adjustment." So what is being adjusted? Not the ocean floor! It is DATA that is adjusted: measured and calculated sea level data, which are adjusted for presumed land movement, including movement of the ocean floor. [David Burton, USA] | Taken into account - "an isostatic adjustment" is replaced by "vertical movement". |
| 13-257 | 13 | 10 | 2 | 10 | 2 | Farrell and Clark [Simon Holgate, UK] | Editorial. |
| 13-258 | 13 | 10 | 2 | | | replace (Farrell; Clark, 1976) by (Farrell and Clark, 1976). [Ernst Schrama, Netherlands] | Editorial. |
| 13-259 | 13 | 10 | 6 | 10 | 13 | as stated this process seems overall to be not related to contemporary climate change - one for the next subsection then? [Mark Siddall, UK] | Accepted. |
| 13-260 | 13 | 10 | 6 | 10 | 13 | States at end that deposition of sediments transferred from land to sea has negligible effect on SLR, and will not be considered. I hope this is backed up by evidence?? [Robert Thomas, USA] | Taken into account. Added sentence "Estimates of sediment delivery to the oceans (Syvitski and Kettner, 2011) suggest a contribution to GMSL of order 0.01 mm/yr." |
| 13-261 | 13 | 10 | 8 | | | replace (Watts 2001) by (Watts, 2001) [Ernst Schrama, Netherlands] | Editorial. |
| 13-262 | 13 | 10 | 12 | 10 | 13 | If it is negligible for contemporary climate change, why is it then discussed in this section? [Uwe Stoeber, Germany] | Taken into account - process is no longer discussed in this section. |
| 13-263 | 13 | 10 | 13 | 10 | 13 | I suggest changing "over" to "on". [Stephen Griffies, USA] | Taken into account. This text has been removed. |
| 13-264 | 13 | 10 | 15 | | | Section 13.2.1.2: All processes named in the first paragraph of this section are explained in detail except for the dynamic response of ice sheets to past climate change. [Uwe Stoeber, Germany] | Taken into account - reference is made to Section 13.2.3 where this process is discussed. |
| 13-265 | 13 | 10 | 22 | 10 | 25 | Two of these five items are treated in a rather inconsistent way. Firstly, the fifth item ("coastal processes resulting in erosion, deposition and compaction of sediment") has already been discussed in the previous section (Chapter 13, page 10, lines 6-13), where it is stated that it "will not be considered further in this chapter" - and it is then considered in the very next section! Further, the second item ("the dynamic response of ice sheets to past climate change") does not appear to be discussed further - I'm not actually sure what it means - is it the present dynamic response of ice sheets to past climate change (e.g. resulting from a non-equilibrium shape of the present ice sheets due to past climate change)? These issues need to be clarified. [John Hunter, Australia] | Taken into account. See responses to comments 13-259 & 13-264. |
| 13-266 | 13 | 10 | 28 | 10 | 28 | Nothing propagates "instantaneously". I presume the real speed is given by an elastic wave. Is that the speed of sound? Clearly this speed is nearly infinite on the time scales of climate change. But I suggest quoting the real physical speed rather than calling it "instantaneous." [Stephen Griffies, USA] | Taken into account. Replaced "instantaneously" with "synchronous with the loading changes". |
| 13-267 | 13 | 10 | 31 | 10 | 31 | Date of Lambeck publication? [Ian Allison, Australia] | Editorial. |
| 13-268 | 13 | 10 | 31 | | | replace (Lambeck; Nakiboglu 1984) by (Lambeck and Nakiboglu, 1984). [Ernst Schrama, Netherlands] | Editorial. |
| 13-269 | 13 | 10 | 32 | | | (same GIA terminology problem as on line 1) [David Burton, USA] | Rejected. This is the formal terminology for this process. |
| 13-270 | 13 | 10 | 39 | | | replace (Sahagian 2000) by (Sahagian, 2000). [Ernst Schrama, Netherlands] | Editorial. |
| 13-271 | 13 | 10 | 45 | | | insert) between after] [Ernst Schrama, Netherlands] | Editorial. |

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| 13-272 | 13 | 10 | 45 | | | something wrong with brackets [Elie Verleyen, Belgium] | Editorial. |
| 13-273 | 13 | 10 | 46 | 10 | 46 | A reference to Watson et al., 2010 (Geophysical Journal International, 182, 781-796, doi: 10.1111/j.1365-246X.2010.04640.x) may be useful here as it indicates a significant change in relative sea level rise as measured by tides gauges (by several mm/year) caused by an earthquake 80 years earlier. [John Hunter, Australia] | Accepted. |
| 13-274 | 13 | 10 | 49 | 10 | 49 | In view of the previous comment giving one example of the effect of an earthquake on relative sea level rise, many decades after the actual earthquake, it may be wise to include just a brief discussion on the way in which earthquakes can confound tide-gauge observations (I'm only thinking of a few sentences). [John Hunter, Australia] | Taken into account. Added text "...and significant secular relative sea level changes due to post-seismic deformation (Watson et al., 2010)" |
| 13-275 | 13 | 10 | 52 | 12 | 12 | Might be prudent to include a short summary paragraph explaining the capacity of the relative models to predict the actual physical response that has been measured to date (or at least the type of models that are closest at this point in time?). Similarly this paragraph could include a discussion on the most sensitive elements for projection modelling. [Phil Watson, Australia] | Rejected. The aim of this section is to only introduce the models used in sea level studies. Model assessment is undertaken in the sections that deal with projections. |
| 13-276 | 13 | 10 | 52 | | | Section 13.2.2. It seems to me that there is some interest in pointing out the differences between the process-based models and the semi-empirical models throughout the chapter. The differences are mentioned for instance in page 4, line 47 and more extensively in section 13.6.1. I would therefore expect that this distinction appeared more clearly in Section 13.2.2, but it does not. I suggest (1) to indicate what a process-based model indicating which type of models belong to this classification (2) change the order of the types of models presented at the beginning of the section so that it matches the order of the paragraphs in page 11 (i.e. 1) AOGCM models, 2)Isostatic, 3) Cryosphere, 4) Storm-surge/waves and 5) finally semi empirical) [Belén Martín Míguez, Spain] | Accepted. A short paragraph has now been included that describes, in general terms, the differences between process-based and semi-empirical models. Regarding the second suggestion, we note that the introductory paragraph to this section has been removed. |
| 13-277 | 13 | 10 | | | | Section 13.2.2 Models used for sea level studies [Catia Motta Domingues, Australia] | See responses to 279 & 280. |
| 13-278 | 13 | 10 | | | | It seems that the idea of this section is to provide a basic description of the various types of models that can be used to simulate/project certain components of sea level change. Two parts of the section, however, need some refinement, the text related to AOGCMs and to the storm-surge and wave models. [Catia Motta Domingues, Australia] | See responses to 279 & 280. |
| 13-279 | 13 | 10 | | | | In the AOGCM part, the text should be more in line with what has been defined in Chapter 9. Here there is no distinction, for example, between OGCMs, AOGCMs and Earth systems models. Also, is the model used by Timmermann et al. a shallow water equation model rather than an AOGCM? In addition, I was wondering why there was no discussion about the care needed prior to analysis of modelled sea level from AOGCMs (i.e., mass or volume conserving model; removal of numerical drifts, etc). Perhaps these issues could be discussed in an appendix or in the sea level/ocean heat content section from Chapter 9 and cross-referenced here. [Catia Motta Domingues, Australia] | Partly taken into account. The reader is referred to Chapter 9 in the introductory paragraph. In addition, a statement has been made to clarify that only models most commonly referred to in Chapter 13 are described (this is why OGCMs and ESMs are not described). The model used by Timmermann et al. (2010) is not an AOGCM but this paper is cited for the result obtained rather than the model. Mass vs volume conservation is a technical detail and so is beyond the scope of this short introductory section. Also, this issue is covered in the papers cited where relevant. A comment about the need for drift subtraction will be made in a later section where it is more appropriate. |
| 13-280 | 13 | 10 | | | | I have no background on storm surge or wave models and I did not find the text informative. Perhaps the authors can elaborate a bit more on a brief description of these models or point the readers to a section where there might be more general (or detailed) information. [Catia Motta Domingues, Australia] | Taken into account. This paragraph has been expanded. |
| 13-281 | 13 | 11 | 1 | | 2 | Delete the "semi-empirical" junk [David Burton, USA] | Rejected. These models appear in the peer-reviewed literature and so must be considered in the assessment report. |

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| 13-282 | 13 | 11 | 10 | 11 | 10 | It may be that these modes are simulated by AOGCMs but they are not, in general, simulated well. My own experience with HadCM3 shows that that ENSO is very weak for example. Such caveats should be included here. [Simon Holgate, UK] | Rejected. Model assessment is not a remit of Section 13.2.2. This statement is more relevant to later sections that deal with model assessment and projections. |
| 13-283 | 13 | 11 | 12 | 11 | 12 | The reference "White et al., 2005a" has been duplicated in the references - i.e. it should only be "White et al., 2005". [John Hunter, Australia] | Editorial. |
| 13-284 | 13 | 11 | 29 | 11 | 29 | Reword: "A number of isostatic models in current use...." [Simon Holgate, UK] | Accepted. |
| 13-285 | 13 | 11 | 29 | 11 | 30 | A number ... Which ones? [Donald Forbes, Canada] | Accepted. The relevant publications are listed. Note that this text has been moved to a later section which deals with model assessment. |
| 13-286 | 13 | 11 | 30 | 11 | 30 | this may be a writing policy but in statements like this regarding multiple examples, it would be better to include multiple original references instead of just one [Mark Siddall, UK] | Accepted. See response to comment 13-285. |
| 13-287 | 13 | 11 | 34 | | | Add: "Unfortunately, as direct GPS-based measurements of land movement have become available, the've showed little consistency with model-derived GIA numbers." [David Burton, USA] | Rejected. Model assessment is not a remit of Section 13.2.2. |
| 13-288 | 13 | 11 | 36 | 11 | 36 | Grammar: "...in the cryosphere are required inputs..." [Simon Holgate, UK] | Editorial. |
| 13-289 | 13 | 11 | 49 | 12 | 2 | Delete the "semi-emperical" junk [David Burton, USA] | Rejected. These models appear in the peer-reviewed literature and so must be considered in the assessment report. |
| 13-290 | 13 | 11 | 50 | 11 | 50 | Add Kemp et al (2001) to the list of citations. [Michael Mann, USA] | Rejected. Only original papers that illustrate the method are cited. |
| 13-291 | 13 | 11 | 50 | 11 | 50 | Vermeer 2009, I suppose? [Belén Martín Míguez, Spain] | Editorial. |
| 13-292 | 13 | 11 | 50 | 11 | 50 | Vermeer reference missing date [Jeff Ridley, UK] | Editorial. |
| 13-293 | 13 | 11 | 50 | | | change Vermeer; Rahmstorf into Vermeer and Rahmstorf [Ernst Schrama, Netherlands] | Editorial. |
| 13-294 | 13 | 11 | 51 | 11 | 51 | here and throughout I would say 'qualitatively argued assumptions' instead of 'physical considerations' [Mark Siddall, UK] | Accepted. Wording changed to "Semi-empirical models are based on empirical relationships connecting..." |
| 13-295 | 13 | 11 | 56 | 11 | 56 | in addition, V&R 2009 include an additional 'instantaneous response' [Mark Siddall, UK] | Noted. |
| 13-296 | 13 | 12 | 4 | 13 | 6 | Through this figure, the explanation is not evident [Michel Boko, Benin] | Rejected. More specific information required. |
| 13-297 | 13 | 12 | 14 | 12 | 14 | I am not comfortable about lumping glaciers and ice sheets into one category. The approach to modelling them, the challenge of understanding them and in particular their contribution in the future is very different in each case. [Mark Siddall, UK] | Rejected. We could have 2 subsections (ice sheets - glaciers) but would leave a very short section on glaciers. Several of the topics covered in this section are generic to ice, for instance steady states and some aspects of dynamics (calving). Having 2 sections would then lead to repetition. |
| 13-298 | 13 | 12 | 14 | 12 | 30 | There seems to be not enough attention paid to ice sheet discharge in a number of places in this chapter. This is one of them. While there is recent evidence that short time-scale oceanographic effects contribute a lot to recent variability in Greenland discharge, there never seems to be a serious attempt to assign how much GIS contribution is from climate variability and change, and how much is due to oceanographic changes [Neil White, Australia] | Noted. Good point but we are limited to the available literature. Clearly a statement of this sort would be very helpful in attribution. This boils down to whether ocean variability currently observed around Greenland (and Antarctica) is the consequence of natural variability or anthropogenic forcing or a mixture of the two (in that anthropogenic forcing amplifies variability in some way). If relevant literature in this are |

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| | | | | | | | emerges, will clearly be cited. |
| 13-299 | 13 | 12 | 14 | 13 | 48 | Similarly to comment 7 (above), might be prudent to include a short summary paragraph explaining the capacity of the relative models to predict the actual physical response that has been measured to date (or at least the type of models that are closest at this point in time?). Similarly this paragraph could include a discussion on the most sensitive elements for projection modelling. [Phil Watson, Australia] | Noted. The author team have agreed to insert text into FOD 13.4.7 which makes an assessment of the models being used and whether they can be used for projections. The aim here is to introduce these models. |
| 13-300 | 13 | 12 | 16 | 12 | 17 | This is a strange dichotomy as any 3D glaciological model will need some sort of SMB representation/assumption (at least for sea level contribution modelling). [Lev Tarasov, Canada] | Noted. True in the absolute but more useful to say that some ice sheet models focus and are designed for studying dynamics and have a crude/virtually nonexistent SMB, while SMB models exist without dynamics models (e.g. RCMs). |
| 13-301 | 13 | 12 | 17 | 12 | 17 | I think there needs to be some clarification on "SMB models" and "ice-sheet dynamics models". At the time of the AR4, it was known that some mechanisms were missing from the ice sheet dynamics models (e.g. the effects of losing ice shelves, and of bottom lubrication from surface meltwater) - this absence was addressed in a simple way by introducing an additional component which scaled with temperature (the "scaled up ice-sheet discharge"). These are often referred to as "dynamic ice effects", which may be confused with "ice-sheet dynamics models", in the sense that the reader may get the impression that "ice-sheet dynamics models" have only just been introduced to incorporate these effects, while in fact, the existing ice-sheet dynamics models are (hopefully) being enhanced with these new processes. [John Hunter, Australia] | Noted. Not clear what is required here. May help to introduce an acronym (ISDM). I think that it is clear in this section that we are discussing models of ice sheet dynamics rather than scaling arguments. Could add paragraph that explains that in contrast to AR4, we will be using models with relevant processes explicitly modelled. This is mentioned in FOD 13.6 but could be added here. |
| 13-302 | 13 | 12 | 17 | 12 | 17 | I think "budget" should read "balance". [John Hunter, Australia] | Rejected. See discussion about these terms in Glossary - we agreed that they are pseudonyms. My personal preference is for budget because it has no implication that terms need to be balanced. |
| 13-303 | 13 | 12 | 24 | 12 | 25 | This statement is only true if the Antarctic ice sheet is in balance. Otherwise, there is an imbalance between the flux through the surface and the outflux through the grounding lines/calving/run-off. Although the following sentences explain this further, I'd add a short attribute "in steady state". [Olaf Eisen, Germany] | Accepted. Have added phrase but this sections needs a wider rewording/reconsideration |
| 13-304 | 13 | 12 | 25 | 12 | 25 | "SLE" hasn't yet been defined. [John Hunter, Australia] | Accepted - now defined. |
| 13-305 | 13 | 12 | 25 | 12 | 25 | de Berg should be van de Berg [Philippe Huybrechts, Belgium] | Accepted. Endnote mangling, have requested change. |
| 13-306 | 13 | 12 | 25 | | | Please introduce SLE. [Uwe Stoeber, Germany] | Accepted. see 304 |
| 13-307 | 13 | 12 | 26 | 12 | 27 | I suggest removing the "5-10%". The authors have no way of knowing whether this is generally true, except perhaps under conditions not too different from those prevailing now in Antarctica. [Robert Thomas, USA] | Accepted. These numbers are there to give an order of magnitude and say that sea contribution is less than total flux. Have replaced with text. |
| 13-308 | 13 | 12 | 27 | 12 | 27 | The arbitrary range 5-10% seems to come out of nowhere. Moreover, the percentage is a function of the period of time. [James G Titus, United States of America] | Accepted. See 307. |
| 13-309 | 13 | 12 | 29 | 12 | 29 | Pollard & Deconto, 2009 is neither the first nor the best to illustrate this point. Gordon Robin and others showed this in the 1960s... [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. May be this is true. On reflection I think that this assertion is sufficiently obvious that a reference is not required. |
| 13-310 | 13 | 12 | 34 | 12 | 35 | As in previous comment, glaciological models need SMB representation/assumption. One can do experiments assuming no change in SMB but the text paints a distorted picture of how the modelling is done. [Lev Tarasov, Canada] | Rejected. See 297. The reviewer is discussing a particular type of ice sheet modelling (paleo work); the material on contemporary change is very different to this 'traditional' use of ice sheet models. |
| 13-311 | 13 | 12 | 35 | 12 | 35 | Delete 'surface' - redundant. [Donald Forbes, Canada] | Accepted. |

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| 13-312 | 13 | 12 | 35 | 12 | 35 | "surface SMB" – here the word surface is superfluous, as the S in SMB already means surface. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. |
| 13-313 | 13 | 12 | 35 | | | "surface SMB" is redundant since S stands for surface [Philip Mote, USA] | Accepted. |
| 13-314 | 13 | 12 | 39 | | | "feedback" is a noun, not a verb [David Burton, USA] | Accepted. Reworded to use as a noun. |
| 13-315 | 13 | 12 | 41 | 12 | 46 | see Siddall et al subm. Phil. Trans. (sent to the appropriate author) [Mark Siddall, UK] | Rejected. Not convinced that this paper discusses the issue in more than passing detail. |
| 13-316 | 13 | 12 | 43 | 12 | 43 | A citation to who is doing the assuming in "is generally assumed" is needed. [James G Titus, United States of America] | Accepted. Wording changed to "Semi-empirical models are based on empirical relationships connecting...". Note that the page number is incorrect (should be 11). |
| 13-317 | 13 | 12 | 44 | 12 | 46 | More detail needed [Michel Boko, Benin] | Rejected. This is a poorly defined comment. What type of additional detail is required? The basic point has been made, which is sufficient here (we are supposed to be introducing general models and issues only). |
| 13-318 | 13 | 12 | 46 | | | change "added to" to "included in" [David Burton, USA] | Accepted. |
| 13-319 | 13 | 12 | 48 | 12 | 54 | This paragraph is rather cryptic, and it is not clear what its specific contribution to the chapter is. This is a problem as the papers cited are not even accepted yet. So why introduce this? The paragraph needs to be made more explicit and clear. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Rejected. This is key to ability to make projections and will be drawn upon later in 13.6. The function of this subsection is to raise general issues that hamper modelling (as well as introduce type of model); this problem really is a major issues and deserves to be highlighted here. The use of submitted papers is allowable under IPCC guidelines. |
| 13-320 | 13 | 12 | 53 | 12 | 52 | ditto with Rae, submitted. This isn't even a published paper. Gregory and Huybechts 2006, for example, shows quite clearly the effect of different climates on SMB. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Rejected. See 319 about submitted papers. G&H use day degrees which obscures that point somewhat |
| 13-321 | 13 | 12 | 57 | 12 | 57 | Do RCMs really have a complete physical representation of climate? Some have different parameterisations due to the higher resolution and others have been specifically tuned to the region, but few have a 'more complete physics'. [Jeff Ridley, UK] | Rejected. The statement is that they have a *more* complete representation, which may be due to many things including their finer resolution. We do not make a statement about their physics. |
| 13-322 | 13 | 13 | 5 | 13 | 5 | This needs a reference, and I am not 100% convinced this statement is justified by the literature. Several studies (see Tijm-Reijmer, TCD, or Janssens and Huybrechts 2000) that suggest it isn't that significant. There again other studies suggest it might be. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. There are several studies (including one by the reviewer) that highlight this as an issue. References added. |
| 13-323 | 13 | 13 | 5 | 13 | 5 | The text says that the regional climate models are unable to model refreezing in the snow. This is not correct, many of these models do, RACMO includes refreezing. Second, this is not a critical fact in the context of predicting SLR from ice sheets, i.e. it is NOT a major source of uncertainty. This statement is not referenced. No effort has been made to see if this is consistent with Chapter 4. [Eric Rignot, USA] | Rejected. It is clearly stated that refreezing is a source of uncertainty in these models NOT that these models do not incorporate refreezing. |
| 13-324 | 13 | 13 | 9 | 13 | 11 | Needs some introduction to the issues being resolved by ice sheet models. Grounding line retreat and sub-shelf cavities are mentioned 'out of the blue' here. [Jeff Ridley, UK] | Rejected. There is a link to Chapter 4 which has set out the issues; hard to justify a more wide ranging discussion. |
| 13-325 | 13 | 13 | 11 | 13 | 11 | "Physical relationships" probably overstates what semi-empirical models are based on. Perhaps "are based on the assumed existence of a physical relationship between..." [James G Titus, United States of America] | Editorial. line/page number must be wrong; this comments is elated to semi empirical which are not discussed in this section. Should be p11 line 49. |

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| 13-326 | 13 | 13 | 11 | 13 | 12 | Unclear what is meant here. Do you mean that the region ocean models are now embedded 2-way in GCM oceans or regional models have been converted to global models for predictive purposes [Jeff Ridley, UK] | Agreed. This has been clarified. The models exist but there use to understand and project these changes is in its infancy. |
| 13-327 | 13 | 13 | 13 | 13 | 16 | This is a confusing sentence. It seems to suggest that the link between ice dynamics and potential surface meltwater effects is clear and then "exact mechanisms linking climate change to enhanced calving...". But the exact mechanism that links surface meltwater and ice dynamics is not clear. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Rejected. It is clearly stated that links to melt water are reasonably clear for lubrication (Greenland) and ice-shelf collapse (Ant); they are less clear for calving in Greenland (as opposed to calving in Antarctica). |
| 13-328 | 13 | 13 | 21 | 13 | 23 | Isn't high resolution needed to reproduce greenland GL migration also? [Ian Allison, Australia] | Rejected. Yes this is theoretically true but the problem most pressing for Antarctic (where it has prevented sensible projections). |
| 13-329 | 13 | 13 | 23 | 13 | 23 | "Qualitatively" seems a strange word here - I'd change it to "quantitatively" or (probably better) miss the word out all together. [John Hunter, Australia] | Agreed. Prefer not quantities which would imply far high degree of agreement - it is that resolution effects whether something (GL retreat) happens or not - this is "qualitative" in nature but agreed wording could be improved. Have tried 'to an unreasonable extent' |
| 13-330 | 13 | 13 | 23 | 13 | 23 | remove 'i.e.' and brackets [Mark Siddall, UK] | Accepted. |
| 13-331 | 13 | 13 | 26 | 13 | 26 | because' not 'since', which implies something sequential [Mark Siddall, UK] | Accepted. |
| 13-332 | 13 | 13 | 26 | 13 | 27 | ...contribution of Antarctica...' is unsupported by literature [Jeff Ridley, UK] | Accepted. This is fairly obvious but have no objection to adding a reference. Have inserted Vaughan 2008. |
| 13-333 | 13 | 13 | 27 | 13 | 27 | First use of SLR - requiring definition occurs on line 16 above [Jeff Ridley, UK] | Accepted. |
| 13-334 | 13 | 13 | 40 | 13 | 40 | I think the authors are now talking about non polar glaciers.?? [Robert Thomas, USA] | Rejected. It is not clear what is meant here. We are now talking about glaciers as opposed to ice sheets. The same processes operate for polar and non-polar glaciers. The reviewer may be confused by the preceding discussion about calving, however it is clear that this discussion is about Greenland. |
| 13-335 | 13 | 13 | 40 | 13 | 48 | Compared to the the description of the ice sheet models in section 13.2.3 and the later rather long dscussion in section 13.5.2, I found this section too short. It has only 1/10 of the section and does not really provide a good overview on the different types of models applied to project changes in glaciers. More regional studies on future glacier development (with important results also for the modelling at a global level) are missing completely and should be added. I am happy to provide some references if required. [Frank PAUL, Switzerland] | Noted. SOD will expand this section. |
| 13-336 | 13 | 13 | 40 | 13 | 48 | the glacier section here stands right out in not being covered so extensively and in being a very different modelling strategy. I suggest putting this in a different sub-section and adding some meat to it. [Mark Siddall, UK] | Rejected. See 297. However, we will add more material to this discussion in the SOD. |
| 13-337 | 13 | 13 | 40 | 14 | 43 | Note here that inventory is incomplete [Ian Allison, Australia] | Rejected. This comment is now out of date. The inventory is now virtually (over 99%) complete. |
| 13-338 | 13 | 13 | 45 | 13 | 47 | Explain how it will be obtained. [Michel Boko, Benin] | Noted. Not clear what 'it' refers to. If calving (line 46) then depends on whether there is literature that attempts to do. |
| 13-339 | 13 | 13 | 52 | 16 | 5 | Flawed by trying to make too much of unrepresentative and highly inaccurate, speculative data, boosted by freequent expressions of "confidence" [VINCENT GRAY, NEW ZEALAND] | Rejected - This is an assessment of the published literature, with more details reported in Chapter 3 and 5 |
| 13-340 | 13 | 13 | | | | Section 13.3.1 The geological record | First comment is for Chapter 5. Re: Figure, Accepted. |

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| | | | | | | <p>After reading this initial text (which I quite liked): "Records of past sea level change provide critical context for understanding current changes and evaluating projected changes. In addition to establishing a longer term reference for placing current rates of sea level rise in the context of natural variability, these records provide insight into the sensitivity of sea level to past climate change." I was expecting to read some discussion on the lines of the text/figure completed for the FAQ5.1: How unusual is the current sea level rate of change? (Chapter 5, page 5-42). Also, it would be very informative to have a figure with a sea level time series from the past 2,000 years (based on salt marsh reconstruction) to present time. If possible, Figure 13.3 could be turned into a composite with 4 panels: (a) paleo-present; (b) instrumental record (tide gauges/altimeter) (1700-2010); (c) altimeter era (1993-2010); (d) GRACE era (2002-2010) (with altimeter sea level and inferred sea level curves from GRACE/ThSL). [Catia Motta Domingues, Australia]</p> | We have developed a similar type figure as the reviewer describes (Fig. 13.3). |
| 13-341 | 13 | 13 | | | | chapter 13.3.1 and 5.5 are largely redundant. I would suggest to present them in one chapter and refer to in the other [Hubertus Fischer, Switzerland] | Rejected. This arrangement is by design with more details given in Chapter 5 and only a summary reported here. |
| 13-342 | 13 | 14 | 1 | 14 | 1 | I suggest citing: PALSEA, 2009: The sea-level conundrum: case studies from palaeo-archives, Journal of Quaternary Science, DOI: 10.1002/jqs.1270 [Mark Siddall, UK] | Rejected - we mainly refer to Chapter 5 for assessments and literature. |
| 13-343 | 13 | 14 | 1 | 15 | 57 | Include time periods in sub headings 13.3.1.1.1 etc [Ian Allison, Australia] | Rejected - time periods are provided in text. |
| 13-344 | 13 | 14 | 7 | 14 | 7 | "Mean global surface temperatures during the middle Pliocene (~3.3–2.9 Ma)". The Pliocene is now defined at 5.3-2.6 Ma (http://en.wikipedia.org/wiki/Pliocene) so 3.3-2.9 Ma is not "middel Pliocene" but late Pliocene. This problem occurs throughout the draft report. [Andrew Glikson, Australia] | Rejected - middle Pliocene is used as commonly defined in literature, and following Chap. 5 |
| 13-345 | 13 | 14 | 7 | 14 | 7 | Some brief explanation is needed on the times used for the paleo data (e.g. "Ma" and "ka") - these are presumably "before present" (which should be stated), but (a) when is "present" and (b) are these "real" dates or carbon dates? [John Hunter, Australia] | Rejected - these are conventional units recommended by IPCC. |
| 13-346 | 13 | 14 | 7 | | | change "~3.3 - 2.9" into "-5.3 - 2.6" [Ernst Schrama, Netherlands] | Rejected - Chapter 5 specifically focuses on this time interval |
| 13-347 | 13 | 14 | 9 | 14 | 10 | Reading these lines prompted a question about the geological sources/sinks of water. Do we know the net amount of water in the climate system has been roughly unchanged since the Pliocene? If not, then how can we infer from sea level the amount of ice? [Stephen Griffies, USA] | Noted. This is an interesting question, but completely unknown, with no literature available to assess. |
| 13-348 | 13 | 14 | 13 | 14 | 13 | "... the most comprehensive estimate of [what?] ..." [Donald Forbes, Canada] | Clarified. |
| 13-349 | 13 | 14 | 13 | | | What is the figure quoted "of 20 ± 10 m" relevant to? Is it equivalent SLR or present day MSL? [Phil Watson, Australia] | Taken into account. It is related to MSL. |
| 13-350 | 13 | 14 | 18 | 14 | 18 | This goes back to comments made on Chapter 5. Here a section on older interglacials would be appropriate. [Roland Gehrels, United Kingdom] | Taken into account - following Chapter 5, marine isotope stage 11 is now included.. |
| 13-351 | 13 | 14 | 19 | 14 | 19 | The dates of the LIG should be defined. [Simon Holgate, UK] | Accepted. |
| 13-352 | 13 | 14 | 19 | | | Section 13.3.1.1.2: Terminology is inconsistent between ch. 5 ("Last Interglacial") and 13 ("Last Interglaciation"). [Robert Kopp, USA] | Accepted - should now be consistent. |
| 13-353 | 13 | 14 | 25 | 14 | 25 | The parenthetical note "(high confidence in high latitudes)" is awkward here -- I believe there is high confidence that high latitudes were significantly more than 2°C warmer than pre-industrial temperatures. [Robert Kopp, USA] | No longer included in revised text. |
| 13-354 | 13 | 14 | 25 | 14 | 25 | this is cited with medium confidence in chapter 5 (with no reference to high latitudes). I copy my response to | Noted. Reference removed. Otherwise, we have |

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| | | | | | | chapter 5 on this: 'It is not acceptable to cite a Nature News and Views in this context.N&Vs is essentially 'grey literature'. This cannot be compared with the rigorous modelling efforts noted later. How large is the uncertainty? It is hard to imagine that the LIG was as warm as the Mid Pliocene with such different forcing...are you actually confident to state 2 degsC on the basis of the evidence presented??' [Mark Siddall, UK] | adopted the assessment from Chapter 5. |
| 13-355 | 13 | 14 | 27 | 14 | 28 | The text should briefly explain why IPCC is confident that the expansion relative to today in the water column was 0.3-0.4m. Without an explanation, one might normally assume that it is very difficult to have any idea about the heat content of the ocean 100,000 years ago. [James G Titus, United States of America] | Taken into account. This is based on paleo-sea surface temperature reconstructions and modeling. Confidence will be reassessed following consultation with Chapter 5. |
| 13-356 | 13 | 14 | 27 | 14 | 29 | If the confidence on the first line is "medium", how can the confidence on the next line be "high"? [Donald Forbes, Canada] | Revised |
| 13-357 | 13 | 14 | 31 | 14 | 31 | ".....Greenland contributed at least 2 m....." [Ian Allison, Australia] | Revised |
| 13-358 | 13 | 14 | 31 | 14 | 33 | Colville actually says 1.6-2.2m. The sentence makes it sound as if it is almost certainly atleast 2m but perhaps even 4m . Colville actually rules out 4m. [Aslak Grinsted, Denmark] | Revised |
| 13-359 | 13 | 14 | 31 | 14 | 33 | The text should explain the basis of IPCC's confidence about the Greenland contribution. [James G Titus, United States of America] | We follow the assessment of Chapter 5. |
| 13-360 | 13 | 14 | 31 | | | This number (~0.6 m) has likely an uncertainty of about +/-50%. I would recommend to add a short comment on that. [Frank PAUL, Switzerland] | No longer in tex. |
| 13-361 | 13 | 14 | 33 | 14 | 36 | There is slightly muddled logic here. 10 m must include EAIS, but 6 m could equally well also include EAIS. I.e. 6 m could be some combination of contributions from all three ice sheets. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | No longer in text. |
| 13-362 | 13 | 14 | 40 | 14 | 44 | I don't believe this is mentioned in Chapter 5 (of which this ought to be a summary?). [Roland Gehrels, United Kingdom] | No longer in text. |
| 13-363 | 13 | 14 | 41 | 14 | 42 | I think there needs to be a little clarification of the distinction between "warmer temperatures", "higher insolation" and "associated nonlinear feedbacks". I assume "temperatures" means "surface air temperatures" and that "insolation" means direct heating from the sun (i.e. not including long wave radiative forcing from the atmosphere). An example of "associated nonlinear feedbacks" (in the context of "insolation") would also be helpful here. [John Hunter, Australia] | No longer in text. |
| 13-364 | 13 | 14 | 45 | 14 | 47 | "3–4oC increase in LIG surface temperature over Antarctica (Jouzel et al., 2007) relative to present remained too low to induce any significant loss from surface melting." Given the warming of West Antarctica over the last few decades (NASA/GISS), does "relative to the present" mean relative to 2011 or some earlier point in time? [Andrew Glikson, Australia] | No longer in text |
| 13-365 | 13 | 14 | 47 | 14 | 48 | What does this mean? What is the connection between mid-depth ocean warming and Antarctic mass loss? [Ian Allison, Australia] | No longer in text |
| 13-366 | 13 | 14 | 48 | 14 | 48 | The text should indicate whether there is any fossil evidence of the boundaries of the ice sheet. [James G Titus, United States of America] | No longer in text. |
| 13-367 | 13 | 14 | 49 | 15 | 49 | refers to the rise of only 1-2 m; what about the rest of the 6 - 10m referred to earlier in para??. Moreover, the SLR rates of 1-2.5 mm/yr are less than those now prevailing.. [Robert Thomas, USA] | Clarified - we do not consider rates when it was rising to the +6m. |
| 13-368 | 13 | 14 | 50 | 14 | 55 | Why almost exclusively rely here for final datings and rates on an as yet unaccepted paper just to report a likely age/duration of the LIG highstand? In a careful, stratigraphically underpinned (unlike most coral studies) study with lots of U-series dating, Thompson et al. (2011) conclude on a range of 129-115 ka. That paper is already published, so has precedent anyway over the cited unpublished study. As I recall Stirling has also determined a similar age range even earlier. The chapter should avoid making subjective choices between studies/methodologies and instead provide a more balanced reflection of the accumulated knowledge and the (chronological) priority of findings, especially if new studies do not significantly alter or advance the knowledge | Dutton and Lambeck is now published, and as was and still is discussed, is in good agreement with Kopp et al. The Thompson study is now referenced in Chapter 5. |

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| | | | | | | from published results regarding the specific aspect that it is cited for. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | |
| 13-369 | 13 | 14 | 51 | 14 | 55 | The first sentence states a confidence (medium-to-high) whereas the second sentence does not. Consistency? [Roland Gehrels, United Kingdom] | Revised |
| 13-370 | 13 | 14 | 55 | 14 | 55 | see also: Thompson W.G., H.A. Curran, M.A. Wilson & B. White, Sea-level oscillations during the last interglacial highstand recorded by Bahamas corals. Nature Geoscience (2011) doi:10.1038/ngeo1253. And please consider my responses to the parallel section to this in chapter 5 (see above) [Mark Siddall, UK] | Chapter 5 now includes this study |
| 13-371 | 13 | 14 | | | | This is an important section. It is good that the authors have stated a level of confidence, but the text needs to also explain the basis of that confidence. [James G Titus, United States of America] | This is based on assessment from Chapter 5, where further explanation is provided. |
| 13-372 | 13 | 15 | 1 | 15 | 2 | Ch. 5 has moderate confidence (rather than high confidence) in the meter-scale fluctuation, which I would agree with. [Robert Kopp, USA] | Accepted |
| 13-373 | 13 | 15 | 1 | 15 | 11 | Why almost exclusively rely here for final datings and rates on an as yet unaccepted paper just to report a likely age/duration of the LIG highstand? In a careful, stratigraphically underpinned (unlike most coral studies) study with lots of U-series dating, Thompson et al. (2011) conclude on a range of 129-115 ka. That paper is already published, so has precedent anyway over the cited unpublished study. As I recall Stirling has also determined a similar age range even earlier. The chapter should avoid making subjective choices between studies/methodologies and instead provide a more balanced reflection of the accumulated knowledge and the (chronological) priority of findings, especially if new studies do not significantly alter or advance the knowledge from published results regarding the specific aspect that it is cited for. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | See response to 13-368 |
| 13-374 | 13 | 15 | 2 | 15 | 2 | The phrase "suggesting substantial sea level variability" could be cut. It seems redundant and it is unclear why a change of 3.5 mm/yr is viewed as "substantial" since it is commonplace today. [James G Titus, United States of America] | Deleted "substantial" |
| 13-375 | 13 | 15 | 5 | 15 | 6 | "... based on a LIG duration that is 3-4 kyr shorter ..." - On what evidence? [Donald Forbes, Canada] | No longer in text |
| 13-376 | 13 | 15 | 5 | 15 | 7 | While the prior distribution used by Kopp et al. is based on the LR oxygen isotope stack, it is not accurate to say that the posterior distribution is based on the age model of the stack. It is influenced by the stack's age model, but that age model is revised in light of data. It is therefore not appropriate to scale rates as suggested here. As can be seen by comparing the LR stack to the posterior PDF (e.g., Figure 4a vs Figure S1 of Kopp et al., 2009), the incorporation of observational data significantly alters the age model: median estimates of the LR-derived GSL curve falls below present values at ~121 ka, whereas median estimates of the posterior curve falls below present values at ~116 ka. This is not to say that considering alternative age models for the prior would not be a useful exercise; the use of the LR curve for the prior is significant because the age constraints on many data points are poor, and for these data points, best estimate ages will tend toward the framework established by the prior and better constrained data. [Robert Kopp, USA] | No longer in text |
| 13-377 | 13 | 15 | 5 | 15 | 7 | There are flaws in the interpretation presented here of both the method used and confidence asserted by Kopp et al in deducing the rate of sea level rise for the LIG. [Michael Oppenheimer, USA] | This has been corrected |
| 13-378 | 13 | 15 | 10 | 15 | 11 | It is not clear what the source of the ~1.0-2.5 m/ky range in the last sentence is, nor what the meaning of "medium-to-high confidence" is. [Robert Kopp, USA] | This has been revised |
| 13-379 | 13 | 15 | 13 | 15 | 20 | How do the relevant contributions to SLR attributable to the Last Interglaciation (LIG) compare to current SLR budgets and forecasts used in projection modelling? [Phil Watson, Australia] | This is discussed in sections 13.5 and 13.8 |
| 13-380 | 13 | 15 | 14 | 15 | 14 | this is stated differently in chapter 5. No statement is given regarding 'limited evidence' Given GIA effects on any one site it is hard to imagine what 'limited evidence' can mean [Mark Siddall, UK] | This has been revised following Chapter 5 |

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| 13-381 | 13 | 15 | 22 | 15 | 22 | This section should contain a reference to: Stanford et al., 2011. "Sea-level probability for the last deglaciation: a statistical analysis of far-field records", Global and Planetary Change, 79, 3-4, 193-203, DOI: 10.1016/j.gloplacha.2010.11.002. [John Hunter, Australia] | This section no longer appears here. |
| 13-382 | 13 | 15 | 24 | 15 | 24 | The dates of the glacial/inter-glacial transitions should be defined. [Simon Holgate, UK] | This section no longer appears here. |
| 13-383 | 13 | 15 | 27 | 15 | 31 | "One strategy to infer possible future rates of sea level rise in a warm climate is to constrain them to be less than the observed rates as former sea level approached or reached the present value. These intervals of sea level rise represented continued ice-sheet disequilibrium response to deglacial forcings rather than a near-equilibrium response to greenhouse forcing. Thus, they only provide upper limits for land ice loss and are not complete analogues for future changes.". In attempting comparisons between the galcial terminations and current warming, the extreme rate of current CO2 rise (>2 ppm/year) needs to be taken into account in terms of its effects on destabilization of the ice sheets. [Andrew Glikson, Australia] | This section no longer appears here. |
| 13-384 | 13 | 15 | 29 | 15 | 30 | The distinction between "disequilibrium" and "near-equilibrium" is highly qualitative. This could be phrased better to indicate that, during the deglaciations, the climate was further from equilibrium than it is now, or is likely to be in the next few century. However, I think a better explanation is that ice sheets take much longer to respond to disequilibrium than ocean warming, so there is a long lag (on century to millennial time scales) before land ice becomes the dominant source or sink for sea-level change - the last deglaciation operated over at least 10,000 years, while present and projected sea-level rise only operates over a few centuries. [John Hunter, Australia] | This section no longer appears here. |
| 13-385 | 13 | 15 | 30 | 15 | 31 | They are clearly not perfect analogies, but it is not an 'upper limit'. You just have to crank up the forcing enough to beat those rates. It is not a physical limitation of the system. [Aslak Grinsted, Denmark] | This section no longer appears here. |
| 13-386 | 13 | 15 | 30 | 15 | 31 | The point should also be made that, during a deglaciation, there is a larger area of land ice to melt compared with today - this is another reason why the sea-level rise rates during a deglaciation are very much an upper limit of "future changes". [John Hunter, Australia] | This section no longer appears here. |
| 13-387 | 13 | 15 | 33 | 15 | 33 | "... within -10 m ..." - Of what? Within 10 m of the present sea level? [Donald Forbes, Canada] | This section no longer appears here. |
| 13-388 | 13 | 15 | 33 | 15 | 33 | "(when sea level was within - 10 m)" doesn't make sense to me - within -10m of what?? [Neil White, Australia] | This section no longer appears here. |
| 13-389 | 13 | 15 | 34 | 15 | 34 | Kopp et al. found that GMSL rates of rise to the highstand extremely likely (95% confidence) exceeded 5.6 m/ky (rather than "likely", as currently indicated). [Robert Kopp, USA] | Accepted and revised |
| 13-390 | 13 | 15 | 40 | 15 | 41 | This section is on the last two deglaciations, but there is only one sentence on the last deglaciation. Clearly there is a paragraph missing here. [Roland Gehrels, United Kingdom] | This section no longer appears here. |
| 13-391 | 13 | 15 | 43 | 15 | 43 | I suggest that SL events in the earlier Holocene (8.2 event and later) are mentioned in parallel with chapter 5. I suggest including Carlson et al 2008 [Mark Siddall, UK] | This section no longer appears here. |
| 13-392 | 13 | 15 | 43 | 15 | 50 | Do not capitalise "late". The late Holocene is not a formal epoch. [Roland Gehrels, United Kingdom] | Accepted. |
| 13-393 | 13 | 15 | 47 | 15 | 48 | Need to explain the basis for this confidence. If possible, explain what happended since this area is covered with ice today. [James G Titus, United States of America] | This is based on assessment from Chapter 5. |
| 13-394 | 13 | 15 | 50 | 15 | 50 | "Late Holocene" is too vague here. Give a period. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted and clarified with ages given |
| 13-395 | 13 | 15 | 50 | 15 | 50 | It would be clearer if here (and in many other relevant instances) "sea level" was replaced by "relative sea level". [John Hunter, Australia] | Noted |
| 13-396 | 13 | 15 | 51 | 15 | 54 | Again, no evidence is cited of a global analysis of sea-level variations in the past 2000 years. The problem is that the conclusion included here (that the amplitude of variation has been smaller than 25 cm) is used in Chapter 5 in several places, also with no reference. My impression is that this study does not exist - I am not aware of it, in any case. The fact that neither here nor in Chapter 5 a suitable reference is cited seems to | This is based on assessment from Chapter 5. |

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| | | | | | | confirm that no such analysis exist. [Eduardo Zorita, Germany] | |
| 13-397 | 13 | 15 | 53 | 15 | 53 | I would quibble with the "+/- 25 cm" because the uncertainty in the estimates varies considerably over the last 6000 years. Kurt Lambeck's global data indicates uncertainties ranging from +/- 1 m (in ~6000BP) to +/- 0.1 m (a few hundred years ago) over this period (uncertainties which I believe represent roughly limits of error) - which would suggest that fluctuations in global sea could have been significantly larger than "+/- 25 cm" during the period 6000-3000 BP. [John Hunter, Australia] | This is based on assessment from Chapter 5. |
| 13-398 | 13 | 15 | 54 | 15 | 54 | references needed and some way of emphasising the contribution of field geologists and dating contributions to understanding these issues more generally [Mark Siddall, UK] | Rejected. The purpose of this report is to assess the relevant literature, not acknowledge the efforts of those who have collected the data. |
| 13-399 | 13 | 15 | 56 | 15 | 56 | The word 'observational' is wrong in my opinion. I suggest 'proxy' record. [Aslak Grinsted, Denmark] | Accepted. |
| 13-400 | 13 | 15 | 56 | | | I would question (or at the very least qualify) the statement "The observational record for the past 2000 years up to pre-industrial times is of the highest precision..". I think the description "highest precision" requires qualification. Compared to what? Digital sea level data and altimetry (post 1990) are a quantum leap more accurate and sophisticated than tide gauge measuring devices pre 1900? Difficult to lump all observational data over the past 2000 years into this category? [Phil Watson, Australia] | Accepted |
| 13-401 | 13 | 16 | 1 | 16 | 3 | "The current 19+-year satellite altimetry time series (Figure 13.3) shows that from 1993–2010, GMSL has risen at a rate of 3.2 ± 0.5 mm yr ⁻¹ . A correction of ~ -0.3 mm yr ⁻¹ has been applied to this value to account for the increasing size of the global ocean basins due to GIA (Peltier, 2009)." I find this bit of text ambiguous. How I read it: the value was corrected by subtracting 0.3 (addition of -0.3), so I deduce from that that the measured value was 3.5. However, if the ocean basins are increasing in size over time, then the measured sea level rise would underestimate the amount of water added to the ocean, no? So should the correction not be positive then, so that the corrected value of water added is larger, but some of it had been taken up by the increasing basin size? I may be getting this wrong, but with me so will others, and the language/signs reported need to be clarified and made unambiguous. It may also be that the correction relates to overall mass redistributions, but then again this is not clear, and the sign of the correction is not intuitive and should be better explained. [Elco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Taken into account. The value of 0.3 mm/year was ADDED to 2.9 mm/year. This discussion has been clarified. |
| 13-402 | 13 | 16 | 13 | 16 | 13 | "almost no data"--Are there any tide gauges in the open ocean? If so, a quick reference to them would be helpful. [James G Titus, United States of America] | Taken into account. Select island gauges are considered "open ocean". We have clarified this text. |
| 13-403 | 13 | 16 | 13 | | | delete the word "almost" -- tide gauges provide data only at the coasts, never the open oceans [David Burton, USA] | Taken into account. Text clarified to identify island gauges which effectively sample the open ocean. |
| 13-404 | 13 | 16 | 14 | | | Same terminology issue with "GIA" -- see above [David Burton, USA] | Rejected. (referring to 13-256). "Adjustment" in GIA is well understood to refer to adjustment (movement) of the land surface through isostasy, not adjustment of data. One makes corrections to elevation data (i.e., from tide gauges) to account for GIA. |
| 13-405 | 13 | 16 | 15 | 16 | 26 | These studies have ignored many of the causes of bias in early readings. Equipment is damaged by storms and was often reinstalled lower. Local land was depressed by buildings, removal of groundwater and minerals. Harbours were modified to take larger ships and the local sea level raised. As a result "long-term" records are suspect. Recent modern methods of leveling and other modifications (particularly with the SEAFRAME programme) have shown hardly any sea level change in a number of places, examples being Australia and the Pacific islands. You give only one example, Figure 13.16, for Palau, which shows no change since 2000 plus a ridiculous "projected" rise. Actual sea level data have been largely suppressed by this Chapter. For much of it you have to give references to the internet which have not appeared in peer reviewed Journal because the results would disagree with the attitude of the Lead Authors of this chapter. It should, be quoted here and used for some of your figures. You should discuss and summarise the work of the PMSL and quote the PMSL website. The work on SEAFRAME should be quoted from the Australian Bureau of Meteorology website and you should also quote my own assessment of the Pacific Sea Level Results which is on the "Climate Science" website.. [VINCENT GRAY, NEW ZEALAND] | Rejected. Any tide gauges with issues as identified would not have been used in the peer-reviewed literature assessed here. Our references are to peer-reviewed published literature; data used in these publications is available on the internet. Data from the PMSL web site has been used in publications. IPCC does not refer to an unpublished assessment on a web site such as yours because it is not peer-reviewed literature. We recommend that you submit your work to a peer-reviewed journal in order for an assessment by the IPCC. |

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| 13-406 | 13 | 16 | 23 | 16 | 23 | "...vertical land motion correction used...." [Ian Allison, Australia] | Accepted - typo corrected. |
| 13-407 | 13 | 16 | 23 | 16 | 23 | typo corrected-> correction [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted - typo corrected. |
| 13-408 | 13 | 16 | 23 | 16 | 23 | "... land motion corrected used is ..."? Something missing here? [Donald Forbes, Canada] | Accepted - typo corrected. |
| 13-409 | 13 | 16 | 27 | | | Add: "GIA corrections increase reported SLR. Unfortunately, without GPS measurements of land movement, corrections for subsidence (which would decrease reported SLR) are not possible. Correcting up for GIA, but not down for subsidence, introduces an unfortunate systematic bias, exaggerating reported SLR. The solution, eventually, will be to substitute GPS measurements of land movement for model-derived GIA corrections; unfortunately, GPS measurements of land movement are still unavailable for most tide gauges." [David Burton, USA] | Rejected - Depending on location, GIA corrections may be correcting for land uplift (e.g. Stockholm) or subsidence (e.g. New York). The comment is inaccurate." |
| 13-410 | 13 | 16 | 28 | 16 | 28 | Given the previous paragraph, it is surprising to read that any general statements about global sea level are possible from tide-gauge data before the middle of the 19th century. [James G Titus, United States of America] | Noted. While the error bars are much larger, general statements are still possible before the mid-19th century from the few available tide gauge records and some paleo data. Text clarified. |
| 13-411 | 13 | 16 | 28 | 16 | 29 | The sentence is not clear . If the acceleration of SLR was almost the same from 1880 till now, why differentiate "before late-19th " and "after"? How to quantify statement "clearly began to rise" ? Suggest to change to " Data show that sea level was stable or slowly rising before late-18th century, when it started to rise at accelerated rate about 0.01 mm/year ² [Pavel Tklich, Singapore] | Accepted and text clarified |
| 13-412 | 13 | 16 | 30 | 16 | 30 | Elsewhere 1.6. Be consistent. See comments 2 and 7. [Roland Gehrels, United Kingdom] | Taken into account. |
| 13-413 | 13 | 16 | 30 | | | Change "~1.7" to "~1.4" because the 1.7 figure includes Peltier's 0.3 mm/yr GIA adjustment for modeled sinking of the ocean floor, which is proper for correcting ocean depth, but not proper for adding to sea level. [David Burton, USA] | Rejected. This statement is incorrect - the 0.3 mm/year correction is not applied to the tide gauge data - the tide gauge data are corrected using a GIA model. |
| 13-414 | 13 | 16 | 36 | | | Add, after the first sentence: "However, in Church and White's 2009 data, all but 28% of that acceleration was gone." [David Burton, USA] | Rejected. Church and White (2011) find an acceleration since 1880 slightly smaller than but not significantly different to their 2006 analysis. |
| 13-415 | 13 | 16 | 42 | 16 | 56 | I suggest remove the word "mean" as I don't think it is appropriate - the ellipsoid is a surface of best fit to the fairly lumpy shape of the Earth. Changes in the shape since we've been defining ellipsoids are orders of magnitude smaller than the differences between the Ellipsoid and the true shape of the Earth. [Neil White, Australia] | Accepted. |
| 13-416 | 13 | 16 | 43 | 16 | 46 | I find this sentence to be thoroughly obscure. Please try reading it aloud, I can't really decipher the meaning. One problem ios with the idea of an abrupt inflection, or inflexion. This is a stange concept as an inflection point is generally viewed as a point where the sign of curvature of a graph changes sign, see the Wikipedia page http://en.wikipedia.org/wiki/Inflection_point for pictures of inflection points, I don't see how one could be 'abrupt'. Do the authors mean 'an abrupt change in slope' in which case say so, that would not be an inflection point. [Howard J. Freeland, Canada] | Accepted. Sentence removed. |
| 13-417 | 13 | 16 | 44 | 16 | 45 | It is in my view not possible to say that the inflexion in the proxy records is more abrupt. This can only be evaluated in proxy and tide-gauge records that are close to each other. The proxy records only overlap with the rising limb in nearby tide-gauge records. To my knowledge there is not a singly proxy reconstruction where the period before and after the inflexion is also captured in a nearby tide-gauge record. It is only in some of the longest tide-gauge records that the inflexion is not very abrupt, but there are no nearby proxy reconstructions to compare them with. [Roland Gehrels, United Kingdom] | Accepted. Sentence removed. |
| 13-418 | 13 | 16 | 46 | | | Add, at the end of the paragraph: "However, no statistically significant acceleration in sea level rise has been detected since the first 1/4-th of the 20th century; i.e., none coincident with the large anthropogenic GHG emissions of the mid- and late-20th century and the first decade of the 21st century." [David Burton, USA] | Rejected. Changes in rates of rise since 1925 have been observed and are beginning to be explained. |

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| 13-419 | 13 | 16 | 48 | 16 | 48 | Operational satellite altimetry did not begin with TOPEX it began with Geosat in the 1980's (if we ignore the short Seasat mission). The statement that precision ocean altimetry began with TOPEX would be more accurate [Jeff Ridley, UK] | Taken into account. Reworded. |
| 13-420 | 13 | 16 | 48 | 16 | 48 | The statement "Satellite altimetry began in 1992..." is incorrect. Satellite altimetry started in 1973 with Skylab. This was followed by GEOS-3, Seasat, GEOSAT and ERS-1, all before the launch of TOPEX/Poseidon in 1992. [Neil White, Australia] | Taken into account. Reworded. |
| 13-421 | 13 | 16 | 49 | 16 | 49 | Should read "...a nearly globally distributed set..." [Simon Holgate, UK] | Accepted. |
| 13-422 | 13 | 16 | 49 | 16 | 49 | Most people would not characterize the satellites as providing "precise sea level measurements" at specific locations. The "precision" comes from the totality. [James G Titus, United States of America] | Noted. At specific locations, satellite altimetry is "precise" to 1-2 cm. |
| 13-423 | 13 | 16 | 49 | | | delete the word "precise" -- it's not very precise! [David Burton, USA] | Rejected. At the scale we're discussing, these measurements are precise. |
| 13-424 | 13 | 16 | 51 | 16 | 51 | Strictly, these satellites are not in identical orbits - the ground tracks are identical, but, due to the (small) time offsets, the orbit planes are slightly offset longitudinally. [Neil White, Australia] | Noted. |
| 13-425 | 13 | 16 | 53 | | 54 | change "...relative to the land, satellite altimetry measures 'absolute' sea level variations with respect to..." to "...relative to the land at the coasts, satellite altimetry measures 'absolute' sea level variations over the open ocean with respect to..." [David Burton, USA] | Taken into account. Reworded (possibly to "relative to the land at the island and coastal locations, satellite altimetry measures sea level variations over the open ocean with respect to") |
| 13-426 | 13 | 16 | | | | Note: this would be a perfect place to carefully define two different terms for the two different types of GMSL! [David Burton, USA] | Rejected. These concepts to bediscussed in 13.2. |
| 13-427 | 13 | 16 | | | | Section 13.3.2 The instrumental record Following the above suggestion of the composite for Figure 13.3, it would be interesting for the instrument record panels (b,c,d) to have "an ensemble plot", with all published global mean estimates available plotted (thin coloured lines) and a best averaged estimate curve highlighted in black & bold, with uncertainties. First, this would show how the scientific community expanded from the availability of one or two estimates to various estimates now, and groups involved. Second, this would highlight whether there are major/minor (dis)agreements between the various estimates in terms of interannual, decadal, multidecadal and longer changes (and possibly to discuss why some differences are expected). Third, panel (d) would help to highlight major/minor (dis)agreements between different observing systems. Now, I am not sure whether this suggestion would best suit this section or the sea level section in Chapter 3. It is not clear to me where the line was drawn in terms of the division/overlap/complementary character between the observed sea level material posted in this Chapter and Chapter 3. I am confused. [Catia Motta Domingues, Australia] | Taken into account. This figure is being redrawn in consultation with Chapter 3. |
| 13-428 | 13 | 17 | 1 | 17 | 5 | Satellite altimetry measurements have levelled in the last few years, suggesting that there is a periodic change. [VINCENT GRAY, NEW ZEALAND] | Noted, although it is most likely reflects natural variability. |
| 13-429 | 13 | 17 | 1 | | | Change sentence into: "The current satellite altimetry time series shown in (Figure 13.3) shows that from 1993-2010, GMSL has" [Ernst Schrama, Netherlands] | Accepted. |
| 13-430 | 13 | 17 | 2 | 17 | 3 | Additional text is needed to clarify what the correction is and why it is needed. [James G Titus, United States of America] | Accepted - this will be clarified. |
| 13-431 | 13 | 17 | 2 | | 3 | Delete the 0.3 mm/yr "correction" -- it corrects ocean depth, not sea level. (But even with the 0.3 mm/yr GIA addition, it's still only 2.8, not 3.2 mm/yr.) [David Burton, USA] | Rejected. The 0.3 mm/yr is an adjustment to satellite altimeter data required to estimate ocean volume. |
| 13-432 | 13 | 17 | 2 | | | "3.2" is incorrect. Change to "2.5" per: [David Burton, USA] | Rejected. "3.2" is correct. |
| 13-433 | 13 | 17 | 2 | | | At end of first sentence add: ", but markedly decelerating since 2004." [David Burton, USA] | Taken into account. This section is being considerably shortened, with same or similar material being covered in Chapter 3. There is some natural |

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| | | | | | | | variability in the rate of rise if short periods are selected. |
| 13-434 | 13 | 17 | 8 | | 14 | Envisat should not be excluded!!! [David Burton, USA] | Rejected. The Topex/Poseidon/Jason-1 and 2 series of satellite are the most accurate. Envisat has significant deficiencies in measuring GMSL. |
| 13-435 | 13 | 17 | 9 | 17 | 10 | Proof needed. There is no demonstration here. [Michel Boko, Benin] | Rejected. Unclear what is not being demonstrated. |
| 13-436 | 13 | 17 | 9 | 17 | 14 | Suggestion: Adding comparable tide gauge time series to figure, since it was discussed in detail in the preceding text. [Carmen Boening, USA] | Taken into account. This figure is being redrawn in consultation with Chapter 3. |
| 13-437 | 13 | 17 | 16 | 17 | 16 | Many people have interpreted text similar to this paragraph as demonstrating that the rate of sea level rise has approximately doubled, though the thorough analysis by Church and White which includes all this data shows that the acceleration has been about 1mm/yr over a century, which is considerably less. This text warns the reader to be skeptical about the doubling, but it does not mention that an analysis that considered all the factors found an acceleration more like 50% than the doubling. An "assessment" is needed here. [James G Titus, United States of America] | Taken into account. We have discussed the fact that tide gauge and altimetry records both identify differences in the rate of rise over the 20th century (p. 13-16 to 13-17), that there is uncertainty in the interannual to decadal variability inferred from individual tide gauges because of differences between curves (p. 13-17), that there are multiple controls on sea level that can give rise to regional and global differences in the rate of sea-level rise on interannual to multi-decadal timescales, and that these can strongly modulate a long-term secular signal (section 13.7; FAQ 13.1). We will reword the paragraph and some of this material is transferred to Chapter 3. |
| 13-438 | 13 | 17 | 16 | 17 | 23 | It is unfair to compare satellite measurements on the open ocean with tide-gauge measurements close to the coast. It is the coastal ones that really matter to the residents and they have not risen in the same way as the satellite results [VINCENT GRAY, NEW ZEALAND] | Rejected. The comparison here is of the tide-gauge estimates of GMSL. |
| 13-439 | 13 | 17 | 16 | 17 | 23 | the 5mm decrease in observed sea level in 2010-11 merits some discussion [Philip Mote, USA] | Taken into account. We have discussed the fact that tide gauge and altimetry records both identify differences in the rate of rise over the 20th century (p. 13-16 to 13-17), that there is uncertainty in the interannual to decadal variability inferred from individual tide gauges. There are multiple controls on sea level that can give rise to regional and global differences in the rate of sea-level rise on interannual to multi-decadal timescales, and that these can modulate a long-term secular signal (section 13.7; FAQ 13.1). The fall in sea level in 2011 is interesting and reflects the importance of (temporary) terrestrial storage. We will add a comment. |
| 13-440 | 13 | 17 | 16 | 17 | 23 | This paragraph should also include a short discussion of the deceleration of sea level rise after 2006 that can be seen in Fig. 13.3. [Uwe Stoeber, Germany] | Taken into account. We have discussed the fact that tide gauge and altimetry records both identify differences in the rate of rise over the 20th century (p. 13-16 to 13-17), that there is uncertainty in the interannual to decadal variability inferred from individual tide gauges because of differences between curves (p. 13-17), that there are multiple controls on sea level that can give rise to regional and global differences in the rate of sea-level rise on interannual to multi-decadal timescales, and that these can strongly modulate a long-term secular signal (section 13.7; FAQ 13.1). |

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| 13-441 | 13 | 17 | 16 | 17 | 23 | There is considerable scope in this section to broaden the discussion and include the published views advised in comment 3 above. [Phil Watson, Australia] | Taken into account. This is an important issue and requires careful wording. We have discussed the fact that tide gauge and altimetry records both identify differences in the rate of rise over the 20th century (p. 13-16 to 13-17), that there is uncertainty in the interannual to decadal variability inferred from individual tide gauges because of differences between curves (p. 13-17), that there are multiple controls on sea level that can give rise to regional and global differences in the rate of sea-level rise on interannual to multi-decadal timescales, and that these can strongly modulate a long-term secular signal (section 13.7; FAQ 13.1). We have now added a discussion of the Houston and Dean (2011) and Watson (2011) papers. We will reword the paragraph and some of this material is transferred to Chapter 3. |
| 13-442 | 13 | 17 | 16 | | | DO NOT CONFLATE SATELLITE AND TIDE-GAUGE MEASUREMENTS! Change "The larger rate of rise since the early 1990s is almost double the 20th century average rate, although..." to "The larger rate of rise measured by satellites from 1993-2003 was almost double the 20th century average rate from coastal tide gauges, although..." [David Burton, USA] | Rejected. Where there is overlap, the tide gauge and satellite data agree. Moreover, this statement can be made from tide gauge data alone. |
| 13-443 | 13 | 17 | 16 | | | add "from satellite altimetry" after 1990s [Ernst Schrama, Netherlands] | Rejected. This larger rate of rise is present in both the tide gauge and sea level data. |
| 13-444 | 13 | 17 | 17 | 17 | 17 | Sentence is garbled. [Aslak Grinsted, Denmark] | Taken into account - sentence will be rewritten. |
| 13-445 | 13 | 17 | 18 | 17 | 18 | Ray and Douglas (2011) has been published [Neil White, Australia] | Noted. |
| 13-446 | 13 | 17 | 19 | 17 | 20 | This should also be stated at the appropriate place near the beginning of this Chapter. [Robert Thomas, USA] | Accepted. |
| 13-447 | 13 | 17 | 19 | 17 | 23 | I don't find this clear. How can Merrifield et al. assert that that the recent trend is distinct from the trend in the 1940s and 1970s if they are relying on observations from the tropical and southern hemisphere oceans which "were sparsely observed in the 1940s and 1970s"? The sentence "However, the 20th century" then seems to partially re-state this, I suspect some rewording will clarify this. [John Hunter, Australia] | Taken into account - this will be reworded to clarify. |
| 13-448 | 13 | 17 | 23 | 17 | 23 | "one curve to another". Not too sure what "curve" refers to: Different authors results or...? [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account - this will be reworded to clarify. |
| 13-449 | 13 | 17 | 23 | | | After this sentence I would add: The GMSL rate estimated from tide gauges is systematically lower (2.8 +/- 0.8 mm/yr as mentioned in (Church and White, 2011)) than the GMSL rate from satellite altimetry where the consensus estimate is 3.2 +/- 0.5 mm/yr. This is partially explained by sampling difference between tide gauges and satellite altimetry. [Ernst Schrama, Netherlands] | Rejected. As the comment demonstrates, these are not statistically different. |
| 13-450 | 13 | 17 | 25 | | | "The overall rate of decline is likely to be accelerating" -- this is unknown. The opposite is clearly true in some places; e.g., this map of Glacier Bay, AK: http://soundwaves.usgs.gov/2001/07/glacierbaymap.gif [David Burton, USA] | Taken into account. This sentence will be deleted. |
| 13-451 | 13 | 17 | 25 | | | Section 13.4: GMSL trends are given and commented in several places for three time intervals (1971-2010, 1993-2010, 2005-2010) without a proper discussion of a possible influence of decadal-scale GMSL variability on the trends. The decadal-scale GMSL variability has been documented by several authors (e.g., Holgate S. J., 2007: On the decadal rates of sea level change during the twentieth century, Geophysical Research Letters, 34) and is also visible in Figure 13.4 of the present report. The extent to which it may influence trends computed over different windows is discussed in Section 3.7 (Chapter 3) but is not apparent from Section 13.4. The authors should either add a discussion here or should mention Section 3.7. [Mirko Orlic, Croatia] | Accepted. We added a sentence to make this clear. |

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| 13-452 | 13 | 17 | 34 | 18 | 15 | Excessive confidence is conveyed throughout 13.4.1 [David Burton, USA] | Rejected. We will consult with Chapter 3, but we argue this is an accurate assessment of the literature and will continue to reassess the contributions and the budget. |
| 13-453 | 13 | 17 | 34 | | | Section 13.4.1 it might be helpful to refer to the potential role of internal variability in the thermosteric contribution to sea level rise when discussing trends over a few years - e.g. the reduced Lyman et al. (2010) trend from 2003-2004 and the 2005-2010 Argo-based estimate of von Schuckmann and Le Traon (submitted). [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-454 | 13 | 17 | 34 | | | This section provides a lot of helpful results, but it would better enhance most people's understanding if it was clear how much warming had caused a given amount of thermal expansion, rather than simply providing an estimate of the expansion. Doing so might involve describing average warming at several depths at least the first time through. [James G Titus, United States of America] | Taken into account. We will consider adding this information. |
| 13-455 | 13 | 17 | 36 | 18 | 15 | This comment applies to Section 13.4.1 and to other Sections as well. This whole Section is basically a set of estimates of thermosteric sea-level rise for different periods, accompanied by references. It would be much easier to comprehend if it were presented in a table with a few accompanying comments (e.g. concerning bias corrections, fall-rate errors). This would render the final (summary) paragraph effectively redundant. [John Hunter, Australia] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-456 | 13 | 17 | 40 | 17 | 41 | Barker et al (2011) should be added to Lyman et al, 2010. Ref: Barker, P.M., J.R. Dunn, C.M. Domingues and S.E. Wijffels (2011), Pressure Sensor Drifts in Argo and Their Impacts. Journal of Atmospheric and Oceanic Technology, 28, 1036-1049, doi:10.1175/2011JTECHO831.1 [Neil White, Australia] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-457 | 13 | 17 | 43 | 17 | 45 | This is a bit confusing. After mentioning Domingues et al (2008) the issue of their estimate being an underestimate due to the lack of full spatial and full depth coverage is raised. But this is an issue for the other estimates too - e.g. Levitus et al (2009) which is mentioned a few lines up. [Neil White, Australia] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-458 | 13 | 17 | 43 | | | change "...observed sea level rise" to "...observed sea level rise over the open ocean, but not the coast" [David Burton, USA] | Rejected. Ocean thermal expansion does affect coastal sea levels through a dynamic response of the ocean. We have changed the text to "observed global-averaged sea-level rise." |
| 13-459 | 13 | 17 | 44 | 17 | 44 | Upper ocean thermosteric rate of sea level rise = 0.5 +/- 0.1, with a reference to Domingues et al. 2008. However, in section 3.7.3, page 27, line 34, as well as in Table 3.1 the value provided is 0.6+/- 0.2. I know that, according to the Table, this value is "updated" but, wouldn't it be better that this updated value was also the one indicated in Section 13.4.1? [Belén Martín Míguez, Spain] | Corrected |
| 13-460 | 13 | 17 | 45 | 17 | 51 | Deep ocean is defined here from 700m-3000m. Is the "abyssal" ocean that below the deep? I suggest this term should be defined for the reader. [Stephen Griffies, USA] | Accepted - it is defined elsewhere in the text, but will clearly identified here. |
| 13-461 | 13 | 17 | 48 | | | This section advises "and reported significant warming of the global abyssal...". Relevant to what or over what timeframe? Could this be merely inter-decadal variability? [Phil Watson, Australia] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-462 | 13 | 17 | 50 | 17 | 50 | Deep-ocean thermosteric rate of sea level rise = 0.7 +/- 0.2, citing Church et al 2011b. However, in section 3.7.3, page 27, line 52, as well as in Table 3.1 the value chosen to illustrate this rise is 0.15+/- 0.10 citing Purkey and Johnson 2010. I find this confusing: Are both references equally relevant? In that case both should appear in both chapters. [Belén Martín Míguez, Spain] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-463 | 13 | 17 | 51 | | | Note the lack of anthropogenic cause for deep/abyssal warming! [David Burton, USA] | Rejected. Here Chapter 13 is reporting observations, not attribution. |
| 13-464 | 13 | 17 | 53 | 17 | 53 | 1.6 mm +/-? [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. Uncertainties inserted |
| 13-465 | 13 | 17 | | | | http://www.avisio.oceanobs.com/fileadmin/images/news/indic/msl/MSL_Serie_ALL_Global_IB_RWT_NoGIA_A_djust.png [David Burton, USA] | Rejected. The web link is to a compilation of altimeter records with no description of how they have been |

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| | | | | | | | computed or what corrections have been done or not done. We prefer to refer to the referred literature. We will reassess all satellite observations in writing the SOD |
| 13-466 | 13 | 17 | | | | <p>Section 13.4.1 Thermosteric contribution</p> <p>I believe the overall content of this section needs a major critical revision to achieve the same standard of the other contribution sections (13.4.2. Glaciers, 13.4.4 Land water storage, 13.4.3 Ice sheets). Some specific comments are listed below.</p> <ul style="list-style-type: none"> - lines 37-38: Domingues et al. (2008) missing from references. - lines 39-40: Gouretski and Koltermann (2007) were the ones who recently found the XBT/MBT biases while many others just elaborated on their discovery to develop more bias corrections (Wijffels et al., 2008; Levitus et al., 2009, Ishii and Kimoto, 2009, Gourestki and Reseghetti, 2010, Good, 2011, etc...). - lines 40-41: Instrumental biases for a small subset of Argo floats (SOLO FSI), largely deployed in the Atlantic Ocean, were reported by Willis et al. (2008) and not Lyman et al. (2010). In addition, Barker et al. (2011) have reported on pressure biases in the dominant type of Argo float (APEX) and their impacts on global and regional thermosteric sea level. - lines 42-45: This text sounds a bit confusing. Whose number is likely a lower bound? - lines 53-55: Why the lower trend estimate for Domingues et al. is not included with the Levitus and Ishii values for 1993-2003(2010)? No error bars for 1993-2003? - line 57: Do we need a cautionary note here? How certain that the perceived reduced rate of ThSL rise since 2003-2004 is not (or at least in part) an artefact created by the changes in the ocean observing system at approximately the same time (eg, from an undersampled ocean to almost global coverage with Argo floats)? Or is it just a coincidence? - lines 58-59: Are the differences found by Llovel et al (2010a) within uncertainties? Or do they point to a major systematic problem? - pg 13-18, line 1: Some text should be attached to these "short term trend" estimates: to explicitly clarify that these values mostly reflect interannual variations (eg., ENSO); to provide an idea of the relative amplitude of the ENSO signal compared to the global change signal (eg, see Roemmich and Gilson, 2011); and to mention the association of large error bars with short length records, sometimes almost with the same size of the signal. <p>I wonder if it is more appropriate for this section to be mostly devoted to discuss the ThSL contribution to sea level with a cross-reference to Chapter 3 where a detailed discussion about the ThSL estimates (background information on all estimates, bias corrections, etc) could be organised.</p> <p>[Catia Motta Domingues, Australia]</p> | Accepted. Most of this material moved to Chapter 3 as recommended. |
| 13-467 | 13 | 17 | | | | global land, island, ocean and coastline distribution maps and area tables in different years, e.g. 1900,2000, 2010,2020,2050, may help to express and understand the changing results [Bing Qiao, China] | No Change. It is not clear what the comment is referring to, |
| 13-468 | 13 | 18 | 3 | 18 | 3 | I think von Schuckmann and Le Traon has been published?? [Neil White, Australia] | Noted. |
| 13-469 | 13 | 18 | 6 | 18 | 9 | This paragraph is redundant as the information is also presented in the following paragraph (except for the references). I would eliminate lines 6-9 and keep lines 11-15, adding the references that appear in lines 6-9. [Belén Martín Míguez, Spain] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise. |
| 13-470 | 13 | 18 | 6 | 18 | 15 | There is duplication in these two paragraphs. [Roland Gehrels, United Kingdom] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise. |
| 13-471 | 13 | 18 | 8 | 18 | 8 | state the extent to which deep warming is global/local [Mark Siddall, UK] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise following consultation with Chapter 3. |
| 13-472 | 13 | 18 | 11 | 18 | 15 | This paragraph could be completed with the references that appear in the previous one (lines 6-9 if eliminated) [Belén Martín Míguez, Spain] | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise. |
| 13-473 | 13 | 18 | 15 | 18 | 15 | Where does 27% come from? If we're using Nerem et al's (2010) figure of 2.1 mm/yr (2005-2010) (Ch 3) then 0.75±0.15 mm/yr represents 29-43% of sea level change. Alternatively, if we take Leuliette and Willis' (2011) | Taken into account. We are substantially shortening some of this material, but if it remains, we will revise. |

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| | | | | | | 1.1±0.6 mm/yr (2005-2010) for the mass component then we have a total sea level change of ~1.85 mm/yr and thermosteric change contributes ~41%. This is also only the shallow contribution. If 0.1 mm/yr of deep expansion is added to the 0.75 mm/yr above 700m then 33-48% of change is thermosteric in origin. Clarification and expansion of this paragraph is required in my opinion. [Simon Holgate, UK] | |
| 13-474 | 13 | 18 | 17 | 19 | 37 | A new paper on the contribution of glaciers to sea level rise is available (Jacob, Wahr, Pfeffer and Svenson, 2012: Recent contributions of glaciers and ice caps to sea level rise, nature, doi:10.1038/Nature10847) with strongly reduced numbers. Assessment of the value of these estimates requires a more thorough discussion of the validity of GRACE data for estimates of changes on such small spatial scales. [Peter Lemke, Germany] | Accepted. We will use input from Chapter 4 which certainly will discuss the Jacob et al. paper. |
| 13-475 | 13 | 18 | 17 | 19 | 37 | Section 13.4.2. Glaciers. This section stands out from the preceding ones, in that there are three long paragraphs dealing with technicalities on how to measure things, whereas the other sections bypassed that and launched straight into what was measured. This may be because of previous media attention to glacier history/projections, but the flow of the chapter is significantly broken by this change of style. I think the section on glaciers should be more in the same style as the other ones, listing the facts/data. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. Section 13.4.2 will be substantially revised and shortened, and this suggestion will be incorporated. |
| 13-476 | 13 | 18 | 17 | 20 | 2 | Sections 13.4.2 and 13.4.3 are more relevant to the Chapter 4 and all mentioned have many repetitions. So, these sections should be replaced in Ch 4 and combined with already existed content. Besides the combined text have to be included the suggested in (Konovalov, 2011) general concept of interaction land glaciers and World Ocean (calving is not considered). This concept reflects two basic types of connection between continental glaciation and ocean: (1) Inflow to the ocean volume of melted ice and old firn, formed within of glaciation area, as part of the surficial river flow, and (2) Seasonal outflow of moisture due to evaporation of melted ice and firn on glaciers in the closed (not drained) basins, i.e., without direct connection with the ocean. As the informational, scientific and methodical basis of the proposed new concept serves solutions of the following tasks. a) Identification components of the annual water balance of river basins, influencing the level of the ocean. b) Generalization of the sets of glaciers, discretely located in river basins, into the characteristic groups with a single set of areal, altitudinal and morphological parameters. c) Modeling and calculation of the glacier runoff as component of equation of water balance for catchment and an independent assessment of the quality of results. g) Use of necessary and sufficient set of initial meteorological, hydrological and glaciological data to calculate the components of the water balance equation. If necessary results of realization the described concept could be provided on the example of the rivers Amudarya (closed river basin) and the Brahmaputra (connected to the Indian Ocean). [Vladimir Konovalov, Russian Federation] | Accepted. Portions of 13.4.2 and 13.4.3 will be moved to Chapter 4 as part of a complete revision and shortening. |
| 13-477 | 13 | 18 | 24 | | | I would recommend to write 'only a limited subset have direct measurements' rather than 'have point measurements'. Point measurements of mass balance exist as well, but they provide a different kind of information than the area-averaged (specific) mass balances and should thus not be mixed-up. [Frank PAUL, Switzerland] | Agreed. This section is to be completely revised, however, and this particular content may disappear. The suggestion will be retained and used if this topic reappears elsewhere. |
| 13-478 | 13 | 18 | 25 | 18 | 29 | I do not doubt this line. However, I just noticed a paper in Nature by Jacobs et al., 2012, which will probably have to be addressed in some manner. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted. We will use input from Chapter 4 which certainly will discuss the Jacob et al. paper. |
| 13-479 | 13 | 18 | 25 | 18 | 31 | Jacob et al., 2012, doi:10.1038/nature10847, make an attempt to measure glacier changes with GRACE [Aimee Slangen, Netherlands] | Accepted. We will use input from Chapter 4 which certainly will discuss the Jacob et al. paper. |
| 13-480 | 13 | 18 | 51 | 18 | 51 | I don't know if any of these glaciers have ice shelves, but it would be worth clarifying whether "marine-terminating" glaciers include glaciers with ice shelves or whether it means only tidewater glaciers. [John Hunter, Australia] | Taken into account. This terminology needs to be clarified, but "marine-terminating" here refers to all glaciers that end in the ocean, where they end in floating ice or not. Incidentally, the meaning of the term "tidewater glacier" is shifting to include glaciers with floating termini; since Alaskan temperate tidewater glaciers (the epitome of the tidewater glacier) are now known to be able to develop floating tongues, there no longer appears to be a physical |

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| | | | | | | | category of marine-ending glaciers that cannot have floating termini. |
| 13-481 | 13 | 18 | 51 | | | This should read ' underrepresented in the direct measurements' (as they are well represented in the glacier inventories) [Frank PAUL, Switzerland] | Accepted. |
| 13-482 | 13 | 18 | 54 | 19 | 2 | I understand that a certain ice mass / volume has a distinct SLE in m, but what are rise rates in terms of SLE? Where does the time scale come from? [Uwe Stoeber, Germany] | Taken into account. The correspondence between glacier mass loss and sea level rise is reckoned as 362 GT (10 ⁹ tonnes) fresh water mass per mm sea level rise. This will to be defined early in the sections on Cryospheric contributions. |
| 13-483 | 13 | 19 | 1 | 19 | 37 | This section never cross references Chapter 4. This is a repeat, sometimes inconsistent, of the major part of Chapter 4. The coordinating authors need to work harder on this. [Eric Rignot, USA] | Taken into account. Section 13.4.2 will be substantially revised and shortened, and be coordinated much more closely with Chapter 4. |
| 13-484 | 13 | 19 | 11 | 19 | 12 | Since AR4 many papers have looked into the GRACE satellite data, I presume that the placeholder text will refer to these papers. [Ernst Schrama, Netherlands] | Taken into account. There is a very large amount of new literature yet to be incorporated, some of which has not yet been submitted. These include several new GRACE analyses. |
| 13-485 | 13 | 19 | 16 | 19 | 16 | Explain "geodetic observations" [Ian Allison, Australia] | Accepted - will be explained. |
| 13-486 | 13 | 19 | 16 | 19 | 16 | The meaning and relevance of "without geodetic observations" is unclear here. [John Hunter, Australia] | Accepted - will be explained. |
| 13-487 | 13 | 19 | 19 | 19 | 21 | Numbers should include error ranges [Regine Hock, US] | Taken into account. Dyurgerov did not supply error ranges; we may be able to extract them from ancillary data. |
| 13-488 | 13 | 19 | 23 | 19 | 23 | Brackets opened '(since ca. mid-19th century....' and not closed [Jeff Ridley, UK] | Corrected |
| 13-489 | 13 | 19 | 23 | 19 | 37 | See Jacob et al, Nature advanced online publication. Also, Cogley et al is for the period 01-05 not 05-09. Given that the paper was published in 2009 its hard to see how it could include that year.... In addition the numbers quoted are wrong. Cogley 09 gets 1.1 mm/yr excl PGIC and 1.4 mm/yr for all GIC incl Gris, AIS. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account - the Cogley details will be corrected. Jacob et al came out long after the First Order Draft was submitted, but will be included in the Second Order Draft. |
| 13-490 | 13 | 19 | 23 | 19 | 37 | Can't follow the logic in this paragraph [Jeff Ridley, UK] | Taken into account - the paragraph will be rewritten for clarity. |
| 13-491 | 13 | 19 | 24 | 19 | 24 | Missing ')'. [Donald Forbes, Canada] | Corrected. |
| 13-492 | 13 | 19 | 24 | 19 | 24 | The name Leclercq is misspelled as Leclercq on this line and at other points in the text. It is spelled correctly in the list of references, please do a global search for Leclercq and replace with Leclercq. [Howard J. Freeland, Canada] | Corrected. |
| 13-493 | 13 | 19 | 24 | 19 | 24 | A closing bracket is missing after (2011). [Roland Gehrels, United Kingdom] | Corrected. |
| 13-494 | 13 | 19 | 24 | 19 | 24 | Leclercq should be Leclercq [Philippe Huybrechts, Belgium] | Corrected. |
| 13-495 | 13 | 19 | 24 | | | Add a brackettLeclercq et al.(2001)).... [Thomas Voigt, Germany] | Corrected. |
| 13-496 | 13 | 19 | 26 | 19 | 26 | As I said in my review of the AR4 FOD: "Pentadal" is an appallingly imprecise word and should (presumably) be replaced by "5-yearly". The word "pentadal" is not even in the Oxford English Dictionary. In addition, the OED defines the noun "pentad" as EITHER a period of 5 DAYS or a period of 5 YEARS (both meanings having been used in climatology). [John Hunter, Australia] | Rejected. "Pentadal" is now well-established in the glacier mass balance literature, having been introduced by Graham Cogley and widely used to refer to an interval of 5 years for at least the past decade. |
| 13-497 | 13 | 19 | 26 | | | Instead of 'most commonly used for averaging' I would write 'a method applied for averaging'. The use of | Accepted. |

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| | | | | | | pentadal intervals is specific to one author and not a common standard when reporting mass balance observations. [Frank PAUL, Switzerland] | |
| 13-498 | 13 | 19 | 30 | 19 | 30 | The peripheral glaciers are not missing! It is their inclusion in inventories that is missing. [Ian Allison, Australia] | Accepted. |
| 13-499 | 13 | 19 | 30 | 19 | 33 | I would recommend to be more specific with the term 'Arctic Canada'. There is likely already a high contribution from glaciers on Baffin Island, but not very much from the regions to the north (Bylot, Devon, Ellesmere Island). These glaciers are mainly cold or polythermal and do not yet generate much runoff (of course, this can change in the future). The same applies to many of the local (peripheral) glaciers on Greenland and the Antarctic Peninsula. [Frank PAUL, Switzerland] | Taken into account. Actually the northern Canadian Arctic is the larger contributor by a wide margin, and this will be specified. |
| 13-500 | 13 | 19 | 34 | 19 | 37 | You may add here that there is a substantial amount of ice below sea level that is not correctly accounted for in current models. For example, much of the ice in the ablation region of Bering and Malaspina glaciers is below sea level and will thus not contribute to it. [Frank PAUL, Switzerland] | Taken into account - we will add this information. |
| 13-501 | 13 | 19 | 41 | 19 | 48 | Section 13.4.3. First paragraph requires references regarding the key processes listed. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Rejected. The purpose of this section is to draw across information from Chapter 4 so that an observational budget can be made. In this respect processes are not important. They are covered in detail elsewhere in the chapter, in particular where they become in projections. |
| 13-502 | 13 | 19 | 46 | 19 | 46 | Replace (the release of icebergs) with outflow across the grounding line. There is sub-shelf melting in both Greenland and Antarctica and both are important parts of their mass budget. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. Have amended text. |
| 13-503 | 13 | 19 | 46 | 19 | 46 | "ice bergs" should read "icebergs". [John Hunter, Australia] | Noted. Relevant text now gone see 502. |
| 13-504 | 13 | 19 | 46 | 19 | 46 | A comma is needed between "accumulation" and "primarily". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Editorial. Have amended punctuation. |
| 13-505 | 13 | 19 | 47 | | | This statement is too imprecise, as the Antarctic mass budget should also take into account evaporation/sublimation and subglacial run-off. Weaken the statement made here, e.g. by changing "comprises" to "is dominated by". Rather than explaining it in more detail, I would add a reference to chapter 4 for the details. [Olaf Eisen, Germany] | Accepted. Have amended text. |
| 13-506 | 13 | 19 | 50 | 19 | 50 | I think that "rate of" should be inserted in between "and this" and "loss is". [John Hunter, Australia] | Accepted. Have amended text. |
| 13-507 | 13 | 19 | 51 | 19 | 53 | These dates and numbers are NOT those in Chapter 4.4.2.2. That has: "The total sea level contribution from the Greenland Ice Sheet has been 5.9 mm (± 1.1 mm) over the period 1992-2009, and 4.5 mm (± 0.8 mm) between 2002 and 2009" [Ian Allison, Australia] | Noted. Have kept the FOD numbers here for the time being but clearly will revise when new numbers are supplied by Chapter 4. X-chapter agreement that these numbers will be available 2 weeks prior to SOD deadline. |
| 13-508 | 13 | 19 | 52 | 19 | 52 | Add 'SLE' after first number. [Donald Forbes, Canada] | Accepted. Have amended text. |
| 13-509 | 13 | 19 | 52 | 19 | 53 | This comparison of rates for 1993-2009 and 2005-2009 is over periods of different length - they are therefore not directly comparable and do not therefore support the statement in the previous sentence that "this loss is likely to have increased over the last two decades". It would be good if this could be rectified by getting two estimates of the rate of loss taken over periods of the same length. This comment applies to all instances in this Chapter where a comparison is made over periods of different length. [John Hunter, Australia] | Noted. The selection of the periods for which observed SLR contributions are measured is a complicated process. The selected periods reflect many issues spanning observations on the ice and oceans, such as the dates at which particular satellites became operational. It would be useful to have periods of the same length however practical issues confound this. It is very hard to explain the reported figures if loss had not increased during the last 2 decades. |

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| 13-510 | 13 | 19 | 52 | 19 | 57 | It seems surprising that error bars for earlier period are far lower than for later period, which had the advantage of GRACE and GLAS observations. This casts doubt on reliability of the error assessments, which can, however, be relied upon to be too low for almost all ice-sheet mass balance estimates. [Robert Thomas, USA] | Noted. This is an issue for Chapter 4. The function of this subsection is simply to draw their numbers across. Have emailed Chapter 4 contact. The figures quoted by Chapter 4 (see 507) do not show this feature but are given as changes rather than rates. Need to agree that Chapter 4 will supply rates. Also, for rates the uncertainty is dependent on the length of the period. |
| 13-511 | 13 | 19 | 54 | 19 | 56 | These dates and numbers are NOT those in Chapter 4.4.2.2. That has: "The total sea level contribution from Antarctica has been 3.4 mm (±1.5 mm) over the period 1992-2009, and 3.0 mm (±1.1 mm) between 2002 and 2009" [Ian Allison, Australia] | Noted. See 507. |
| 13-512 | 13 | 19 | 56 | 19 | 56 | The uncertainties have here been added linearly and not in quadrature - this needs some explanation, and also a general explanation (covering the whole Chapter or, better, all the Assessment Reports) similar to Box 11.1 in the TAR Chapter 11. [John Hunter, Australia] | Accepted. Have amended text. Emailing to CLA asking whether there is general guidance about this and whether an explanation is required in Chapter 1. |
| 13-513 | 13 | 19 | 56 | 20 | 1 | Again, different from Chapter 4 [Ian Allison, Australia] | Noted. See 507. |
| 13-514 | 13 | 19 | | | | Section 13.4.3 Ice sheets - I am missing comments on the knowledge (or lack thereof) about the ice sheets contributions before 1993. [Catia Motta Domingues, Australia] | Noted. Whether numbers are generated or not for periods prior to 1993 depends on Chapter 4. we will need to ensure that there is text the recaps whatever decision chpt 4 makes about the longer periods (1901-1990 and 1971-2010). |
| 13-515 | 13 | 20 | 4 | 21 | 6 | Consider the shrinkage of Arctic lakes (Carroll et al., 2011) when estimating terrestrial reservoir contribution to sea level rise. Carroll, M. L., J. R. G. Townshend, C. M. DiMiceli, T. Loboda, and R. A. Sohlberg (2011), Shrinking lakes of the Arctic: Spatial relationships and trajectory of change, Geophys. Res. Lett., 38, L20406, doi:10.1029/2011GL049427. [David Parker, United Kingdom of Great Britain & Northern Ireland] | Taken into account. We will assess this literature as to whether this effect is a significant contributor. |
| 13-516 | 13 | 20 | 4 | | | Section 13.4.4 and 13.4.5: Somewhere in here, I think the Riva et al., 2010, paper should be discussed. It has implications for the land-water storage, and is the complement to the other GRACE studies in 13.4.5 [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted. This is a good suggestion |
| 13-517 | 13 | 20 | 6 | 20 | 6 | Add comma after "variability" to improve readability, or otherwise rephrase. [John Hunter, Australia] | Editorial. Have amended punctuation. |
| 13-518 | 13 | 20 | 8 | 20 | 9 | water is also stored in permafrost (Lawrance et al. 2005) but no mention here [Jeff Ridley, UK] | Accepted - reference will be made to Milly et al, 2010. |
| 13-519 | 13 | 20 | 18 | 20 | 23 | The paragraph claims complete scientific understanding of interannual changes in sea level and their relation to ENSO. Even though, the high correlation between ENSO indices and interannual changes in total sea level (Nerem et al., 2010) as well as terrestrial water storage from models (Llovel et al., 2011) indicates a connection between these events and sea level fluctuations, the relative roles of steric and eustatic sea level still require more detailed investigation to understand the underlying processes. [Carmen Boening, USA] | Taken into account. Unfortunately we have no space to go into the details of the processes. But we can quote recent papers that discusses possible processes (Boening et al, 2012; Cazenave et al., 2012.). |
| 13-520 | 13 | 20 | 18 | 20 | 23 | This section could mention a recently submitted paper (Boening et al., 2012) that studies the processes underlying ENSO-related sea level variability in more detail. This case study examining the 2010/11 La Nina determines the relative contributions of mass and cooling during the event. Analysis of data from ARGO, GRACE, altimetry, and CERES yield that during the event water mass transport from the ocean to land dominated and cooling only played a minor role. [Carmen Boening, USA] | Taken into account. Unfortunately we have no space to go into the details of the processes. But we can quote recent papers that discusses possible processes (Boening et al, 2012; Cazenave et al., 2012.). |
| 13-521 | 13 | 20 | 18 | 20 | 23 | Having these new observing systems in place provides a base for understanding the underlying physics and role of the influence of climate modes on sea level. In addition to understanding present day effects of ENSO on sea level, the evolution and changes in climate modes in a warming world and the potentially changing impact on ocean heat uptake and precipitation/evaporation/convection/winds requires further investigation. Thus, it needs to be emphasized that even though a connection between interannual sea level, water storage, and ENSO is obvious, the processes need to be studied more intensely to eventually help to improve climate model physics and future predictions. [Carmen Boening, USA] | Taken into account. Unfortunately we have no space to go into the details of the processes. But we can quote recent papers that discusses possible processes (Boening et al, 2012; Cazenave et al., 2012.). |

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| 13-522 | 13 | 20 | 22 | 20 | 23 | What evidence is there to link the down turn in Fig. 13.3 with La Nina? [Ian Allison, Australia] | Taken into account. This is currently debated. There are at least 2 papers (submitted or in revision) that discusses the processes at work which will be assessed in the SOD. |
| 13-523 | 13 | 20 | 23 | 20 | 23 | Boening et al., 2012: Boening, C., J.K. Willis, F.W. Landerer, R.S. Nerem, J. Fasullo, 2012: The 2011 La Niña: So Strong, the Oceans Fell, Nature, submitted. [Carmen Boening, USA] | Accepted - will refer to paper. |
| 13-524 | 13 | 20 | 23 | 20 | 23 | Nina and Nino are sometimes spelled with the tilde over the second n and sometimes not, I suggest doing a global search for Nino and Nina and correcting them all after deciding which you want to do. [Howard J. Freeland, Canada] | Accepted. We will be consistent. |
| 13-525 | 13 | 20 | 34 | 20 | 34 | Does "the last half century" mean the second half of the 20th century, or the last 50 years. Specifying a date range would be better. [Neil White, Australia] | Accepted - now modified. |
| 13-526 | 13 | 20 | 42 | 20 | 44 | Water 'mining'? Wouldn't 'extraction' be a better term? [Donald Forbes, Canada] | Taken into account. The correct term is ground water depletion. |
| 13-527 | 13 | 20 | 43 | 20 | 43 | It is probably worth mentioning here that groundwater withdrawal could well taper off in the future as fossil aquifers are exhausted and extraction from non-fossil aquifers becomes more difficult as they get deeper due to overextraction. In addition some cities (e.g. Bangkok) are limiting groundwater extratction because of subsidence issues. [Neil White, Australia] | Taken into account. There is a new paper by Wada et al that provides projections for future ground water withdrawal. We now quote it. |
| 13-528 | 13 | 20 | 47 | 21 | 43 | Information on GRACE is incomplete. For example, GRACE data has been successfully used to determine groundwater changes, detect droughts, and finds application in water management. Several additional references on GRACE processing and errors as well as sea level and terrestrial water storage studies using GRACE, can be found here: http://grace.jpl.nasa.gov/publications/ . [Carmen Boening, USA] | Taken into account. Unfortunately we have no space to go into the details of the processes. |
| 13-529 | 13 | 20 | | | | Section 13.4.4 Land water storage - pg 13-21, lines 1-6: The summary (contribution/confidence) could be placed in the context of previous IPCC reports in which the contribution of terrestrial water storage was stated to be highly uncertain and possibly a large contribution. [Catia Motta Domingues, Australia] | Accepted - we will do this. Good suggestion. Will do that |
| 13-530 | 13 | 21 | 1 | 21 | 4 | "In summary, model-based and GRACE-based estimates of total land-water storage indicate that climate-related trends are small and do not contribute more than ~ 0.1–0.2 mm yr ⁻¹ to observed sea level rise. This is unlike human-induced changes, which is several times larger in amplitude over the second half of the 20th century." Here plain use of the wording 'human-induced changes' is confusing, as the climate change is also (at least in part) human induced. What you seem to mean is something like 'human reservoir building, freshwater diversion, and groundwater mining'? It's best to be specific so there is no confusion that anyone could latch on to. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. We agree with the reviewer and will be more specific; |
| 13-531 | 13 | 21 | 2 | 21 | 2 | The "~0.1-0.2 mm yr ⁻¹ " is not a "summary" (see line 1) at all - it is the first time these numbers have appeared. They should appear earlier in Section 13.4.4, both in relation to the models and to GRACE data. [John Hunter, Australia] | Accepted - they will appear earlier. |
| 13-532 | 13 | 21 | 3 | 21 | 3 | "...human induced change, which is..." [Ian Allison, Australia] | Accepted - now modified. |
| 13-533 | 13 | 21 | 3 | 21 | 3 | Insert 'climate-driven' before 'changes'. Or change the whole phrase to 'anthropogenic climate-driven changes'. [Donald Forbes, Canada] | Accepted - now modified. |
| 13-534 | 13 | 21 | 3 | 21 | 3 | I would rephrase "which is several times larger in amplitude" with "which have components which are several times larger in magnitude". This still isn't ideal and complete rephrasing of the sentence would be better. [John Hunter, Australia] | Accepted - now modified. |
| 13-535 | 13 | 21 | 10 | 21 | 20 | To my understanding the stated range of GIA corrections is too small by about a factor of two. See, e.g. Cazenave and Chen (2010), Earth and Planetary Science Letters 298 (2010) 263–274, section 6. This is still being argued about, and I think there should be more emphasis on how large the true uncertainties in this data | Taken into account. This issue was controversial until recently but the problem seems to have been resolved, leading to much better agreement between |

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| | | | | | | are. I think there is a tendency in these communities to underestimate measurement errors (see line 41 on the same page). [Neil White, Australia] | the model-based values. The text has been modified to reflect this. |
| 13-536 | 13 | 21 | 15 | 21 | 26 | This information should be updated and be based on the upcoming new release of GRACE data. I would like to recommend contacting the processing centers about further information (JPL (Michael M. Watkins), CSR (Srinivas Bettadpur), GFZ (Frank Flechtner)) if this hasn't already been done. [Carmen Boening, USA] | Taken into account. We will update information as it is published. |
| 13-537 | 13 | 21 | 19 | 21 | 19 | I am not sure I understand the reason for the reference to Paulson et al., 2007, here. While this is the model that the Chambers et al., 2010, paper is based, I don't believe that it addressed the issue discussed on this line. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted - will be deleted. |
| 13-538 | 13 | 21 | 22 | 21 | 23 | The discussion here needs to be better aligned with that on lines 37-40. It's not clear how the rate on line 22 and the rate on line 37 are related. [Michael Oppenheimer, USA] | Modified |
| 13-539 | 13 | 21 | 23 | 21 | 23 | Please specify "somewhat less than estimates ...from other sources". How much is "somewhat less" and what are the "other sources"? [Carmen Boening, USA] | Accepted. We agree that this was not clear. We will be more specific here |
| 13-540 | 13 | 21 | 37 | 21 | 37 | The "1.1 +/- 0.6 mm/yr SLE over 2005-2010" is inconsistent with the earlier "increase in global average ocean mass since 200 range from 1-1.5 mm/yr SLE", which both come from the same paper. This should be fixed - a "summary" should summarise! [John Hunter, Australia] | Accepted. The text has been revised so that numbers are consistent. |
| 13-541 | 13 | 21 | 37 | 37 | 52 | The last paragraph in section 13.4.5 and the first paragraph in section 13.4.6 seem to be contradictory. The first indicates that observed gain in ocean mass cannot be reconcile with the observed loss of Greenland, Antarctica and glaciers, the reasons for this discrepancies being unknown The following paragraph opening section 13.4.6. states that the sea-level budgets are balanced over all three time periods considered. [Eduardo Zorita, Germany] | The text has been revised according to new values for land ice melt available from chap. 4 |
| 13-542 | 13 | 21 | 38 | 21 | 38 | I feel this should say that the agreement is with large error bars. The quoted errors on the GRACE estimate are >50%. Going back to my general comment for this chapter, I feel that this is an illustration of the overly rosy view of the closing of budgets presented, in particular, in the executive summary. The sum of the 2005-2010 components is 1.1 + 0.75 = 1.85 mm/yr which is 88% of the 2.1 mm/yr estimated by Nerem et al (2010). This is for the best observed period in history. I can't believe that we can close the budget from 1960 or 1970 anything like as well as that which means we have larger uncertainty that I feel is presented here. [Simon Holgate, UK] | Accepted. We agree with the reviewer and will change the text to reflect that there may be an agreement within the quoted uncertainties. However, note that the length of the period, the uncertainty of the measurements and their distribution are all important for determining the uncertainty on trends. Especially for 2005-2010, trends can be more uncertain because of the shortness of the period, especially if there are small systematic errors. |
| 13-543 | 13 | 21 | 40 | 21 | 40 | "Greenland, and Antarctic" should be replaced by "Greenland, Antarctic and land water storage". [John Hunter, Australia] | Accepted - now modified. |
| 13-544 | 13 | 21 | 42 | 21 | 43 | This dammed glacier melt explanation does not appear satisfactory because the combined terrestrial storage has zero contribution (see section 13.4.4). So, if you dam a portion of the glacier melt, then you have to compensate with more pumping. (If you include impoundment, then you also have to include pumping). [Aslak Grinsted, Denmark] | Noted. This sentence has been removed. |
| 13-545 | 13 | 21 | 47 | 21 | 52 | This paragraph reads like a table caption. [Uwe Stoeber, Germany] | Accepted - now modified. |
| 13-546 | 13 | 21 | 55 | 22 | 3 | In Table 13.1, there is a slight inconsistency in that the table entries are in terms of sea-level rise and yet two rows are labelled "Greenland SMB" and "Antarctic SMB" (which have the opposite sign to sea-level rise). It think this could be addressed simple by a note in the caption indicating that these table entries are of opposite sign to SMB. [John Hunter, Australia] | Accepted. Footnote added. |
| 13-547 | 13 | 21 | 55 | 22 | 3 | Table 13.1 Name of lines in the part "Observed contributions" means, that we have some system of direct measurements, which provide users with data entirely and representatively covering the region (e.g., Glaciers, Greenland, and so on) as a whole. It is not correct (probably, excluding Land Water Storage), see comment 7 and sections 13.4.2-13.4.3. Thus, taking into account the described there a lot of uncertainties and disadvantages in glacier inventories and mass balance measurements, in the part "Observed contributions" hid the limited and unrepresentative samples of data, which used further for Modelling. Values of modelled | Rejected. We base our discussion on the published literature and the attendant methodologies that have been developed therein. In assessing these methodologies, we clearly describe their shortcomings. We will also refer to Chapter 4 where applicable for further discussion of these issues. |

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| | | | | | | glacier contributions are wrong in four time intervals, because of methods of thier receving ignore water balance of watersheds, where glaciers exist and different coefficient of runoff for marine-terminated glaciers and those ones located thousands kilometers far from mouth of rivers. [Vladimir Konovalov, Russian Federation] | |
| 13-548 | 13 | 21 | 55 | 22 | 10 | Table 13.1 How were errors estimated for the model results, and what do these error estimates mean? Surely not an indication of how close the estimates are to reality, so presumably a measure of uncertainty within some model world, which itself of course may have little resemblance to reality. Inclusion of such error estimates without honest and clear explanation is extremely misleading. To the average reader they tend to suggest that we know far more than we really do. [Robert Thomas, USA] | Rejected. It is already clear in the text that the uncertainties indicate the model spread, as the reviewer correctly says, so it is not misleading. We have rearranged 13.4 in order to be able to compare the observational and model estimates of each contribution directly, and thus help the reader to appreciate the extent of model uncertainty. |
| 13-549 | 13 | 21 | 55 | 22 | 10 | Table 13.1: My impression is that most of the ice-body models (and probably others also) are tuned to what we think we know about what really happened, so it is not surprising that they show agreement with observations. It is not really clear how they help unless the processes responsible for whatever happened are understood and included adequately in the models. This certainly is NOT true for models of ice-sheet dynamics, so the inclusion of low error estimates for Greenland and Antarctic dynamic contributions is misleading. Perhaps the purpose is to give the reader a false sense of confidence in predictions coming later in the Chapter?? [Robert Thomas, USA] | Taken into account. We have rearranged the table to make clear that we do not have an a-priori model estimate of the ice-sheet dynamical contribution. We reject the allegation that our intention is to mislead the reader about confidence; we have extensive discussion of our level of confidence in this contribution in 13.5 and 13.6. We note the reviewer's first sentence. Obviously it is true that all models are formulated in a way which is consistent with our scientific understanding and observation. We agree with that. However it is not usually a trivial matter for observations and models to agree; evaluating the extent of this agreement is a major purpose of 13.4, which we have rearranged so that this purpose is clearer. |
| 13-550 | 13 | 21 | 55 | | | Table 13.1: reported periods are not consistent with chapter 4.Same periods should be reported where possible [Regine Hock, US] | Taken into account. Chapter 4 was not using the agreed periods, but Chapter 13 was. This will be corrected for consistency. |
| 13-551 | 13 | 21 | 55 | | | Table 13.1: The line 'Modelled Greenland SMB for observed climate' is confusing because it doesn't add to the total. It should be placed in a way that it is obvious that it is not included. [Uwe Stoeber, Germany] | Taken into account. The table has been rearranged. |
| 13-552 | 13 | 21 | | | | Section 13.4.6 GMSL budget [Catia Motta Domingues, Australia] | Response below. |
| 13-553 | 13 | 21 | | | | - Although it is mentioned in the text that the numbers provided in Table 13.1 are based on direct estimates for the contributions (Chapters 3, 4 and land storage from Church et al), it is still obscure to me how exactly some of these numbers were pulled together given that certain contribution estimates have a wide range. [Catia Motta Domingues, Australia] | Taken into account. This is now clarified |
| 13-554 | 13 | 21 | | | | - Some of the error bars look rather small, i.e., for ThSL contribution over 2005-2010. [Catia Motta Domingues, Australia] | Taken into account. More realistic error bars are now used |
| 13-555 | 13 | 21 | | | | - There is no mention in the text that other minor terms have been omitted from the SLBudget. [Catia Motta Domingues, Australia] | Accepted. This will be corrected. |
| 13-556 | 13 | 21 | | | | Table 13.1: In the last row of the fourth column a number is missing. [Mirko Orlic, Croatia] | Accepted and corrected. |
| 13-557 | 13 | 22 | 1 | 22 | 9 | I note that the observed and modelled totals are very close for all 3 periods. They are in fact so close that it seems rather unlikely to happen by chance if the uncertainties are correct. I calculate assuming independence that the probability of the numbers being so close 3 times in a row to be $P(1971)*P(1993)*P(2005)=0.2*0.3*0.3=0.016$. For that reason I do not think the agreement is quite as impressive as it appears. They could have been equal, but if the uncertainty is still huge then that i tend to think "what a coincidence?". [Aslak Grinsted, Denmark] | Noted. |

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| 13-558 | 13 | 22 | 1 | 22 | 11 | The modelled contributions should not be listed here. They are obviously wrong for the ice sheets and reflect the arbitrary selection of some models or some averaging of multiple models rather than an assessment. I don't know of a single peer-reviewed paper that detected a large increase in SMB in Antarctica for the time periods mentioned. Secondly, there is no way that Antarctic ice dynamics has been modeled to EXACTLY repeat observations, this must be an unfortunate typo or a major misunderstanding. There is not a single paper that has replicated any of these changes using climate forcing and ice sheet geometry alone. The only partially successful attempts had to force the models to fit the data in order to improve their performance. [Eric Rignot, USA] | We reject the first point. One of the purposes of 13.4 is to compare models and observations, so it is essential to do so in the table. This is part of model evaluation. In the text, we discuss the increasing trend in modelled Antarctic SMB. Observations do not confirm this, but are not sufficient to exclude it (chapter 4). The second point has been taken into account by rearranging the table to indicate that we do not have an a-priori model estimate of the ice sheet dynamic contribution. |
| 13-559 | 13 | 22 | 2 | 22 | 2 | It should be made clear that the last line of Table 13.1 is the observed sea-level rise and not some total based on the observed contributions. I would replace "Observed total" with "Observed sea-level rise". [John Hunter, Australia] | Taken into account. The presentation of the table has been clarified |
| 13-560 | 13 | 22 | 2 | 22 | 2 | On the last line of Table 13.1, the "1993-2010" column is missing a number. [John Hunter, Australia] | Accepted and corrected. |
| 13-561 | 13 | 22 | 2 | 22 | 3 | comparing the observed glacier contributions in Table 13.1 with Table 4.4, the means are the same but the 5-95% values are different. Please reconcile. [Philip Mote, USA] | Taken into account. Now corrected using final values provided by Chap 4. |
| 13-562 | 13 | 22 | 2 | 22 | 3 | Table 13.1, modelled Antarctic component: kudos for clearly showing this. The fact that the models get the SIGN wrong for Antarctica for the observed periods of record shows that they MUST NOT be used for projections. This approach to projecting future Antarctic contributions has no basis in observations, and clearly leads to egregious underestimates in the projections for global sea level rise. Although the modelled budget closes, this seems to be because the large negative error in Antarctic mass balance is offset by smaller positive errors in other terms, and points toward a flaw in the predictive approach. Also, is the mean value missing from the "observed total" row, 1993-2010 column? [Philip Mote, USA] | We reject the main point. Observations indeed do not confirm the increasing trend in modelled Antarctic SMB, but cannot exclude it (Chapter 4). Moreover, there is strong theoretical basis and paleo evidence of a direct relationship between increased temperature and increase precipitation and it is universally predicted by physically based models for the future. The last point is accepted and corrected. |
| 13-563 | 13 | 22 | 2 | 22 | 3 | The value of "Observed total" at the bottom line of this table reads +/-0.5 which is wrong. [Ernst Schrama, Netherlands] | Corrected |
| 13-564 | 13 | 22 | 14 | 22 | 15 | This improved understanding of the sea-level budget is very significant and possibly needs further emphasis - on reading the Executive Summary, I didn't get much of a message that this is important - it is! - not just for further progression of the science but as something that needs to be communicated to non-scientists - it is a major step forward. [John Hunter, Australia] | Taken into account. A more thorough discussion is now added in this section |
| 13-565 | 13 | 22 | 14 | 22 | 17 | There is alternative and more substantiated method to calculate change of World Ocean Level (WOL), which described, for example, in Malinin (2009), see list of references in the Supplement. The method based on application of annual water balance equation for the World Ocean in the form: $\Delta H_e = (P_{wo} - E_{wo} + R_c + I - \Delta V_{sh} + \Delta S - \Delta A) / F_{wo}$. Here ΔH_e - is eustatic component of WOL, F_{wo} - is area of World Ocean, P_{wo} and E_{wo} - are consequently volumes of total precipitation and evaporation, R_c - is continental runoff, comprising surface and subsurface waters, I - is iceberg runoff (calving), ΔV_{sh} - is change of WOL due to melting (freezing) of shelf glaciers in the Antarctic, ΔS - is melting of sea ice and permafrost, ΔA - is anthropogenic influence on inflow of water in the World Ocean. The next quotation from (Malinin, 2009) looks undoubtedly true: "Melting of mountain glaciers, which indeed is quite significant, can directly contribute to WOL variations only in the case of glaciers on islands in the Arctic Ocean and in Alaska. The mountain glaciers in Europe, Asia, Africa, and South America can affect WOL only via river water inflow to the ocean. Continental glaciers form very small relative part of annual inflow from river basins to the Ocean (0.3-0.8% and much more less. This is reviewer's calculation.) if this inflow is measured in the mouth of river. Therefore, it appears not reasonable to directly take into account their contribution to WOL trend". According to the data in (Global Water Balance and Water Resources of the Earth. 1974. Leningrad, Hydrometeoizdat, 638 p.) total inflow from continental rivers to the World Ocean equals 47000 km ³ . Some estimations of river inflow to the Ocean and its long-term variability illustrate Fig.5 and Table 3 in the Supplement from reviewer. After determining of all components of annual water balance equation for the World Ocean Malinin (2009) received rather acceptable difference between calculated and measured trend of WOL in 1993-2003, equalled 0.2 mm/year. [Vladimir Konovalov, Russian] | Rejected. We have not been given the reviewer's supplement and assume Malinin (2009) is "Variations of global water exchange under changing climate" in Water Resources (not Water Resources Research), which is unfortunately a journal to which we do not have access and we have therefore only seen the abstract. This paper has not been cited according to ISI. Although a method based on fluxes would work in principle, in practice we do not know any of these fluxes observationally with the required accuracy. Melting of sea-ice and ice-shelves contributes negligibly to sea level, since these masses are already afloat. It is quite true that mass loss from glaciers mostly enters the ocean through river runoff and is only a small fraction of river runoff, but that does not mean it is a negligible contribution to sea level rise. On the contrary, this very result points to the practical difficulty referred to above in using fluxes to calculate the change of mass of the ocean. It is |

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| | | | | | | Federation] | much more reliable to quantify changes in stores of water. |
| 13-566 | 13 | 22 | 30 | 22 | 31 | If "their trends were about 10% less than the observed trends but closer to the observations than the models without natural forcing", does this mean that the modelled trends without natural forcing were more than 10% above the observations (since the modelled trends without natural forcing are higher than modelled trends with natural forcing)? I assume that the answer depends on exactly when the volcanos occurred relative to the period over which the trend was estimated, but this all needs to be made a bit clearer. [John Hunter, Australia] | Taken into account . This will be clarified. |
| 13-567 | 13 | 22 | 36 | 22 | 39 | The words "and reduced solar activity" should be included in the discussion at the bottom of page 13-22. [Terje Wahl, Norway] | Taken into account by mentioning "other forcing changes". |
| 13-568 | 13 | 22 | 37 | 23 | 2 | If there is a decreased rate of ocean warming due to negative aerosol radiative forcing, why is the rate of expansion larger after volcanic eruptions? [Uwe Stoeber, Germany] | Taken into account by inserting "tropospheric" before "aerosol" i.e. the earlier point does not refer to volcanoes. Also, immediately following an eruption there is a cooling of the ocean followed by a period of recovery. |
| 13-569 | 13 | 22 | 38 | 23 | 13 | This sentence in line 22- 38 is quite unclear. The reference Church 2011b is not in the reference list. I have checked the other reference included in the list Church et al 2011, but there there is no mention of models lacking aerosols forcing, which in itself would be very remarkable Does this sentence refer to volcanic aerosols or to anthropogenic aerosols? Does the word ' rate' in this sentence refer to the rate of sea-level or the rate of thermal expansion? if the former is correct, this sentence indicates a disagreement between modelled and observed sea-level, a disagreement that would be actually larger because models do not include the observed increase in ocean mass that according to this report has been considerable in the last 20 years. I think this section should more thoroughly discuss the possible sources of disagreement between models and observations. The disagreement is also over a relatively long period of 20 years for a variable like sea-level for which the internal variations should be smaller than the externally forced trend. The paragraph on page 23 then offers another explanation of why models would underestimate the thermosteric contribution (Gregory 2010) related to the spin-up of models, which I think is not consistent with what is shown in these paragraphs. This explanation would also apply to the whole 20th century period. So, why do models reproduce the observed sea-level rate in 1971-2010 ? Also this paragraph glosses over the lack of between the simulated model rate and the observed previous to 1970, which is very obvious in figure 13.4.d All in all, figure 13.4.d displays a quite obvious disagreement between observations and models through the whole period. Why does this disagreement occur? [Eduardo Zorita, Germany] | Taken into account. First, the sentence refers to tropospheric aerosol has been clarified. Second, the rate referred to is ocean warming and hence thermal expansion. Third, 13.4 has been rearranged to facilitate comparison of models and observations, as the reviewer advocates. Fourth, the spin-up problem decreases with time so is more serious earlier in the 20th century. Fifth, the discussion of the totals has been expanded. |
| 13-570 | 13 | 22 | 39 | 22 | 39 | Why was "negative aerosol radiative forcing" not included in the model simulations? This should be clarified. [John Hunter, Australia] | Accepted. What is not included is an increase in aerosol forcing in the 2000s. |
| 13-571 | 13 | 22 | 39 | 23 | 2 | That "rebound" effect is counter-intuitive! Following volcanic eruptions, there should be cooling. In any event, this paragraph is yet another example of the report failing to note that thermal expansion cannot cause coastal sea level rise. [David Burton, USA] | Taken into account by clarifying. The "rebound" means recovery from cooling, which does indeed take place first. We reject the second comment; Ocean thermal expansion does affect coastal sea levels through a dynamic response of the ocean. |
| 13-572 | 13 | 22 | | | | Section 13.4.7 Modelled budget - Lines 24-31: This text needs a bit more refinement and to be in line with what has been mentioned in the ocean heat content section in Chapter 10. [Catia Motta Domingues, Australia] | Accepted. This will be improved. |
| 13-573 | 13 | 22 | | | | Table 13.1: there is a number missing in bottom line, 4th column. [Roland Gehrels, United Kingdom] | Accepted and corrected. |
| 13-574 | 13 | 22 | | | | Table 13.1: How is it that the error bar on the trend for thermosteric sea level from 1971-2010 is less than for 1993-2010 or 2005-2010 when we actually have measurements? These may be the formal errors but they don't represent the true uncertainty. Surely something should be said about that? A similar comment must be made about other error bars, especially those for glaciers. [Simon Holgate, UK] | Noted. The length of the period, the uncertainty of the measurments and their distribution are all important for determining the uncertainty on trends. Especially for 2005-2010, trends can be more uncertain becuse of the shortness of the period, especially if there are |

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| | | | | | | | small systematic errors. Nevertheless, the uncertainties need to be checked. |
| 13-575 | 13 | 22 | | | | Table 13.1: Are the figures for uncertainty (standard error) in the Therospheric component correct? They imply a greater uncertainty per year over the period 1993-2010 than over the period 1971-2010 despite fewer observations prior to 1993. In addition the Greenland SMB appears to be negative (i.e. net contribution to sea level rise) prior to 1990. I dont know any model that suggests SMB negative over this period, or indeed, that SMB is currently negative. [Jeff Ridley, UK] | Noted. The length of the period, the uncertainty of the measurments and their distribution are all important for determining the uncertainty on trends. Especially for 2005-2010, trends can be more uncertain becuse of the shortness of the period, especially if there are small sytematic errors. Nevertheless, the uncertainties need to be checked. Well spotted on Greenland - the implications are that the contribution is very small prior to 1993, but the uncertainties would allow for a positive or negative contribution. I think there are suggestions the the Greenland contribution is indeed small prior to 1993. We will investigate this further. |
| 13-576 | 13 | 22 | | | | Table 13.1. This table is confusing to me, as there seem to be two blocks of rows totalling the observations (top and bottom blocks), and the numbers are not the same? This may just be me failing to understand the presentation, but the caption does not help, and I fear others may struggle with me. Why not merge the two 'observed' blocks of rows, instead of keeping them separated? Or, alternatively, improve the labelling so that it clearly specifies what the two 'observed' blocks exactly refer to – and also update the caption to clarify things. In addition, there is a loose ± 0.5 in the 4th column, bottom row. Is there a central value missing? Is it 0.0 perhaps (if so, then don't omit it, but specify it). Overall, I feel that such a potentially important summary table must be immediately clear to everybody. It isn't, and it must be improved to become clear and unambiguous, in my opinion. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. The table has been rearranged to make it clearer, and the missing number inserted. |
| 13-577 | 13 | 22 | | | | Table 13.1: I think the authors should carefully think about what they want to convey with this table. Frequently the values listed here seem to be numbers from particular studies, and not numbers that represent ranges from the literature, which I think would be a better choice. A couple of examples to illustrate this point. Only the Cogley values are used for the glaciers, with error bars suggesting that this is by far the best constrained contributor to sea level rise. Second, the thermosteric contribution from 2005-2010 is the von Schuckmann and Le Traon value, which seem to be at the upper end of the range listed in Chapter 3, page 27, line 39. While this approach may help to close the budget, I do not believe it adequately reflect our understanding. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Taken into account. We will reevaluate how the numbers are reported. |
| 13-578 | 13 | 22 | | | | Table 13.1: An observed mass change line would be nice. The observed total for 1993-2010 seems to be missing a value. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Corrected. |
| 13-579 | 13 | 23 | 1 | 23 | 1 | I think "modelled" should be inserted between "the" and "rate of". [John Hunter, Australia] | Accepted. |
| 13-580 | 13 | 23 | 1 | 23 | 58 | This entire section does not belong to Chapter 13 but to Chapter 4, and some of the statements made reflect a poor knowledge of the recent literature. Same goes on on page 24 which is a poor summary of the state of knowledge. [Eric Rignot, USA] | Rejected. We presume the reviewer is referring to the parts about land ice rather than the part about thermal expansion, which is obviously not a subject of Chapter 4. Chapter 4 is about observations of the cryosphere. This discussion is about models, which are not covered by chapter 4, and are discussed here in order to evaluate their fitness for making projections. |
| 13-581 | 13 | 23 | 7 | 23 | 10 | I don't understand this sentence - is the "artifact of experimental design" the same as the effect caused by spinning up a model without volcanic forcing - or is it something different? [John Hunter, Australia] | Taken into account by clarifying, but the reviewer has understood it correctly anyway. |
| 13-582 | 13 | 23 | 12 | 23 | 13 | I don't understand why, or how, "this effect is not included in the results in Table 13.1" - in fact I don't think I understand the paragraph from line 4 to 13 - it requires some rewriting. [John Hunter, Australia] | Accepted. |
| 13-583 | 13 | 23 | 29 | 23 | 29 | A comma is needed between "(1998)" and "van de Wal". [John Hunter, Australia] | Editorial. This text has been removed in the second draft. |

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| 13-584 | 13 | 23 | 33 | | 34 | The statement "This is probably an underestimate..." is likely incorrect. Lack of observed acceleration in SLR in response to the war 1990s and 2000s, compared to the cool 1970s, suggests the opposite. [David Burton, USA] | Rejected. The reviewer apparently thinks this sentence is about the rate of sea level rise, whereas actually it is about the sensitivity of glacier mass balance to global warming. This text has been removed in the second draft. |
| 13-585 | 13 | 23 | 34 | 23 | 34 | Leclercq should be Leclercq [Philippe Huybrechts, Belgium] | Accepted. |
| 13-586 | 13 | 23 | 40 | 23 | 44 | Using a model analogous to Rahmstorf's (2007) model, to estimate glacier contribution, is interesting, but could be undermined if elsewhere the Rahmstorf method were to be 'attacked' within the IPCC report. Section 13.6 is rather sceptical about the semi-empirical approach of Rahmstorf. This would reflect on the statement I address here on p. 13-23. Is this a sufficiently consistent attitude/approach? [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Noted. This text has been removed in the second draft, because this approach is not used. |
| 13-587 | 13 | 23 | 42 | 23 | 42 | What does "by construction" mean? [Ian Allison, Australia] | Noted. This text has been removed in the second draft. |
| 13-588 | 13 | 23 | 42 | 23 | 43 | I think "for recent decades" could be omitted - the glacier model reproduces the observations quite well over all periods. [John Hunter, Australia] | Noted. This text has been removed in the second draft. |
| 13-589 | 13 | 23 | 43 | 23 | 44 | The sentence "Observed glacier mass influence on climate" requires some brief explanation, or at least discussion - why did this mass loss occur? [John Hunter, Australia] | Accepted; comment added. |
| 13-590 | 13 | 23 | 43 | 23 | 44 | Please, add a reference to support the sentence that glacier mass loss had stated before 'substantial anthropogenic climate influence' (Orlemanns, xxxx?). Also, I would suggest to replace 'anthropogenic influence' by 'anthropogenic greenhouse gas forcing', since anthropogenic climate influence may comprise land-use changes and others that were already substantial, at least regionally, in the 19th century [Eduardo Zorita, Germany] | Accepted. |
| 13-591 | 13 | 23 | 43 | | 44 | The final sentence of this paragraph is true, and the most important thing for the reader to know about glacial retreat. It needs to be said much more prominently, along with the note that we're unsure whether that rate has increased. [David Burton, USA] | Rejected. Our chapter is reporting observations, but is not involved with attribution. |
| 13-592 | 13 | 23 | 51 | 23 | 51 | There seems to be some "double-dipping" in the terms "sublimation" used here and "ablation" used on line 55. I understand sublimation to be direct conversion of ice or snow to water vapour. I also understand ablation to be the sum of sublimation, melting and evaporation of meltwater. If accumulation is precipitation minus sublimation and SMB is accumulation minus ablation, then according to my definitions, sublimation has been subtracted twice in order to derive SMB. So, either: (a) accumulation must be defined as precipitation minus sublimation and SMB must be defined as accumulation minus melting and evaporation of meltwater or (b) accumulation must be defined as precipitation only and SMB must be defined as accumulation minus ablation. In any event, the terms "sublimation" and "ablation" need defining. [John Hunter, Australia] | Taken into account. In fact ablation does not include sublimation here. |
| 13-593 | 13 | 23 | 51 | 23 | 52 | What is the period for the "average and standard deviation of model-based accumulation" of "589 +/- 77 Gt/yr"? [John Hunter, Australia] | Accepted that it should be stated (1961-1990). |
| 13-594 | 13 | 23 | 52 | | | How does that number relate to Table 13.2? [Uwe Stoeber, Germany] | Taken into account by clarifying (it's additional information, not in the table). |
| 13-595 | 13 | 23 | 55 | 23 | 55 | See comment to Chapter 13, page 23, line 51, re. the word "ablation". [John Hunter, Australia] | Taken into account. In fact ablation does not include sublimation here. |
| 13-596 | 13 | 23 | 55 | 23 | 56 | I don't understand the statement "All models indicate that Greenland ice sheet SMB started decreasing in the early 1990s" - this is not shown by the model results of Figure 13.4 (c), which indicate a steady rise in the sea-level contribution or a steady, negative, SMB. Nothing significant seems to happen to the "Greenland SMB" line in Figure 13.4 (c) in the early 1990s. [John Hunter, Australia] | Accepted and clarified in the discussion. This discrepancy arises from the difference between observed and modelled regional climate trends. |
| 13-597 | 13 | 23 | 56 | | | That "3%" figure could easily confuse the reader! You need to also state the annual change as a percentage of the total Greenland ice sheet mass, for clarity. [David Burton, USA] | Rejected. The mass of the ice sheet is not relevant, only the rate at which mass is transferred to the |

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| | | | | | | | ocean. |
| 13-598 | 13 | 23 | 57 | 24 | 8 | I had to read this portion a number of times before it made sense - I then realised that Figure 13.5 relates to regional climate models, while the last two sentences relate to global models. Perhaps I'm a bit slow, but it would probably be good to put a bit more emphasis on the fact that this is all about comparing regional and global models. [John Hunter, Australia] | Accepted and clarified. This relates to 13-596. |
| 13-599 | 13 | 24 | 2 | 24 | 8 | This NAO explanation seems like a reasonable hypothesis, but the final sentence of the paragraph makes it sound as if this IS the explanation. [Aslak Grinsted, Denmark] | Accepted. The explanation being suggested is variability, but not necessarily NAO. |
| 13-600 | 13 | 24 | 14 | 24 | 14 | See comment to Chapter 13, page 23, line 51, re. the word "sublimation". [John Hunter, Australia] | Taken into account. In fact ablation does not include sublimation here. |
| 13-601 | 13 | 24 | 19 | 24 | 19 | "...sparse accumulation measurements..." [Ian Allison, Australia] | Editorial. |
| 13-602 | 13 | 24 | 20 | 24 | 22 | Magand, O., C. Genthon, M. Fily, G. Krinner, G. Picard, M. Frezzotti, and A. A. Ekaykin (2007), An up-to-date quality-controlled surface mass balance data set for the 90°–180°E Antarctica sector and 1950–2005 period, J. Geophys. Res., 112, D12106, doi:10.1029/2006JD007691. [Jeff Ridley, UK] | Noted, but that is not the complete ice sheet. |
| 13-603 | 13 | 24 | 38 | 24 | 40 | Graversen is a 10 km resolution zeroth order SIA model so how can it "reproduce the rapid changes in ice sheet outflow observed in recent decades". This is a misleading statement. This model broadly and crudely approximates the change in flux but in no way does it reproduce observations of changing dynamics of glaciers (e.g. Helheim, Kanger...). It then goes on to say "however dynamic change is not well constrained by obs." This seems somehow inconsistent with the previous sentences. The subsequent sentences were hard to follow and required several readings. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account by rewording. See also 13-605. |
| 13-604 | 13 | 24 | 38 | 24 | 48 | This backwards reference to AR4 seems unhealthy. It is not that the estimates may by coincidence come out to be the same, it is how our understanding has moved forwards that is relevant. [Jeff Ridley, UK] | Accepted. AR4 reference removed. |
| 13-605 | 13 | 24 | 38 | 25 | 2 | These two paragraphs are full of fast successions of acronyms and lots of 'techno-speak'. One has to be a specialist just to keep up with the fast successions of words like 'imbalance', 'change', 'decrease' and 'increase' that often lack unambiguous qualification of what they exactly refer to. True, it is possible to get there because the grammar overall is good, but it takes a lot of effort from the reader, and I would hate to be a non-specialist trying to make sense of the text here. A bit of careful editing and regard for the reader would not be out of place. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Taken into account. The discussion has been revised. |
| 13-606 | 13 | 24 | 50 | 24 | 52 | This sentence is in contradiction with page 22-36, stating that the thermosteric contribution from OAGCMs exceeds the observations in 1993-2010 [Eduardo Zorita, Germany] | Taken into account by enlarging the discussion. In fact the total matches better than the individual contributions. |
| 13-607 | 13 | 24 | | | | This discussion skirts the central issue: if the net mass balance is so far in error for the observed periods of record, can this approach do any better than the extrapolations of Rignot at predicting future net Antarctic mass balance? Much hinges on getting this right. [Philip Mote, USA] | Taken into account by enlarging the discussion. |
| 13-608 | 13 | 25 | 1 | 25 | 58 | Table 13.2 is unfit to Chapter 13, and it is not even included in Chapter 4 because of lack of space and because of lack of relevance. Listing all published estimates of SMB is not an assessment, and the list compares models with high skills (RACMO) with models that are tuned to fit limited observations and let loose elsewhere (i.e. these models have no error bounds). Computing an average estimate from that is not good science. [Eric Rignot, USA] | Rejected. Chapter 13 is concerned with evaluating models, which is not a concern of chapter 4. |
| 13-609 | 13 | 25 | 5 | 25 | 34 | Error estimates in Table 13.2 should also be explained. [Robert Thomas, USA] | Accepted. |
| 13-610 | 13 | 25 | 6 | 25 | 6 | There is no sea level equivalent shown in this table. [Ian Allison, Australia] | Accepted for the anomaly columns (the last three); the other numbers are not SL contributions. |
| 13-611 | 13 | 25 | 6 | 25 | 6 | "Negative SMB trend means positive sea level rise trend..." [Ian Allison, Australia] | Accepted in a modified form. |

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| 13-612 | 13 | 25 | 13 | | | Insert: "However, historical records of the 400+ year Viking Greenland agricultural settlement, during the Medieval Warm Period, when Greenland was warm enough to grow lumber (which it no longer is), strongly suggest that no such "tipping point" is likely to be encountered for at least several hundred years." [David Burton, USA] | Rejected. There is no discussion of tipping points here. This comment may have been misplaced. |
| 13-613 | 13 | 25 | 16 | 27 | 42 | This box seems completely out of place. [Aslak Grinsted, Denmark] | Rejected - it complements the sea level budget and would be out of place elsewhere as well. We have added a sentence making the connection to sea level clearer and the box will be referred to in other chapters. |
| 13-614 | 13 | 25 | 16 | 27 | 42 | I have made numerous comments on Box 13.1. This is because I think it is a very important box. I enjoyed reading it and learned a lot from it. It just needs a bit of cleaning. [John Hunter, Australia] | Thank you |
| 13-615 | 13 | 25 | 18 | 27 | 42 | The Global Energy budget is based on the belief that the earth can be considered to be flat, that the sun shines all day and all night, that the energy entering equals the energy leaving and that all the figures are constant. All of these assumptions are ridiculous. The earth is round and there is a fundamental difference between night and day. By day the earth warms and is cooled by convection and evaporation of water. By night the earth cools and is warmed to some extent by restoration of heat previously gained by the earth and atmosphere. No part of the earth is ever in equilibrium and there is overwhelming evidence from geology that energy entering is always different from energy leaving. The earth's energy is decided by convection and turbulence in the atmosphere, by air pressure, by cyclones and anticyclones, by ocean currents and oscillations, and only to a minor extent by radiation exchanges apart from that supplied by the sun. This is traditional meteorology and it is far more successful in predicting future climate, despite limitations due to lack of knowledge of fluid movements, than any "model" based exclusively on radiation exchanges. [VINCENT GRAY, NEW ZEALAND] | Rejected - the assessment has not made this assumptions. |
| 13-616 | 13 | 25 | 18 | | | Box 13.1: Why is this in the sea level chapter? It seems like it belongs earlier. [Robert Kopp, USA] | Rejected - it complements the sea level budget and would be out of place elsewhere as well. We have added a sentence making the connection to sea level clearer and the box will be referred to in other chapters. |
| 13-617 | 13 | 25 | 28 | 25 | 29 | Yes the SL budget and the energy budget has to be consistent, but they give very little constraint on each other. H55. So even if you only have a small uncertainty in the ratio of melt to steric in the sea level budget, then it leads to a huge slack in the energy budget. [Aslak Grinsted, Denmark] | Rejected - The two budgets need to be consistent and Church et al. found some useful constraints. Indeed, for the example quoted, if more sea-level rise came from melting ice sheets, that would have very large implications for the energy budget. The energy budget also puts constraints on how much thermal expansion is possible. |
| 13-618 | 13 | 25 | | | | Box 13.1 The global energy budget I appreciate the discussion of the energy budget and links/need for consistency with the sea level budget. And, despite knowing that the sea level chapter is an integration chapter, my first reaction was to question what is the likelihood of any reader to intuitively look for a discussion of the energy budget on the sea level chapter? In addition, I also thought: why should there be only a discussion of the energy budget and not also of the global hydrological (freshwater) budget or any other budget that might be linked and should be consistent with the sea level budget? (and whether IPCC should organise the discussion of the various budgets in a dedicated location). Following the major concern above, these are minor concerns: - What exactly is meant by consistency between the budgets? It might be important for general readers to understand. - In general, the energy budget has always been discussed in terms of W/m ² and not J (as in this chapter and the ocean chapter). This makes harder to place these estimates in the context of other estimates. Would it be valuable to transform the units into W/m ² ? - I find that the energy budget section is missing the same level of content/discussion as done for the | Rejected - the broader issues are outside our scope. We have added a sentence making the connection to sea level clearer and the box will be referred to in other chapters. The uncertainties are given in the chapters that have provided the various time series. |

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| | | | | | | contribution terms of the sea level budget. The forcings in panel (a) are missing error bars. Why aren't modelled contributions discussed for the energy budget? How relevant is the energy budget material in this chapter for the TOA budget discussed in Chapter 2 and for climate sensitivity discussion in other chapters. [Catia Motta Domingues, Australia] | |
| 13-619 | 13 | 25 | | | | Hanna et al. (submitted) in Table 13.2 has been published as: Hanna, E., and Coauthors, 2011: Greenland Ice Sheet surface mass balance 1870 to 2100 based on Twentieth century reanalysis, and links with global climate forcing. Journal of Geophysical Research, 116, D24121, doi:10.1029/2011JD016387. [Philippe Huybrechts, Belgium] | Comment is out of order (relates to 13.4.7). But noted and publications will be updated. |
| 13-620 | 13 | 25 | | | | Box 13.1: I do not understand why the box on the global energy budget is in this Chapter. I well see the point with the following Ch. 13.5.1 (Ocean heat uptake), but would clearly expect the explanation and details of the energy budget in another Chapter. [Frank PAUL, Switzerland] | Rejected - it complements the sea level budget and would be out of place elsewhere as well. We have added a sentence making the connection to sea level clearer and the box will be referred to in other chapters. |
| 13-621 | 13 | 26 | 4 | 26 | 20 | All the numbers in this paragraph do not agree with Box 13.1, Fig, 1a [Uwe Stoeber, Germany] | Accepted - the numbers will be updated. |
| 13-622 | 13 | 26 | 5 | 26 | 5 | "changes in" should be inserted between "input from" and "solar forcing". [John Hunter, Australia] | Accepted |
| 13-623 | 13 | 26 | 18 | 26 | 18 | Radiative forcing chapter is chapter 8, not chapter 7. [Teruyuki Nakajima, Japan] | Accepted |
| 13-624 | 13 | 26 | 41 | 26 | 41 | I suggest "but the absolute calibration" - and perhaps there could be more emphasis on how large the "adjustments" to this data set are - ~6 W m ⁻² ! It may also be worth mentioning that this calibration uses an existing estimate of the Earth's heat budget. This, in turn, can lead the unwary to prejudge new estimates of the heat budget. [Neil White, Australia] | Accepted - inserted "absolute". Rejected - not made more general comments given space limitations and the need to focus on the most pertinent issues. |
| 13-625 | 13 | 26 | 44 | 26 | 44 | "(Murphy et al., 2009)" should read "Murphy et al. (2009)" [John Hunter, Australia] | accepted - corrected. |
| 13-626 | 13 | 26 | 46 | 26 | 47 | "the climate feedback parameter lambda, which is related to the equilibrium climate sensitivity (see Box 12.1)" is not very helpful. The phrase "climate feedback parameter" does not appear in Box 12.1, and only appears once in the whole of Chapter 12 (and then only in a Figure caption). Given that the nomenclature surrounding climate feedback parameter and climate sensitivity is somewhat confusing in the literature (climate sensitivity can have two different meanings (including the reciprocal of itself), and for example Gregory and Forster (2008) equate "climate feedback parameter" and "climate sensitivity parameter"), it would greatly help if these terms (including the distinction between equilibrium and transient sensitivities) were clearly defined in a few sentences here (although I do realise that it may well be opening a can of worms to make the entire WGI report consistent in this respect), [John Hunter, Australia] | Accepted - will make consistent with Chapter 12 |
| 13-627 | 13 | 26 | 47 | 26 | 49 | I couldn't find where these values of climate feedback parameter come from - presumably from another Chapter of the WGI reports - it would help if there was a reference to the source. [John Hunter, Australia] | Accepted - cross reference added. |
| 13-628 | 13 | 26 | 50 | 26 | 50 | Given that I'm not sure where the values of climate feedback parameter came from, it would be good to check that "The uncertainties quoted are one standard deviation" is correct. [John Hunter, Australia] | Taken into account - To be checked |
| 13-629 | 13 | 26 | 50 | | | One standard deviation? What kind of a CI is that? Two SDs is customary. [David Burton, USA] | Rejected - the text is clear |
| 13-630 | 13 | 26 | 55 | | | change "...but there is also warming in the deep..." to "...but there is also presumed to be warming in the deep..." (since this presumption is unconfirmed by reliable measurements) [David Burton, USA] | Rejected - Now refer to Box 3.1 |
| 13-631 | 13 | 26 | 56 | 26 | 56 | Box 3.1 says "219 x 10 ²¹ J" rather than "225 x 10 ²¹ J". Also, the reader should be referred to Box 13.1, Figure 1b, which also shows this (the light and dark blue regions at 2010). [John Hunter, Australia] | Accepted - corrected |
| 13-632 | 13 | 26 | 56 | | | The number in Box 3.1 is 219+50 x 10 ²¹ J until 2009. [Uwe Stoeber, Germany] | Accepted - corrected |
| 13-633 | 13 | 26 | 58 | 27 | 1 | 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 unaffected by thermal expansion of the ocean. [David Burton, USA] | Rejected - this is a misunderstanding of the science as thermal expansion of the oceans is also felt at the coast through dynamical ocean adjustment. |

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| 13-634 | 13 | 27 | 2 | | | Insert between the sentences: "Indeed, it has probably been warming since the end of the Little Ice Age (in the mid 19th century)." [David Burton, USA] | Rejected - outside of the scope considered here |
| 13-635 | 13 | 27 | 6 | 27 | 6 | I think "magnitude of the" should be inserted between "The" and "residual". [John Hunter, Australia] | Accepted |
| 13-636 | 13 | 27 | 7 | 27 | 8 | "A positive residual...": The residual is negative. [Uwe Stoeber, Germany] | Accepted - reworded |
| 13-637 | 13 | 27 | 7 | | | J is a unit for energy, not for energy flux. [Uwe Stoeber, Germany] | Accepted - reworded |
| 13-638 | 13 | 27 | 8 | 27 | 8 | Replacing "means" with "would mean" would make this clearer (since the residual you are talking about is not positive). [John Hunter, Australia] | accepted |
| 13-639 | 13 | 27 | 10 | 27 | 10 | The phrase "For the central value of lambda" should be moved from line 11 to line 10 (to the start of the sentence which currently begins "The residual increases in"). It might also be worth moving some, or all, of "in Figure 1b (red solid line)" from line 12 into this sentence. [John Hunter, Australia] | accepted |
| 13-640 | 13 | 27 | 19 | 27 | 19 | It is stated that "the residual is smaller than the uncertainties" - it would be good to know what the uncertainties actually are (I can't find them - if they are there, they should be more clearly visible). [John Hunter, Australia] | Accepted - the uncertainties are given in the various chapters providing the time series. |
| 13-641 | 13 | 27 | 23 | 29 | | Please also refer to the recent work of Palmer et al. (2011). The authors show simulations from three coupled models where internal climate variability can produce decadal changes in the top-of-atmosphere radiation balance of > 0.1 Wm ⁻² (see their Figure 1). They also highlight the role of oceanic heat re-distribution during decades when SST is negative but the TOA is positive. In addition, they show that the ocean is capable of re-distributing large quantities of heat on decadal timescales as deep as 4000m. This has implications for monitoring of the thermal expansion contribution to sea level rise on decadal time scales, since Argo observations only extend to about 2000m. Palmer, M. D., D. J. McNeall, and N. J. Dunstone (2011), Importance of the deep ocean for estimating decadal changes in Earth's radiation balance, Geophys. Res. Lett., 38, L13707, doi:10.1029/2011GL047835. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland] | Accepted - reference added |
| 13-642 | 13 | 27 | 28 | 27 | 29 | I pondered the sentence "While these natural energy budget" for a while. My first reaction was that, to "stall" global warming for a decade, you need a decade-long internal flux of the same order as the forcing. However, we are only talking about surface air temperature, which only relates to a small proportion of the thermal capacity of the climate system (represented by a part of the light blue region in Box 13.1, Figure 1b). I think that this is a very significant (though obvious with a little thought) point, and one which should be clarified and emphasised - that only a small amount of heat (relative to the total anthropogenic forcing) is required to temporally slow or stop the rise in surface air temperature. [John Hunter, Australia] | Accepted - The first part of the comment is not quite correct but the emphasis requested has been added |
| 13-643 | 13 | 27 | 29 | 27 | 29 | I don't think "variations" is the right word here (even if the forcing were constant, the statement would still be true). I would replace "forced variations" with "forcings" or possibly "anthropogenic forcings". [John Hunter, Australia] | accepted - reworded |
| 13-644 | 13 | 27 | 38 | 27 | 40 | The "2 x 10 ²¹ J" and "1 x 10 ²¹ J" are not fluxes - they are quantities of heat. I assume that they relate to fluxes over a specific period - presumably 1970-2010. This should be clarified. [John Hunter, Australia] | Accepted - reworded |
| 13-645 | 13 | 27 | 45 | 42 | 8 | Sheer speculations which are out of touch with what is currently observed. Plus simulations which do not prove cause and effect and have never successfully predicted any future observations [VINCENT GRAY, NEW ZEALAND] | Rejected. These projections are based on the most complete scientific understanding available, as embodied in the models used, and evaluated where possible against observed past changes. They are not speculations. |
| 13-646 | 13 | 27 | 47 | | | This section is good in that it provides a scientific context for the assessment a context that is missing for other sections and reveals an inconsistency in approach for the authors of the different sections [Jeff Ridley, UK] | Noted. |
| 13-647 | 13 | 27 | 49 | 27 | 50 | Please note that in Box 3.1 this statement is given for the time period 1970-2009. [Uwe Stoeber, Germany] | Noted. 1970-1999 is a multiannual period, to which Box 3.1 applies. |

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| 13-648 | 13 | 27 | 57 | | | Insert between the sentences: "However, it will affect only satellite-measured sea levels over the open ocean, not coastal sea levels." [David Burton, USA] | Rejected. This comment is scientifically incorrect. Sea level change is dynamically determined, and not equal to in-situ expansion, as the reviewer believes. |
| 13-649 | 13 | 28 | 1 | 28 | 2 | Some commas would help here (e.g. between "scenario" and "based" and between "results" and "for"). [John Hunter, Australia] | Accepted. |
| 13-650 | 13 | 28 | 1 | | | Change 0.09 to 0.16 m SLE (Radic; Hock 2010) to 0.07 to 0.18 since this is the range from 10 GCMs used in this study. [Valentina Radic, Canada] | Accepted. |
| 13-651 | 13 | 28 | 12 | 28 | 16 | too technical for common readers. [Pavel Tkalich, Singapore] | Rejected. It is a technical point, but the formulae are not complicated, and it is important to appreciate. |
| 13-652 | 13 | 28 | 14 | 28 | 14 | It should be noted that "transient climate response = $F_{2x} / (\alpha + \kappa)$ " is only an approximation unless κ includes components of the climates system other than just the ocean (i.e. the land, atmosphere and ice) [John Hunter, Australia] | Accepted. |
| 13-653 | 13 | 28 | 14 | | | add closing parenthesis to equation like ($=F_{2x}/(\alpha+\kappa)$), [Ernst Schrama, Netherlands] | Accepted. |
| 13-654 | 13 | 28 | 16 | 28 | 16 | I think "," should be "and" in two instances. [John Hunter, Australia] | Accepted. It looks like a bizarre behaviour of Endnoteweb. |
| 13-655 | 13 | 28 | 16 | 28 | 19 | This sentence ("For a given forcing Raper et al., 2002") should be explained. I don't understand its reasoning. The ocean uptake is given by $F_{2x} / (1 + \alpha/\kappa)$ which seems to me to be just as sensitive to (proportional) uncertainties in α as to (proportional) uncertainties in κ . I obviously don't understand this so it needs to be clarified. [John Hunter, Australia] | Taken into account by adding more explanation. This is not an inference purely from the foregoing. |
| 13-656 | 13 | 28 | 22 | 28 | 22 | Should be "parameterised" [Simon Holgate, UK] | Editorial. |
| 13-657 | 13 | 28 | 25 | 28 | 25 | I don't understand the reference to "in the lower half of the AR4 SRES ranges". I assume this relates to the results of the AOGMs. However, for each SRES scenario, there will be one "AR4 global SAT projection" which I assume is compared with the AOGCM projection for the same scenario. It seems to me that "the lower half of the AR4 SRES ranges" should be replaced by something like "in the lower half of the range of AOGCM projections for each SRES scenario". [John Hunter, Australia] | Accepted. |
| 13-658 | 13 | 28 | 36 | 28 | 36 | I think "Figure 13.5" should read "Figure 13.6". [John Hunter, Australia] | Accepted. |
| 13-659 | 13 | 28 | 38 | 28 | 38 | After "2010", I would add something like "and the ocean heat uptake is still positive (because the radiative forcing, although stabilised, is still finite), and so can warm up the surface ocean as it is cooled from below". This is important, as if the ocean heat uptake is turned off (i.e. the radiative forcing is zero), thermal expansion or contraction can only occur by redistribution of the heat within the ocean (due to the non-constant thermal expansivity). [John Hunter, Australia] | Accepted. The text has been adjusted accordingly. |
| 13-660 | 13 | 28 | 40 | 28 | 41 | Put in some numbers (e-folding times) for the time scales. [Aslak Grinsted, Denmark] | Accepted. |
| 13-661 | 13 | 28 | 44 | 28 | 44 | I've never seen the term 'thermal expansivity' before, thermal expansion is OK, but expansivity I think is an original term. I suggest rephrasing this. [Howard J. Freeland, Canada] | Accepted. The text will be reformulated. |
| 13-662 | 13 | 28 | 44 | 28 | 46 | Sentence "Since the thermalRussell et al., 2000)" - this is a good point, not to be missed, as the thermal expansivity varies by a factor of around five in the ocean (it may be worth saying this too). [John Hunter, Australia] | Noted. |
| 13-663 | 13 | 28 | 44 | | | change "Vellinga; Wood" into "Vellinga and Wood" [Ernst Schrama, Netherlands] | Accepted. |
| 13-664 | 13 | 28 | 47 | | | Insert: "The observed difference between satellite and tide gauge measurements might be attributable, at least in part, to the fact that thermosteric changes affect the former, but not the later." [David Burton, USA] | Rejected. This aspect is discussed extensively in section 13.3. |
| 13-665 | 13 | 28 | 53 | 28 | 53 | if glaciers are considered separately here then they need introducing separately earlier - see my comment #87 [Mark Siddall, UK] | Noted. This will be considered in the revision. |

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| 13-666 | 13 | 28 | 53 | 32 | 10 | In this part of Chapter 13 we have more or less full estimation of informational capacity of the available glaciological measurements and inventories. Described there some efforts to model and calculate influence of glaciers to the World Ocean Level and especially long term projecting of this influence were objectively qualified as unsuccessful due to lack first of all the initial data at the global scale. It again justifies our critique presented above in the proper comments. [Vladimir Konovalov, Russian Federation] | Rejected. We did not conclude that our projections were unsuccessful. More importantly, these projections are being reassessed with the new complete inventory of global glaciers (Randolph inventory), which will be reported in the SOD. |
| 13-667 | 13 | 28 | 53 | 32 | 10 | Much of the text on pages 29-31 reaches an unnecessary level of detail, that surely the authors could summarize in a more palatable manner. My impression is that "glacier" contribution to SLR by 2100 could be a few tens of cm with very high uncertainty, the reasons for which could be summarized fairly briefly and preferably just once. The various projections detailed in the text could be listed in a Table (they may already be in Table 13.3??), with important explanations in an extended caption. [Robert Thomas, USA] | Accepted. This section is being completely rewritten and will be substantially shortened and simplified. Having said that, the ice sheet contribution is also likely to be a few tens of cm with very high uncertainty, as will the steric contribution (although with lower uncertainty), so one would assume that the terms would receive more or less equal attention in the chapter. |
| 13-668 | 13 | 28 | 53 | | | Section 13.5.2 Glaciers: I found this Section to be over-long, poorly structured, and with papers repeatedly discussed in different places. It isn't easy reading. It needs some reordering and tightening up. [John Hunter, Australia] | Accepted. This section is being completely rewritten and will be substantially shortened and simplified. |
| 13-669 | 13 | 28 | 53 | | | As mentioned above, for section 13.5.2 I will see the point to describe here the state of knowledge about the future contribution of glacier melt to sea-level rise in more detail, but I found the broad discussion of the four main studies way too long. I will not reduce their merit, but methods come and go and change with the available data sets. So their half-life is rather short and does in my opinion not require such a broad treatment. When 'reviewing' these studies individually, there is also overlap in some of the main points. For an assessment report I would prefer to have a comparison of the methods / input data used, clearly list the shortcomings of each approach and find explanations for the differences in the reported values. [Frank PAUL, Switzerland] | Accepted. This section is going to be shortened significantly and discussions of glaciological methods will be moved to Chapter 4 or eliminated. |
| 13-670 | 13 | 28 | 53 | | | Scientific context on glacier mass balance required before launching in to sea level contributions - precip minus ablation, tidewater glaciers, volume change vs terminus etc. [Jeff Ridley, UK] | Accepted. Various introductory concepts, definitions, etc will be sorted out and placed early in the chapter in the upcoming revision. |
| 13-671 | 13 | 28 | 55 | 29 | 1 | Slangen et al., 2011, doi: 10.1007/s00382-011-1057-6, present a 21st-century sea level contribution from glaciers ranging from 0.11 to 0.22 m. [Aimee Slangen, Netherlands] | Noted - this will be included in the list. |
| 13-672 | 13 | 29 | 1 | 29 | 58 | This section is incredibly long and has too many details. It reads like a literature review. Table 13.3 is not a great idea and again lists all studies - somehow - but does not make an assessment. Calculating an average or a range is not an assessment. [Eric Rignot, USA] | Accepted. This section is going to be shortened significantly and discussions of glaciological methods will be moved to Chapter 4 or eliminated. |
| 13-673 | 13 | 29 | 9 | 29 | 10 | This phrase is repeated too often in this chapter . [Jeff Ridley, UK] | Noted. Fortunately for everyone, a large part of the problem is now solved with the publication of the Randolph Glacier Inventory, so this phrase can be largely deleted. |
| 13-674 | 13 | 29 | 14 | | | I would not write that 'power-law scaling estimates ...based on measured areas' are 'evolving rapidly'. Indeed, they will be applied to the new data sets but they are a dead end. What currently rapidly evolves is the modelling of glacier thickness distribution from simplified approaches using the available topographic information from digital elevation models. I assume you are aware of these studies, otherwise I am happy to provide you the related references. [Frank PAUL, Switzerland] | Rejected. Power law scaling is not at a dead end. It has become a universally accepted and widely used tool for estimating volume-area relationships, not for individual glaciers, but for large populations of glaciers. Newer methods of modeling thickness distributions (e.g. Farinotti et al, 2009) are very useful additional tools that accompany and supplement power law scaling but by no means displace it. |
| 13-675 | 13 | 29 | 15 | | 18 | Tbl. 13.3 doesn't present new estimates of global glacier area and volume. It contents projected SLR at 2100, as mentioned at p.30 line 49-50. [Thomas Voigt, Germany] | Accepted. This is an error - the correct table reference is 4.3 |
| 13-676 | 13 | 29 | 18 | 29 | 18 | "This volume of 0.6 ± 0.7 m" - SLE? [Donald Forbes, Canada] | Accepted. Several typos conspired here; The correct |

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| | | | | | | | value is 0.6 ± 0.07 m SLE, and the reference is Table 4.3. |
| 13-677 | 13 | 29 | 18 | | | This volume of 0.6 ± 0.7 m ...' Do you mean 0.6 ± 0.07 m? Why volume? 0.6 m is not really a volume but a length. Please clarify. [Frank PAUL, Switzerland] | Accepted. Several typos conspired here; The correct value is 0.6 ± 0.07 m SLE, and the reference is Table 4.3. |
| 13-678 | 13 | 29 | 26 | 29 | 29 | Line 27: "rates" of what?? This para is confusing; presumably the projections on line 28/9 are total SLR by 2100. I assume this is for "glaciers", but then Line 26 mentions the Greenland and Antarctic ice sheets?? Quoted error estimates are totally unrealistic. [Robert Thomas, USA] | Taken into account. The rates are loss rates from glaciers and ice sheets. The numbers are from a generally well-known paper published in 2007 by Meier et al in Science. |
| 13-679 | 13 | 29 | 27 | 29 | 27 | "Trend" is not a good word here - "acceleration" would be better (even if Meier et al. used the word "trend"). [John Hunter, Australia] | Agreed. The wording will be changed. |
| 13-680 | 13 | 29 | 29 | 29 | 29 | Trend is not a good word here - "acceleration" would be better (even if Meier et al. used the word "trend"). [John Hunter, Australia] | Agreed. The wording will be changed. |
| 13-681 | 13 | 29 | 37 | 29 | 37 | "is highly variable" should be "are highly variable". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Noted. |
| 13-682 | 13 | 29 | 47 | 29 | 47 | I think "increase" should read "decrease" (otherwise the scaling factor will be negative?). [John Hunter, Australia] | Yes! Thinking in magnitude instead of sign. This will be corrected. |
| 13-683 | 13 | 29 | 51 | 29 | 51 | "Surface mass balance-to-calving ratio" isn't quite right. For one thing the hyphens should either be removed or extended. Also, this is really the ratio of the change in surface mass balance to the change in calving rate - but I'm not sure how you would refer to this in a concise way. [John Hunter, Australia] | Agreed. The wording here is awkward and bit too compressed for clarity. This will be fixed. |
| 13-684 | 13 | 29 | 53 | 29 | 54 | It would be a bit clearer if "0.47 m SLE" were moved so that it is between "However, the" and "calving contribution". Also "of" should be changed to "to" in this sentence. [John Hunter, Australia] | Agreed. |
| 13-685 | 13 | 30 | 2 | 30 | 16 | I could not understand this paragraph at all until I referred to Bahr et al. (2009) (which is, itself, very concise and clear) - a little further explanation is needed. [John Hunter, Australia] | Accepted. Bahr's original wording will be consulted. |
| 13-686 | 13 | 30 | 3 | 30 | 3 | The "accumulation area ratio" should be defined. [John Hunter, Australia] | Noted. |
| 13-687 | 13 | 30 | 18 | 10 | 25 | How to achieve this with GCM models ? [Michel Boko, Benin] | Comment location appears to be mislabeled. No action possible. |
| 13-688 | 13 | 30 | 22 | 30 | 22 | It may be worth inserting "surface" between "and a" and "mass-balance model". [John Hunter, Australia] | Noted. |
| 13-689 | 13 | 30 | 25 | 30 | 28 | Raper and Braithwaite 2006 is a Nature Letter so the description is very brief, a more complete description is to be found in: Raper, SCB and Braithwaite, RJ, 2009 Glacier volume response time and its links to climate and topography based on a conceptual model of glacier hypsometry. The Cryosphere, 3, 1-13. [Sarah Raper, United Kingdom of Great Britain & Northern Ireland] | Noted. The reference will be updated. |
| 13-690 | 13 | 30 | 27 | | | That the largest valley glaciers (that contain a substantial amount of the global ice volume in their flat and thus thick tongues) can retreat to higher, stable altitudes in warming conditions is a misconception. The point is, they most often cannot adjust by retreat to a new climate, as the slope of their beds is often very low and thus stays at low elevations. Instead, the mass balance-altitude feedback will remove all the ice volume contained in the tongues very rapidly (by downwasting). I suggest to add at least a short remark on these processes. [Frank PAUL, Switzerland] | Noted. This may be the case, but this is the reasoning on which Raper and Braithwaite (2006) was based. In the interests of shortening this section, the discussion will probably be cut off rather than extended. |
| 13-691 | 13 | 30 | 44 | 30 | 44 | "with" may be deleted. [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Noted. |
| 13-692 | 13 | 31 | 7 | 31 | 7 | Final word "and" in this line should be "an". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Noted. |

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| 13-693 | 13 | 31 | 15 | | | I suggest to write 'significantly extended' rather than 'significantly improved'. The basic elements of a detailed inventory had been there before. [Frank PAUL, Switzerland] | Agreed. |
| 13-694 | 13 | 31 | 17 | 31 | 19 | If true, this does not follow in a simple way from the assumption of a triangular area-elevation distribution, which is a projection of the glacier area onto a vertical coordinate. The adoption of the triangular shape is due to the idea that the greatest area will be around the elevation of the greatest mass flux, though glacier thickness is also clearly a factor here. This idea is upheld by the observations, which also in general show a triangular shape. However, the triangular shape does not restrict the ELA to be at the altitude of greatest area, indeed the ELA is highly variable. In the hypothetical case of a glacier at equilibrium, the ELA would be at the elevation of maximum area specifically only in the case of a linear mass balance profile with altitude and a symmetric triangular area altitude distribution: Raper and Braithwaite 2006, Glacier volume response time and its links to climate and topography based on a conceptual model of glacier hypsometry. The Cryosphere, 3, 1-13. [Sarah Raper, United Kingdom of Great Britain & Northern Ireland] | Noted. This level of discussion may be eliminated in revision of this section. |
| 13-695 | 13 | 31 | 17 | 31 | 23 | Slangen and Van de Wal 2011, doi:10.5194/tc-5-673-2011, provides a sensitivity study of the uncertainties associated with global glacier modelling and find that the scaling factor in the power law is not the determining uncertainty, but that the uncertainties are dominated by mass balance sensitivity and choice of glacier inventory. Also the choice of climate model and climate scenario plays a large role. [Aimee Slangen, Netherlands] | Noted, and good to know. This section is being substantially revised, and details such as what model factors control uncertainty will be more prominent in the discussion. |
| 13-696 | 13 | 31 | 19 | 31 | 20 | It is not only the poorly constrained multiplier that is critical. Further problematic issues are that (1) glacier area is highly ill-defined (e.g. for glaciers with many tributaries, icefields and ice caps with a complex shape), (2) glacier area is a bad predictor for glacier thickness (as the latter mainly depends on slope), (3) the multiplier changes from glacier to glacier and thus works only for large samples of glaciers, (4) a likely upper limit of the thickness for very large glaciers is not taken into account and the volume of large glaciers is thus overestimated, and (5) the large uncertainties of this method are seldom reported. My recommendation is to add at least some of these points when reflecting about critical issues in current assessments. [Frank PAUL, Switzerland] | Rejected. These are all issues that have been discussed extensively in the literature. Glacier area may not be perfectly well-defined, but it is - fortunately for us - well enough defined to be used as a variable. Glacier area actually is an amazingly good predictor of thickness, as the well-documented statistics of glacier area-thickness observations attests. It is true that area-thickness scaling works only for large samples of glaciers; this is one of the fundamental tenets of volume-area scaling, and one that does not present much of an obstacle when the goals are global assessments. The uncertainties in this method have to be understood and expressed, and there are, unfortunately, a number of authors who have picked up power law scaling improperly and treated it simplistically. This is a risk for any complex method, however, and this assessment does not seem to be the place to offer remedial lessons. |
| 13-697 | 13 | 31 | 19 | 31 | 21 | This is as much a question of the model being constrained by the data. Even in this simple representation, where a single value for the area-volume scaling index for all regions is adopted, there is a lack of data to define the index with confidence for larger glaciers, exactly those glaciers that contain most of the ice. This lack of data also leads to the uncertainty in the potential for sea level rise from glaciers. [Sarah Raper, United Kingdom of Great Britain & Northern Ireland] | Noted. This level of discussion may be eliminated in revision of this section. |
| 13-698 | 13 | 31 | 33 | 31 | 34 | This statement is true in general, but I would not restrict this to 'the time scale of their disappearance'. It is the case for all glaciers where the time scale of dynamic adjustment (i.e. decreasing the size of their ablation area) is longer than the forcing. Due to the mass balance altitude effect, runoff can increase even for stabilized temperatures when the area does not shrink too much (as the same surface area comes progressively to lower elevations with more melt). [Frank PAUL, Switzerland] | Agreed. |
| 13-699 | 13 | 31 | 40 | 31 | 47 | The period of reference should be indicated. Is-it possible to add an mean tendency as a synthesis ? [Michel Boko, Benin] | This figure is being redesigned, and the reference period will be indicated, and mean tendencies will be calculated for different forcing scenarios. |

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| 13-700 | 13 | 31 | 49 | | | I suggest to add in this section also uncertainties due to ice below sea level, and cold/polythermal ice that does not contribute much to sea level before it is temperate (as much of the meltwater refreezes). [Frank PAUL, Switzerland] | Agreed. This should be added. |
| 13-701 | 13 | 31 | 53 | 31 | 55 | Does this number (7.65Gt per year) include ice below sea level? In this case the Gt cannot be directly converted to sea level contribution as the ice below sea level is just replaced by the water. [Frank PAUL, Switzerland] | It does not, but the correction for ice grounded below sea level was made, and it is small. (Most of the 7.65 GT/yr comes from ice advected from upstream ice ground above sea level.) During the course of the 1996-2007 interval, the sea level equivalent of ice transferred from the glacier to the ocean had to be corrected by about 5%. This correction will be made and noted in the revision. |
| 13-702 | 13 | 31 | | | | Figure 13.7. For the studies using several GCMs and scenarios it would be good to give the whole range (as upper and lower curve) instead of just the mean value as it is now. [Valentina Radic, Canada] | Noted. This figure is being redrawn, and the scenarios will be split out. |
| 13-703 | 13 | 32 | 6 | 32 | 10 | It is a separate issue whether some of the water is captured by other landstorage. It should be dealt with in the same section where extra land storage from precip is dealt with. So it should be in the projections of terrestrial storage, but left out of the glacier section in my opinion. (to avoid double accounting) [Aslak Grinsted, Denmark] | Noted, but some indication of the presence of this problem is warranted here, even if a solution is not identified. We will work with other section of the chapter to eliminate double counting. |
| 13-704 | 13 | 32 | 6 | 32 | 10 | It is evident axiom that all continental glaciers, ice caps and ice sheets should be hydrologically differentiated as: (1) marine-terminated, (2) not marine-terminated but having connection to the World Ocean through the river flow, and (3) not marine-terminated but located in closed river basins (not drained to the World Ocean). In calculations of glaciers surficial runoff as input to the World Ocean have to be applied the next approximate values of coefficient of runoff: (1) 0.9-1.0; (2) 0.3-0.6, depending on the distance between river mouth and location of glaciers; (3) 0.0 [In this group special calculation should be done to estimate outflow of moisture from melted surface of the ice and old firm. See Comment 16]. Since the coefficient of runoff is not included in the calculations of past, current, and future contribution of glaciers to the World Ocean, which presented in the Ch 4 and Ch 13, such results have to be revised only on this reason. But there are many (listed in the Chapters 4, 13, Comments, and illustrated in Supplements) other reasons to revise unreliable or erroneous results based on uncertainties in the initial data for glaciers, which were extrapolated to the regional and global levels. [Vladimir Konovalov, Russian Federation] | Takn into account. Some accounting of the degree of glacier connectivity to the ocean, for example by the means suggested here, would be a very valuable improvement to our present ability to project future glacier sea level contributions. However, the mapping required to make these distinctions is one of the many essential tasks that the small group of non-ice sheet glacier researchers has been unable to do. The task is simple, but the tasks are too many and the people and support too sparse. The completion of the Randolph Glacier Inventory will make this task easier to complete, but probably not in time to be if use in AR5. |
| 13-705 | 13 | 32 | 13 | 32 | 14 | I only checked the "Bahr et al. (2009)" values in this table and it is evident that, although they are undoubtedly based on Bahr et al., they do not come directly from the paper. Bahr et al. say very little about detailed time scales ("2100" isn't mentioned in their paper) and they certainly give no time series as implied in Figure 13.7. This is no real problem, but the extra assumption used to present these values (and the curves in Figure 13.7) should be stated. This probably applies to other entries in this Table, but I haven't checked the original papers. [John Hunter, Australia] | Accepted. The values given are the Bahr et al (2009) final steady state values scaled to their estimated partially-equilibrated values at 2100 using area-response time scaling, done as an update to the publication by D. Bahr. This needs to be noted in the Table. |
| 13-706 | 13 | 32 | 13 | | | Table 13.3 caption does not say what base year the projected SLR is relative to. "SLR at 2100" should be "SLR from xxx to 2100" (and xxx should be near the release date of the report). [David Burton, USA] | Noted. |
| 13-707 | 13 | 32 | 13 | | | Does the last estimate (AR5) include or exclude calving? [Regine Hock, US] | It excludes calving - this will be noted. |
| 13-708 | 13 | 32 | 13 | | | Table 13.3: As indicated aboe (comment 11) I suggest to provide the differences in the applied approaches (e.g. in considered glacier area), the core of the method, in a more comparable form (table) and reduce the details about each paper in the main text. [Frank PAUL, Switzerland] | Accepted. A revised table along these lines is planned. |
| 13-709 | 13 | 32 | 22 | | | "experiment"? Playing with computer models is not an experiment! It is a calculation! (Same problem at line 56 of 13-37.) [David Burton, USA] | Rejected. It is a calculation, indeed, but it is also an experiment, and this is usual terminology. |
| 13-710 | 13 | 32 | | | | Table 13.3. Radic & Hock 2011: If a range from 10 GCMs is given then it should be 0.07 to 0.18 m [Valentina Radic, Canada] | This table has been extensively revised. |
| 13-711 | 13 | 33 | 5 | | | Section 13.5.3.1 Geenland: I find the discussion of Greenland SMB very confusing. Firstly it contains several acronyms (AMOC, EBM, PDD) which are not defined the first time they are used (EBM and PDD are later | Taken into account by clarifying the text and acronyms. Apologies for boring the reviewer. |

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| | | | | | | defined in Table 13.4). More importantly, it is hard for the new reader to even grasp whether the Greenland SMB is presently positive or negative. The reader will already have seen Table 13.1 which shows a positive modelled sea-level contribution from Greenland SMB, implying a negative SMB. However, Page 13-35, lines 2-3 say that "there is a critical threshold in surface warming when the totals surface mass balance over Greenland becomes negative" implying that the present SMB is positive. A clue comes from Page 13-33, lines 7-8, which say that "all recent studies indicate a positive sea level contribution from Greenland SMB because the increase in ablation (mostly runoff) outweighs that in accumulation (mostly snowfall)". From this, I gather that Greenland SMB is presently positive but decreasing. The confusion arises because, if the Greenland SMB is presently positive and decreasing (while staying positive), it is incorrect to say that there is a "positive sea level contribution from Greenland SMB" - the correct statement is that there is a "positive sea level contribution from Greenland" (if previously the contribution was zero). It is very difficult to get this message quickly from the text and it still seems to be inconsistent with Table 13.1. Overall, I found Section 13.5.3.1 tedious to read, with no clear "take home messages". [John Hunter, Australia] | |
| 13-712 | 13 | 33 | 5 | | | Some expansion needed on the issues such as tidewater glaciers, narrow/steep ablation zone, meltwater refreeze [Jeff Ridley, UK] | Accepted the point about narrow ablation zone. Tidewater glaciers are dealt with in 13.2, and meltwater refreezing is discussed here. |
| 13-713 | 13 | 33 | 16 | 33 | 16 | and the other 40% is due to? [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Rejected. The next sentence supplies the answer. |
| 13-714 | 13 | 33 | 24 | | | Please introduce EBM. [Uwe Stoeber, Germany] | Accepted. |
| 13-715 | 13 | 33 | 41 | 33 | 41 | What is PDD? One for the glossary perhaps. I realise that it is defined in table 13.2 on page 13-25 but that takes some searching. [Simon Holgate, UK] | Accepted. |
| 13-716 | 13 | 33 | 41 | | | Please introduce PDD. [Uwe Stoeber, Germany] | Accepted. |
| 13-717 | 13 | 34 | 2 | 34 | 18 | Table 13-4 tells me that total SLR from Greenland surface mass balance by 2100 could be between 0 and 17 cm, or 9+/-9 cm? Table 13-6 has a range of 1-13 cm for Greenland SMB, and also shows glacier contributions of 9-19 cm compared to 4-37 cm in Table 13-3?? [Robert Thomas, USA] | Taken into account by adding more explanation in 13.6.1 about the connection between 13.5 and 13.6, that was confined to the Appendix. |
| 13-718 | 13 | 34 | 4 | 34 | 4 | Please double check yoshimori numbers. The ZOD said 3-17 cm. Which is it? (i dont have access to the paper at the moment) [Aslak Grinsted, Denmark] | Accepted. |
| 13-719 | 13 | 34 | 4 | 34 | 4 | Does ralf greve really have a negative rate? [Aslak Grinsted, Denmark] | Noted. Yes, he does. |
| 13-720 | 13 | 34 | 20 | 34 | 25 | The seasonality of both DT and DP changes are critical to how they affect SMB. Winter warming has relatively small effect (warming snowpack but not increasing melt directly) whereas summer warming has big impact on melt. Summer increase in DP means more rain and latent heat input. Need to discuss consistency of seasonal trends in climate here as it is so important. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. The text will be changed accordingly. |
| 13-721 | 13 | 34 | 27 | 34 | 27 | Any literature on the implications of Arctic Ocean sea-ice loss on GIS climate and SMB? [Donald Forbes, Canada] | Accepted. The literature needs to be added. |
| 13-722 | 13 | 34 | 36 | 34 | 37 | I have never seen any suggestion that the Atlantic thermohaline circulation could cease, I think what the authors are actually referring to is the AMOC or Atlantic Meridional Overturning Circulation, which is a different thing entirely. I also note that Chapter 3 refers to the various MOCs, including the AMOC, so please use the same term, and use the right one. [Howard J. Freeland, Canada] | Accepted. This is a matter of definition. The authors are aware of the dispute about the wording. For consistency we agree to use the term AMOC. The text will be corrected accordingly. |
| 13-723 | 13 | 34 | 37 | 34 | 37 | p. 13-11 line 13 uses AMOC. Here THC is used in reference to the Atlantic. For consistency, one form or the other should be chosen. [Simon Holgate, UK] | Accepted. This is a matter of definition. The authors are aware of the dispute about the wording. For consistency we agree to use the term AMOC. The text will be corrected accordingly. |
| 13-724 | 13 | 35 | 1 | 35 | 58 | It may be true that these models predict sea level decrease with warming, but the section does not mention that none of these models explain the lack of ice sheet growth at present; in other words, these models have no skill in terms of reconstructing SMB at present. How can we therefore place any level confidence in their | Partially accepted. While the models do have some predictive skill, this issue will be discussed in detail now. |

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| | | | | | | projection? [Eric Rignot, USA] | |
| 13-725 | 13 | 35 | 1 | | | "Model results suggest" is weasly cover for a poorly-supported assertion, and "like other climatic subsystems" is a dubious editorial comment. I'm a Systems Scientist, and one thing I've noticed is that pronounced tendency toward stability, rather than instability, in natural systems. [David Burton, USA] | Rejected. This is a frequently applied language and hopefully conveys the idea. |
| 13-726 | 13 | 35 | 1 | | | The referenced section "12.6.4.4" does not exist. Unclear what is referred to. [Olaf Eisen, Germany] | Accepted. This will be fixed. |
| 13-727 | 13 | 35 | 9 | 35 | 10 | For unexperienced readers, the statement that debris-covered glaciers experience more melt is in contradiction to the statement in ch4 that debris shields glaciers from melting by insulating. Needs to be clarified, the difference between debris-covered and dirty. [Olaf Eisen, Germany] | Accepted. This needs to be clarified. |
| 13-728 | 13 | 35 | 9 | 35 | 10 | debris-covered ice experience more melt' is likely not what you mean (as debris cover in general reduces melt). Maybe just write 'because darker surfaces experience more melt'. [Frank PAUL, Switzerland] | Accepted. This needs to be clarified. |
| 13-729 | 13 | 35 | 11 | 35 | 11 | Delete 'between'. I assume this threshold is a temp anomaly, thus add 'change' after 'temperature'. [Donald Forbes, Canada] | Accepted. |
| 13-730 | 13 | 35 | 12 | 35 | 12 | The threshold has to be assessed from a fully coupled ice-atmosphere models. The changing geometry of the ice sheet will be a major forcing of the atmospheric circulation. Solgaard and Langen (from centre for ice and climate in copenhagen) has submitted a paper on how it has a stabilising effect. (i do not know the status of this paper though). As the ice sheet shrinks the 'desert effect' is reduced (more of the moisture is able to make it to the interior as snow). [Aslak Grinsted, Denmark] | Noted. |
| 13-731 | 13 | 35 | 14 | 35 | 23 | I found this a very useful summary of the "Greenland" issue. [John Hunter, Australia] | Noted. |
| 13-732 | 13 | 35 | 18 | 35 | 23 | Does any of these simulations consider reinforcement feedbacks due to the huge lake that might form once the ice has disappeared (e.g. by calving / water temperature)? [Frank PAUL, Switzerland] | Taken into account. Neither of these models referred to include such a lake and this is now mentioned in the text. |
| 13-733 | 13 | 35 | 19 | 35 | 19 | Using GtC here which is difficult to compare to other emmision values quoted as different units. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. This will be adjusted. |
| 13-734 | 13 | 35 | 20 | 35 | 20 | "(Ridley et al., 2010b)" should be "Ridley et al. (2010b)" [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted. |
| 13-735 | 13 | 35 | 23 | 35 | 23 | It would be clearer if "has been lost" were inserted between "more" and ", it regrows". [John Hunter, Australia] | Accepted. |
| 13-736 | 13 | 35 | 33 | 35 | 39 | The section "Atmospheric circulation changes results (Krinner et al., 2007)" is not very clear and needs a bit more explanation (taking just one example - what does "anomaly methods in regional climate and SMB projections with global or regional atmospheric models" mean? [John Hunter, Australia] | Accepted. |
| 13-737 | 13 | 36 | 9 | 41 | 7 | Section 13.5.4. As far as I can see few if any of the modeling studies described here explicitly models dynamic responses, which are generally either assumed or largely ignored. So the predictions listed in Table 13.5 can only be misleading in that they give the typical reader the impression that we have really modeled the dynamic response. Using these values to provide a range of what might happen (which is what I suspect authors will do) is a bit like playing darts blindfold. [Robert Thomas, USA] | Rejected. This is untrue. Examples of models that explicitly assess dynamical effects are: Nick, Price, Greve, Joughin, Gladstone etc. The darts analogy is useful - we may not be able to hit the bulls eye but we can at least start to constrain where the dartboard is! |
| 13-738 | 13 | 36 | 9 | 42 | 8 | I feel section 13.5.4. is quite unbalanced and overly emphasises very extreme scenarios of ice-dynamical changes that are extremely unlikely to occur in the 21st century because they are physically unachievable. The summarizing statement that ice dynamics may contribute 2-25 cm to SLR during C21 is perhaps not totally unreasonable, though still high, but the preceding discussion too much conveys the message that 'the sky is the limit' and SLR contributions in excess of 1 m are also possible. In fact, a sound physical basis for confidently projecting the ice-dynamic contribution to SLR is still lacking, as was the case in AR4. [Philippe Huybrechts, Belgium] | Rejected. The section is about dynamics - there is a very significant literature on possible dynamic processes and their implications. This must be assessed. It would be a mistake not to address dynamics explicitly given the issues raised in AR4. This inevitably raises the profile of dynamics above the norm, however this is not in itself a reason for not considering dynamics explicitly. |
| 13-739 | 13 | 36 | 9 | | | Section 13.5.4 Ice-Sheet Dynamical Changes: This suffers from the same problem I noted for Chapter 13, Page 12, line 17 - what has been lost here is the distinction between dynamic ice sheet models and the | Noted. See 301. Will add material to 13.2.3 that makes this distinction clear and emphasizes how the |

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| | | | | | | "dynamical" changes such as melting of ice shelves and enhanced bottom lubrication, both of which may enhance ice flow. The reader is left with the impression that there are SMB models (used in previous IPCC Assessments) and some new models which attempt to simulate these "dynamical changes". There have, of course, been dynamic ice sheet models prior to this AR5 Assessment. The (quite simple) way in which the results of these models were incorporated into earlier (e.g. AR4) projections should be summarised (e.g. in AR4, the 5% +/- 5% subtraction from the Antarctic SMB and the "scaled-up ice sheet discharge" to account for "accelerated ice flow"). I'm not suggestion any major additions here - just some additional explanation to distinguish SMB models, dynamic ice sheet models and the extra processes which need to be implemented in the latter to account for accelerated ice flow. This would make life much easier for poor non-glaciologists like myself. [John Hunter, Australia] | dynamics models used here differ from the dynamics scaling used in AR4. I think 13.2.3 is the best place for this text and it would be wasteful to repeat here. |
| 13-740 | 13 | 36 | 9 | | | The usage of the word 'plausibility' in this section can be misleading. In general, I would understand that 'plausibility scenario' denotes the most likely scenario, whereas it seems that the authors actually mean 'the most extreme scenario deemed possible'. Either the meaning of 'plausible' in this context is explained or another word should be used. [Eduardo Zorita, Germany] | Agreed. The author team have discussed the use of the plausibility limit and have now rejected this idea. The strategy will now be to 1/ attempt to define limits on the very likely range 2/ if this fails (which is probable given issues with constraining high end SLR from ice sheets) then will attempt to define limits on likely range. |
| 13-741 | 13 | 36 | 11 | 36 | 16 | first para of 13.5.4 states that "a new generation of ice-sheet models have been developed that are capable of simulating them (changes in ice-sheet dynamics). We are therefore now in a position to make meaningful projections of SLR due to ice-sheet dynamics over the next century". My impression is that these models are not much better than the AR-4 era models. Later discussion in this section reveals how far we are from being able to model rapidly accelerating glaciers. [Robert Thomas, USA] | Accepted. This sentence presents an overly rosy view of our ability to project. In the IPCC parlance, I think we have observed these effects and are now well on the way to understanding them (both are advances from AR4) but may still be short of being able to project. This sentence removed and replaced with "Since the publication of the AR4, a great deal of effort has been invested in understanding the relevant effects as well as developing a new generation of ice-sheet models that are capable of simulating them, enabling us now to make a quantitative assessment of SLR due to ice-sheet dynamics for the next century. However, these efforts are still in their infancy, and the published literature does not yet offer a sufficient basis for a time-dependent or scenario-dependent assessment. Much uncertainty still exists in our basic understanding of the mechanisms, and modelling them presents strong challenges in terms of processes included and their numerical implementation." which sits in 13.2.3. |
| 13-742 | 13 | 36 | 14 | 36 | 15 | The statement that 'We are therefore now in a position to make meaningful projections of SLR due to ice-sheet dynamics over the next century' does not seem warranted by the ensuing discussion in section 13.5.4. that is on the contrary very speculative on what might actually happen by lack of a good physical understanding of the forcing and response mechanisms. There is large ambiguity between this statement and further statements in other sections that indeed stress that confidence in estimating the ice-dynamic contribution is very limited. [Philippe Huybrechts, Belgium] | Accepted. See 741. |
| 13-743 | 13 | 36 | 14 | 36 | 15 | I suggest more clarity here: the "meaningful" projections, presumably those in table 13-5, don't come from continental scale models but rather regional models and various estimation techniques. It's important to clarify this point so as not to give the reader excessive confidence in current ice sheet models. [Michael Oppenheimer, USA] | Accepted. See 741. |
| 13-744 | 13 | 36 | 14 | 36 | 16 | These two sentences seem contradictory: on the one much uncertainty in our basic understanding still exists; on the other hand we are now in position to provide meaningful SLR projections. I doubt that lay persons can make sense of these two sentences. [Eduardo Zorita, Germany] | Accepted. See 741. |

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| 13-745 | 13 | 36 | 26 | 36 | 26 | "two criteria" should be three criteria, the third being adequate definition of the boundary conditions. This is discussed later and is clearly essential (as discussed elsewhere see top of following page for example), particularly subglacial topography and bathymetry. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted I would argue that the local climate forcing is one of the boundary condition for the ice response. The reviewer is worried about bedrock topography, which is a different type of boundary condition which I think falls within the basic mechanism. I have added text to indicate better data is also required in order to model the basic mechanisms. |
| 13-746 | 13 | 36 | 32 | 36 | 33 | There are two many "and"s in this sentence - some restructuring is needed. [John Hunter, Australia] | Accepted. Text reworked and moved to 13.2.3. |
| 13-747 | 13 | 36 | 39 | 36 | 40 | The warming amounts given here for "decadal" rates imply a whopping warming over a century. Something needs to be explained or corrected. [Michael Oppenheimer, USA] | Accepted. Figures are decadal means over 21st century NOT decadal rates. Have made this clearer in text. |
| 13-748 | 13 | 36 | 39 | 36 | 41 | These two sentences suggest that the way in which the ocean forces ice sheets is simply via temperature - i.e. that all we need to do is take the warming from AOGCMs and apply it to an ice sheet/ice shelf model. It is more complicated than this - to melt an ice shelf you don't just need to warm the adjacent water - you need to get it under the ice shelf. This requires the ocean density to be suitable - which depends on local sea-ice production (among other things). Sea-ice production will undoubtedly change as the climate warms, and so is another important forcing for an ice sheet/ice shelf model. There needs to be a short summary here of climate-related properties (other than just water temperature) which influence ice shelves and therefore ice sheets. [John Hunter, Australia] | Accepted. Text has been rewritten and suggestion that the only process that operates is warming of the global ocean has been removed. Now discussion of CIRCULATION changes as well. |
| 13-749 | 13 | 36 | 43 | 36 | 59 | This paragraph is repetitive and could be improved. [Neil White, Australia] | Accepted. Paragraph moved to 13.2.3 and has been reworked and shortened. |
| 13-750 | 13 | 36 | 52 | | | A good comprehensive section on Greenland. The style of effectively bullet-points of recent papers and outcomes is different from other sections in that it does not structure around a story. I suggest changing the units of SLR to be consistently in mm or m. [Jeff Ridley, UK] | Noted. Agreed that will use m or mm. One instance of cm changed to mm. |
| 13-751 | 13 | 36 | | | | Section 13.5.4.1: Section reads poorly with many paragraphs on 13-37 and 13-38 reading as a simple reporting of results in the form of a list. [Simon Holgate, UK] | Agreed. This section will be rewritten as new results become available - this is a reflection of literature at FOD |
| 13-752 | 13 | 37 | 4 | 37 | 10 | It is important to mention here that as well as speed ups, several of these glaciers (Helheim and Kanger e.g.) have slowed down to close to their pre-speed-up value (Howat, et al 2007) [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Noted. This text will likely be removed as is already covered in chapter 4. have added remarks about slow down. |
| 13-753 | 13 | 37 | 5 | | 6 | Rate of ice loss needs to be related to total Greenland ice mass, for context. [David Burton, USA] | Rejected. The aim of the sentence in question (p. 37, lines 5-6) is to suggest that dynamics are significant and growing. Linking it to total mass is not easy or necessary at this stage and would confuse matters. We have clarified wording. |
| 13-754 | 13 | 37 | 9 | 27 | 10 | don't make much sense. Surely the important question is What caused the accelerated flow? The observations suggest it was something happening near the calving fronts; acceleration then resulted in thinning so that retreat of the calving front was inevitable. Modeling this requires knowledge of what happened near the calving fronts plus any knock-on effects that modulate the acceleration. [Robert Thomas, USA] | Noted. This paragraph serves as a reminder of the importance of calving it is not meant to be a detailed discussion of the observations. In its not clear in what way this does not make sense. The next paragraph attempts to show that models (in the limited places that they have been applied) can reproduce some of the observations. |
| 13-755 | 13 | 37 | 12 | 37 | 12 | There is wider evidence of a slowdown in southeast Greenland than just the Helheim Glacier. I feel there should be clarification that not all of Greenland's glaciers are behaving in the same way at the same time. See: [Simon Holgate, UK] | Agreed. See 752. |
| 13-756 | 13 | 37 | 12 | 37 | 12 | Murray, T. et al., 2010. Ocean regulation hypothesis for glacier dynamics in southeast Greenland and implications for ice sheet mass changes. Journal of Geophysical Research, 115(F3). Available at: | Noted. The aim of this paragraph is simply to say that this can modelled. Chapter 4 discusses observations |

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| | | | | | | http://www.agu.org/pubs/crossref/2010/2009JF001522.shtml . [Simon Holgate, UK] | and their interpretation in more detail. |
| 13-757 | 13 | 37 | 20 | 37 | 20 | Add van de Wal et al 2008, which is the onlgest GPS time series andshows important results. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Noted. Not really relevant in this context. The aim of this paragraph is to 1/ note that water gets to the bed (the references are more directly linked to this than vdW) and 2/ to say progress has been made in modelling. If aim was a detailed discussion of effect then vdW very useful BUT this is what chpt 4 will be doing. |
| 13-758 | 13 | 37 | 29 | | | Sections 13.5.4.1 and 13.5.4.2: As for glaciers, I found the level of detail provided to the individual papers too high and would prefer a more condensed version that compares the different results in view of the different approaches / input data. [Frank PAUL, Switzerland] | Agreed. See 751. |
| 13-759 | 13 | 37 | 36 | 37 | 36 | "Dynamic SLR" is used elsewhere to refer to regional sea level changes associated with ocean dynamics. Terminology should be clarified. [Robert Kopp, USA] | Agreed, Have reworded. |
| 13-760 | 13 | 37 | 42 | 37 | 43 | Recent work by the VUB group indicates that neither basal lubrication by surface meltwater nor the inclusion of longitudinal stress gradients makes a noticeable difference in the ice-sheet volume response of the Greenland ice sheet on decadal to centennial time scales as compared to the response from a classical SIA model. We expect to meet the IPCC deadline to submit this work. [Philippe Huybrechts, Belgium] | Noted. Can only refer to submitted work. |
| 13-761 | 13 | 37 | 44 | 37 | 44 | The significance of the presence or omission of the simulation of "longitudinal transmission of stresses" should be explained somewhere. [John Hunter, Australia] | Noted. The importance of this is discussed in 13.2.3 |
| 13-762 | 13 | 38 | 1 | 38 | 1 | A simple explanation of why "doubling sliding equates to a fourfold increase in flow" would be helpful here. [John Hunter, Australia] | Accepted. Text revised. |
| 13-763 | 13 | 38 | 1 | 38 | 58 | Some efforts in there, but is it necessary to explain long details of each study? Summarizing the results in a table is not making it easy for the average reader to understand what is going on. A graph would be much better, with an assessment and error bars, and explanation of how these were derived. What is the assessment of IPCC in this chapeter? What should one think of these ten or so numbers listed in the Table? As a side note, Ren et al (2011) is a paper that should have never been published; the model is unverified (Ren is running Navier-Stokes on his laptop for the entire ice sheet), and the authors find less loss in ice mass in 2100 than at present. [Eric Rignot, USA] | Noted. SOD will focus on assment of projections however at the FOD stage their were so few projections that it seemed worthwhile to dwell on details. I have similar concerns with the Ren paper but put it in to test the water, so that this comment is very helpful. |
| 13-764 | 13 | 38 | 3 | 38 | 4 | What time interval? [Donald Forbes, Canada] | Accepted. Have amended text. |
| 13-765 | 13 | 38 | 4 | 38 | 4 | The terminology "dynamic component" is confusing, in the context of a dynamic ice-sheet model (see also comments to Chapter 13, Page 12, line 17 and Chapter 13, Page 36, line 9). You need a clear and unambiguous name for these components, used consistently throughout this and other Chapters of the report - for example, "accelerated dynamic component". [John Hunter, Australia] | Noted. There has been discussion in chapter meetings about agreeing on terminology and this clearly needs to be improved for the SOD. |
| 13-766 | 13 | 38 | 20 | 38 | 37 | The mixed units (mm and m) are confusing here - while I think I can see the rule behind it, it is still a bit confusing. [Neil White, Australia] | Noted. Have used mm except where this is unfeasible. |
| 13-767 | 13 | 38 | 22 | 38 | 23 | Comparison would be clearer if presented in the same units, e.g. 93 mm and 470 mm. [Donald Forbes, Canada] | Agreed. Have moved this and other instances of vales < 1 m to mm. |
| 13-768 | 13 | 38 | 30 | 38 | 32 | It is seems like a misrepresentation to say that Pfeffers "low estimate" is a "upper bound of the likely range". I suggest you ask him if it is an accurate characterisation of his work. [Aslak Grinsted, Denmark] | Agreed. Neither Low 1 and Low 2 form a maximum lower bound (infimum). They are only likely values at the low end at the range of plausible future dynamic scenarios. |
| 13-769 | 13 | 38 | 30 | | | The division between calving and basal lubrication is unclear: basal lubrication can presumably enhance ice flow leading to more calving. [Michael Oppenheimer, USA] | Accepted. Have added caveat. |
| 13-770 | 13 | 38 | 31 | 38 | 31 | Pfeffer 2008 (low scenario) is taken as the likely upper bound and high as plausibility limit. These needs some sort of justification. Is plausibility limte defined somewhere? [Jonathan Bamber, United Kingdom of Great | Taken into account. The Pfeffer et al (2008) Low 1&2 scenarios do not represent lower bounds (see 13- |

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| | | | | | | Britain & Northern Ireland] | 768), but High 1 does represent an upper bound to the extent that values higher than 2.0 m SLE start to require physically unreasonable agencies. High 1 is, however, a "loose" upper bound (i.e. not a supremum); arguments could be made (possibly easily) to reduce the magnitude of the 2.0 m SLE of High1. "Plausibility Limit" is not defined, and was not used by Pfeffer et al (2008). It will no longer be used. |
| 13-771 | 13 | 38 | 33 | | | "plausibility limit" should be clearly defined somewhere. It's certainly not in the IPCC uncertainty guidance. [Michael Oppenheimer, USA] | Taken into account. "Plausibility Limit" is not defined, and was not used by Pfeffer et al (2008). It will no longer be used. |
| 13-772 | 13 | 38 | 34 | 38 | 34 | The figure of 0.22 m from Greve et al. (2011) refers to a period of 500 years and is therefore wrongly combined with the other numbers to give 69 cm. The Pfeffer et al (2011) number of 47 cm is totally unrealistic to actually happen during the 21st century and should be assessed within the context of its assumptions. [Philippe Huybrechts, Belgium] | Noted. Both these numbers refer to the plausibility limit which will be dropped in the SOD. See 740. |
| 13-773 | 13 | 38 | 40 | 38 | 40 | This is a table of the "total contribution" (including "ice dynamics"). [Aslak Grinsted, Denmark] | Noted. The table clearly states "contribution from changing ice dynamics in caption". |
| 13-774 | 13 | 38 | 40 | 38 | 41 | Table 13.5 is in the wrong place. It should come after Antarctic discussion. [Ian Allison, Australia] | Editorial. Will place more sensibly in SOD. |
| 13-775 | 13 | 38 | 40 | 38 | 41 | The term "physically based constraint" is misleading in my opinion, because it makes it seem like it is a physical law (like the speed of light). "Heuristic" is better. [Aslak Grinsted, Denmark] | Noted. The author team have discussed how best to refer to these different types of approaches and will adopt a more rigorous approach in the SOD. |
| 13-776 | 13 | 38 | 40 | 39 | 1 | Projections in Table 13.5 show a wide range of possibilities, which is not surprising. The truth is that we do NOT understand enough about dynamic responses to model them reliably. But we do have enough observations to show that they bear no simple relationship to anything likely to emerge soon from a GCM prediction of future climate. 25 years ago, most glaciologists, and probably all the ice-sheet modelers, would have scoffed at the idea that the loss of the small Jakobshavn floating ice tongue would cause a near instantaneous doubling of its already high discharge velocity. So, today, we should resist the temptation to use what are only marginally better models to predict future dynamic changes, and to provide uncertainty limits on these changes. The massive observed changes on Jakobshavn, and on some Antarctic glaciers were probably caused by changes in deeper ocean conditions, and I suspect we have next to no idea of what these might be over the next 90 years. [Robert Thomas, USA] | Rejected. The comments could have been made at the time of the AR4 and ignored much of the progress made in understanding and modelling these effects since the AR4. There are now convincing studies that suggest we can model recent changes in JI and PIG. This work may however fall short of the ability to make full projections, hence our revised methodology for making projections (see 740 and 786). |
| 13-777 | 13 | 38 | 41 | 38 | 41 | I do not believe that the column title "likely upper bound" accurately reflects all the studies in this table. [Aslak Grinsted, Denmark] | Noted. Not helpful if a reason for this belief is not given. This is an assessment of the process-based literature. This relates to the plausibility issue and a better explanation of what constitutes the likely range. Assume that the reviewer would like to see the plausibility numbers become likely upper bounds. This table will be more convincing when more process based studies are available and there is less reliance on Pfeffer. |
| 13-778 | 13 | 38 | 41 | 38 | 41 | I think this table should include the low estimates as well. (i.e. Pfeffer's low estimates, and Price) [Aslak Grinsted, Denmark] | Accepted. They are mentioned in the text and should also have been in table. |
| 13-779 | 13 | 38 | 41 | 38 | 41 | Please provide other types of estimates in this table as well. Even statistical extrapolations because they are useful comparisons for the bounds. A constant rate extrapolation is probably the lower bound of what we can expect given that the imposed forcing is only going to be stronger in the future. [Aslak Grinsted, Denmark] | Noted. This point was discussed at some length and it was decided that extrapolations should not be used as projections (indeed the papers that generate functional forms that could be extrapolated are very wary about using these forms as projections) and |

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| | | | | | | | should not therefore appear in the table. It would however be useful to provide context for the numbers in this table by way of comparing them with extrapolations. This was done in the ZOD however was not done in the FID because of uncertainty about the relevant literature (i.e. that related to observations of the recent mass budget of the ice sheets), this context will be provided in the SOD if Chapter 4 can supply relevant numbers. |
| 13-780 | 13 | 38 | 41 | 38 | 41 | Table 13.5 caption: has "plausibility scenario" been defined anywhere? Is this the best word? [John Hunter, Australia] | Accepted. See 740. |
| 13-781 | 13 | 38 | | | | Table 13-5: just looking at the table, it is unclear why a "Likely upper bound" is several times smaller than a "'Plausibility' Scenario". Should be clarified by extending the table caption. [Olaf Eisen, Germany] | Accepted. See 740. |
| 13-782 | 13 | 38 | | | | Table 13.5: I consider the numbers in the 'plausibility' column as rather outrageous. Thought experiments have been published in the literature based on back-of-the-envelope estimates or by stretching crucial model parameters to the extreme, but such results should be assessed with much more restraint. I would rather qualify such high numbers as 'implausible' and for sure as extremely unlikely. [Philippe Huybrechts, Belgium] | Accepted. See 740. |
| 13-783 | 13 | 38 | | | | Table 13.5: The likely upper bound for Antarctica from Katsman et al. (2011) is likely in error as the lower end (0.17) is higher than the upper end of the quoted range (0.15) [Philippe Huybrechts, Belgium] | Accepted. Typo error. |
| 13-784 | 13 | 38 | | | | Table 13.5: the 0.12-0.22 m numbers for the Greve et al. (2011) study refer to a period of *500 years* but are erroneously quoted in the 'plausibility' column for the *21st century*. Actually, dividing these numbers by a factor 5 would give a number that might fit more into the first column. From first principles doubling the sliding coefficient (as was done in this experiment) would just double the calving flux. All other things being equal (no feedback on driving stress, etc...) this gives a maximum of about 7.5 cm of SLR during 1 century. But such a doubling is just a thought experiment, and there are little indications that all Greenland outlet glaciers would just double at a constant rate for 100 years. The chapter should be much more reluctant in using such speculative arguments as a basis for estimating the ice-dynamic contribution. This refers also to most of the other entries in Table 13.5, with the notable exceptions of the Price et al. (2011), Graverson et al. (2011), and Joughin et al (2010) studies that all used observation-based arguments, not wild speculations on sustained accelerations that are extremely unlikely to occur during the 21st century. [Philippe Huybrechts, Belgium] | Accepted. Error in calculation - thanks to the reviewer for spotting this. In regard to other issues raised in comments - see 738 |
| 13-785 | 13 | 38 | | | | My suggestion for Table 13.5 and all of section 13.5.4. is to clearly distinguish between model studies that (i) start from reality (Price et al. (2011), Graverson et al. (2011), and Joughin et al (2010)) and use only those type of studies to produce the likely range and (ii) those that are just Gedankenexperimente on assumptions that are extremely unlikely to occur during the 21st century (foremost the high scenarios of Pfeffer et al., 2008). The word 'plausible' to characterize the latter category is misplaced but I don't readily have a better suggestion. [Philippe Huybrechts, Belgium] | Noted. This was done by use of * and + but will explore ways of making the distinction a/ clearer and b/ more nuanced. The definition of plausible which is very loose. We have agreed to drop this term for several reasons, one of which being this point. SOD will limit itself to very likely/likely ranges which have a definition within uncertainty language. |
| 13-786 | 13 | 39 | 3 | 41 | 37 | Section 13.5.4.2: Again, the modeling described here leaves little confidence that reality is well simulated. This is inevitable in our present state of comparative ignorance. And the authors of this Chapter clearly have to review what is being done. But I think they also have to make our comparative ignorance clear to readers. In this situation, perhaps the most relevant predictions come from efforts such as those of Pfeffer et al, which simply assume "reasonable" maximum glacier discharge rates based on what they see happening on real existing glaciers. Then, at least their results can easily be tuned as observations narrow (or broaden!) the range of "reasonableness". [Robert Thomas, USA] | Noted. See 740. Comparison with comment 782 reveals a wide difference of opinion between reviewers. There has been a great deal of discussion amongst the authorship on how to address this issue. The concept of split probability seems to offer a useful way forward, in which different levels of confidence are assigned to different SLR. For instance we may well be able to characterize likely range (as opposed to very likely) and make a different (less concrete) statement about the high-end range. |
| 13-787 | 13 | 39 | 3 | 41 | 37 | As the authors point out, even if we had reliable models for fast glaciers, realistic predictions will require reliable modeling of future interactions between the ocean and ice shelves, and this is a long way off. In this | Noted. The Joughin and Gladstone papers that discuss the fate of PIG, the former has one scenario |

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| | | | | | | situation, extremes can perhaps be considered by assuming total loss of an ice shelf. As the authors indicate elsewhere, this is unlikely for the big Ross and Ronne ice shelves, but Amundsen Sea ice shelves are thinning fast, and could become vulnerable. As far as I can see, none of the modeling discussed here considers the possibility of ice-shelf breakup in this region. One study that does (Accelerating ice loss from the fastest Greenland and Antarctic glaciers, Thomas et al. GRL, 38, 2011) concludes that acceleration of just Pine Island Glacier (PIG) following breakup of its ice shelf could increase SLR by 1.5 mm/yr, which could result in a total SLR by 2100 similar to the predictions in Table 13.5 for all of Antarctica! [Robert Thomas, USA] | of the type mentioned by the reviewer. The Thomas paper should be included but does not add much to the Joughin and Gladstone studies. |
| 13-788 | 13 | 39 | 3 | 41 | 37 | Although the authors probably omitted mention of this because of the simplicity of the model used, it should be noted that a similar model was also used to predict (ie before it happened) the later-observed increase in PIG velocity to more than 3 km/yr following retreat of the grounding line into deeper water (Force-perturbation analysis of Pine Island Glacier, Antarctica, suggests cause for recent acceleration, Annals. Glac. 39, 2004). By contrast, modeling studies reviewed by the authors show agreement with stuff that had already happened. [Robert Thomas, USA] | Noted. See 787 |
| 13-789 | 13 | 39 | 17 | 39 | 17 | "SLR" appears suddenly without definition. It may be obvious but it is not used elsewhere. [Simon Holgate, UK] | Accepted. Acronym SLR now defined in 13.2. |
| 13-790 | 13 | 39 | 19 | 39 | 24 | The statement made here (no ice shelf collapse by 2100) should be counterbalanced by the collapse of Larsen A in 1995, Larsen B in 2002 and Wilkins Ice Shelf (not mentioned) in 2010, and signs of weaking of the northern part of Larsen C and George VI ice shelf. [Eric Rignot, USA] | Noted. The preceding paragraph goes into some detail about the on-going changes in the Peninsula. |
| 13-791 | 13 | 39 | 23 | 39 | 23 | Fykes should be Fyke [Philippe Huybrechts, Belgium] | Editorial. |
| 13-792 | 13 | 39 | 26 | 39 | 35 | This is wrong. Take a look at Fig 1 of Rignot et al 2008. Dynamic losses are greates in the ASE, followed by the AP. Cook and Totten are somewhat equivocal and no accel. of these glaciers has been observed. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. Rignot et al 2008 is not the only source of mass estimates. Have added Peninsula, although not strictly relevant to the point being made (i.e. mass loss from AIS by dynamics) and toned down reference to EAIS. |
| 13-793 | 13 | 39 | 35 | 39 | 35 | This paragraph is based on old literature. The paper by Jacobs et al Nature 2011 describes oceanic changes in Pine Island Bay. They suggest that the increase in melt is due to ice shelf thinning that enables a greater inflow of heat under the cavity. Again, this is mentioned at length in Chapter 4. [Eric Rignot, USA] | Noted. this positive feedback but does not in itself explain the initial retreat. The contention that CDW upwelling does not vary through time is at odds with the lit for the ASE. |
| 13-794 | 13 | 39 | 37 | 39 | 41 | This paragraph is focused on Joughin et al. (2010) paper. It does not mention Thomas et al GRL 2010 paper which has predictions of speed up and contributions to SLR that greatly exceed the Joughin paper. Interestingly, Thomas et al. model was applied to Pine Island in 2003 and correctly predicted the velocity of that glacier in 2010. So at least this paper should be mentioned in the assessment as having some skill. Joughin et al paper has a disputable treatment of errors in basal melt rates of hundreds of meters per year on grounded ice, constrains the model by forcing it along hypothetical shear margins of the ice shelf, forces the grounding line retreat, and employs addhoc ocean forcing and unclear reasoning for modulating temporal variations in melt rates. [Eric Rignot, USA] | Agreed see 787 |
| 13-795 | 13 | 39 | 37 | 39 | 45 | Must include Thomas et al 2011 in this discussion who look at both PIG and Jakob. The force balance model of Thomas (1979) is one of the few studies shown to have had a good predictive skill for recent obs of GL migration rate. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Agreed see 787 |
| 13-796 | 13 | 39 | 56 | 39 | 56 | "...characterised by complete collapse..." [Simon Holgate, UK] | Agreed reworded. |
| 13-797 | 13 | 39 | 56 | | | "complete collapse" needs to be defined precisely [Michael Oppenheimer, USA] | Agreed see 796 |
| 13-798 | 13 | 40 | 1 | 40 | 58 | This entire page is a technical review of papers. I find it difficult to review because the text is too long and does not have the right focus. The second part of that page on "irreversibility" is overlapping with Chapter 4. [Eric Rignot, USA] | Agreed. This section will be rewritten in the SOD. The division between 4 and 13 was that 13 would to anything associated with projection; irreversibility clearly falls in this area and discussion in 4 will need to be revised. |

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| 13-799 | 13 | 40 | 2 | 40 | 3 | "is already displacing ocean water; its loss therefore has a reduced impact on sea level" seems a funny place to say this and a funny way to say it (unless I completely misunderstand). This whole Section (13.5.4.2) is devoted to ice shelves which, by necessity, require that the ice is grounded below sea level and that a portion of the grounded ice is therefore below sea level. Surely it should be noted at the beginning of this Section (or earlier) that, when the grounding line retreats, the resultant sea-level rise is caused by the total volume of fresh water produced by melting minus the volume of seawater water displaced by the ice that was previously below sea level (I'm ignoring any secondary effects related to the equation of state of seawater). [John Hunter, Australia] | Agreed. Will move this text to 13.2. |
| 13-800 | 13 | 40 | 3 | 40 | 9 | More references to "SLR" [Simon Holgate, UK] | Noted. Definition now in 13.2 |
| 13-801 | 13 | 40 | 5 | 40 | 5 | the' should be removed between 'not' and 'incorporated'. [Philippe Huybrechts, Belgium] | Accepted. |
| 13-802 | 13 | 40 | 9 | 40 | 16 | Qualifying the Pfeffer et al. (2008) assumption of a 'an order of magnitude' velocity increase of all EAIS glaciers as 'high but still plausible' is over the top. It is very difficult to imagine how all EAIS outlet glaciers could suddenly accelerate by a factor 10 in a sustained way over the remainder of the 21st century or even beyond. I presume the assumption is based on extrapolating observations from the Antarctic Peninsula after the collapse of Larsen B, but this needs to be put in the right perspective (are the characteristics of AP glaciers really comparable to the entire EAIS coast?). One can speculate about the implications in a what-if approach (as Pfeffer did) but one can not qualify this as a 'plausible' or realistic outcome. This is extremely unlikely to occur during the 21st century. [Philippe Huybrechts, Belgium] | Taken into account. The scenario discussed here is the 2.0 m High 1 scenario, designed to be at the very limit of physically possibility. The term "plausible" was not applied to this scenario in Pfeffer et al (2008); in fact it was applied to the Low 1&2 scenarios in contrast to High 1. More importantly, High 1 was not designed to establish a working estimate for a supremum (lowest upper bound) sea level estimate. It was designed to be a robust invalidation of even higher sea level estimates: if even the extreme assumptions contained in High 1 yield only 2 m SLE, by 2100 how likely is 3.0 m SLE by 2100? The issue here is perhaps the definition of plausible which is very loose. We have agreed to drop this term for several reasons, one of which being this point. SOD will limit itself to very likely/likely ranges which have a definition within uncertainty language. |
| 13-803 | 13 | 40 | 21 | 40 | 25 | inconsistency of units (mm and m). [Simon Holgate, UK] | Agreed. Now using mm |
| 13-804 | 13 | 40 | 27 | 40 | 36 | Text based on unpublished material (submitted). What is the status of the submitted work? (Little et al.) [Eduardo Siegle, Brazil] | Rejected. this is within rules (pdf available) |
| 13-805 | 13 | 40 | 33 | 40 | 34 | inconsistency of units between mm and m. [Simon Holgate, UK] | Agreed. Now using mm |
| 13-806 | 13 | 40 | 38 | 40 | 57 | The previous paragraphs are all about the 21st century. Suddenly we lurch into much longer time scales (thousands of years for Bamber et al) without any new heading or other warning that we have moved to a different set of time scales! [Neil White, Australia] | Noted. context here is explained. This is to get an estimate from pollard. This discussion would be better in the paleo section. |
| 13-807 | 13 | 40 | 45 | 40 | 45 | See also: Young, D. A., A. P. Wright, J. L. Roberts, R. C. Warner, N. W. Young, J. S. Greenbaum, D. M. Schroeder, J. W. Holt, D. E. Sugden, D. D. Blankenship, T. D. van Ommen, and M. J. Siegert, 2011, A dynamic early East Antarctic Ice Sheet suggested by ice covered fjord landscapes, Nature, 474, 72–75, 10.1038/nature10114. [Ian Allison, Australia] | Agreed. Reference added |
| 13-808 | 13 | 41 | 2 | 41 | 3 | Text based on unpublished material (submitted). What is the status of the submitted work? (Gladstone et al.; Little et al.) [Eduardo Siegle, Brazil] | Rejected. this is within rules (pdf available) |
| 13-809 | 13 | 41 | 2 | 41 | 4 | Please provide a argument for why these studies are an "upper bound" of the likely range. It is not how i read e.g. Pfeffers paper. [Aslak Grinsted, Denmark] | Noted. Chapter has had extensive discussions on how to progress. Text will be added to SOD which explains how very likely or likely range will be addressed. |

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| 13-810 | 13 | 41 | 7 | 41 | 8 | using both m and mm as units - pick one. Or pick cm. [Philip Mote, USA] | Agreed. Now using mm |
| 13-811 | 13 | 41 | 7 | 41 | 9 | Does it make sense for a plausibility limit to have a range? [Aslak Grinsted, Denmark] | Agreed see 740. |
| 13-812 | 13 | 41 | 7 | 41 | 9 | Mixed units again (see comment 18). [Neil White, Australia] | Agreed. Now using mm except where value about 1 m |
| 13-813 | 13 | 41 | 9 | 41 | 9 | I think it is fair to end this paragraph by highlighting the problem that all these estimates are independent of emmision scenario. [Aslak Grinsted, Denmark] | Agreed. Test added. |
| 13-814 | 13 | 41 | 11 | 41 | 21 | This paragraph starts out by talking about "beyond the 21st century" and ends up with a conclusion about the 21st century. This needs to be sorted out, possibly by splitting the paragraph into two, one talking about the 21st century and the other beyond the 21st century. [John Hunter, Australia] | Agreed. This is a typo. |
| 13-815 | 13 | 41 | 33 | 41 | 37 | It is surprising that the authors fail to point out that actual observations (as summarized in Chapter 4) DO show net ice loss already since 2000, which is totally contrary to the model results. Surely reality deserves a mention. I would have thought that the failure of the "currently-available climate ice-sheet models" here should deter the authors from using them to predict future ice-sheet contributions to SLR. [Robert Thomas, USA] | Noted. I am not certain where the reviewer finds the mismatch between models and observations that he eludes to. It does come as a surprise that the observations are totally contrary to the model results. As indicated in replies to the reviewer's previous comments, models are now beginning to be able to replicate observations. A good example is the Gladstone study, in which great attention is paid to comparing model with observations (of thinning and GL retreat); similarly the Joughin work goes to some lengths to test model against observations. The authorship team discussed the need for an explicit test of models section which will now appear in SOD 13.4 |
| 13-816 | 13 | 41 | 37 | 41 | 37 | Please mention that these coupled models still don't include key processes such as grounding line migration and higher order stresses. [Aslak Grinsted, Denmark] | Agreed. Text added. |
| 13-817 | 13 | 41 | 43 | 41 | 43 | Value of SLE given in 13-40 41 is 3.3 m (including some EAIS contribution), not 3.7 m [Ian Allison, Australia] | Noted. Already aware that better comms required with chpt 4. |
| 13-818 | 13 | 41 | 43 | 41 | 43 | "... downward-sloping ..." - 'downward' is not helpful - Intuitive interpretation is seaward. Perhaps use 'poleward' or 'inward'? [Donald Forbes, Canada] | agreed 'towards interior' |
| 13-819 | 13 | 41 | 43 | 41 | 44 | "downward sloping bedrock" should be replaced with "becrock which slopes downward inland" or something like it. [John Hunter, Australia] | agreed see 818 |
| 13-820 | 13 | 41 | 46 | 41 | 46 | "(Bamber et al., 2009)" should be "Bamber et al. (2009)" [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Editorial. |
| 13-821 | 13 | 41 | 46 | 41 | 46 | Is Bamber et al., 2009, the correct reference? [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted. Have removed. |
| 13-822 | 13 | 41 | 46 | | | Once again, the term "models" is used too loosely. Which variety of model? [Michael Oppenheimer, USA] | Accepted see 821 |
| 13-823 | 13 | 41 | 47 | 41 | 48 | Line 47/48: "parameterized ice flow across the grounding line" sounds ominous. What does it mean? I suspect velocity is not determined by physics, but by some "parameterization". [Robert Thomas, USA] | Accepted see 821 |
| 13-824 | 13 | 41 | 50 | 41 | 52 | These two sentences seem to come out of the blue and I didn't understand what they were referring to or why they were here. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Agreed. These are nonsense. |
| 13-825 | 13 | 41 | 51 | 41 | 51 | It would be helpful to turn these numbers into rates for comparison i.e. 1-7 mm/yr. [Simon Holgate, UK] | Noted. This text should be in paleo. Have deleted. |
| 13-826 | 13 | 41 | 54 | 41 | 54 | Please provide a reference for the number. [Aslak Grinsted, Denmark] | Noted. Text has been moved to paleo. |

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| 13-827 | 13 | 41 | 54 | 41 | 55 | There is a problem with the logic of "... to cause about 13 m SLE rise and is thus potentially subject to marine ice-sheet instability". The reason for its potential for marine ice-sheet instability is because the sea bed slopes downward inland, rather than anything to do with the amount of potential sea-level rise. [John Hunter, Australia] | Agreed but see 826 |
| 13-828 | 13 | 42 | 4 | 42 | 4 | In my opinion not all the studies that go into "likely upper bound" can really be considered to be "upper bound" estimates. [Aslak Grinsted, Denmark] | Agreed see 809 etc |
| 13-829 | 13 | 42 | 4 | 42 | 4 | I find the 22 mm to be highly unlikely, and perhaps even stretching the "plausibility limit". Presently we get ~1mm per year from the ice sheets. And the imposed forcing is only going to increase. I consider an extrapolation of 0.1 m to be a lower bound. [Aslak Grinsted, Denmark] | Rejected. extrapolation can not be the means of generating the lower likely limit. Extrapolation assumes that the processes currently operating continue; this may not necessarily be the case. Examples include calving in Greenland which may have a depletion effect, also GL retreat in WAIS which may stabilise. We will use extrapolation as a 'sanity check' but see little role for them as projections in their own right. see 779 |
| 13-830 | 13 | 42 | 4 | 42 | 4 | It is not clear where the 22mm is coming from. Please put the relevant numbers in table 13.5 in an extra column. [Aslak Grinsted, Denmark] | Accepted. Will clarify. This comes from 18 (ant) plus 4 (Greenland, figure 6 +/- 2) |
| 13-831 | 13 | 42 | 4 | 42 | 4 | I think "the next century" should read "this century" or, better, "the 21st century". [John Hunter, Australia] | Agreed have changed to 21st century |
| 13-832 | 13 | 42 | 4 | | | using both m and mm in the same sentence - pick one for consistency. [Philip Mote, USA] | Noted. Have used mm for numbers less than 1 m and m for above. 1240 mm seems odd. |
| 13-833 | 13 | 42 | 5 | 42 | 5 | Please, explain briefly how the authors come up with the number 1.24 m. It will be probably the most quoted number in this report, so it would be useful that the origin of this number is explicitly made clear in the summarizing paragraph [Eduardo Zorita, Germany] | Agreed. Have made this explicit. In SOD should expand although very conscious of space. |
| 13-838 | 13 | 43 | 22 | 43 | 24 | The most important effect of human water storage is the reduction in evaporative cooling that it causes [VINCENT GRAY, NEW ZEALAND] | Rejected. This statement is not supported by the peer-reviewed literature. |
| 13-839 | 13 | 43 | 28 | 43 | 33 | A constant rate for the pumping seems very unlikely. Why not use the population scaling idea with konikows numbers to provide the lower estimate? [Aslak Grinsted, Denmark] | Rejected. We would rather use a wider range, since there is so much uncertainty in this contribution. |
| 13-840 | 13 | 43 | 30 | 43 | 30 | Add "SLE" after '22-44 mm' [Donald Forbes, Canada] | Accepted. |
| 13-841 | 13 | 43 | 31 | 43 | 33 | This assumption that groundwater extraction will scale by population looks extremely suspect to me - see comment 14. [Neil White, Australia] | Taken into account. In the SOD we cite the projection of Wada et al. (2012), which supersedes the scaled-up estimate we gave here and provides a comparison with Rahmstorf. |
| 13-842 | 13 | 43 | 46 | | | Some discussion of the Aswan High Dam, and its effect (or lack of effect) on sea level, would be a useful addition here, since it dwarfs all other reservoirs. [David Burton, USA] | Rejected. This is untrue. The Aswan High Dam has a large reservoir, indeed, but it is just one of many reservoirs and responsible for only a small proportion of the global total of impounded water. |
| 13-843 | 13 | 43 | 47 | 63 | 54 | Sheer speculations based on flawed climate models; out of touch with actual current observations of sea level [VINCENT GRAY, NEW ZEALAND] | Rejected. These projections are based on the most complete scientific understanding available, as embodied in the models used, and evaluated where possible against observed past changes. They are not speculations. |
| 13-844 | 13 | 43 | 47 | | | Section 13.6 Projections of Global Mean Sea Level Rise: Do the RCP numbers have a decimal or not (e.g. RCP26 or RCP2.6)? The numbering should be consistent. [John Hunter, Australia] | Editorial. |
| 13-845 | 13 | 43 | 49 | | | Section 13.6.1: In this section projections for GMSL rise during the 21st century are given on the basis of | Taken into account by further work on the assessment |

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| | | | | | | process-based and semi-empirical models. The shortcomings of both types of models are discussed, it is admitted that it is not properly understood why there is difference between the two sets of projections, and then the preference is given to the projections stemming from process-based models. The authors should (1) more clearly explain why they prefer process-based over semi-empirical models or (2) mention both sets of projections in the concluding part of the section. In fact, the latter approach is followed in Executive Summary of Chapter 13, so presumably it should be also followed here. [Mirko Orlic, Croatia] | of models in 13.4 and the discussion in 13.6.1.3. |
| 13-846 | 13 | 43 | 51 | | | The section on projections is excellent, but there is one additional feature that it needs to make it "extremely excellent". For each discreet model run (or collection of model runs) that makes a projection for the next century, we also need to know what that specific model run (or collection of model runs) estimates for the historical period. That information is needed for purposes of impact analysis and planning. The chapter already tells us what the models say about the historic contribution for purposes of "closing the sea level budget". But we need the projections and historical projections to be pared, so that we know what the models say about the projected acceleration. Using a past example, if IPCC's low scenario said 10 cc by 2100, some people construed that as a deceleration given that the historic rise was 17 cm. But if that model said the historic rise was 5 cm, then actually, even the low implied a 5 cm acceleration, not a deceleration. The same concept might apply in reverse for the higher scenarios. Therefore, some impact analyses choose to focus on the acceleration implied by the models. (See e.g. Titus and Narayanan 1996, The Probability of Sea Level Rise. US Environmental Protection Agency for further explication of this logic; see the US Climate Change Science Program 2009 report "Coastal Sensitivity to Sea Level Rise" as an example assessment that focussed on acceleration. Note that acceleration is often more expedient than absolute rise when researchers have access to local rates of sea level rise based on tide guages since they can just add acceleration to those trends. [James G Titus, United States of America] | We note the positive comment, thank you. While the suggestion is reasonable, we reject it because the connection between historical and future simulations is not as close as the reviewer assumes especially because there is not a strong correlation between past and future forcings. However, the model-based simulation of the past rate of sea level rise matches observations well (see 13.4); the absence of a significant discontinuity between the past and the projections obviates some of this concern, which may arise from previous IPCC assessments. |
| 13-847 | 13 | 43 | 55 | | | "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, USA] | Rejected. This reference period is used consistently for projections. A multiannual mean is needed for comparison, rather than a single year, because the latter is affected much more strongly by unforced interannual variability. The RCP projections begin in 2006. |
| 13-848 | 13 | 43 | | 50 | | It is well justified to give more weight to model-based sea level rise projections than to the much higher semi-empirical projections. [Terje Wahl, Norway] | Noted. |
| 13-849 | 13 | 43 | | | | Section 13.6.1.2 Semi-empirical projections I found this section very informative and I can sense in the text a large and careful effort by the authors to provide a critically balanced background on semi-empirical models and associated projections. [Catia Motta Domingues, Australia] | Noted. |
| 13-850 | 13 | 44 | 9 | 44 | 9 | It is excellent to see time series of projections for the 21st century again after the "AR4 lapse" - planners and policymakers need these! [John Hunter, Australia] | Noted. |
| 13-851 | 13 | 44 | 10 | 44 | 10 | This sentence can be misleading, as it can be understood as that all central projections are below 0.05m. I would change to 'the spread in the central projections is 0.05m' [Eduardo Zorita, Germany] | Accepted. |
| 13-852 | 13 | 44 | 12 | 44 | 12 | Is the time span 1985 to 2005 inclusive of the end years or not. That is, is it from January 1985 to December 2005 (21 years), or from 1985.0 (00:00:00 on 01-Jan-1985) to 2005.0 (00:00:00 01-Jan-2005) - 20 years. This may seem a trivial point, but it causes confusion later on - e.g. for people comparing projections with observations after the report has been published. This also applies to other time spans that are quoted. Please state up front what the quoted time spans actually mean. [Neil White, Australia] | Accepted. Actually it should say 1986. That was a mistake. |
| 13-853 | 13 | 44 | 15 | 44 | 15 | Perhaps a little more emphasis should be placed on the fact that the central value of these projections now matches present observations reasonably well, which was not true of the TAR and AR4 projections (the observations were ~50% higher than the central values of the TAR/AR4 projections). This seems to me to be a very significant improvement. [John Hunter, Australia] | Accepted. |
| 13-854 | 13 | 44 | 26 | 44 | 27 | This sentence is incomprehensible. [Michael Oppenheimer, USA] | Taken into account by rewording. |

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| 13-855 | 13 | 44 | 29 | 44 | 34 | I think this comparison with AR4 should be explored a bit further, as at present it tends to suggest that there is a large increase in the projections from AR4 to AR5 (the central value for A1B increases by 50%). However, the "AR4" projection does not include the scaled-up ice sheet discharge, which for A1B is in the range (-0.01 - 0.13). If this is added to the AR4 A1B projection, it becomes 0.20 - 0.61 m, the upper limit being close to the AR5 value of 0.65, and the central AR5 value being only 26% higher than the central AR4 value. Also, the AR5 land-ice contribution (0.13 m) is about twice the central value of the scaled-up ice-sheet discharge for the AR4. I think it is a bit unfair (to the AR4) to say that: "The largest increase relative to the AR4 is from the land ice dynamics This term was largely omitted in the AR4" - the term wasn't omitted; it was merely underestimated by a factor of about 2. To me the take-home message is that the 95% upper limits are about the same, but the central values have increased (by about 26%) and the 5-95% range has decreased by a factor of about 0.68. [John Hunter, Australia] | Taken into account by enlarging the discussion, which has moved to 13.6.1.3. |
| 13-856 | 13 | 44 | 44 | 45 | 6 | The captions for figures 13.9 and 13.10 and the title for table 13.6 need to be changed to indicate that they were "produced with process-based models". The caption and titles will otherwise suggest that they are the only type of projections that can be developed based on our current scientific understanding, which is not the case. [Virginia Burkett, United States of America] | Accepted. |
| 13-857 | 13 | 44 | 49 | 45 | 1 | Explain how 5% and 95% sea level rise totals are obtained. They are not the some of the contributions - and should not be if these are not correlated. [Ian Allison, Australia] | Accepted; explained in the caption. |
| 13-858 | 13 | 44 | 49 | 45 | 1 | Table 13.6 is problematic. The most probable 95 year projection is simply a continuation of the current ~1.4 mm/year global average rate of SLR, which adds up to just 0.133 meters in 95 years. But in this table, the low end "5% likelihood" projection for the lowest SLR scenario in the table is twice that! That's absurd. [David Burton, USA] | Rejected. As stated in 13.6.1.3, it is very likely that the time-mean rate of global-mean sea-level rise in the 21st century will exceed that of recent decades. We have confidence in this assessment on physical grounds and because of the agreement of models and observations. The reviewer has apparently prejudged the magnitude of future sea level rise by disregarding the influence of expected anthropogenic climate change. |
| 13-859 | 13 | 44 | 49 | 45 | 1 | It is unlikely that the RCP projections should have tighter confidence intervals than those from the A1B scenario which has had the majority of the studies, and the RCP projections is based on the semi-empirical methods of 13.A. The forcing in RCP6 is almost like A1B, yet the greenland SMB contribution appears to be better known for RCP6 than for A1B. please explain. [Aslak Grinsted, Denmark] | Accepted. More explanation is needed in the Appendix, and a reference to the Appendix from the table caption. |
| 13-860 | 13 | 44 | 49 | 45 | 1 | Table 13.6: I don't recall the text explaining how the % uncertainties were derived [Robert Thomas, USA] | Accepted. A reference is needed to the Appendix, and a note has been added about uncorrelated uncertainties. |
| 13-861 | 13 | 44 | 49 | 45 | 1 | Please add the historic contribution from the comparable model for each of these projections. The historic total rise would be sufficient. [James G Titus, United States of America] | See 13-846. |
| 13-862 | 13 | 44 | 49 | | | In the text related to Table 13-6, some explanation of the calculation of the dynamical contribution should be given. It's too important to defer to an appendix. [Michael Oppenheimer, USA] | Accepted. More explanation has been included in 13.6.1. |
| 13-863 | 13 | 44 | | | | Plus, this table includes thermal expansion as if it matters, when, in fact, it has no significant effect on coastal sea levels. [David Burton, USA] | See 13-648. |
| 13-864 | 13 | 44 | | | | The intervals chosen are odd, too. Usually, 95% CIs are used (which conveniently equate to ~2xSD for normal distributions). But in this table the CI used is just 90%. [David Burton, USA] | Rejected. 5-95% CIs are used consistently in the AR5, as in the AR4. |
| 13-865 | 13 | 44 | | | | 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 | Taken into account by including a note in the caption about combination of uncorrelated uncertainties and a reference to the Appendix, where further detail is given. Some uncertainties are correlated, others are not. We do not correlate uncertainties in the formulation of SMB changes for Antarctic and |

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| | | | | | | used for the Greenland SMB and Antarctic SMB numbers. [David Burton, USA] | Greenland, because the former is dominated by accumulation and the latter by melting and refreezing, and because the patterns of regional climate change are not strongly related. |
| 13-866 | 13 | 44 | | | | 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, USA] | Taken into account. See 13-865. |
| 13-867 | 13 | 45 | 3 | 45 | 7 | note that Siddall et al Phil Trans. Subm. Covers a semi-empirical model for ice sheet contribution. A draft has been sent to the appropriate authors. Unlike the other S-E models, it does explicitly calculate ice sheet contributions [Mark Siddall, UK] | Noted. Not assessed because the paper is not current under review, in press or published. |
| 13-868 | 13 | 45 | 3 | | | Secton 13.6.1.2: Clearly, this section needs attention. While I don't necessarily disagree with discussion here, it seems to go into much greater detail of the faults of the semi-empirical projections than is necessary. In comparison, the shortcomings of the process-based model are glazed over. I think that the text in this section could be significantly tightened. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | The comment about process-based models has been taken into account by reorganising 13.4 in order to bring out its role in evaluating those models, and this evaluation is pointed out in 13.6.1.3. We note that the reviewer agrees with the discussion, but we do not wish to shorten it significantly because this is a controversial area in which all the arguments must be carefully laid out. |
| 13-869 | 13 | 45 | 3 | | | Section 13.6.1.2 is a reasonable and comprehensive review of the semi-empirical models. The attempt to explain why these models find a relatively high sensitivity of sea level rise to global temperatures is quite useful, and in fact it is what past assessments should have done to reconcile observations of sea level rise with what the models would have predicted. Perhaps you are a bit too kind to this analysis, in that there is essentially no theory to support these models. Consider the reasons why the temperature-projections section in a different chapter does not include a projection based on a historic regression of temperature on CO2 concentrations, or whether the authors of that chapter would be as kind to those (climate skeptics?) who have put forth such studies in the past. Perhaps you are more kind to semi-empirical than they would be because AOGCM's are better than ice-sheet dynamic models, so there is a greater need for a simplistic model here than there. Better to emphasize the relative need, than to leave open the possibility that such models are any better for projecting sea level than for projecting temperature. [James G Titus, United States of America] | Noted. The role of 13.6.1.2 is only to assess semi-empirical models. It is left to 13.6.1.3 to compare semi-empirical and AOGCM-based methods, and there we comment that the difficulty of developing reliable dynamical ice-sheets models is one of the motivations for using semi-empirical models. As the reviewer notes, there is a greater motivation for the use of such models for global sea level rise projections than for global temperature projections because of the poorer understanding embodied in currently available models for sea level. |
| 13-870 | 13 | 45 | 3 | | | The only omission I see in 13.6.1.2, is a discussion of the inherent limitations of time series regression analysis as a tool to estimate dynamic lags in relationships between an independent (e.g. temperature) and a dependent variable. Has Rahmstorf and others made the (unrealistic) assumption of an instantaneous response of sea level to temperature, the regression coefficients would have found an unrealistically low sensitivity. By playing around with different assumed functional specifications of the lagged response, one can derive all sorts of equilibrium sensitivities. And if one assumes that the change in sea level is a function of temperature, one has embedded a functional form guaranteed to over-estimate sea level rise at some point in the future. Needless to say, my comments are quite informal, but if you could find a way to treat this inherent limitation of time-series analysis, your review of these models will be even more complete. [James G Titus, United States of America] | Noted. It is our role only to assess the existing literature, not to carry out our own investigations. The aspect referred to is covered to some extent in the papers cited by Holgate, Rahmstorf and Schmith. |
| 13-871 | 13 | 45 | 3 | | | It might be worth pointing out somewhere in this section, that the semi-empirical models, while flawed, may be an appropriate way for planners to capture the complete range of uncertainty, while at the same time, for purpose of an assessment such as AR5, they do not significantly add to the knowledge base. [James G Titus, United States of America] | Noted. This issue is discussed in 13.6.1.3. |
| 13-872 | 13 | 45 | 5 | 47 | 13 | Semi-empirical models can give answers to questions like what is the difference in sea level between RCP26 and RCP85. This is unlike process models which rely on scenario independent estimates of the dynamic contributions. You should highlight this very important virtue of semi-empirical models. [Aslak Grinsted, Denmark] | Rejected. Our assessment is that predictions of this contribution cannot be made reliably from indicators of global climate change calibrated from the evidence of the 20th century, since the ice sheets were not responding to global climate change or greenhouse gas forcing, in contradiction to the reviewer's |

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| | | | | | | | assumption. In the SOD we have moved this point from 13.6.1.3 to 13.6.1.2. |
| 13-873 | 13 | 45 | 8 | 45 | 8 | I like 'physically motivated' and would add 'qualitatively argued' [Mark Siddall, UK] | Rejected. We think "physically motivated" conveys both senses. |
| 13-874 | 13 | 45 | 16 | 45 | 18 | You are correct to emphasize the need that motivated the semi-empirical models. But the casual reader might be left with the impression that they do "simulate recent acclerations in ice flow and..." and of course they don't do so either. The second reason, by contrast, does highlight something the semi-empirical models do. [James G Titus, United States of America] | Accepted and clarified. |
| 13-875 | 13 | 45 | 38 | 45 | 39 | For completeness, the duration of the calibration data for Grinsted et al. (2010) should be given (~last 2000 years). [John Hunter, Australia] | Accepted. |
| 13-876 | 13 | 45 | | | | Scrap section 13.6.1.1 Semi-Emperical Projections. It is discredited junk science. [David Burton, USA] | Rejected (assuming the reviewer means 13.6.1.2). It is essential to assess this relevant, prominent and widely cited body of literature. |
| 13-877 | 13 | 46 | 1 | 46 | 17 | Yes - there is sensitivity to both inputs and assumptions, as I am sure there is for practically every type of model used in the AR5. It is hardly surprising. This sensitivity leads to uncertainty, and that uncertainty is slowly mapped out as more and more studies uses different approaches and input data. It is simply not reasonable to expect any single study to map out the whole uncertainty. I think this is a biased critique of semi-empirical models, because it applies to virtually any kind of model projection. I suggest you delete the whole paragraph. [Aslak Grinsted, Denmark] | Taken into account to some extent by limited rewording to try to avoid the impression of bias, which is not the intention; our aim is to produce an objective assessment. |
| 13-878 | 13 | 46 | 3 | 46 | 3 | The Kemp et al. (2011) is a local sea-level reconstruction from North Carolina. It is NOT a GMSL dataset. [Roland Gehrels, United Kingdom] | Taken into account by noting that Kemp et al. suggest their record can be regarded as GMSL change with some uncertainty. |
| 13-879 | 13 | 46 | 7 | 46 | 8 | I don't understand why Rahmstorf et al. (2011) would assert that the "Church and White (2011) data imply model parameters that are inconsistent with reconstructed paleo-temperatures" (or even what this means). A little more explanation would be helpful. [John Hunter, Australia] | Accepted and clarified. |
| 13-880 | 13 | 46 | 13 | 46 | 15 | I would argue that our error covariance C-matrix estimate for sea level, is much more conservative than the error bars published for other GMSL estimates (where there is little information on the covariance). In fact i am pretty sure that our uncertainties are so flexible that they allow for these two other GMSL estimates. [Aslak Grinsted, Denmark] | Noted. However, we can assess only what is in the peer-reviewed literature. |
| 13-881 | 13 | 46 | 13 | 46 | 15 | Is this question a published analysis or an unsubstantiated hunch? - please provide a reference. [Aslak Grinsted, Denmark] | Taken into account by rephrasing. It is simply a statement that these datasets were not used. |
| 13-882 | 13 | 46 | 19 | 46 | 26 | Perhaps it is not such a bad approximation to scale up e.g. pumping with a proxy for anthropogenic activity. The alternative is to project it using the other statistical extrapolations that are done in section 13.5.5. (This is really a minor minor/misplaced criticism -especially considering that terrestrial storage adds up to ~zero in the observation period) [Aslak Grinsted, Denmark] | Noted. |
| 13-883 | 13 | 46 | 33 | 46 | 33 | the models either respond to radiative forcing or global temperature changes, not broadly to 'climate change' [Mark Siddall, UK] | Accepted. |
| 13-884 | 13 | 46 | 56 | 46 | 56 | Although Rahmstorf (2007) indeed claimed that the temperature variability in the ECHO-G simulation was mostly due to volcanic forcing, there is no study whatsoever to support that assertion, which thus can be considered as a pure invention by Rahmstorf. [Eduardo Zorita, Germany] | Rejected. Our text does not make any statement specifically about ECHO-G; we report Rahmstorf's argument as a general one. |
| 13-885 | 13 | 46 | 57 | 46 | 57 | Please point out that the von storch paper also demonstrates (rather unsurprisingly) using radiative forcing (instead of SAT) makes for a near ideal predictor for modelled sea level rise. This is used in Jevrejeva's recent semi-empirical papers. [Aslak Grinsted, Denmark] | Rejected. Von Storch et al. do not show a correlation with forcing. They show that there is a near-perfect correlation between the rate of ocean heat uptake (not radiative forcing) and the rate of thermal expansion. |
| 13-886 | 13 | 47 | 2 | 37 | 2 | "...the ablation area..." [Ian Allison, Australia] | Taken into account by rewording. |

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| 13-887 | 13 | 47 | 2 | 47 | 3 | This is true technically. But it will hardly be matter in 100 years. Try the glacier model in section 13.A with and without the Mg/M0 term. Compare the difference to the uncertainties. [Aslak Grinsted, Denmark] | Rejected. It makes a large difference (tens of percent) if the area scaling is omitted (Meehl et al., 2007). New references have been added. |
| 13-888 | 13 | 47 | 2 | | | will tend to decrease', this is only the case after all low-lying ice masses have disappeared (other wise it will increase, see comment 14), and before a generally increased ELA reaches the altitude of the main ice fields and icecaps. These ice masses will have a much higher sensitivity to warming than 'normal' shaped valley glaciers that are increasingly step towards their highest elevations. [Frank PAUL, Switzerland] | Rejected. The area loss is generally more important than the thinning. New references have been added. |
| 13-889 | 13 | 47 | 14 | 47 | 14 | There is a fifth reason why the semi-empirical models might overestimate sea-level rise in the 21st century. As heat mixes down in the ocean, it encounters colder water, which has a lower thermal expansion coefficient. The semi-empirical models effectively simulate total heat flux rather than sea-level rise, and are calibrated using 20th century (or earlier) data (when the effective thermal expansion coefficient may be relatively large). They are then used to project sea-level rise during the 21st century (when the effective thermal expansion coefficient may be smaller because the the heat has been mixed deeper in the ocean). Given that the semi-empirical models effectively assume that the thermal expansion coefficient is constant, they would therefore overestimate sea-level rise in the 21st century. [John Hunter, Australia] | Noted, but not acted upon because this point is not discussed in the existing literature. We suspect it would be a small effect. We note also that the expansion efficiency would increase, rather than decrease, if the heat uptake occurred predominantly at high latitude, since thermal expansivity increases with pressure. |
| 13-890 | 13 | 47 | 16 | 47 | 24 | Table 13.7 caption should explain the %, as 13.6 does [Robert Thomas, USA] | Taken into account. In the SOD the ranges of 13.6 are labelled as likely ranges rather than 5-95% ranges. Here, they are actually 5-95% ranges from the literature. |
| 13-891 | 13 | 47 | 21 | 47 | 21 | "2000-2099" should read "2090-2099". [John Hunter, Australia] | Accepted. |
| 13-892 | 13 | 47 | | | | Table 13.7: one wonders how useful the Rahmstorf et al. (2011) result is that is calibrated with proxy data, when they treat a local data set (Kemp et al. 2011) to represent global sea level. [Roland Gehrels, United Kingdom] | See 13-878. |
| 13-893 | 13 | 48 | 7 | 48 | 7 | "0.1-0.2 m" is the range of values given in the AR4 SPM, covering all emission scenarios. However, Table 10.7 of the AR4 give ranges for each scenario (-0.01 - 0.13 for A1B and -0.1 - 0.17 for A1FI) - perhaps these should be used here instead. [John Hunter, Australia] | Rejected, because we are giving here the range of upper bounds (note "maximum") from the AR4. |
| 13-894 | 13 | 48 | 10 | 48 | 10 | I haven't checked Nicholls et al. recently, but is it really 2.4 m? [Donald Forbes, Canada] | Noted. Yes, it is 2.4; that number is from paleo-evidence. See 13.6.1.3 for comment. |
| 13-895 | 13 | 48 | 27 | 48 | 32 | This is a repetition of what has already been said in 13.6.1.2 [Ian Allison, Australia] | Noted. It is a only a slight repetition. The first sentence summarises semi-empirical models; that is necessary for context. The second sentence is a new point relevant to the discussion of this section. |
| 13-896 | 13 | 48 | 45 | 48 | 46 | This sentence must be wrong... Are you seriously proposing that radiative forcing and ocean temperatures have little influence on ice sheet changes on long time scales, or has I misunderstood the intent behind the sentence? Obviously ice loss will depend on whether we choose RCP26 or RCP85? - This is also in conflict with polland and deconto's work where they show dramatic influence of ocean temperatures on the response of WAIS on long time scales. [Aslak Grinsted, Denmark] | Taken into account by modifying the statement to indicate we are talking about decadal variations specifically during the 20th century, not about relationships on longer timescales. |
| 13-897 | 13 | 48 | 49 | 48 | 55 | Last paragraph. This appears to be a rather biased representation of what is discussed in section 13.3.1.2. The 'reliable' rates mentioned are not at all given in 13.3.1.2. There is mention of such a type of rate in 13.3.1.3, but that is a section about the Late Holocene when sea level gradually approached the present level. Only 13.3.1.2 is about past interglacial rates when sea-level was above the present; that is, when ice volume was smaller than today, as indeed stated in the present paragraph's second sentence, then contradicted in the present paragraph's third sentence, and then again re-instated in the present paragraph's fourth sentence. Most surprisingly, the stratigraphically constrained (in contrast to multi-region compilations of 'loose' dated coral points) work of Thompson et al (2011) is not even taken into account here, or anywhere else in this chapter.... That study clearly demonstrates that 26 cm per century is the very MINIMUM rate, which is a well-constrained lower limit value that is already higher than the here presented 'reliable' values.... Note that in all | Taken into account. This will be revised following consultation with Chapter 5, where the actual assessment on this subject is being done. We have requested a copy of the paper referred to by Grant et al. |

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| | | | | | | <p>studies, the timing, and thus rates of the oscillations in the last interglacial are not 'best estimated' by establishing the time-span when sea-level was above 0 m. Instead, one needs to find dates for the time-span over which the oscillations were happening, excluding any plateaus (e.g., around 0 m). This is the main difference between the Red Sea estimates, which focus on the specific highstand oscillations which played within an interval of about 5 ky, even though sea-level was within error around or above 0 m over a longer period. That study directly admitted that it did not date both intervals very well in an absolute sense, but the distinction of interval duration was clear. For information, recent work has solved this issue. We have now constrained the Red Sea chronology by U-Th ages over the entire last 150,000 years (Grant et al., in review with Nature); the peak highstand with its oscillations sits between 131 and 126 ka, while the general conditions at (within 85% confidence limits) or above 0 m extend over a twice as long period of 131 to 121 ka. Rates of rise in the oscillations above 0 m were 1-1.5 m/century. I'm not advocating that this should be used, but it does give a good indication that the most 'reliable' estimates of 0.1-0.25 m/century in the present paragraph are not so reliable at all, as was shown already by the excellent work for constraining the minimum rate of rise in the oscillations by Thompson et al. (2011).</p> <p>Any study that uses coral datings (or any other sea-level tool) without having a strict stratigraphy is highly likely to miss or underrepresent the oscillations, and so to underrepresent the rates of rise. I advocate a rethink of the presentation here, and that the rates presented here are presented as 'at least' estimates.</p> <p>[Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland]</p> | |
| 13-898 | 13 | 49 | 3 | 49 | 3 | I think you need to briefly expand on the 'scenario of Hansen 2007' or reduce the statement to: '...physically untenable such as the semi-qualitative argument of Hansen 2007.' or even '(I.e. ruling out the large values implied by Hansen 2007)' [Mark Siddall, UK] | Accepted. We have described Hansen's argument as "heuristic". |
| 13-899 | 13 | 49 | 13 | 49 | 27 | I find this paragraph very heavy going. Perhaps it could be reorganised and/or broken up? [Neil White, Australia] | Taken into account by focussing the paragraph on the bounds, rather than the likely ranges. |
| 13-900 | 13 | 49 | 19 | 49 | 22 | The explanation of confidence, or lack thereof, should be amplified to explain more fully the evidence leading to this judgment. [Michael Oppenheimer, USA] | Taken into account by simplifying the paragraph, in order to make it clearer that the next paragraph explains the basis for confidence, and pointing out that the projections are consistent with those for SAT change in chapter 12. |
| 13-901 | 13 | 49 | 21 | 49 | 22 | If the authors have only medium confidence in their assessment, they should probably redo it. I assume however that they rather have medium confidence in the published literature or available studies or similar. [Uwe Stoeber, Germany] | Taken into account by rephrasing. We have medium confidence in our likely ranges. This is IPCC phraseology. |
| 13-902 | 13 | 49 | 29 | 49 | 41 | In this paragraph you concisely describe the factors that restrict confidence in results of process-based models. The most important constraint in the process-based models may be their inability to capture rapid ice sheet change, which is why the semi-empirical models should be considered in their own class of "projections". Is the consensus about the reliability of process-based models so much higher than the consensus revealed in the recent literature that suggests that the rate of sea level rise was grossly underestimated in AR4? In AR4 there were very important statements about sea level rise being potentially higher than in the table that contained the future projection up to 0.59 m. A different type of mistake is being made in the chapter 13 draft by presenting the WG1 "projections" that rely totally on the process-based approach. Please remember that coastal communities and resource managers will consider your table the "consensus" of international scientific opinion. The table of AR5 sea level rise projections will become the basis for regulations, policy, and adaptation. Placing all of our confidence in one approach, which has the short-comings that you cite in this paragraph, seems risky. [Virginia Burkett, United States of America] | Rejected. We are particularly sensitive to this issue and have given it careful consideration. We are not placing "all of our confidence in one approach". The higher projections of the semi-empirical models are an important reason why we have only medium confidence and give likely (rather than very likely) ranges. Another reason is our low confidence in projections of ice-sheet dynamical change on these timescales. In fact, as we state, there is not a consensus in the community about the reliability of semi-empirical models. There is much greater agreement about projections of the contributions, apart from ice-sheet dynamics, but even in that area it is now possible to give quantitative projections. Because this was not possible at the time of the AR4, they were omitted from the ranges given; the AR4 did not underestimate their importance, but was not able to quantify them at all. We are able to quantify them, although with large uncertainties, which we have assessed; we also point out, like the AR4, that larger |

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| | | | | | | | sea level rise cannot be ruled out. |
| 13-903 | 13 | 49 | 34 | 49 | 35 | Further explanation would be helpful on ".....that quantifies observational constraints or the effect of carbon-cycle uncertainties on GMSL". I don't really understand what either of these mean but realise that this may be fixed by the indicated Placeholder which should contain an explanation to "reconcile the treatments of Chapter 12 and Chapter 13". [John Hunter, Australia] | Accepted. We have amplified the statement somewhat, but reference is needed to chapter 12 for details. The likely ranges for GMSL rise in the SOD are derived from those for SAT change, unlike in the FOD i.e. we have "reconciled the treatments". |
| 13-904 | 13 | 49 | 40 | 49 | 40 | add '...or indeed that the semi-empirical models are not reliable.' [Mark Siddall, UK] | Accepted. Text modified by stating this alternative in other words. |
| 13-905 | 13 | 50 | 7 | 50 | 17 | The approach described here is NOT for independent variables (as mentioned in l. 11) but 1st order error propagation for perfectly correlated errors. [Uwe Stoeber, Germany] | Accepted. The text has been changed accordingly. |
| 13-906 | 13 | 50 | 11 | 50 | 17 | One reason why they must be interdependent is that they rely on the same model formulations/parameterisations, and if there is a bias in e.g. the calving law, or the SIA/SSA, then it most likely will bias both ice sheets with the same sign. [Aslak Grinsted, Denmark] | Noted. Based on the model results presented, the uncertainty is thus rather overestimated than underestimated. This is now noted in the text. |
| 13-907 | 13 | 50 | 12 | 50 | 14 | This sentence seems jumbled. [Michael Oppenheimer, USA] | Noted |
| 13-908 | 13 | 50 | 14 | 50 | 14 | First word "where" should be "were". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Corrected. |
| 13-909 | 13 | 50 | 38 | 50 | 38 | "outlets" should be "outlet". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Corrected. |
| 13-910 | 13 | 50 | 39 | | | increasingly less contact with the ocean': see my comment 22, will the interior of Greenland not be filled with water through narrow channels such as below Jacobshaven Isbrae? In such a case the perimeter in contact with water might extremely increase. [Frank PAUL, Switzerland] | Noted and partially accepted. Most of the interior lies on bedrock less than 200 m below sea level with only few isolated connections to the ocean, in which case these areas would tend to act as lakes with a finite ability to store ice bergs etc. We will provide an additional comment in the SOD. |
| 13-911 | 13 | 50 | 44 | 50 | 44 | "models" should be "model's". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Corrected. |
| 13-912 | 13 | 50 | 54 | 50 | 54 | "GMT" hasn't been defined. [Neil White, Australia] | Corrected. |
| 13-913 | 13 | 50 | 54 | | | Please introduce GMT. [Uwe Stoeber, Germany] | Corrected. |
| 13-914 | 13 | 51 | 5 | 51 | 6 | Table 13.8. The 2500 values for glaciers require more than the footnote a. A value of 17 cm for high scenario compared to 13-40 for 2300 needs further explanation. It sort of implies (??) that GIC will lose a maximum of 4 cm in 200 yrs from 2300-2500 in the high scenario. This seems implausible. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted. The text will be revised accordingly. |
| 13-915 | 13 | 51 | 5 | 51 | 8 | A column for 2100 is necessary for comparison with previous tables. This is needed in order to gain confidence in the long term projections, and also to better gauge where "low" is compared to RCP26. [Aslak Grinsted, Denmark] | Accepted. The column will be introduced. |
| 13-916 | 13 | 51 | 20 | 51 | 20 | "contribution" should be "contributions". [Eelco Johan ROHLING, United Kingdom of Great Britain & Northern Ireland] | Accepted - text revised |
| 13-917 | 13 | 51 | 23 | | | Section 13.7 I am not sure what "region" means. I suppose the term "region" is vague on purpose, so that "regions" can be as large as the authors wish in each chapter, but I am missing some references to "smaller" regions such as the Mediterranean Sea or the Japan Sea or Northern sea, where interesting examples of this regional variability can be found . [Belén Martín Míguez, Spain] | Accepted - text revised to include a better explanation. |
| 13-918 | 13 | 51 | 25 | 51 | 25 | I don't think one can say that this issue is really "discussed" throughout Chapter 3, even though I do agree that it is convenient to make this kind of cross-references between sections of the report. I would suggest to be more specific "As mentioned in Section 3.7" [Belén Martín Míguez, Spain] | Accepted - text revised. There is now a different mix of information about sea level in Chapt. 3 and 13. |

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| 13-919 | 13 | 51 | | | | Section 13.7 Regional sea level changes [Catia Motta Domingues, Australia] | Editorial |
| 13-920 | 13 | 51 | | | | One of the major issues raised in past IPCC reports (eg, TAR) is the spread of the 20th century regional patterns in the various model simulations. This topic has not been discussed in this section and it was only quickly mentioned that it remains uncertain in the synthesis and key uncertainties section (13.9). There seems to be no progress in this topic over the last 10 years. Why is that so? What directions should be taken in science which would help to reduce this uncertainty? [Catia Motta Domingues, Australia] | Accepted. This topics is now being discussed in detail based on new results emerging from CMPI5. |
| 13-921 | 13 | 51 | | | | By reading the previous IPCC reports in sequence (just before reviewing this chapter), I realised that the sections on "How to reduce uncertainties" might have played a large role in steering scientific directions that helped progress the sea level science reported in the following assessments. [Catia Motta Domingues, Australia] | Noted. |
| 13-922 | 13 | 52 | 12 | 52 | 12 | First of all, the reference should be Becker et al.2012, not 2011. Besides, Becker et al. 2012 in their abstract they talk about the subdecadal (not multidecadal) ENSO signature for the whole western tropical Pacific region. As far as I understand, the sea level rise at Tuvalu, despite being highlighted, is not explained as a consequence of ENSO in their paper. [Belén Martín Míguez, Spain] | Accepted text revised |
| 13-923 | 13 | 52 | 15 | 52 | 21 | This paragraph needs some re-thinking in my opinion. Regional sea level changes are simply not mainly thermosteric during recent decades. This makes no sense and goes against the evidence from GRACE (e.g. Chambers, D.P., 2011. ENSO-correlated fluctuations in ocean bottom pressure and wind-stress curl in the North Pacific. Ocean Science Discussions, 8(4), pp.1631-1655.). In the Pacific the regional changes over the past two decades are clearly wind driven (e.g. Merrifield and Maltrud, 2011). If mass redistribution played no part in regional sea level change then we would have to conclude that all mass that enters the ocean (50-75% of recent sea level change) is evenly distributed over the globe, which seems unlikely. Bottom pressure will equalise very quickly but dynamic changes will occur over longer timescales. Hence, the regional sea level change may have a thermosteric signature, but it is not thermosteric in origin. The effect of the freshwater forcing is actually highlighted in section 13.7.3.1. [Simon Holgate, UK] | Accepted - text revised accordingly. |
| 13-924 | 13 | 52 | 15 | 52 | 21 | I agree that regional sea level trend shows a special trend according to each sea or basins, and thermosteric changes are dominated. It will be better to include more regional cases for several regional seas or basins to understand the patterns in more regions. As a special case for regional sea level rises in the marginal basin sea of the Northwestern Pacific the sea level rise in the East/Japan Sea shows that sea level rise of 5.4+10.3mm/yr in early satellite era from 1993 to 2001 is about two times GMSL and thermosteric sea level rise explains more than 80% in southern East/Japan Sea (Kang et al. 2005, JGR. 110,co7002, doi:1029/2004JC002565). It may be one of special examples of regional sea level rise. [Sok Kuh Kang, South Korea] | Accepted - text revised by including the suggested regional example. |
| 13-925 | 13 | 52 | 19 | 52 | 19 | I miss a reference here, after "thermosteric changes". Landerer 2007, maybe??? (already in the reference list) [Belén Martín Míguez, Spain] | Accepted - text revised |
| 13-926 | 13 | 52 | 41 | | | Section 13.7.2: In the beginning of the section it is mentioned that regional sea level changes depend on wind forcing and on changes in the ocean heat and freshwater content, whereas air pressure forcing is mentioned only at the end of the section - almost as an afterthought. Air pressure may considerably influence regional sea levels, as was, for example, the case in the Mediterranean in the second half of the 20th century (e.g., Tsimplis M. N. et al., 2005: Mediterranean sea level trends: atmospheric pressure and wind contribution, Geophysical Research Letters, 32). It seems that the section would be much more clear if all the forcing agents are mentioned in the beginning and then discussed in turn. [Mirko Orlic, Croatia] | Accepted - text revised |
| 13-927 | 13 | 52 | 43 | 52 | 43 | I don't understand what is meant by "only the dynamical component of sea level". [John Hunter, Australia] | Accepted - text revised |
| 13-928 | 13 | 53 | 2 | 53 | 2 | The word "CMP5" is spelling mistake,should be "CMIP5". [Juncheng ZUO, China] | Accepted - text revised |
| 13-929 | 13 | 53 | 3 | | | Based on Fig. 13.12a I would rather assume a RMS of 60 to 70mm, i.e. much below 10cm [Uwe Stoeber, Germany] | Accepted - text revised |
| 13-930 | 13 | 53 | 52 | 53 | 52 | "...as obtained from a ..." [Simon Holgate, UK] | Accepted - text revised |

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| 13-931 | 13 | 54 | 8 | | | I would prefer to avoid mentioning "inverted barometer effect" and refer instead to "atmospheric pressure loading of the ocean". The inverted barometer effect is an old-fashioned concept. [Ernst Schrama, Netherlands] | Accepted - text revised |
| 13-932 | 13 | 54 | 10 | 54 | 11 | Millibars not SI (then again nor is 'yr' or 'kyr'). Perhaps give SLE. [Donald Forbes, Canada] | Editorial |
| 13-933 | 13 | 54 | 13 | 54 | 50 | In both paragraph or in references (Stammer 2008; Stammer et al, 2011), response time scales from several years to several months, for instance, to Pacific, are quite different from each other. In lines 44-45 it is described that Enso-like response in the Pacific within a few months. It would be convincing if related references can be added to show any observational evidence, supporting this quick telecommunication. However, I admit that such an observational evidence or similar result from related works is not necessary required in present report. [Sok Kuh Kang, South Korea] | Rejected; no observational evidence does yet exist. |
| 13-934 | 13 | 54 | 15 | 54 | 15 | Add 'SLR' after 'rise' [Donald Forbes, Canada] | Accepted - text revised |
| 13-935 | 13 | 54 | 15 | 54 | 17 | There is scope for confusion here - Stammer and Hutterman talk about inverse barometer-type loading of the ocean surface, but elsewhere in this chapter there is talk about loading in the geophysical sense (GIA etc). Could this be clarified? [Neil White, Australia] | Accepted - text modified |
| 13-936 | 13 | 54 | 27 | 54 | 28 | "... which to first order will not project on GMSL" - Can this be stated in a simpler way? [Donald Forbes, Canada] | Accepted - text modified |
| 13-937 | 13 | 54 | 32 | 55 | 5 | Addressing increased freshwater forcing due to ice sheet mass imbalance, the cited studies (e.g., Stammer 2008, Stammer et al., 2011) feature model experiments in which additional runoff is specified, adjacent to an ice sheet (Greenland or Antarctica). While useful, these studies are somewhat idealized, as a substantial fraction of the ice sheet imbalance is associated with increased iceberg calving. The freshwater associated with icebergs is redistributed in the ocean quite differently from that associated with meltwater. This could be noted, if not in section 13.7.3.1, then elsewhere (see comment 11) [Robert Marsh, United Kingdom of Great Britain & Northern Ireland] | Accepted - text revised |
| 13-938 | 13 | 54 | 34 | 54 | 35 | Also loss of sea ice. [Donald Forbes, Canada] | Rejected - sea ice does not contribute to sea level changes directly. |
| 13-939 | 13 | 54 | 35 | 54 | 36 | While the information about the increase in freshwater is communicated around the ocean basins in days, the rise in the global mean sea level is instantaneous. [Simon Holgate, UK] | Accepted - text revised accordingly. |
| 13-940 | 13 | 54 | 39 | 54 | 39 | Kawase (1987), Cane (1989) and Johnson and Marshall (2002) demonstrated the communication of changes in the N Atlantic circulation prior to Stammer (2008). [Simon Holgate, UK] | Accepted - references included |
| 13-941 | 13 | 54 | 39 | 54 | 40 | I think the reference to Stammer, 2008, should be dropped as it is very misleading. According to the paper Lorbacher et al this mechanism accounts for only 3.5% of the of the expected rise one sees if we include the actual added mass. That it might take a long time for the ocean to equilibrate I think is irrelevant if the change is so tiny. In fact the statement as it stands is actually incorrect in that a complete baroclinic adjustment will never occur, it will always be transitory as long as we can measure carefully enough. I think that the Stammer 2008 result is terribly misleading and could potentially damage the credibility of the chapter if left in. [Howard J. Freeland, Canada] | Rejected - The Stammer (2008) paper is widely known and has to be assessed. The paper refers to the steric component which is important for the regional distribution of sea-level change. |
| 13-942 | 13 | 54 | 47 | 54 | 48 | Deos fresh water in "Fresh water input to the North Atlantic" denote Greenland ice melting input? It would be better to add freshwater source, considering the audience who do not read the references. [Sok Kuh Kang, South Korea] | Accepted - text revised |
| 13-943 | 13 | 54 | 53 | 54 | 53 | "The combination of this dynamic sea level rise ..." - but this may be counteracted by gravitational fingerprinting if the greater part of SLR comes from the GIS. [Donald Forbes, Canada] | Accepted - text modified |
| 13-944 | 13 | 55 | 5 | 55 | 5 | This statement neds a reference [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account |
| 13-945 | 13 | 55 | 7 | 55 | 7 | make clear the contibution to this modelling of paleo sea-level data [Mark Siddall, UK] | Rejected - the subject is specifically related to contemporary, and there are sufficient references |

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| | | | | | | | provided on this general subject. |
| 13-946 | 13 | 55 | 9 | 55 | 11 | What is "fingerprints" ? The description is not clear enough. [Juncheng ZUO, China] | Accepted, text revised by including an explanation. |
| 13-947 | 13 | 55 | 9 | 55 | 24 | Bamber & Riva 2010 calculated the RSL fingerprint of all contemporary land ice melt and each major component. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Taken into account |
| 13-948 | 13 | 55 | 9 | 55 | 24 | Gomez et al, 2010a and 2010b are the same paper. Riva et al., 2010a and 2010b are the same paper. Perhaps this paragraph could be condensed a bit (reduction in the references to the same papers). [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted - references corrected. |
| 13-949 | 13 | 55 | 13 | 55 | 15 | Slangen et al., 2011, doi: 10.1007/s00382-011-1057-6, present rates that are based on a certain amount of ice sheet mass loss. [Aimee Slangen, Netherlands] | Accepted - text revised |
| 13-950 | 13 | 55 | 28 | 55 | 28 | Ditto. They also show what it would look like for 4xmelt compare to GIA etc. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Noted |
| 13-951 | 13 | 55 | 32 | 55 | 35 | The statement of regional sea level change is too broad. Some examples of typical regional sea can be given. [Juncheng ZUO, China] | Accepted - text modified |
| 13-952 | 13 | 55 | 47 | 55 | 47 | Has 'SSH' been defined? [Donald Forbes, Canada] | Accepted, text revised. |
| 13-953 | 13 | 55 | 49 | 55 | 50 | Perhaps not for regions such as eastern Canadian Arctic close to the GIS. [Donald Forbes, Canada] | Accepted - text revised |
| 13-954 | 13 | 56 | 24 | 56 | 24 | "... in some regions land uplift is significant ..." - Tide gauges such as Churchill or Helsinki (PSMSL) and numerous papers showing raised marine features - (e.g. St-Hilaire-Gravel et al. 2010. Arctic 63 (2), 213-226). A critical issue for many Arctic communities is whether (when) accelerated SLR will overtake uplift, leading to a switch from emergence to submergence (State of the Arctic Coast 2010 - www.arcticcoasts.org). [Donald Forbes, Canada] | Accepted - text expanded. |
| 13-955 | 13 | 56 | 27 | 56 | 27 | What does the "RSL" mean? Can't find the full name of it. [Juncheng ZUO, China] | Editorial |
| 13-956 | 13 | 56 | 32 | 56 | 38 | Ditto. They show that for present day, and predicted future (see fig 13.9; 50% of GIC melt is in the Arctic) land ice melt the gravitationally consistent signal is enhanced in the Western Pacific and Indian Oceans and that this RSL pattern is stationary for a given ice melt distribution. This is important for the Atolls and islands in this region. Their euastatic contribution for 2000-2009 was estimated at 1.4 mm/yr which is broadly consistent to the 1.49 mm/yr obtained by Jacob et al (Nature 8th Feb) for 2003-2010. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted - text expanded. |
| 13-957 | 13 | 56 | 41 | 56 | 41 | Figs 13.14 and 13.15. I realise these are published results but without reading the whole paper carefully it is easy to misinterpret these as providing an indication of what the future RSL pattern might look like. This is not the case as the authors have used AR4 ice sheet estimates (small) mixed with updated glacier values. Caution is required, therefore, in interpreting these plots. [Jonathan Bamber, United Kingdom of Great Britain & Northern Ireland] | Accepted - Figs. Modified and text revised |
| 13-958 | 13 | 56 | 49 | 56 | 49 | Australian Bureau of Meteorology and CSIRO, 2011 - missing reference. [Donald Forbes, Canada] | Editorial |
| 13-959 | 13 | 57 | 20 | 57 | 30 | crease -> "increase" [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Editorial |
| 13-960 | 13 | 57 | 22 | 57 | 22 | the GIA/rotational/gravitational effects are also discussed in this section but there is no equivalent discussion of uncertainty/sensitivity...in general 13.7 is not well threaded together between the different aspects of regional sea level considered [Mark Siddall, UK] | Accepted - text expanded. |
| 13-961 | 13 | 57 | 22 | 57 | 55 | In addition to ocean/climate model formulations and parameterizations, uncertainty (in predictions of sea level change) may be associated with specified freshwater forcing. Whether or not an ocean model is coupled with an ice sheet model, the forcing should distinguish between runoff and iceberg flux. The only attempt (so far) to explicitly represent iceberg drift and melting in a fully-coupled climate model is reported by Martin and Adcroft (2010). This limitation of contemporary simulations could be noted, with reference to the new scheme. | Accepted - text expanded. |

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| | | | | | | Reference: Martin, T., and A. Adcroft (2010). Parameterizing the fresh-water flux from land ice to ocean with interactive icebergs in a coupled climate model. Ocean Modelling, 34, 111-124. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland] | |
| 13-962 | 13 | 57 | 29 | 57 | 29 | Problems with knowledge of air-sea fluxes should be added to this list. [Simon Holgate, UK] | Accepted - text revised |
| 13-963 | 13 | 57 | 57 | 62 | 35 | The discussion about surges and wind generated waves looks into smaller scale processes, while the chapter discusses sea level change at larger time scales. It seems more appropriate to have this discussion on waves and storm surges in Chapter 3, while the discussion about sea level change rised in Chapter 3 should be concentrated in Chapter 13. [Eduardo Siegle, Brazil] | Chapter 3 describes observations, while Chapter 13 deals with 21 st century projections |
| 13-964 | 13 | 57 | 57 | | | Section 13.8: The discussion of sea level extremes concentrates on extratropical and tropical storms as the key drivers of extreme events, tides are briefly mentioned, but the other atmospherically driven contributions are ignored. One of those is sea level variability driven by planetary atmospheric waves (e.g., Pasarić M. et al., 2000: Response of the Adriatic sea level to the air pressure and wind forcing at low frequencies (0.01-0.1 cpd), Journal of Geophysical Research, 105), which is pronounced in mid-latitudes and could be even more important under a higher GMSL because it may be expected to support the flooding events lasting much longer than the events related to travelling cyclones. The other contribution is sea level variability generated by mesoscale atmospheric perturbations (see Orlic M. et al., 2010: Fresh evidence relating the great Adriatic surge of 21 June 1978 to mesoscale atmospheric forcing, Journal of Geophysical Research, 115, and references cited therein), which is responsible for some of the most dramatic flooding events around the world; since these events are usually local in character, the authors may decide that the process is not of interest for the present report, but it should then be at least mentioned in Section 13.1 (together, for example, with tsunamis). [Mirko Orlic, Croatia] | Noted. However, the present section 13.8 of the chapter focusses mostly on 21st century projections |
| 13-965 | 13 | 57 | | | | Figure 13.16 : How come that the projections are not illustrated with interannual variability as it is done with the current SLR? [Valentina Radic, Canada] | Accepted. We are considering how we should include the interannual variability in the projections in a readable manner. (Note - this comment was misplaced and has been moved here) |
| 13-966 | 13 | 58 | 11 | 58 | 13 | similar to comment #8 [Pavel Tkalič, Singapore] | Not able to track down this comment |
| 13-967 | 13 | 58 | 15 | 58 | 17 | Until I came to this line, I was going to make a comment that the potential for tidal amplification with SLR had been overlooked. Can be very significant in places like the Bay of Fundy (e.g. Shaw et al. 2010. Can J Earth Sci. 47 (8), 1079-1091; Shaw & Ceman. 1999. The Holocene, 9 (4), 439-451; Gehrels et al. 1995. Marine Geology, 124, 71-85). [Donald Forbes, Canada] | Rejected. Changes in tides also could be anthropogenic activities, such as dredging, changes in local topography etc. Moreover, this chapter is on projections. |
| 13-968 | 13 | 58 | 21 | | | Closing parenthesis at sentence end is missing [Ernst Schrama, Netherlands] | Noted |
| 13-969 | 13 | 58 | 49 | | | Section 13.8.2.2: Numerical projections often overestimate the central pressure of tropical cyclones (TCs) owing to the spatial resolution of the models and so on. Therefore, the quantitative projection of future TC-induced storm surges is difficult. [Hiroyasu Kawai, Japan] | This is a finding reported and discussed in Chapter 14. We have made a statement that site-specific tropical cyclone projections have low confidence. |
| 13-970 | 13 | 59 | 22 | 59 | 22 | Who is "they" that showed? [Simon Holgate, UK] | Noted and clarified |
| 13-971 | 13 | 59 | 22 | | | Who is 'they'? [Uwe Stoeber, Germany] | Noted and clarified |
| 13-972 | 13 | 59 | 36 | 59 | 36 | "CM2.1 projection" needs to be defined. I presume this is the projection from the GFDL CM2.1 climate model. Regardless, this term needs definition. [Stephen Griffies, USA] | Noted and explained |
| 13-973 | 13 | 59 | 55 | | | Please be more specific about the scale parameter. What is its physical meaning? What does it represent? [Uwe Stoeber, Germany] | We have removed this sub-section. |
| 13-974 | 13 | 60 | 45 | 60 | 50 | Mori (2012) discussed the future tropical cyclone projections for extreme wind impact based on stochastic tropical cyclone models with global climate change effects. Based on the analysis of general circulation models and stochastic tropical cyclone models, both a decrease in cyclogenesis frequency and changes in | Noted and cited in the text |

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| | | | | | | track have regional impacts to annual tropical cyclone frequency and 100 years extreme winds. Mori, N. (2012) Projection of Future Tropical Cyclone Characteristics based on Statistical Model, In Cyclones Formation, Triggers and Control, Eds. K.Oouchi and H.Fudeyasu, Chapter 12, Nova Science Publishers, 24p., in press. [Nobuhito Mori, Japan] | |
| 13-975 | 13 | 60 | 52 | | | Section 13.8.3.2: Is there any prediction of change in wave period and direction? These parameters depend on the projected wind fields and affects the safety of coastal areas. [Hiroyasu Kawai, Japan] | We now have a new figure which clarifies changes in wave period and direction |
| 13-976 | 13 | 62 | 1 | 62 | 5 | These sentences contradict Chapter 4 and ignore the importance of melt processes in Greenland. [Eric Rignot, USA] | We have removed this sub-section. |
| 13-977 | 13 | 62 | 4 | | | Section 13.8.3.3: Increase in river discharge can be one of important factors for coastal inundation in some areas, such as the Bay of Bengal. [Hiroyasu Kawai, Japan] | We have removed this sub-section. This is moved to Chapter 5 of WG II |
| 13-978 | 13 | 62 | 37 | 63 | 50 | Section 13.9 needs work. It's the place where the uncertainties need to be summarized so that those reading the report don't get anchored in numbers which don't have a strong basis. Tables like 13-6 and 13-8 need to be revisited with a more complete discussion of the factors which could lead to changes in future assessments. For example, the uncertainty related to ice dynamics is related to one sentence (p.63, line 25-26). [Michael Oppenheimer, USA] | In the second order draft, a new figure (Fig.13.8) will be included, which will clarify changes in wave period and direction |
| 13-979 | 13 | 62 | 37 | | | Section 13.9: The same comment as on Section 13.6.1. [Mirko Orlic, Croatia] | Accepted - section to be rewritten |
| 13-980 | 13 | 62 | | | | Section 13.7.4 Net regional SSH Sorry it is not clear to me what is exactly meant by net SSH changes. [Catia Motta Domingues, Australia] | Accepted - section to be rewritten |
| 13-981 | 13 | 63 | 12 | 63 | 12 | this will confuse the reader. How about 'Part of the reponse of oceans, glaciers and ice sheets will occur on time scales longer than one century.' the point is that all of these systems have both longer and slower responses [Mark Siddall, UK] | Accepted - section to be rewritten |
| 13-982 | 13 | 63 | 14 | 63 | 16 | The important point that Antarctica is expected (based on Table 13-6) to make a positive contribution to sea level rise for most combinations of SMB and dynamics is thoroughly obscured in this sentence. [Michael Oppenheimer, USA] | Accepted - section to be rewritten |
| 13-983 | 13 | 63 | 25 | 63 | 25 | I think 'relevant independent case studies' or 'independent examples' would be better than 'analogues' [Mark Siddall, UK] | Accepted - section to be rewritten |
| 13-984 | 13 | 63 | 49 | 63 | 49 | older paleo evidence indicates that even higher sea levels are possible and these higher sea levels are commensurate with the loss of the WAIS and marine based sectors of Greenland and the EAIS' the point to emphasise is that the paleo estimates are commensurate with the total loss of less stable bits of the existing ice sheets [Mark Siddall, UK] | Accepted - section to be rewritten |
| 13-985 | 13 | 64 | 1 | 65 | 48 | FAQ 13.1: The language and explanations in this FAQ are well tailored to a non-specialist audience - I have no specific changes to suggest. [David Wratt, New Zealand] | Noted. |
| 13-986 | 13 | 64 | 2 | 64 | 4 | The reason local sea level is different from the supposed "global average" is that the "global average" is both incorrect and lacking information on its unreliability. People should rely on what they can actually see and experience and ignore the advice of the people associated with this report who are obsessed with trying to justify their completely unrealistic climate models [VINCENT GRAY, NEW ZEALAND] | Rejected. Observations clearly demonstrate that regional variations are important. The assertions about the global average are unsubstantiated. |
| 13-987 | 13 | 64 | 5 | 64 | 7 | The sentence "For example of sea level change" is virtually a restatement of the previous sentence ("Local sea level change uniform signal"). [John Hunter, Australia] | Taken into account. This text has been revised. |
| 13-988 | 13 | 64 | 10 | 64 | 18 | The description is clear. However, the corresponding FAQ 13.1 Figure 1a is too small for some general spatial pattern with skipped scale bar to be seen. I recommend that time series data need to be shown separately, or in the lower half of the page. [Sok Kuh Kang, South Korea] | Taken into account. Fewer tide gauge records will be shown and the figure reformatted to maximise the size of the map in Fig. 1a. |

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| 13-989 | 13 | 64 | 27 | 64 | 29 | I think what this means is that the tide gauge rate over the longer time span of the tide gauge record is substantially lower than the altimeter (or tide gauge) rate over the shorter altimeter period. This is exactly what we would expect. Please clarify this. [Neil White, Australia] | Taken into account. Text revised to make this point explicit. |
| 13-990 | 13 | 64 | 30 | 64 | 30 | changes that -> "changes than" [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted - text revised. |
| 13-991 | 13 | 64 | 33 | 65 | 13 | These two paragraphs are confusing and/or repetitive. Are the two important contributions to sea floor movement: (1) steady ductile deformation in the first paragraph and (2) GIA in the second the same, or different. I suspect that they are the same - please combine and rewrite these two paragraphs. Please also use the accepted terminology of GIA (Glacial Isostatic Adjustment) where appropriate. [Neil White, Australia] | Taken into account. The text has been shortened and simplified. |
| 13-992 | 13 | 64 | 56 | 65 | 2 | While you have said earlier that your are talking about regional and global scales, there are some who take this to mean that because GIA models do not describe the vertical uplift observed by GPS, the GIA models are obviously deficient. However, in many far-field locations, even a "perfect" GIA model would not explain the GPS observation due to local processes which frequently contribute to the observations. Thus, I worry that the word "dominant" encourages this wrong train of thought. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Taken into account. The word "dominant" removed. |
| 13-993 | 13 | 64 | | | | FAQ 13.1, Fig 1: Please consider a figure layout to optimize page space. 1.5 pages for three panels will not be difficult. [Thomas Stocker/ WGI TSU, Switzerland] | Noted. |
| 13-994 | 13 | 65 | 18 | 65 | 21 | If sea level rising pattern due to melting of Greenland and Antarctic ice sheet occurs in FAQ Figure 1c, Such a pattern probably can be reflected in GMSL pattern (such as FAQ Figure 1a), since recent melting is dominating and several months are enough for the melting signal to be seen globally (in comment number 6 below, referenced in Stammer et al., 2011). However, such a pattern, even though it is rough, is not seen at a first glance for FAQ Figure 13.1a. If signal is small, special analysis should show it. From this sense, can it be said that the result of FAQ Figure 13.1c is accurate or convincing enough? [Sok Kuh Kang, South Korea] | Taken into account. Text added to clarify that the ice melt signal not apparent in Fig. 1c due to the dominance of oceanographic variability at decadal time scales. |
| 13-995 | 13 | 65 | 51 | 67 | 35 | FAQ 13.2: The language and explanations in this FAQ are well tailored to a non-specialist audience - I have no specific changes to suggest. [David Wratt, New Zealand] | Noted. No action required |
| 13-996 | 13 | 65 | 53 | | | FAQ 13.2: The figure related to this section (FAQ 13.2, Figure 1) is not invoked in the text. [Mirko Orlic, Croatia] | Accepted. We are in discussion with Chapter 4 on replacing this figure with one based on real data (eg subglacial bedrock). To facilitate this, we have added chpt 4 LA Tavi Murray as a CA to 13. |
| 13-997 | 13 | 65 | | | | FAQ 13.2, Fig 1: Figure overall is difficult to interpret. Colors used in the map need full explanation in a legend - eg, grey shading, light grey shading etc not currently explained. There is ample space on the figure to include such a complete legend. In addition, it is unclear why Greenland does not show any areas of thinning (red) in the future, and unclear why the outline of the ice areas do not change in the future. [Thomas Stocker/ WGI TSU, Switzerland] | Accepted. We are in discussion with Chapter 4 on replacing this figure with one based on real data (eg subglacial bedrock). To facilitate this, we have added chpt 4 LA Tavi Murray as a CA to 13. |
| 13-998 | 13 | 66 | 1 | 66 | 3 | Use of IPCC calibrated language is clumsy and ambiguous in this FAQ. For example it might be clearer to say: "Increased accumulation will cause global mean sea level to fall, while increases in surface ablation and increases in outflow,....., will cause it to fall. Each of these changes is likely with warming" [Ian Allison, Australia] | Accepted. Advice from IPCC TSU is now to remove uncertainty language. Revision has removed uncertainty language and replaced as far as possible phrases such as 'likely'. |
| 13-999 | 13 | 66 | 4 | | | "We are confident" is very subjective. Would be appropriate to use something like "best educated guess". [Olaf Eisen, Germany] | Accepted. Advice from IPCC TSU is now to remove uncertainty language. Revision has removed uncertainty language and replaced as far as possible phrases such as 'likely'. |
| 13-1000 | 13 | 66 | 11 | 66 | 11 | "where it is subsequently lost" [Ian Allison, Australia] | Accepted. Suggestion incorporated. |
| 13-1001 | 13 | 66 | 12 | 66 | 12 | "and submarine melt..." [Ian Allison, Australia] | Accepted. Suggestion incorporated. |
| 13-1002 | 13 | 66 | 19 | 66 | 19 | "polar atmosphere to carry moisture" [Ian Allison, Australia] | Accepted. Suggestion incorporated. |

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| 13-1003 | 13 | 66 | 33 | 66 | 33 | "The concept of 'marine ice sheet instability'..." [Ian Allison, Australia] | Accepted. Suggestion incorporated. |
| 13-1004 | 13 | 66 | 33 | 66 | 38 | The velocity of the thicker ice has to increase also as the grounding line retreats, The present flux through the thicker section is in (near) balance with the discharge at the present GL. [Ian Allison, Australia] | Accepted. Text changed so that it is clear this is not purely a geometrical effect. |
| 13-1005 | 13 | 66 | 34 | 66 | 35 | "...an ice sheet resting on bedrock below sea level increases as ice thickens at the grounding line..." [Ian Allison, Australia] | Accepted. Suggestion incorporated. |
| 13-1006 | 13 | 66 | 44 | 66 | 44 | "the ice shelf collapse and subsequent..." [Ian Allison, Australia] | Accepted. Suggestion incorporated |
| 13-1007 | 13 | 66 | 46 | 66 | 47 | Why is this unlikely? The latitude of some of the fringeing ice shelves of EAIS (e.g. Shackleton) is similar to that of Larsen B. Is it because the model project greater regional warming the Peninsula than EAIS? [Ian Allison, Australia] | Noted. Correct, regional warming is far stronger over the AP than elsewhere in Antarctica. Have clarified this by addition of regional and warming |
| 13-1008 | 13 | 66 | 49 | 66 | 51 | Chapter 4 rates this increase as stronger than an "indication" [Ian Allison, Australia] | Accepted. Have made text stronger "Estimates of the contribution of the Antarctic ice sheets to sea level over the last few decades vary widely, however there are strong indications that enhanced outflow (primarily in WAIS) outweighs any increase in snow accumulation (primarily in EAIS) implying a tendency towards sea level rise." |
| 13-1009 | 13 | 66 | 50 | 66 | 51 | The wording here, "beginning to outweigh" is very odd, considering that the current Antarctic mass balance is likely negative. The current mass balance plus the recent acceleration in the Amundsen Sea support+H19 a somewhat more confident statement about the future Antarctic contribution. [Michael Oppenheimer, USA] | Accepted. Have made text stronger "Estimates of the contribution of the Antarctic ice sheets to sea level over the last few decades vary widely, however there are strong indications that enhanced outflow (primarily in WAIS) outweighs any increase in snow accumulation (primarily in EAIS) implying a tendency towards sea level rise." |
| 13-1010 | 13 | 66 | 55 | 66 | 55 | It is not correct that precipitation has increased in Greenland. Where is the citation? RACMO in fact reports a slight decrease in overall precipitation over the GRACE time period for instance. [Eric Rignot, USA] | Accepted. Have discussed this with RACMO group and rephrased this sentence - "In Greenland, it is unclear whether a slight observed thickening of ice in the interior is related to a trend towards increasing accumulation." |
| 13-1011 | 13 | 66 | 55 | 66 | 56 | This is counter-intuitive. Please explain why there is more accumulation in a warmer atmosphere. [Uwe Stoeber, Germany] | Accepted. Comment removed. See 1010. |
| 13-1012 | 13 | 67 | 18 | 67 | 20 | Greenland has marine-based sectors as well, so discounting marine ice sheet instability in Greenland is not justified. [Eric Rignot, USA] | Rejected. While some sectors of Greenland do lie on bedrock below sea level, the depths below sea level are shallow (<200 m) and spatially restricted. In comparison, a very high proportion of West Antarctica rests on bedrock kms below sea level. It would therefore be misleading to discuss marine instability in the context of Greenland. |
| 13-1013 | 13 | 68 | 33 | 68 | 33 | Becker 2012 [Belén Martín Míguez, Spain] | Taken into account |
| 13-1014 | 13 | 70 | 4 | 70 | 4 | de Berg should be van de Berg [Philippe Huybrechts, Belgium] | Taken into account |
| 13-1015 | 13 | 72 | 9 | 72 | 9 | Hanna et al. (submitted) has been published as: Hanna, E., and Coauthors, 2011: Greenland Ice Sheet surface mass balance 1870 to 2100 based on Twentieth century reanalysis, and links with global climate forcing. Journal of Geophysical Research [Philippe Huybrechts, Belgium] | Taken into account |
| 13-1016 | 13 | 73 | 11 | 73 | 13 | Remove these lines; Huybrechts et al, SG, 2011 is duplicated in reference list [Philippe Huybrechts, Belgium] | Taken into account |

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| 13-1017 | 13 | 73 | 54 | 73 | 56 | Use full initials of authors: Kemp, A.C., Horton, B.P., Donnelly, J.P., Mann, M.E., Vermeer, M., Rahmstorf, S., Climate related sea-level variations over the past two millennia, Proc. Nat. Acad. Sci., 108, 11017-11022, 2011. [Michael Mann, USA] | Taken into account |
| 13-1018 | 13 | 74 | 35 | 74 | 35 | Leclercq should be Leclercq [Philippe Huybrechts, Belgium] | Taken into account |
| 13-1019 | 13 | 75 | 23 | 75 | 24 | Lowe et al. (2010) is not referenced correctly - it is a chapter of Church et al. (2010) (referenced in Chapter 13, Page 69, lines 43-44). [John Hunter, Australia] | Taken into account |
| 13-1020 | 13 | 81 | 15 | 81 | 18 | Duplicate reference for White et al., 2005 [John Hunter, Australia] | Taken into account |
| 13-1021 | 13 | 81 | 45 | 81 | 46 | Reference (Wouter, Chamber and Schrama, 2008) in GRL is superseded by E.J.O. Schrama and B. Wouters, 2011, Revisiting Greenland Ice Sheet Mass Loss observed by GRACE, JGR Vol 116, B02407 doi:10.1029/2009JB006847. [Ernst Schrama, Netherlands] | Taken into account |
| 13-1022 | 13 | 83 | 1 | 83 | 51 | Sheer speculations based on flawed climate models; out of touch with actual current observations of sea level [VINCENT GRAY, NEW ZEALAND] | Rejected. The models used for projections are evaluated against observations in 13.4 and the agreement is good. |
| 13-1023 | 13 | 83 | 17 | 83 | 26 | There is a critical issue with this formulation: This semi-empirical model of glacier loss has the problem that the glaciers will continue to waste away forever for any small warming. There is no equilibrium ... [Aslak Grinsted, Denmark] | Taken into account by noting this point. The reviewer is correct but it is not a problem for the 21st century. |
| 13-1024 | 13 | 83 | 24 | 83 | 26 | I think you do not sample the full uncertainty when you use $T_0=0.32-0.65/s$. I believe you should allow for some uncertainty in the observations that you use to pick that relationship. [Aslak Grinsted, Denmark] | Rejected. The observational uncertainty is reflected by the uncertainty in s, given on line 19. |
| 13-1025 | 13 | 83 | 24 | 83 | 26 | Please provide references or a more detailed explanation for why you would choose $T_0=0.32-0.65/s$ [Aslak Grinsted, Denmark] | Accepted. An explanation has been added. |
| 13-1026 | 13 | 83 | 28 | 83 | 35 | Please verify that this captures the full range of uncertainty for A1B, and central best estimate, if you use A1B forcing. [Aslak Grinsted, Denmark] | Taken into account by comparing the resulting range with that which comes from the RCMs in 13.5.3 and 13.5.4. |
| 13-1027 | 13 | 83 | 31 | 83 | 33 | Greenland SMB results seem to have been multiplied by a factor 1.2 based on more recent models. Which models? Actually, our latest results for ice2sea indicate the contrary: imposing SMB changes directly from RCMs consistently produce lower SLR as compared to imposing T and P anomalies from the same RCMs in a PDD model. Hopefully more on this by the IPCC cut-off date for paper submission. [Philippe Huybrechts, Belgium] | Taken into account by referring to 13.5.3 and comparing with the ranges given there. Note that the spread is enlarged as well as the mean being raised. |
| 13-1028 | 13 | 83 | 37 | 83 | 38 | These are all the observations we have, and then you arbitrarily set it at 50%. You may believe that present trends are "ice sheet weather" rather than climate. But this is an unsupported hypothesis and even if true, then it might as well be the low rates of loss in the 80s & 90s that was ice sheet weather. The "ice sheet weather viewpoint" also seems to clash unfortunately with one of the key claims made in AR5: namely that the present day budget is closed, and that "process models" show close correspondence to these observations, and that this gives confidence in these models. [Aslak Grinsted, Denmark] | Taken into account by recasting the discussion of the role of ice sheet dynamics in the budget of 13.4. We reject the comment that the choice of 50% is arbitrary. This is the best estimate from the albeit limited observational knowledge. This apportioning makes no comment about "ice sheet weather", rather it is the split between the dynamic response of the ice sheets and the SMB. |
| 13-1029 | 13 | 83 | | | | Appendix 13.A: Please verify that this approach works using A1B temperatures, so that it gives the same central estimate, and confidence intervals as shown for A1B in figure 13.9. [Aslak Grinsted, Denmark] | Taken into account by referring to 13.6.1.1 and Fig 13.9. The methods described in the Appendix are those which were used to generate Fig 13.9, including the results for A1B. |
| 13-1030 | 13 | 84 | | | | For the figures, in particular Fig 13.9 and 13.10, consider to be more clear on what is CMIP5 output and what is an expert combination of components; related to these comments, we encourage you to please clearly mark all sources of information. [Thomas Stocker/ WGI TSU, Switzerland] | Accepted |
| 13-1031 | 13 | 85 | | | | As far as I know, ice shelves do not influence sea level directly because they are already floating on the | Noted. Ice shelves are important because they |

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| | | | | | | ocean. So I recommend to delete 'and shelves' from the figure or alternatively state in the caption that ice shelves influence local sea level but do not have a direct effect on the water budget entering the ocean [Elie Verleyen, Belgium] | impact how rapidly ice sheets can discharge into the ocean. |
| 13-1032 | 13 | 86 | | | | Figure 13.2: The term "physically based constraint" implies it is a physical law, rather than a heuristic limit. "heuristic" is a much more accurate representation. [Aslak Grinsted, Denmark] | Accepted. Figure will be updated. |
| 13-1033 | 13 | 86 | | | | Figure 13.2: The glaciers box also relies on semi-empirical models (as evident in section 13.A) [Aslak Grinsted, Denmark] | Accepted. Figure will be updated. |
| 13-1034 | 13 | 86 | | | | Figure 13.2: Are there really any process models for the ground water and reservoirs? There is rahmstorfs semi-empirical population scaling model, and then there is a statistical extrapolation in section 13.5. [Aslak Grinsted, Denmark] | Noted. Will add SEMs |
| 13-1035 | 13 | 86 | | | | Figure 13.2: Add reference to PO discussion of thermal expansion during the Last Interglacial (5.3.2) and regional sea level records (5.3). [Robert Kopp, USA] | Accepted. We will also add PO to the "gravity and solid Earth effects" box. |
| 13-1036 | 13 | 86 | | | | Figure 13.2 Is is written "SEM - semi-emperical model" instead of "semi-empirical" [Belén Martín Míguez, Spain] | Taken into account - spelling error will be corrected. |
| 13-1037 | 13 | 86 | | | | Figure 13.2: The figure seemingly implies that the global ocean mass contributes to the global ocean volume that in turn contributes to the regional sea level. It would be better to indicate that both the global ocean mass and the global ocean volume contribute to the regional sea level. [Mirko Orlic, Croatia] | Rejected. global ocean mass contributes to the global ocean volume that in turn contributes to the regional sea level |
| 13-1038 | 13 | 86 | | | | Figure 13.2: The box labeled "gravity and solid Earth effects" should be moved up with arrows pointing to both "global ocean volume" and "regional sea level". [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Taken into account. This could cause some confusion as gravity and solid Earth effects do not change the volume of ocean water but they do affect the properties we measure (relative and absolute sea level change) to estimate changes in global ocean volume. In this respect it is correct to have this process pointing to both "global ocean volume" and "regional sea level". We will consider adding a line in the caption to clarify (assuming the change is made). |
| 13-1039 | 13 | 87 | 2 | 87 | 10 | Figure 13.3 would be a more powerful tool for policymakers if it had some of the projection modelling trajectories superimposed as well (in particular the lowest bound projection, highest bound projection and some intermediate ones) but, on a timescale that starts with the commencement of the respective models (\approx 1990). Perhaps some of the trajectories from Figure 13.10 (a) would be ideal. These type of composites enable policymakers and communities to directly compare current GMSL measurements with the projection modelling over the same time period. [Phil Watson, Australia] | Noted. We are considering such composite figures. |
| 13-1040 | 13 | 87 | 8 | 87 | 8 | There is a minus sign missing in front of the 0.3 mm/yr. [Mark Tamisiea, United Kingdom of Great Britain & Northern Ireland] | Accepted |
| 13-1041 | 13 | 87 | | | | The graph in Figure 13.3 has numerous problems, all of which tend to exaggerate SLR: It excludes Envisat, it hides the disagreement between the satellites, it erroneously adds the Peltier's 0.3 mm/yr GIA (which corrects depth, not sea level), the caption overstates the secular trend, and the caption fails to note the deceleration in SLR. [David Burton, USA] | Rejected. However, a diagram showing the agreement between satellite missions may make a useful addition. |
| 13-1042 | 13 | 88 | 1 | 88 | 10 | I like the idea of Figure 13.4 but it still needs a bit of work. I have listed the comments separately. The first is that panel (c) is very unclear. It is not clear which are modelled or observed data (I assume from the text that the SMB lines are both modelled). The "Greenland dyn." line seems to be mainly hidden under the "Antarctic dyn." line. Are the "Antarctic dyn." and "Greenland dyn." lines modelled or observed data? It would probably be best to put "terrestrial storage" in a separate panel, as it doesn't really fit with "ice" data. [John Hunter, Australia] | Accepted - The figure will be redrawn. |
| 13-1043 | 13 | 88 | 1 | 88 | 10 | Since the agreement between models and observations is better after about 1995, it may be better to set the curves to the same value at some time between 1995 and the present, rather than in 1971. This would allow a | Accepted- The figure will be redrawn. |

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| | | | | | | better assessment of the agreement between the curves in recent years (when the observations are best, and observations and models do agree better) as they would lie closer together at these times. I realise, however, that there may well be other constraints that dictate that the curves be lined up at 1971. [John Hunter, Australia] | |
| 13-1044 | 13 | 88 | 1 | 88 | 11 | Line patterns for different models are very hard to follow. Legend shows line patterns for "Antarctica dyn" and "greenland dyn", but they don't seem to be shown. [Ian Allison, Australia] | Accepted- The figure will be redrawn. |
| 13-1045 | 13 | 88 | 9 | 88 | 9 | The observational time series is not shown in black (dashed) in panel (c). [John Hunter, Australia] | Accepted- The figure will be redrawn. |
| 13-1046 | 13 | 89 | | | | Fig 13.5: Please include uncertainties for Greenland mass balance if possible. [Thomas Stocker/ WGI TSU, Switzerland] | Taken into account. We will see if they are available. |
| 13-1047 | 13 | 90 | 34 | 90 | 34 | "The uncertainties quoted are one standard deviation" does not make much sense here, since no uncertainties are mentioned - this could be restated in terms of the separation of the three values of lambda (it they are indeed one standard deviation apart). [John Hunter, Australia] | Accepted. Figure to be revised. |
| 13-1048 | 13 | 90 | 35 | 90 | 35 | There is only one "uncertainty" quoted. [Ian Allison, Australia] | Accepted. Figure to be revised. |
| 13-1049 | 13 | 90 | | | | Box 13.1, Figure 1b: It is clear that the budget has to be closed, and here you illustrate how you can constrain lambda if you know the other contributions perfectly. Unfortunately, in reality we do not know the other contributions perfectly in the budget. Full depth ocean heat content and land-ice loss has substantial uncertainties. We know pretty good the rate of total sea level rise, but that does not provide a useful constraint on the energy budget because a tiny shift in the balance of how much is due to melt vs. expansion will yield vastly different numbers for the energy spent. Quoting from Trenberth 2009: "melting ice is a factor of 40–70 times more effective than thermal expansion in raising sea level when heat is deposited in upper 700 m, or the factor is 90 when heat is deposited below 700 m depth". For this reason I think that this box is misplaced in this chapter, but is much better suited in a chapter where climate sensitivity, or energy budget and observations are discussed. [Aslak Grinsted, Denmark] | Rejected - The two budgets need to be consistent and Church et al. found some useful constraints. Indeed, for the example quoted, if more sea-level rise came from melting ice sheets, that would have very large implications for the energy budget. The energy budget also puts constraints on how much thermal expansion is possible. |
| 13-1050 | 13 | 91 | 2 | 91 | 7 | Figure 13.6 needs a key of some description to understand the colours? [Phil Watson, Australia] | Accepted - this figure was a placeholder only. |
| 13-1051 | 13 | 92 | 1 | 92 | 2 | I only checked the "Bahr et al. (2009)" values in this Figure - the values at 2100 for Bahr et al. do not seem to agree exactly with those in Table 13.3. [John Hunter, Australia] | Taken into account - these will be checked for consistency. |
| 13-1052 | 13 | 92 | | | | Figure 13.7: Bad quality, I could not read the legend [Uwe Stoeber, Germany] | Taken into account - figure quality will be improved. |
| 13-1053 | 13 | 94 | 1 | 94 | 8 | Are the ice sheet dynamics and land storage terms included in each scenario sum? How are the ranges for the sum calculated? [Ian Allison, Australia] | Taken into account. Yes, they are included in each scenario, and their ranges come from the text. These points will be clarified in the caption. |
| 13-1054 | 13 | 94 | | | | Figure 13.9: If you look at the tendency of Antarctic total mass balance with RCP-intensity, then you would conclude that warmer climate is associated with a larger Antarctic ice sheet. I think you need to very visibly state that we cannot presently project dynamical contribution for individual scenarios, but that dynamical loss will be greater for the most intensive scenarios. [Aslak Grinsted, Denmark] | Taken into account. We will note that we cannot give a scenario dependence. We do not have sufficient evidence to support the reviewer's last statement. |
| 13-1055 | 13 | 95 | 1 | 95 | 2 | I suggest the two figures are marked with (a) and (b), to distinguish them. [Juncheng ZUO, China] | Editorial. |
| 13-1056 | 13 | 95 | | | | Figure 13.10 doesn't even get the current rate of coastal SLR correct: about 1.4 mm/year, and even its lowest scenarios postulate improbably high accelerations in SLR. [David Burton, USA] | Rejected. The current rate of sea level rise of about 3 mm/yr is in good agreement with the figure. |
| 13-1057 | 13 | 96 | 1 | 96 | 2 | Colours are hard to distinguish [Ian Allison, Australia] | Accepted. Figure to be revised. |
| 13-1058 | 13 | 96 | 1 | 96 | 2 | Note in caption that Antarctica and Greenland are SMB terms only [Ian Allison, Australia] | Accepted. |
| 13-1059 | 13 | 96 | 1 | 96 | 10 | The error bars on the 2100 projection seem unrealistically low. [Eric Rignot, USA] | Rejected. The reviewer does not identify which error bars, nor provide justification for this statement. |
| 13-1060 | 13 | 96 | 6 | 96 | 6 | What horizontal lines? [Ian Allison, Australia] | Noted. The short black horizontal lines - caption will |

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| | | | | | | | be more complete on this if figure style remains the same. |
| 13-1061 | 13 | 97 | 1 | 97 | 2 | I suggest the two figures are marked with (a) and (b),to distinguish them. [Juncheng ZUO, China] | Accepted. |
| 13-1062 | 13 | 98 | 1 | 98 | 2 | I suggest the two figures are marked with (a) and (b),to distinguish them. [Juncheng ZUO, China] | Accepted. |
| 13-1063 | 13 | 99 | 1 | 99 | 2 | The Figure13.14 is not in right proportions. [Juncheng ZUO, China] | Accepted. Figure to be revised. |
| 13-1064 | 13 | 100 | | | | Figure 13.15: The range of RSL given in the caption does not agree with the figure. The difference is about a factor 10. [Uwe Stoeber, Germany] | Accepted. Figure to be revised. |
| 13-1065 | 13 | 101 | 4 | | | "f when" -> "when" [Ernst Schrama, Netherlands] | Editorial - will be corrected. |
| 13-1066 | 13 | 101 | 5 | 101 | 9 | Some typical regional sea like Western Pacific warm pool or the Kuroshio Extension can be given instead of the key locations around the globe. [Juncheng ZUO, China] | Rejected. The locations used designed to illustrate the variability with respect to the trends. |
| 13-1067 | 13 | 101 | 7 | 101 | 7 | Legend of Figure 13.16. I would use the word "reconstructed" instead of "gridded" so that the legend matches the word in the Figure. [Belén Martín Míguez, Spain] | Accepted. Figure to be revised. |
| 13-1068 | 13 | 101 | | | | Fig 13.16: This will be a very nice figure, but we wonder if; A) Is it feasible to combine Cook Island reconstructions, with tide gauge records from a different regional station (which according to FAQ Fig 13.1a seem to show differences in change over the more recent period). B) Is it feasible to then expand these local records with scenario results which are based on coarse resolution models? [Thomas Stocker/ WGI TSU, Switzerland] | Taken into account. The figure is being redrawn. |
| 13-1069 | 13 | 102 | 1 | 102 | 10 | Are semi-empirical models included in this plot? This plot suggests that sea level rise will be less than one meter by 2100. What level of confidence is placed on that? What is the rationale for "eliminating" the results of semi empirical models in this assessment? Or are they not? The argument that they do not capture the correct physics is weak. Numerical ice sheet models are not even able to replicate the evolution of ice sheets in the past decades; at least empirical models do that; yet this chapter seems to put a lot more trust on these numerical models. This may reflect an opinion rather than correspond to an assessment. Personally I think these semi empirical models went a long way suggesting to ice sheet modelers that may be the skills of their models are actually very low. [Eric Rignot, USA] | This figure will be deleted. |
| 13-1070 | 13 | 104 | 1 | 104 | 10 | The figure only shows increased snowfall in Antarctica. Why not in Greenland? [Eric Rignot, USA] | Taken into account For Greenland, melting is the dominant signal but increased snowfall can be added on as well. |
| 13-1071 | 13 | 105 | 3 | | | figure caption "mmyr-1" should become "mm yr-1", a space is missing [Ernst Schrama, Netherlands] | Editorial |
| 13-1072 | 13 | 105 | | | | FAQ 13.1, Figure 1: Color code is missing in the uppermost part of the figure. [Mirko Orlic, Croatia] | Accepted. The figure is being redrawn. |
| 13-1073 | 13 | 105 | | | | Figure 13.20a: colorbar missing, labels too small [Uwe Stoeber, Germany] | Accepted. The figure is being redrawn. |
| 13-1074 | 13 | 107 | 4 | 107 | 11 | Plots show the "equilibrium line" NOT the "equilibrium line altitude" [Ian Allison, Australia] | Noted. This figure is being replaced. |
| 13-1075 | 13 | 107 | 4 | 107 | 11 | This map is inconsistent with Chapter 4. There is no ice sheet growth in south Greenland, there is no ice sheet growth in the Bellingshausen sector of Antarctica, the mass loss from the northern tip of the Peninsula is missing. The statement that contact with the ocean will be limited makes little sense given how wide of an area is grounded below sea level in the north. Cook Ice Shelf is not experiencing thinning, but Shackleton Ice Shelf (not shown) is thinning. I have no idea which references were used to generate this map but there are some major problems with this map. Any revision should explain which references were used and should also cross referenced the results presented in Chapter 4. [Eric Rignot, USA] | Noted. This figure is being replaced. |
| 13-1076 | 13 | 107 | 29 | 107 | 30 | As far as I know, the North Atlantic around Greenland and in the Southern Ocean are not the typical regions which are sea level rising strongest, Western Pacific warm pool or the Kuroshio Extension can be given instead of them. [Juncheng ZUO, China] | Unclear what this comment refers to - not relevant to this page. |

Expert Review Comments on the IPCC WGI AR5 First Order Draft -- Chapter 13

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| 13-1077 | 13 | 107 | | | | These are pretty crude schematics. Where do the distributions of thinning and thickening come from? Bedrock in EAIS below sea level is more extensive than shown here. [Ian Allison, Australia] | Noted. This figure is being replaced. |
| 13-1078 | 13 | 107 | | | | FAQ13.2, figure 1: It is too early to say that Jakobshavn will lose contact with the ocean already in ~100 years. That may well be true, but i believe this depends strongly on longitudinal stresses and the implementation of ice/ocean interaction. Please double check against SeaRise runs as they become available. [Aslak Grinsted, Denmark] | Noted. This figure is being replaced. |
| 13-1079 | 13 | 107 | | | | FAQ 13.2, Figure 1: There is no reference to the figure in the text. The figure needs however more explanation. There are no visible changes in the ice contour. [Uwe Stoeber, Germany] | Noted. This figure is being replaced. |