SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1434	4	0	0	0	0	From my understanding the IPCC opted to not distinct between 'glaciers' and 'ice caps' and to refer to all these ice bodies as 'glaciers'. This was well respected in chapter 2&3, but not entirely in chapter 4. Check the following occurrences: p.12, l.14; p.12, l.40; p.12, l.29; p.22, l.8; p.42, l.31. Maybe add a footnote on this somewhere? Or will this be included in (eventual?) glossary? [Harry Zekollari, Switzerland]	IPCC refers to "glaciers and ice caps" in combination in context of mass budgets and flux terms but in more gneral contexts, may use the two terms together or separately. Ice sheets are usually treated separately from either. As long as the meaning is clear in each context, and tables and figures are appropraitely labeled, this is not problematic.
11752	4	0	0	0	0	It is better to send a general call to countries to provide their existing information and then report them with integration so that deficiencies can be identified and completed by completing projects and field activities. [Hanieh Zargarlellahi, Iran]	Noted; beyond the remit of chapter authors
19086	4	0	0	0	0	Missing information on limits to adaptation and loss and damage as a result of sea level rise. See e.g. Storlazzi et al. 2018 on the implications of SLR for the inhabitability of atoll islands [Carl-Friedrich Schleussner, Germany]	Taken into account in revisions to FOD and revised 4.4.5. Work by Storlazzi and others is included in the assessment
2208	4	0	0			Well written document, but some need to specify in clear terms what are low lying islands or reference made to cross chapter Box where information has been inserted. More examples and cases from African and Indian Ocean region may be included. [Chandani Appadoo, Mauritius]	Noted and taken into account; with reference to cross chapter boxes as well as spotlighting representative examples to illustrate global realities, including Africa and Indian Ocean
11694	4	0	0			Overall this is a good first draft - congratulations. It is very long - in my view too long. Some sections read like tutorials and/or literature reviews rather than an assessment. The references look incomplete but I have not done a complete check. [John Church, Australia]	Accepted and taken into account with more focused attention on assessment, and continued effort to build literature foundation as basis for assessment, in SOD.
12066	4	0	0			There is no statement that in paleoclimate sea level has risen in the order of meters per century on occasion. Why is this absent? Given models are imperfect statements of fact about processes we do not fully understand give boundaries. This is especially important because we are warming the planet (ocean) dozens of times faster than it has previously warmed in Earth's history. [Michael Casey, Germany]	The paleo section discusses in particular the gological evidence that is directly relevant for the current sea level in a possible warmer world. For this reason we discuss extensively the sea level high stand during the Pliocene and The Last Interglacial warm period. We address the Pliocene because Milankovitch conditions are similarly to the present-day climate and we address the Last Interglacial warm period because of the only slightly warmer conditions than today. So we think that we captured the most important aspect of paleo sea level. In general constraints on rates are poor, but we have added some information on this.
13700	4	0	0			This comment applies to other parts of the report also: the relative scale of importance between different issues and climate change impacts does not emerge sufficiently (one example: tourism vs settlements, where the one affects large proportions of total population, the other a small sub-set, yet they are treated as equals. Coastal urbanization, informal settlements, and SLR involve a huge proportion of the global population) It seems that if governments are going to make transformative decisions at all, they will do so on the basis of large-impact 'bang-for-buck' issues, and not based on ocean science. The authors should aim to look at the report from a policy maker's perspective, and see if the most important issues are sufficiently highlighted, mentioned earlier, receive more detailed attention, and appear clearly in summaries, tables and diagrams. If literature is missing regarding hugely important issues, this should be specially mentioned. [Debra Roberts and Durban Team, South Africa]	Taken into account Sections 4.3 and 4.4 of the SOD include more focused attention on the distinct realities prevailing in different contexts, and consequently the highly variable priorities, and distinct processes underway to respond to SLR. The policy and practice relevant implications from post-AR5 literature are highlighted in the context of this emerging literature. These insights inform the various tables, figures, text and assessment.

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13702	4	0	0			Sec 4.4.6 & 4.4.7 Serious imbalance between space and attention given to small islands vs coastal cities and global North vs South. Is this purely result of what is in the literature? [Debra Roberts and Durban Team, South Africa]	Accepted and taken into account: The SOD has sought to provide more comprehensive and balanced assessment of the different realities faced in different coastal contexts, including the differences prevailing in urban vs rural settings; so called North vs South; etc. The SOD has sought to ensure that relevant literature that reveals these distinctions is robustly assessed.
13704	4	0	0			SPM: one statement on mountain glaciers is called for. [Debra Roberts and Durban Team, South Africa]	Noted; addressed in other chapters and cross-chapter boxes.
14376	4	0	0			This comment is for the whole Chapter 4 and involves both categories: editorial and substance: compared to other chapters in this report, Chapter 4 lies behind in quality. The text is soemtimes entangled, confusing, and it misses references near key statements. Furthermore, it is sometimes also not really consistent. Below I give some specific and certainly not exhaustive examples. --- Confusing text. On Lines 16-18 of Page 12: "Because of their, on average high accumulation and ablation rates compared to the ice sheets, they are sensitive indicators of climate change and respond fast to change in the climate system, with a response time scale in the order of decades". --- Reference missing. On Lines 9-11 of Page 28: "Since AR5, such formal studies have attributed changes in 10 individual contributions of sea level change (i.e., thermosteric sea level change and glacier mass loss), and in 11 the total global mean sea level, to anthropogenic forcing." --- Inconsistency. On Line 23 it is stated that "Human activity was the predominant cause of global mean sea level rise since 1970 (high confidence)." On the other hand, on Lines 6-7 of Page 28 it is stated that "... it is very likely that there is an anthropogenic contribution to the observed trend in global mean sea level rise since 1970." There is a huge difference between being "the predominant cause" and being "a contribution". [Sérgio Henrique Faria, Spain]	Noted and taken into account in revisions to the SOD - on both substantive and editorial levels. There are too many revisions made to mention them individually, but together the revisions to SOD address the underlying concern raised in this comment
14508	4	0	0			PREVIOUS LINE 49 SHOULD NOT BE TAKEN INTO CONSIDERATION, I AN UNABLE TO EDIT OR DELETE IT [Christophe Deissenberg, Luxembourg]	Noted
14516	4	0	0			PREVIOUS LINE 54 SHOULD NOT BE TAKEN INTO CONSIDERATION, I AN UNABLE TO EDIT OR DELETE IT [Christophe Deissenberg, Luxembourg]	Noted
17896	4	0	0			It would be helpful if there was some further coordination and cross referencing of information between Ch3 and Ch4, particularly in relation to the important topic of Antarctic contribution to sea level, and modeling of the Antarctic ice sheet. [Haroon Kheshgi, USA]	Accepted and taken into account in post-FOD revisions; see 4.2
20496	4	0	0			Lack of consistency in using acronyms or full terms. [Frank Oliva, Canada]	Editorial revisions made in SOD
20500	4	0	0			This is generally a very comprehensive and detailed chapter. Although I'm surpised to not see more on modern and paleo- hurricanes, tropical cyclones and storm surges which all have massive implications. [Frank Oliva, Canada]	cyclones and storm surges are treated extensively, the whole section on ESL sea level is much more advanced and expanded than in earlier assessments
20502	4	0	0			Many of the figures need to be improved (low quality, unwarranted changes in font and colour). [Frank Oliva, Canada]	Accepted and taken into account; with ongoing refinements continued beyond SOD

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20982	4	0	0			The chapter is well written and the reader can follow the flow quite easilty. My only comment is that I would like to see more links with regard to policy implications and policy making for protecting local communities and future generations. Implications can derive from case studies or from the theoretical discussion on communities in low lying islands [Stella Tsani, Greece]	Accepted and taken into account. Revisions to 4.4.4 and 4.4.5 explicitly address this comment
22536	4	0	0			The terms "steric", "thermosteric" and "halosteric" appear to be used in an unsystematic manner throughout this chapter. [Toshio Suga, Japan]	we have improved this and further improvements will follow in the final version following a terminology paper which is currently under review. Based on the outcome of this paper we will adjust the nomenclature where needed
23936	4	0	0			The chapter includes very little explicit references to sustainable development. The Sustainable Development Goals (SDGs) could be stressed a bit more. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted. There is limited literature explicitly linking responses to SLR to the SDGs and climate resilient development pathways. We recognize the significance of this issue and have a section on this topic that is under development.
23938	4	0	0			Some sections still have a certain text book style. Here, there is potential to cut some text. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted and taken into account; and this will continue to be an important undertaking post-SOD
23940	4	0	0			There should be a more balanced approach to building on AR5. Some sections do this very, if not too extensively, while others do not. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted and taken into account
23942	4	0	0			define all acronyms at first mention [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account
24588	4	0	0			sustainable development goals should include sustainable coastlines, this balance to be discussed. [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - section on climate resilient development pathways at the coast explicitly addresses this comment
24648	4	0	0			In the impacts section p. 56 to 78, with few exeptions text is qualitative and could be more quantitative and spot-on for selected examples. This includes specific risk assessments (burning embers) for those global and regional examples, considering limits to adaptation, e.g. at regional level. Section 4.3.3.3. is a highlight with respect to specificity reached and level of quantitative information given. [Hans-Otto Poertner and WGII TSU, Germany]	Noted - where quantitative information underpins the assessment this is reflected in content (e.g., 4.4.3 Economic approaches); in some sections, e.g., 4.4.4 Community-based approaches and 4.4.5 Barriers etc., qualitative information is necessary and appropriate to reflect the literature and address substantive issues. In latter, case study experience is commonly drawn upon.
14520	4	0	55	12	55	Water mass changes ==> water mass exchanges ??????? [Christophe Deissenberg, Luxembourg]	sentence has been rephrased
2724	4	1	0	108		This is a better draft, especially Section 4.4 Responses to SLR, which is much improved and expanded and of more value to policymakers. [Poh Poh Wong, Singapore]	Noted
13988	4	1	0	139		While the "physical science" elements of the chapter are well written it is 55 pages into a 139 page chapter before "social science issues" emerge - this makes it extremely daunting reading for any policy maker. Some level of integration would be highly desirable. This difficulty is further compounded by the frequent use of multiple acronyms (especially in the physical science sections) which often make the reading very difficult for the non-specialist. [Debra Roberts and Durban Team, South Africa]	Noted. The introduction seeks to provide an integrative starting point for readers, including policy makers. The need to focus in depth on the physical basis of observed and projected sea level change is compelling because this understanding is foundational for prudent policy-making and practice. Hence the need for comprehensive assessment of post-AR5 literature on this topic before assessing the literatures on impacts, risks and responses as covered in 4.3 and 4.4. Nonetheless, this comment is taken seriously and the relationship between all major sections is addressed through revised text and figures. We also have attmepted to achieve a balance between using acronyms (once defined) and providing a clear text for the non-expert, a difficult line to walk.

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14000	4	1	0	139		There seems to be a great deal of overlap in the discussion of the ice sheets and glaciers between Chapter 3 and 4 [Debra Roberts and Durban Team, South Africa]	Accepted and taken into account through explicit division of tasks in the assessment of relevant literature.
20608	4	1	0	139		Overall I really liked this chapter. Just one commnet which is related to section 4.2.3.4.3 Effects of cyclones. Please relook into it and provide more details. [Pushp Raj Tiwari, UK]	the effects of cyclones has been expanded and is revised
21282	4	1	0	139		The chapter clearly explains the implications of sea level rise to low lying countries and Islands. It has a very good summary explaining sea level rise from geological, historical and present day point of view. It clearly stablish tha the sea is rising and that we are responsible for sea level rise in the modern era. it explains what it was known in the scientific community, that extreme sea level which have distructive impacts will become frequent events by 2100 no matter what we do. It explains that there is only 3 kinds of responses which are hold the line, retreart or manage retreat. It favours a community based adaptation as the most succesful way forward [Alejandro Souza, Mexico]	Noted
21612	4	1	1	6	19	Does sea-level rise and climate change in coastal areas over any opportuniites or is it just threats? We were asked this question in the AR4 and could not find much if anything. I think you have a similar view, but this is implicit and the author teams view could be explicitly stated. [Robert Nicholls, UK]	Accepted. This matter has been discussed in the course of the assessment process leading up to the SOD draft. Threats predominate and our assessment of the balance between threat and opportunity is clearly reflected in the Executive Summary statements. This will be a matter for further deliberation and articulation post-SOD.
21614	4	1	1	6	19	The conference statement for SeaLevel 2017 explicitly suggested considerations of sea-level rise should not stop in 2100. https://www.wcrp-climate.org/events/sl-statement-2017 and also stated by Stammer et al (2018) in EOS (https://eos.org/meeting-reports/sea-level-2017-conference-looks-to-coastal-sea-level-rise-impact). The Executive Summary could better address this recommendation [Robert Nicholls, UK]	Noted and accepted. The SOD includes text and figures that explicitly address this matter.

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16402	4	1	1	36	49	May be because this chapter is contributed separately by numerous authors and compiled afterwards, the flow and connections between two subtopics are sometimes not smooth. This should be improved, otherwise it looks obvious on the gaps, and less proefssional on the presentation of the chapter [Lee-Sim Lim, Malaysia]	Accepted and taken into account in preparation of SOD; and will continue to be addressed post-SOD
21646	4	1	1	108	1	There are lots of new results about changes in surges and waves and the importance of additional factors contributing to extreme water levels. I am not saying any of these results are wrong and the importance of sea-level rise in increasing wave heights in depth-limited situations is basic physical oceanography and coastal engineering -- and a factor that has often NOT been considered in earlier analyses. But increasing wave heights in depth limited situations is not a new insight -- it was mentioned in papers in the 1990s on sea-level rise and is already included in coastal defence assessment methods and design of new structures in countries like the UK. Rather these studies are reminding us of the importance of this process and need to be presented in this way. Increases in wave heights and surges not connected with sea-level rise will be much more site specific and there will be increases and there will be decreases. Hence, I caution about the interpretation of these results in the context of an IPCC report. [Robert Nicholls, UK]	This report is about coasts and low-lying island so we tried to address this as cautiously as possible. Bringing sea level problems to local scales introduces discussion of many more processes and for that reason there is quite some attention in this report to extreme sea level.
21648	4	1	1	108	1	Many references are missing or lack details such as et al. There is a significant job in tidying up the manuscript for the SOD [Robert Nicholls, UK]	Accepted and taken into account.
21650	4	1	1	108	1	The Chapter is very long and yet significant material still seems to be missing. The size and the missing material hinders review. [Robert Nicholls, UK]	Accepted and taken into account.

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21652	4	1	1	108	1	In general, I found cases where observations and model-based prognosis and insights are mixed -- need to be clearer about what is being observed and what may be observed in the future. [Robert Nicholls, UK]	Noted and taken into account.
21752	4	1	1	108	1	There are quite different styles apparent in the Chapter -- some sections like Section 4.3.3.1 are strongly focussed on post-AR5 literature and earlier references are sparse. Other sections like Section 4.3.3.2 are more like text books with a lot of and in some parts even a dominance of pre-AR5 literature. The CLAs need to consider this and encourage more harmony and consistency. [Robert Nicholls, UK]	Accepted and taken into account.
21764	4	1	1	108	1	There is a fair bit of repetition in the document with the same ideas repeating and also inconsistency between sections on those concepts and ideas. Effort is required to harmonise these perspectives. [Robert Nicholls, UK]	Accepted and taken into account.
6236	4	1	1	109	70	There are too many acronyms which make the chapter hard to read. Many shoud be avoided (e.g. ESL, AIS, TC, ETC, CB ...) [Regine Hock, USA]	Noted and taken into account - within the scope of what is practical to provide a robust assessment of relevant literatures that include a wide array of technical terms, many of which are commonly used acronyms in relevant fields of scholarship.
8232	4	1	1	139	1	Check in all Chapter: "modelling" or "modeling"? [APECS Group Review, Germany]	Editorial revisions ongoing
8234	4	1	1	139	1	Check all references tagged as "In Press" and "Submitted" before publish (to define the correct year) [APECS Group Review, Germany]	Accepted and taken into account
17644	4	1	1	139	1	Chapter 6 uses GrIS as the acronym for the Greenland Ice Sheet, which is generally more common than GIS as the latter is also very commonly referred to as Geographic Information System and can appear in the same context. I suggest consistency across the SR and would suggest GrIS as the preferred option [Jonathan Bamber, UK]	Noted; editorial action
18404	4	1	1	139	20	Despite several mentions of "long histories" of cultural response, regional flooding events, community adapation, etc., the words "archaeology" and "heritage" are both entirely absent from this chapter. It is impossible to truly discuss ILK, or a community's history of climate response and environmental interaction without recognition of these entities. Also, since implications to communities is explicity stated in the chapter's title, I feel it is remiss to have absolutely no discussion of such communities' cultural heritage--particularly heritage elements either at risk from climate change impacts and those that might assist towards adaptation and resilience. [Jeneva Wright, USA]	Accepted; where possible relevant literature available on this topic is incorporated into the SOD; and will be spotlighted in sections still under development (4.4.4.1 and 4.4.4.2; 4.4.4.4).
22244	4	1	1	139	20	A general comment--there are quite a few grammatical mistakes/poor phrasing in this chapter that need to be addressed before publication [Andra Garner, USA]	Accepted and taken into account; editorial
8230	4	1	1	139	58	Please, check all references (and year) cited. For example, Arthern and Williams, 2017. In some pages (pag. 34, 35), there are Arthern and Williams, 2015. [APECS Group Review, Germany]	Accepted and taken into account - as part of ongoing process of checking and rechecking references. This will continue post-SOD.
8236	4	1	1	139	58	Few references about South American and Africa are cited or exemplified. If there are few studies in these regions (and elsewhere in the World), it should be highlighted along all the report. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	Accepted and taken into account; and work is ongoing to reveal under-represented regions where published literature may less readily available.
8238	4	1	1	139	58	Check if the correct is "21st Century" or "21st century" [APECS Group Review, Germany]	Editorial changes made in SOD

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23174	4	1	49	1	49	Why not geographical areas instead of "Geographies" strange sounding expression [Sebastian Weissenberger, Canada]	Editorial
13998	4	3	0	6		The focus on coastal urban areas in the Exceutive Summary does not mirror the attention that they are receiving in the policy space see e.g. The future we dont want: How Climate Change Could Impact the World's Greatest Cities UCCRN Technical Report February 2018. Given the decision of the Panel to increase the focus on cities during this cycle it is important that this chapter has a point in the Excutive Summary that pulls togther the challenges, costs and opportunities related to cities in the coastal zone. [Debra Roberts and Durban Team, South Africa]	Accepted and taken into account in SOD, including more detail on impacts and risks faced in coastal cities and urban areas. Addressed in number of sections of the text and Executive Summary.
4140	4	3	1	3	47	The Executive Summary makes very little mention of sea level rise for the period post-2100. The long-term commited sea level rise is an important aspect of climate change that should be included in this seciton (in my opinion). Even under the most aggressive mitigation scenarions (e.g. RCP2.6) global sea level will continue to rise for several centuries. If the focus of this Chapter and/or SROCC is the 21st Century changes, then this should be made very clearly at the start of the chapter/report. [Matthew Palmer, UK]	Accepted and taken into account.
21588	4	3	1	4	20	There is hardly any mention on sea level after 2100. This misses the commimtment to sea-level rise apparent even if we follow the Paris Agreement, and the much greater commiitment if we have higher emissions. This is mentioned in the main text and deserves some mention here, or possibly at the end of the Executive Summary as it has profound implications, including the need for adaptation pathways to respond to sea-level rise even if we sucessfully mitigate (Nicholls et al., 2018). Reference: Nicholls RJ et al. 2018. Stabilization of global temperature at 1.5°C and 2.0°C: implications for coastal areas. Phil.Trans. R. Soc. A 376: 20160448. http://dx.doi.org/10.1098/rsta.2016.0448 [Robert Nicholls, UK]	Commitment is now discussed (without using that word) in the very first bullet of the ES.
24932	4	3	1	6	19	Consider reducing the number of summary points. Fewer points will assure that the points are read by the casually interested reader. [Elizabeth Weatherhead, USA]	Noted, accepted and taken into account.
12062	4	3	3	3	47	The current speed and level of ocean heat uptake along with the acceleration of this process needs to be empahasised in a paragraph in the summary. It needs to be emphasised that regardless of what is done about reducing climate forcing thisexcess heat influences climate and stability of ice. Very few policy makers realise the true role of the Ocean in controlling the climatef or shorelines. Ice formation at the contact points with the ocean require that ocean water be cooled to -2C before ice can form due to the waters salt content. The infusion of warmer waters from lower latitudes via currents and surface winds disrupts this process. As the ocean is gaining a significant magnitude of heat very rapidly relative to other periods in earth's history a continuation and acceleration of this process will occur. Some effort should be made to compare the cumulative ocean heat up takedue to increased forcing by each rcps and then compared to annual energy use. This is necessary to give people a sense of scaleand speed of the changes afoot. [Michael Casey,	we have added the rate of sea level rise now in the executive summary to stress the importace of the expected changes

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6480	4	3	3	4	20	In the executive summary on sea level change, the rate of sea level rise in 2100 (table 4.3, page 40) should be reflected. Actual rates are 3.5 mm/y (2005-2015; table 4.1, page 23). Under RCP8.5, the projected rate in 2100 amounts to 18 mm/y, which would have grave (devastating) complications for the stability of sandy shorelines worldwide. The last time we had these rates, the North Sea region was inundated, Indonesia detached from Asia! At least for "developed" countries, the challenge of managing coastal retreat will probably become larger than the challenge of responding to higher storm surges (by building higher sea walls). Proposal: "Under all emission scenarios, the rate of global mean sea level rise by the end of this century will be significantly higher than today (high confidence). Actual rates of global mean sea level rise are 3.5 mm/y (3.3-3.7). Projections of the rate of global mean sea level rise in 2100 under RCP2.6 results in 5 mm/y, under RCP4.5 in 8 mm/y and under RCP8.5 in 18 mm/y." [Jacobus Hofstede, Germany]	Yes it is correct that rates of sea level rise are important, for various adaptation strategies so we emphasized this point by mentioning it in the executive summary
12798	4	3	5	3	6	The opening sentence makes it sound like temperature and sea-level are strongly correlated. But presumably the picture is more complicated with time lags, abrupt changes etc. and the driver of sea-level change is really changes in the energy balance. It is good to start with a simple, high-impact statement but I wonder if this isn't going to be too much of an oversimplification for the experts? [Collins Matthew, UK]	sentence rewritten, eliminating this issue.
24930	4	3	5	4	20	Consider re-ordering these summary points, perhaps starting with past observations and then moving on to future expectations. [Elizabeth Weatherhead, USA]	The section has been reordered and moves over time and also from global to local.
11886	4	3	5			there is need providing an already existing literature in order to substantiate this prediction, before introducing the second sentence. [Chukwuma Anoruo, Nigeria]	referencing in the ES is limited to in-cha-pter sections, generally at the end of each statement.
20488	4	3	6	3	7	Consider re-phrasing. Sentence incomplete: "Geological evidence of sea level […change?], and tide gauge and satellite observations […]". [Frank Oliva, Canada]	This sentence has been rewritten.
11888	4	3	6			the conjuction used in this context should be placed in a simple manner. Example, sealevel,tide guage and satellite. [Chukwuma Anoruo, Nigeria]	This sentence has been rewritten.

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2940	4	3	10	3	16	However, insolation was quite different during the Last Interglacial; the implication that the difference between LIG and modern sea level is entirely due to global mean surface temperature is a bit misleading [Robert Kopp, USA]	An appropriate caveat, referring to insolation chagnes, has been added.
11890	4	3	10			provide evidence with literature. [Chukwuma Anoruo, Nigeria]	In an ES, references are restricted to chapter subsections at end on bullets, as done here.
12536	4	3	11	3	12	while true, this statement about global mean temperature is misleading and irrelevant because what is important is polar temperatures. It would be more honest to say "Peak global mean temperature during the Last Interglacial (130 to 115 thousand years ago) is estimated to be only 0.5°C–1.0°C warmer than pre-industrial, but polar temperatures reached levels comparable to those expected in 2100 under some scenarios. the resulting sea level was 6–9 m higher (medium confidence)." your current formulation guides the reader to think that a 1 degree global warming due to GHG (as now) is enough to raise sea level by 6-9 m, which almost no-one believes. [Eric Wolff, UK]	see response to comment 2940.
23176	4	3	11	3	14	LIG is used on line 14 but has not been defined previously. Add (LIG) after Last Interglcial on line 11 [Sebastian Weissenberger, Canada]	Accepted and changed.
11892	4	3	11			should be 115 to 130. this should make clear an ascending year order. [Chukwuma Anoruo, Nigeria]	Accepted and changed.
18388	4	3	12	3	12	The estimates of LIG sea level are not certain, so assigning 'medium confidence' is correct but saying 'sea level WAS 6-9 m higher' is too certain, perhaps change to 'sea level is thought to have been 6-9m higher' [Nicholas Golledge, New Zealand]	As long as the statement is accompanied by a confidence assessment, such langauge is clear and appropriate.
23944	4	3	12	3	12	please be consistent with the style of writing 1°C/1.0°C (in the Chapter text and in other Chapters it is 1°C) [Hans-Otto Poertner and WGII TSU, Germany]	MAYA: Accepted and addressed
11894	4	3	12			the sea level should be presented in this format"6m-9m" for clear comparison of height [Chukwuma Anoruo, Nigeria]	Editorial point to be decided according to IPCC foramting rules.

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8240	4	3	13	3	14	Please specify the sea level along with temperature at MPWP in meters (even if its approximate). It has just been stated 'higher' how much higher and with how much certainity . [APECS Group Review, Germany]	We do not wish to specify how much because the uncertainty is too large, as now made clearer.
1436	4	3	14	3	14	LIG: was not defined earlier [Harry Zekollari, Switzerland]	Accepted and addressed.
20490	4	3	14			Add acronym to line 11: "Last Interglacial (LIG) [Frank Oliva, Canada]	See response to comment 1436
18272	4	3	15	3	15	Please avoid using the term "deeply uncertian" here, please use "highly uncertain". [Laurens Bouwer, Netherlands]	Deep uncertainty is defined in CCB-4 of this report so this term is appropriate.
8242	4	3	15	3	16	Are there any studies if yes, then what do they suggest what is the extent of sea ice at LIG and Mid Pliocene Warm Period. With how much certainity level the present day RSL and GMSL can be compared to these paleo events sea level. [APECS Group Review, Germany]	Please see chapter subsection for details. Given the uncertainty, now made clearer, more detail woud not be appropriate for the ES.
17356	4	3	16	3	16	Specific to this point but also general to this chapter: use of the word "uncertain" can be taken (especially by a policy maker) to mean that ice sheets may not respond to even these MPWP or LIG periods, so sea-level rise might not be that significant. It is therefore important to specify that "uncertain" in this and other contexts means that there is a range, and the rate and magnitude of SLR is uncertain but that SL was a mimimum X meters but might have been higher. [Pamela Pearson, USA]	Given the level of uncertainty attached to the paleo studies, we prefer not to give specific values in the ES. These can be found, with appropriate details, in the underlying chapter subsections.
8244	4	3	18	3	18	"The rate of sea level rise is accelerating". This implies the third order differential. Consider changing to "sea level rise is accelerating" or "the rate of sea level rise is increasing" [APECS Group Review, Germany]	Accepted and changed.
8246	4	3	18	3	18	Consider adding a time scale to lead statement - e.g. the rate of sea level rise has increased over the last 150 years. [APECS Group Review, Germany]	This statement has been removed and material combined with another bullet and shortened.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4048	4	3	18	3	21	I strongly disagree with this statement as it is completely misleading. The satellite altimetry era is very short and embedded with internal climate mode forcings that unless they can be identified and removed, will bias any particular trend analysis, let alone determine whether the increase in MSL is accelerating? Modelling is nothing more than a synthetic, mathematically derived tool developed for prediction purposes. To suggest that the results of "modelling" is somehow relevant in this context is not a sound scientific finding supporting the conclusion. I have recently completed doctoral studies on the topic "Improved Techniques to Estimate Mean Sea Level, Velocity and Acceleration from Long Ocean Water Level Time Series to Augment Sea Level (and Climate Change) Research" (Watson, 2018) which applied sophisticated time series analysis techniques to the longest tide gauge records available around the USA (Watson, 2016) and Europe (Watson, 2017) exceeding 80 years in length. This analysis is the most extensive undertaken to date searching for signs of acceleration in these records. The conclusions reached have far ranging implications and include: • Real-time measured velocity and acceleration provide an improved understanding of the time-varying properties of mean sea level; • The comparatively low time varying velocities and associated accelerations evident over the majority of historical records analysed, deem that acceleration is unwisely measured as a simple metric. Until such time as the apparent real-time velocities and accelerations in the mean sea level signal are sufficiently large not to be obscured by complex influences inducing decadal to multi-decadal variability and other background noise, the search for accelerations in these records require more intuitive, diagnostic considerations. For example, the search for acceleration is perhaps more practically inferred by considering whether or not peaks in the instantaneous velocity and acceleration time series are increasing, becoming more sustained or statistically abnormal (or different) over time in the context of the historical record. This type of approach will continue to be important until the extent of sea level rise (due to climate change) is sufficient to be statistically differentiated from the remnant historical record with widespread spatial coherence; • Although sea level has risen around continental USA over the period of available tide gauge records (1853 - 2014), clear and differing spatial signatures in sea level rise have emerged between the Central Pacific and west coast compared to those within the Gulf of	Most of the papers mentioned in this review comment are now considered in the section on the observational record of sea level rise. They allow to put the recent results concerning the acceleration in global mean sea level and concerning the regional comparison between climate models and tide gauge records in a wider context
4574	4	3	18	3	21	I am wondering why the acceleration is only discussed with respect to the recent decades. GMSL has started to accelerate in the 19th century (e.g. Kopp et al., 2016) and rates are currently further increasing. Of course, the early onset of GMSL is probably largely driven by natural variability (the recovery from the littler ice age), but it should be mentioned here more specifically [Sönke Dangendorf, Germany]	These details, which are important, are discussed in the chapter for sake of brevity. The ES generally highlights new findings.
12800	4	3	18	3	21	Can these rates be quatified? This seems like a key summary point for the whole report. [Collins Matthew, UK]	See response to comment 8246
12826	4	3	18	3	21	In executive summary - what is the historical sea-level rise? And what is the best estimate of SLR at 1.5C / 2C by the end of the century? These are likely to be important in the FAQ too. Audience will want to know these key points - how much can mitigation affect SLR. [Stephen Cornelius, UK]	See response to comment 8246. These details can be found in the sbusections of the chapter and are not needed in this ES.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17730	4	3	18	3	21	It is unclear from the report how the different sources of sea level information are combined to a single estimate of the rate of rise and the accelaration stated here. Obviously, data can in principle not be mixed as an ocean-wide observation of sea level using satellites is different from a local measurement with a tide gauge. It is now left to the reader to guess how these results have been combined. Especially the measurements with satellites should be expected to suffer from statistical uncertainty as only a few decades of observations are available, making a projection into the future highly uncertain. A similar argument is valid for tide gauge observations which are (sometime) considerably longer but lack spatial coverage. All in all the reviewer feels that the confidence level for this conclusion is too high [Hessel Voortman, Netherlands]	These important details are not appropriate for the ES and are provided in subsection 4.2.3.1.
23178	4	3	18	3	21	I think that before presenting the fact that SLR has accelerated more than previously thought, it would be good to present the general conclusions about SLR acceleration, e.g. from AR5, i.e. how it has gone from around 0.3 mm.y-1 during the preindustrial age to 1-3 mm.y-1 during the last century to over 3 mm.y-1 since satellite measurements. The recent upwards adjustment of this accelerating trend is a secondary information. [Sebastian Weissenberger, Canada]	See response to comment 8246
24926	4	3	18	3	21	It is difficult to merge the “high confidence” associated with “the rate of sea level rise is accelerating” with the “medium confidence” on the “sea level rise in the last several decades has accelerated more than shown previously.” I recognize the subtle distinction, but this differentiation perhaps does not belong in an executive summary. [Elizabeth Weatherhead, USA]	Section rewritten and differentation removed.
11896	4	3	18			provide evidence with literatures. [Chukwuma Anoruo, Nigeria]	Statement removed.
14356	4	3	19			modeling' not 'modelling' in US English [Christopher Fogwill, UK]	Editorial - IPCC-level decision
8248	4	3	20	3	20	Consider changing "the pace of sea level rise ... has accelerated" to "the pace of sea level rise ... has increased". i.e. the rate of change has increased. Otherwise, use sea level rise has accelerated. [APECS Group Review, Germany]	Section removed and material rewritten appropriately in another bullet.
11898	4	3	20			substantiate with literature after "earlier estimates" [Chukwuma Anoruo, Nigeria]	see comment 8248 response
2942	4	3	23	3	25	It seems overly strong to say that attribution of exteme sea-level events is not yet possible; if mean sea level rise can be attributed, then the enhancement of an extreme sea level event by a higher base level is also attributable. [Robert Kopp, USA]	Sentence moved and rewritten accordingly.
4050	4	3	23	3	25	Similar to and based on the same conclusions as the previous comment, I strongly disagree with the descriptive assertion of “high confidence” attributed to such a broad, profound statement. [Phil Watson, Australia]	Line 23 has ben moved and this assessment is supported by the cited chapter subsection. The assertion of the other sentence has been changed but in any event, the confidence statement does not apply to the second sentence.
8250	4	3	23	3	25	What is the rationale behind the statement "Human activity was the predominant cause of global mean sea level rise since 1970 (high confidence)" it is true at certain specific regions, but not for all regions, and also please provide the proof of direct human influence on sea level rise. (with references) [APECS Group Review, Germany]	Please refer to the chapter subsection cited for the evidenc behind this statement. Note that it refers to global mean sea level only. The underlying discussion within the chapter and the subsequent ES sentence make clear that such an attribution cannot be asserted for most locations.
17732	4	3	23	3	25	Based on tide gauge data, sea level rise is occurring since at least 1880 albeit data is sometimes limited. It is therefore not realistic to attribute the rise completely to anthropogenic forcing starting in 1970. [Hessel Voortman, Netherlands]	The statement does NOT say that human activity is the only cause. Rather, we state that it is the "predominant" cause. For other causes, the chatper subsection cited provides evidence.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
11900	4	3	23			activity should be in plural, as well as cause. There stand to exist more than one activity that causes warming, leading to sea level rise as the result of glacier. Also an evidence should be provided to make strong claim. [Chukwuma Anoruo, Nigeria]	"activity" is a generic term used often by IPCC to cover many activities. Evidence is provided in the subsection cited - such details are not appropriate for an ES.
1398	4	3	24	3	25	Approximately, attribution of regional and local mean sea level change and individual events of extreme sea level is possible [Ali Mahmood, Iraq]	We have modified this statement upon revision to take account of a few attributions in the literature for RSL and ESL.
17590	4	3	27	3	27	It's not really retreat and rapid needs some sort of definition because it sounds potentially alarmist without justification otherwise. I would prefer wording more like "Substantial wastage of some…" with a number for substantial (either Gt or SLE). [Jonathan Bamber, UK]	This language has been eliminated.
11620	4	3	27	3	28	potential for dynamical ice processes and its effect on accelerate to sea level rise has to be comprehensively integrated with greater accuracy in ice-atmosphere interaction and dynamic ice dicharge models. [Ramesh Soysa, Sri Lanka]	We are unsure what edit is suggested by the reviewer.
17592	4	3	27	3	31	AS it reads, it suggests that Greenland will be a minor contributor to 21st C SLR compared to AIS but this is not the case as shown later. Needs rewording. [Jonathan Bamber, UK]	Comparison with Greenland has been eliminated.
8252	4	3	28	3	30	Greenland ice melt if not contributing to the dynamical ice discharge to oceans so significantly, would however, contribute to the rise in sea level (how much percent) compared to Antarctic ice discharge. [APECS Group Review, Germany]	See response to comment 17592
24492	4	3	28	3	31	This might have to be doublechecked with the Executive Summary of Chapter 3 which says about Antarctica and Greenland: “For both ice sheets, there is high confidence that ocean-ice sheet interaction drives key ice sheet mass loss processes.” as well as "The large mass loss in Greenland is very likely caused by enhanced surface melt, runoff and glacier flow.” [Hans-Otto Poertner and WGII TSU, Germany]	See response to comment 17592
11902	4	3	29			provide evidence after" discharge of the ocean" [Chukwuma Anoruo, Nigeria]	See response to comment 17592
6478	4	3	33	3	38	With respect to global mean sea level rise projections, only the high emission scenario (RCP8.5) is reflected in the executive summary. This gives an incomplete picture of possible futures and "invites" critisism and scepticism. It is urged to consider the RCP2.6 and RCP4.5 scenarios in the executive summary as well (table 4.3, page 40), e.g.: "For low and medium emission scenarios, different modelling studies confirm the AR5 projections on mean sea level rise with a small tendency for higher values (high confidence). Projections of global mean sea level rise under RCP2.6 results in 0.40 m (0.31–0.51 m, likely range) for the period 2081–2100, and 0.44 m (0.33–0.56 m) in 2100. Projections of global mean sea level rise under RCP4.5 results in 0.54 m (0.43–0.66 m, likely range) for the period 2081–2100, and 0.59 m (0.47–0.73 m) in 2100." [Jacobus Hofstede, Germany]	yes we agree and have added that

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17734	4	3	33	3	38	This conclusion is conditional on the emission scenario and appears to implicitly neglect a number of sources of uncertainty. Going from an emission scenario to sea level rise implies a number of causal relations, all uncertain. For instance, is uncertainty in climate sensitivity incorporated when establishing this conclusion? Is the fact that the response of Antarctica and Greenland rests on a limited number of samples combined with modelling properly translated in uncertainty and is subsequently this uncertainty incorporated when establishing the confidence level expressed here? Several other steps with related uncertainties can be identified. All in all, the possible range of sea levels in 2081-2100 is possibly larger than indicated here. In a decision-making context, the actual development over time is of prime importance. A qualititative indication would already be helpful: should we prepare for periods with sudden rapid changes of sea level or should we prepare for a gradual, although accelerating, rise. The actual pattern of rise is of prime importance for decision-making, especially on adaptation [Hessel Voortman, Netherlands]	The report reflects an assessment of existing literature and includes uncertainties related to emission scenarios and indeed on a limited number of studies capturing the Antarctic contribution. This brings in a certain uncertainty estimates as explained in the text. We can not quantify the uncertainty in the unknowns. Time evolution of the sea level is provided so that is covered and shows no abrupt changes.
20728	4	3	33	3	38	Not substantiated - see later comments. Also needs to define 'significantly greater', as this could imply more than the ~30 cm increase. [Tamsin Edwards, UK]	Indeed the upper end of the GMSL range here is more than 30cm greater than the corresponding AR5 range.
21584	4	3	33	3	38	Should the benefits of the Paris Agreement be mentioned? [Robert Nicholls, UK]	This is covered in SR15.
24928	4	3	33	3	38	This summary point is too technical and is out of line with the other summary points for this chapter and for the other chapters. [Elizabeth Weatherhead, USA]	Total GMSL rise values are of great importance to policy makers and there is ample precedent for including them in the ES.
11904	4	3	33		34	provide evidence with literature [Chukwuma Anoruo, Nigeria]	See the subsection referenced at end of the bullet for evidence. No room in an ES for all details, even some important ones.
14358	4	3	33			modeling' not 'modelling' in US English [Christopher Fogwill, UK]	EDITORIAL - IPCC will decide.
20492	4	3	34			Consider using decimetre for clarity. [Frank Oliva, Canada]	We believe using centimeters is more consistent with IPCC style and the literature on which these numbers are based.
18390	4	3	35	3	38	This sentence is extremely misleading and must be changed. It implies a consensus between models that ‘dynamic contributions’ from AIS will lead to very high SLR, but this is simply not true. A range of SLR based on different models must be shown here. [Nicholas Golledge, New Zealand]	The values shown are the assessed range based on the ranges produced by the relevant models in conjucntion with expert judgment.
11884	4	3	35		37	Include a confidence level, to substantiate the comment. [Chukwuma Anoruo, Nigeria]	The medium confidence statement for the bolded bullet pertains to the entire statement. The specific numbers are the details on which the "several tens of centimeters" is based.
11906	4	3	36			make clear the comparison in meters "0.66m-1.13m. Also 0.82m-1.33m [Chukwuma Anoruo, Nigeria]	We do not understand the reviewer's point.
4162	4	3	37	3	38	could add a qualifying parenthetical statement here "(not including the several tenths of a meter assessed by AR5)" [Peter Clark, USA]	This is not correct. The change in SROCC is that the "several tens of meters" has been quantified and asessed as a likely range here.
11726	4	3	37	3	38	I think it is also important to note that there is a very significant overlap in the likely range between the AR5 projections and the present results. This is even more true when the AR5 comments on a potential MISI contribution are included. I would hve thought that this would not normally been interpreted as significantly greater? [John Church, Australia]	See comment 11726. The SROCC likely range, which is what we give here, is signifcantly greater than the AR5 likely range. We give ample attention in the underlying chapter to the AR5 assessment on this point but noting it here is not appropriate for an ES.
12802	4	3	37	3	38	Maybe good to breifly explain why the magintude and range is significantly greater than earlier assessments. [Collins Matthew, UK]	This issue is amply convered in the underlying chapter and would occupy too much space in the ES to adequately explain.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
6234	4	3	40	3	40	replace cliff by sheet: marine ice sheet instabliliy. While there is abundant literature on MISI there is only 2 papers (same authors) suggesting an ice cliff instability. This should not be elevated to an excecutive summary at this point. [Regine Hock, USA]	Please see our response to comment 4602. MISI is dealt with in th likely range in SROCC. MICI comes into play for outcomes with sea level rise above the likely range and over the longer term. The latter is the subject of this bullet.
4602	4	3	40	3	44	There is no basis for a Likely Range, and this must be changed. Every known marine-ending glacier under sufficiently warm conditions has lost its ice shelf and calves from a cliff by processes that are not well understood but that must increase calving with increasing cliff height. Most models used to assess future sea-level rise (e.g., Ritz et al., 2015) do not include cliff calving. The Pollard et al and DeConto and Pollard work, which includes cliff calving, does NOT provide a worst-case scenario, as noted in comments on Ch. 3, but includes parameterizations of physical processes that are known to occur and omitted from other models. There is simply no basis to use models omitting known physics to define a likely range, and then to put the physically more complete model into a "tail" of the probability distribution without explanation. This needs to go back to the WG1 AR4 wording. Essentially, the "likely" range being used here is a likely range around a lower limit on sea-level rise, not a best estimate. The IPCC would never include results from a model excluding water-vapor feedback in a "likely" estimate of climate sensitivity; the IPCC similarly should not use results from models excluding known physics that contribute to sea-level rise in estimating the likely range for sea-level rise. This is important; the current treatment is wrong. The AR4 approach,quantifying what is known and identifying what cannot be quantified, was not popular with those who want everything to be quantified, but it is the most accurate way to deal with the real issues. (There is some chance that another paper or two will be published bearing on this issue before the final deadline, but note that if another paper or two changes the likely range, it is not determined with the level of confidence normally associated with the IPCC.) [Richard B. Alley, USA]	This comment raises some important issues and we thank the reviewer for pushing us to clarify the basis of our judgments. We wish the reviewer had commented on the underlying chapter material because that is the place where many of the details of our argument are discussed. First, it is not true that IPCC does not assign a likely range to results draw from models that are inncomplete, that is, where identified processes are not rperesented. IPCC does this regularly, for instance, by assigning a likely range for climate sensitivity while only those processes related to fast feedbacks are taken into account in the models considered and where some known processes, like permafrost feedback, are not well understood. It is the role of the authors to make judgments of the likelihood that such missing processes will contribute to the range of likely outcomes and indicate the confidence in that judgment. In this case (Ch.4), the range was based primarily on two models as well as additional considerations as detailed in the chapter (and made clearer in the SOD, thanks in part to the comments of this reviewer). One of these model does contain a parameterized version of ice cliff instability. The other does not but we believe this covers current literature views on whether these processes are important in the century-scale context. There is also the issue of the upper range of outcomes allowed by this parameterization. In the SOD, we will address this issue more clearly than we do here, thanks in part to this comment. In other words, it has yet to be seen whether MICI is as important to ice sheet dynamics in the 100-year timeframe as water vapor feedback is to climate sensitvity. Current evidence argues that it is unlikely to be the case but we have only medium confidence in this judgment. Thus we account for the uncertainties in the literature available in two ways: by situating the outcomes the reviewer posits as higher than those in the likley range but certainly with non-negligble probability, and then indicating only limited confidence in this judgment. This is our judgment as authors and we believe it closely aligns wiht current views in the relevant expert community. The situation is markedly different from AR4 where
17594	4	3	40	3	44	It's not just MICI that makes it uncertain and it is important here (and elsewhere) not to focus solely on the +ve feedbacks but all. So, e.g. the role of viscoelastic rebound is also a factor in the uncertain contribution. [Jonathan Bamber, UK]	See comments 4602 and 6234. Rebound is discussed in the underlying chapter and is not appropriate for the ES.
11908	4	3	43			provide evidence using gloss. After "21 century and beyond" [Chukwuma Anoruo, Nigeria]	We do not understand the reviewer's request.
18274	4	3	46	3	46	Please replace "climate scenario" with "emission scenario". [Laurens Bouwer, Netherlands]	RCPs are concentration scenarios tied to both emissions and climate outcomes. We believe climate scenario is more informative because each RCP is tied to a specific tempeature range but the relation to emissions scenarios is more complex.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4134	4	3	46	3	47	I'm not sure that I agree with this statement. The sea level rise is dependent on the climate scenario - "strongly" is a subjective term. Sea level rise at the end of the 21st Century is much less scenario-dependent that global surface temperature. In addition, I think there is considerable uncertainty around the scenario-dependence of the Antartic contribution - more so than for other terms. Several studies suggest that ocean-ice-sheet interaction is important driver of potential future instability, with small-scale wind-driven processes playing a key role. It is not clear that climate forcing will dominate the fate of Antarctica on these timescales. [Matthew Palmer, UK]	We believe it is justified to use the word strongly here as in our assessment the mean values for RCP8.5 are outside the likely range of the estimates for RCP4.5 we specified this more explicitly in the statement by adding the reference to RCP8.5 in the sttement
6238	4	3	46	3	47	why especially in terms of Antarctica. If an ice sheet instability is already underway (some papers argue it is), then Antarctica is the place where climate matters least, i.e. the scenario has little impact. [Regine Hock, USA]	The addition especially for Antarctica is made because the difference in our assessment is 20 cm for the central values between RCP.5 and RCP4.5.
20730	4	3	46	3	47	Not substantiated - see later comments. [Tamsin Edwards, UK]	see reply to comment 4134
12804	4	3	46	4	2	Is this an appropriate point to say something about commitment and post-2100 sea level rise? [Collins Matthew, UK]	Point accepted and dealt with in SOD ES.
12828	4	3	46	4	2	There is an important message that needs further highlighting - that deep and rapid cuts to GHG emissions will lead to lower SLR by end of the century. Link to SR1.5C if possible. [Stephen Cornelius, UK]	We agree with this point and have clarified this in the new 2nd executive statement
21586	4	3	46	4	2	Sea-level rise is a very long-term process. Our work for 1.5 degrees suggests that mean sea-level rise is dependent on emissions pathways in 2100 but not strongly dependent (Nicholls et al., 2018). The effect is much more pronounced in the 22nd and 23rd Centuries. I also note Figure 4.14 and associated work with a method which looks at this for extreme sea level -- the argument needs more development and better cross referencing. Reference: Nicholls RJ et al. 2018. Stabilization of global temperature at 1.5°C and 2.0°C: implications for coastal areas. Phil.Trans. R. Soc. A 376: 20160448. http://dx.doi.org/10.1098/rsta.2016.0448 [Robert Nicholls, UK]	see reply to comment 4134
16074	4	3	47	4	1	Amend: "This points to the potential role of greenhouse gas mitigation in minimizing risk to low-lying coastlines, islands and States." [Nathan Ross, New Zealand]	Passage eliminated in revision
24590	4	4	0			Executive summary should highlight quantifiable impacts and response measures, as well as flag risks, adaptation and limts to adaptation. [Hans-Otto Poertner and WGII TSU, Germany]	Noted and taken into account.
2944	4	4	4	4	4	Whether or not flooding is disasterous depends upon exposure and vulnerability, not just the changing hazard. More appropriate to say "historicaly rate levels of flooding" without making assertions about consequences. [Robert Kopp, USA]	Noted and taken into account in edit for SOD.
8254	4	4	4	4	4	Remove emotive language "disasterous" [APECS Group Review, Germany]	Accepted and done.
24400	4	4	4	4	4	Add clarification on ESL (eg storm surges) [Hans-Otto Poertner and WGII TSU, Germany]	Accepted and edited accordingly
17142	4	4	4	4	5	I would suggest to say 'more common', as just 'common' suggests that it is happening everywhere, while lateron in the statement this is refined to saying 'at many low-lying coastal areas', which is slightly contradctory. [Aimee Slangen, Netherlands]	Noted; editorial action taken to clarify intended meaning.
18276	4	4	4	4	5	Adaptation can (and probably will) mitigate much of the disastrous flooding associated with ESL events, so this statement is not correct. It needs at least a qualifier, such as "without adaptation". [Laurens Bouwer, Netherlands]	Noted; addressed in substantive and editorial changes made in ES

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4136	4	4	4	4	12	The text states that sea level rise with "amplify the height and frequency" of ESLs. I think this Is potentially misleading/confusing to the reader. Surely the explanation is that the sea level rise has increased the baseline water level on which drivers of extreme levels are superimposed? I would suggest that the text indicate that changes in ESL are likely to be dominated by the change in baseline water level, causing ESLs that are historically rare to become much more common. Do the authors wish to be specific about "rare" events becoming "annual" events? I would say that this is not a very robust statement, since it must vary widely geographically? [Matthew Palmer, UK]	Text edited to be clearer and simpler.
8256	4	4	4	4	12	I disagree with the "high confidence" given to the statement. The Chapter includes a few studies that demonstrate changes to return period / frequency of extreme events with increasing mean sea level. The statement that 0.01 annual probability events will be annual probability events appears to originate, as far as I can tell, from work performed for this report and is not peer reviewed. Please consider changing the statement to represent the findings of the other published studies discussed i the Chapter. Also note that the studies referenced do not include, for example decadal and inter-annual variability and local, relative sea level variability that is not included in climate model projections, such as vertical land movement from subsidence, so confidence in the projections cannot be high. [APECS Group Review, Germany]	The statement is based on one published study, one submitted study, and the findings of SR15. The changes predicted by these studies are so large at many locations that we can have high confidence that other factors would not challenge the assertion here. Nevertheless these factors are discussed in the section referenced.
12806	4	4	4	4	20	Cross reference to chapter 6. Coulds the probablities be expressed in terms of return leves? [Collins Matthew, UK]	We use both frequency and level sin the chapter text but prefer to keep the ES presentation simple and so have chosen frequency here.
14360	4	4	4			comma between flooding and which [Christopher Fogwill, UK]	Editorial
11140	4	4	5	4	5	"become more common by 2100" would be better than "become common by 2100". [Inseong Han, Republic of Korea]	Noted; addressed in substantive and editorial changes made in ES
24496	4	4	5	4	5	Please clarify which emission scanario(s) you refer to. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted and edited accordingly
2946	4	4	8	4	8	I suggest "1% annual probability". Over what time period is the 'historical' here? [Robert Kopp, USA]	We are trying to uniformly use annual prboability rather than % as we believe it is more easily understood. The definition of historical is location dependent and varies with the types of records available. We believe the generic term is appropriate here.
14362	4	4	8			no comma needed after parenthesis [Christopher Fogwill, UK]	Editorial
4164	4	4	9	4	9	would be helpful to define "low lying" here [Peter Clark, USA]	Noted; this definition is best framed in Ch1 of SROCC
2948	4	4	10	4	10	"annual ESLs" -> "ESLs annually" [Robert Kopp, USA]	Edited accordingly
24934	4	4	14	4	14	“ocean wave characteristics?” I don’t see mention of storm surge. Does this belong here? [Elizabeth Weatherhead, USA]	Storm surge covered under ESL, where it will be mentioned explcitly in the SOD.
2950	4	4	14	4	15	Weirdly constructed. Why are changes in ocean waves important to estimating future changes in mean RSL? [Robert Kopp, USA]	Issue eliminated in editing for SOD
4138	4	4	14	4	19	Since you are higlighting some non-climatic factors, what about tectonic activity? These processes are also very important in some regions. [Matthew Palmer, UK]	Other factors covered in chapter sections but in most places studied, subsidence is the biggest issue.
8258	4	4	14	4	20	Consider summarising wave direction effect on coastal erosion [APECS Group Review, Germany]	Covered in chapter section, not appropriate for ES.
11910	4	4	17		18	provide evidence to this claim [Chukwuma Anoruo, Nigeria]	See referenced sections of main text.
2952	4	4	18	4	18	Find another word for "relative", given the usage of RSL. [Robert Kopp, USA]	Change accepted and implemented.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14364	4	4	18			no hyphen needed between level and rise [Christopher Fogwill, UK]	Editorial
18278	4	4	22	4	43	Please add here that with regard to attribution, there is also strong evidence of i) adaptaton actions being taken to reduce vulnerability and expsoure. Now the text is very pessimistic about these drivers. And ii) there is also empirial evidence of coastal risks (especially vulnerability) being reduced, as a result of better forecasting of ESLs and eraly warning and other response options, inlcuding improved protection/adaptation. I give some references in my other comments, below. [Laurens Bouwer, Netherlands]	Accepted and taken into account in SOD, including more detail about responses and their variable impacts in different coastal settings (e.g., 4.4.3).
12240	4	4	22	5	5	Beyond the effects of climate change-related impacts on coastal systems, it need be stressed that major fatalities observed globally can be attributed to inevitable human interactions characterised by ill planning and preparations, incomplete understanding of the science of coastal system equilbrium and an array of illegalities in coastal use and resource explotation. [Effiom Edem Antia (Prof), Nigeria]	Accepted the influence of non-climatic anthropogenic drivers of coastal risk are spotlighted; and reiterated in the ES.
4166	4	4	24	4	24	provide some examples of these drivers [Peter Clark, USA]	Noted - examples of drivers are outlined in the relevant section of the chapter; space does not permit elaboration in the ES.
636	4	4	24	4	31	This text suggests that non-climatic drivers continues to play the major role in increasing exposure throught to the mid-21st century. However, Section 4.2.3.4 i general, and in particular Figure 4.10 therein, paints a very different picture. The accompanying text describes: "In summary extreme sea level estimates as presented in this paragraph, clearly show that as a consequence of sea level rise, events which are currently rare (e.g., with an expected return period of 100 years), will occur yearly or more frequently at many locations for RCP8.5 by the end of the century (high confidence). For some locations, this change will occur already by mid-century for RCP8.5." In other words, in some locations, climatic drivers alone can turn a current 100 year coastal flood into an annual or even more frequently event in some locations. This climate-driven increase in exposure is not at all adequately represented in the current text. (The cross-reference to Section 4.2.1.2, which deals with ice sheets and shelves, does not make sense.) [Hans-Martin Füssel, Denmark]	Noted and taken into account in revisions to the SOD - including substantive and editorial revision in the ES; and more explicit articulation of interrelationships between non-climatic and climatic drivers of coastal risk (4.3), and the associated responses to mitigate these risks (4.4)
12808	4	4	24	4	33	A nice pat-on-the-back for all those publishing on coastal risk, but it would be ncie to have some examples. [Collins Matthew, UK]	Noted - examples are included in the relevant section of the chapter; word limits do not permit elaboration in the ES
13982	4	4	24			"Anthropogenic non-climatic drivers" many local makers at the local level (where sea level rise has the greatest impact will not understand what this means so some examples would be useful. Especially as many will only read the Executive Summary. [Debra Roberts and Durban Team, South Africa]	Taken into account in revised SOD ES.
14366	4	4	25	4	26	…the course of the last century; in the absence of adequate, proactive adaptation, they will continue to do so through to the mid-21st century…' [Christopher Fogwill, UK]	Editorial changes made in SOD
17170	4	4	26	4	26	through to the mid-21st century is not correct [Jiahong Wen, China]	Taken into account in editorial changes to SOD
24498	4	4	27	4	33	Suggst to focus on more policy-relevant messages with confidence statements and keep information about the development of the quality of the assessment within limits. [Hans-Otto Poertner and WGII TSU, Germany]	Noted and taken into account in revisions in SOD.
11912	4	4	27		29	there is need to substantiate statement with literature." This has been confirmed by recent literature" please list the and/or those literature(s) [Chukwuma Anoruo, Nigeria]	Noted; relevant literature is found in the relevant section; not in ES
4168	4	4	28	4	28	SLR not prevously defined [Peter Clark, USA]	Editorial

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4170	4	4	30	4	30	Statement should be more specific rather than just saying that things have improved [Peter Clark, USA]	Noted and taken into account in revisions to SOD ES
8260	4	4	30	4	30	Replace "effect on" with "contribution to" since ice mass loss (and discharge to the oceans) directly contributes to sea level rise [APECS Group Review, Germany]	We cannot identify which passage this comment refers to.
17602	4	4	32	4	32	Bamber et al, 2018, ERL discusses the SLB and land ice budget and compares ocena mass estimates from both [Jonathan Bamber, UK]	We cannot identify which passage this comment refers to.
20494	4	4	37	4	37	Consistency: use ESL instead of extreme sea levels [Frank Oliva, Canada]	Accepted
22246	4	4	37	4	37	The first sentence of the explanation here is a bit lengthy and convoluted, which makes it a bit difficult to follow. I suggest shortening or re-wording if possible. [Andra Garner, USA]	Noted and taken into account in revisions to SOD ES
13554	4	4	37	4	39	It is not very effective to start an important statement with an uncertainty qualifier. Start by saying "There is compelling evidence..." and then qualify it, if necessary. Punchy statements need to remain punchy. [Debra Roberts and Durban Team, South Africa]	Noted and taken into account in revisions to SOD ES
11914	4	4	37		40	substantiate with literature. [Chukwuma Anoruo, Nigeria]	ES does not include citations
23180	4	4	40	4	43	Short-term is questionable. Most factors that lead to high vulnerability are structural, i.e. related to land occupation and the presence of infrastructure, which can not be changed in the short-term. How would one reduce Venice or London's vulnerability in the short term. [Sebastian Weissenberger, Canada]	Noted and taken into account in revisions to SOD ES
23182	4	4	40	4	43	exposure might not been the right term, since in the common (but not the only) terminology of vulnerability as resulting from exposure, sensitivity and adaptation capacity, exposure denotes mainly the external, climatic, factors, affecting a system, whereas sensitivity includes all the components of a system, natural or anthropic, which make it more vulnerable. This would be the more appropriate term for what the authors try to say. [Sebastian Weissenberger, Canada]	Noted and taken into account by the way in which we define these terms; consistent with the IPCC glossary.
4172	4	4	42	4	43	another vague statement unsuitable for Exec. Summary [Peter Clark, USA]	Noted and taken into account in revisions to the SOD ES.
16076	4	4	47	4	49	Amend: Add "States" (or "nations") to list. [Nathan Ross, New Zealand]	Noted and taken into account in revisions to the SOD ES; the focus on coastal systems rather than coastal states / nations to avoid having to explain the significant differences in different coastal nations.
15954	4	4	47	4	51	This section could have further substance added, as it seems to be missing the bulk of the discussion. [Tim Riding, New Zealand]	Noted and taken into account in revisions to the SOD ES.
17172	4	4	47	4	51	encourage to present more specific impacts and severity. [Jiahong Wen, China]	Noted and taken into account in revisions to SOD ES
13556	4	4	47			Some numbers please. [Debra Roberts and Durban Team, South Africa]	Noted and taken into account in revisions to the SOD ES.
6482	4	4	50	4	50	In the executive summary on impacts, the example of "coastal erosion/recession" should be inserted among "coastal flooding" and "salinization". [Jacobus Hofstede, Germany]	This comment is redundant in the light of changes to the SOD ES, with a focus on observed impacts as opposed to projected impacts / risk.
6484	4	4	50	4	50	In the executive summary on impacts, the words "and erosion" should be inserted behind the word "flooding". [Jacobus Hofstede, Germany]	Repeat comment; see above
4174	4	4	53	4	53	Chapter is about sea level, not climate change at large [Peter Clark, USA]	Noted and taken into account.
11008	4	4	53	4	55	To the list of impacts, "impacts on biodiversity and ecosystem services, coastal infrastructure, community livelihoods, and cultural and aesthetic values " I would suggesst that you include the loss of habitability of regions of established residence, whether due to flooding, erosion, salinization, etc. See some specific citations, below. [Ben Orlove, USA]	Taken into account; habitability together with the other dimensions spotlighted addresses this concern.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24500	4	4	53	4	55	Suggest to concentrate on aspects that reflect the focus of the chapter. [Hans-Otto Poertner and WGII TSU, Germany]	Noted and taken into account.
24936	4	4	53	5	5	I am surprised by the “medium evidence, medium agreement” of this statement. Are the authors sure? If so, please give more back-up. [Elizabeth Weatherhead, USA]	Noted. Taken into account in revisions to SOD
13558	4	4	55			"attribution": attributing impacts to climate change or to human activities? Again, qualifiers should appear at the end. [Debra Roberts and Durban Team, South Africa]	Noted and taken into account in SOD and ES
3730	4	4	56	4	57	Please insert "tectonic movement" into brackets as one of the non-climatic causes of SLR [Serhat Sensoy, Turkey]	See resposne to comment 4138
24502	4	4	57	5	2	This statement may be confusing as usually impacts on these ecosystems are described (not impacts coming from them or their nature). Suggest to keep this consistent, especially in paragraphs that also adress non-experts like the Executive Summary. [Hans-Otto Poertner and WGII TSU, Germany]	Noted and taken into account in revised SOD ES
21590	4	4	57			There is talk here and elsewhere of "sediment transport" as a local process. Problems really arise from the integration of sediment transport and "sediment supply" or "sediment budget" would be much better terms. [Robert Nicholls, UK]	Noted and taken into account in revised SOD ES
4176	4	5	4	5	4	unfamiliar with term "coastal squeeze" [Peter Clark, USA]	Noted and taken into account in revised SOD ES
21592	4	5	4			"sediment starvation upstream" -- better stated as "reduced sediment supply from upstream" or something similar. [Robert Nicholls, UK]	Editorial changes made
11478	4	5	7	5	7	Congratulations on getting to FOD, a huge achievement. As a general comment, I was looking for key messages surrounding the law (at all levels, international e.g UNCLOS and UNFCCC through to national and local) and its role in responding to the challenges presented by this report. I also searched for legal content in the body of this chapter and found none. I highly recommend that you fill this gap for the SOD. As a further 'general comment the role of economics does not feature in the key messages and yet there is considerable substantive content towards the end of the chapter. [Kirsten Davies, Australia]	Noted and accepted. Work is underway to include insights from legal scholarship on SLR but this will only be completed for the FGD (4.4.4.2). Revisions in SOD have strengthened the focus on the economics of SLR. And key messages in this regard are more explicit in the SOD ES.
11480	4	5	7	5	7	This is a comment more for your thinking. SIDs is a commonly accepted term but perhaps in the context of this report we could be thinking of them more as `big ocean' island states? [Kirsten Davies, Australia]	Noted - an issue for framing the overall SROCC
11512	4	5	7	6	19	The response section in the executive summary fails to address limits to adaptation. This is an important omission that needs to be addressed. [Taehyun Park, Republic of Korea]	Noted and accepted. Work is underway to strengthen this aspect of the report chiefly through 4.4.5 but further refinement for the FGD is needed to address this important concern.
21596	4	5	7	6	19	I think an important point for coasts is that there is a long term of adaptation to climate and non-climate risk in the coastal zone, which means that there is a lot of relevant experience to respond to the challenges defined in the Chapter. [Robert Nicholls, UK]	Noted, accepted and taken into account, with further work post-SOD to locate SLR responses in wider societal context of coastal governance initiatives, etc. (4.4.4.1 and 4.4.4.2).
21604	4	5	7	6	19	Is the right balance between more bottom up and more top-down responses correct -- community-based adaptation appears in three bullets, while there is less on top-down policy driven approaches. The material on commuunity-based adaptation also lacks a narrative and should be reviewed for what are the key messages in the context of this chapter and its aims and objectives. [Robert Nicholls, UK]	Noted, accepted and taken into account in revisions to SOD ES. Work for the FGD is planned to further strengthen the narrative of 4.4.4.4 and 4.4.5 to address the balance issue raised and the key messages to be spotlighted.

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256	4	5	7			Add concepts related to adaptive design and risk management -- see Ayyub and Wright (2016) on resilience, sustainability and adaptive design and risk management doi:10.4172/2167-0587.1000e118 . Also see the anticipated ASCE Climate Resilience Infrastructure: a Design Manual on Adaptive Design and Risk Management - https://ndma.gov.in/iwdri/ppts/16jan/TS%202B/IWDRI2018_TS2B_Ayyub_UMaryland%20ASCE.pdf . https://www.asce.org/structural-engineering/news/20180418-climate-resilient-infrastructure-challenges-and-a-design-philosophy/ [Bilal Ayyub, USA]	Noted and taken into account in SOD revisions; with ongoing work on planning and design advances since AR5 post-SOD (4.4.4.2)
21594	4	5	7			"Responses" to what? Responses to sea-level rise" -- or "climate change" or "growing risk", or what? I found this too terse. The following text needs to be consistent in terms of to what is being responded. [Robert Nicholls, UK]	Noted, the term responses is explained in the opening section of 4.4.
638	4	5	8	5	12	This text downplays the role of observed and projected climate change in increasing (standards for) coastal protection. It seems to be based on a sentence on p. 4-76, ll. 45-47, which is not supported by any reference. In Europe, the continent I am most familiar with, most coastal countries have already updated, or are in the process of adapting design standards for coastal protection in response to past and future climate change. Many countries have also made dedicated funds available to improve coastal protection standards. Further information is available e.g. in the 7th National Communications under the UNFCCC. [Hans-Martin Füssel, Denmark]	Accepted and addressed in the revised SOD ES and relevant sections
4178	4	5	9	5	9	by advance, do you mean advance (move) into/towards the coastal zone? Is this really an adaptation strategy? [Peter Clark, USA]	See explanation in revised section 4.4.2 and 4.4.3
2954	4	5	9	5	11	Note that chapter 1 does not mention 'advance' as a strategy. This should be worked out between chapters. [Robert Kopp, USA]	Noted
16078	4	5	9	5	15	Although relocation is occurring, note that it can have its own significant adverse impacts in terms of (1) landlessness, (2) joblessness, (3) homelessness, (4) marginalisation, (5) food insecurity, (6)increased morbidity, (7) loss of access to common property, (8) community dislocation, and (9) loss of culture. See: * Michael M Cernea "Impoverishment Risks, Risk Management, and Reconstruction: A Model of Population Displacement and Resettlement" (Keynote Paper presented to the UN Symposium on Hydropower and Sustainable Development, Beijing, October 2000) * Nathan Jon Ross "Risks to Representative Government in Kiribati" available https://nathanrossconz.files.wordpress.com/2016/11/nathan-jon-ross-climate-change-risks-to-representative-government-in-kiribati-21-cljp-jdcp-91.pdf * Mariya Gromilova "Revisiting Planned Relocation as a Climate Change Adaptation Strategy: The Added Value of a Human Rights-Based Approach" (2014) 10(1) Utrecht Law Review 76 * Sophie Pascoe "Sailing the Waves on Our Own: Climate Change Migration, Self-Determination and the Carteret Islands" (2015) 15(2) QUT Law Review 72 [Nathan Ross, New Zealand]	Accepted and taken into account in revised section in SOD (4.4.2)

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24938	4	5	9	5	15	“Responses seldom explicitly target sea level rise (high evidence, medium confidence).” This is far from my experience. Numerous climate risk companies exist that are directly addressing these issues, and numerous government agencies are directly addressing sea level rise. Many examples are available from Charleston, Miami, NYC, San Francisco, Venice, Bangladesh, India, etc. [Elizabeth Weatherhead, USA]	Noted and taken into account in revised SOD ES. The point was poorly communicated and has been addressed to recognize that SLR responses are seldom addressed in isolation of intersecting climatic and non-climatic factors shaping coastal risk.
13560	4	5	10			"around the world" - please be specific. All around the coast of Africa and South America? In industrialised countries? Retreat (people simply moving further from the sea because they have no choice) and engineered coastal protection measures cannot be lumped together into "adaptation response" without explanation. Neither can natural dunes / mangroves be lumped together with actively restored ecosystems. How widespread is this? In a truly global sense? Same for "widespread" in line 10 and "worldwide" in lin 27. Safeguarding existing protective ecosystems [Debra Roberts and Durban Team, South Africa]	Accepted and addressed in the revised SOD ES and relevant sections
18280	4	5	11	5	11	Very surprised about this statement. I think that very few coastal vulnerability studies since the last 20 years ommit SLR. Also most IFIs are very aware of climate change and sea-level rise, and include this in an explicit manner in planning studies. So what is this statement based on? Could not find any support in section 4.4.2. [Laurens Bouwer, Netherlands]	Noted and taken into account in revised SOD ES. The point was poorly communicated and has been addressed to recognize that SLR responses are seldom addressed in isolation of intersecting climatic and non-climatic factors shaping coastal risk.
11916	4	5	12		15	substantiate with literature. There is need to make short the sentences. This will make flow the reader. [Chukwuma Anoruo, Nigeria]	Noted and taken into account in revised SOD ES.
12398	4	5	17	5	17	"advanced"? [Sylvain Ouillon, France]	Rejected - advance measures are distinguished from other PAR responses; see 4.4.2
17598	4	5	17	5	17	advance => advanced [Jonathan Bamber, UK]	Rejected - advance measures are distinguished from other PAR responses; see 4.4.2
17736	4	5	17	5	24	Reviewer agrees with the general conclusion. The conclusion that there is only medium confidence that performance of protection can be predicted appears not to be justified, at least not in Deltas where explicit performance requirements are the starting point for design [Hessel Voortman, Netherlands]	Noted and taken into account in revised text in relevant sections and SOD ES to better reflect incredible diversity of responses and the mix of synergistic interactions that are sometimes complementary and sometimes antagonistic.
21598	4	5	19			Suggest replace "in" with "through" the 21st Century [Robert Nicholls, UK]	Noted and taken into account in revised text in relevant sections and SOD ES to better reflect incredible diversity of responses and the complex sequencing of diverse responses.
15956	4	5	20	5	24	I feel that this short section could benefit from some discussion about the potential costs of hard and soft engineering options in densely populated areas; specifically the loss of functional 'natural' intertidal and high tidal ecosystems, and concomitant ecosystem and societal services. [Tim Riding, New Zealand]	Accepted and taken into account in revisions to this section of the SOD ES
8262	4	5	31	5	32	"substantial" economic benefits contradicts "actual size". Maybe omit "substantial" [APECS Group Review, Germany]	Noted and taken into account in revisions to this section of the SOD ES
8264	4	5	37	5	37	"Deep uncertainty" is too emotive and inaccurate. There is a difference in the language used by statisticians, earth scientists and the general public - consider simply "uncertainty". [APECS Group Review, Germany]	Accepted and taken into account in revisions to the SOD ES
8266	4	5	37	5	37	"Long-term", quantify e.g. "centennial time scale" [APECS Group Review, Germany]	Noted and taken into account in revisions to this section of the SOD ES

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17738	4	5	37	5	39	Long term should be defined as this might be radically different for different disciplines involved. The time reference for engineered systems is typically 50 to 100 years, with exceptions both higher and lower in special cases. It is clear that uncertainty will increase as the time horizon of the projection is pushed forward. For decision-making, the uncertainty within the time frame of the decision should be specified. [Hessel Voortman, Netherlands]	Noted and to be addressed for SROCC as a whole.
24504	4	5	37	5	39	Suggest to carve out the message more clearly by rephrasing e.g.: “Decision analysis can support the progress of adaptation in the short term even if deep uncertainty remains about long-term mean and extreme sea levels (medium evidence, high agreement). Methods range…” [Hans-Otto Poertner and WGII TSU, Germany]	Noted and taken into account in revisions to this section of the SOD ES
12400	4	5	41	5	44	This sentence is too vague in my opinion, it can be deleted or completed. Due to its "limited evidence, medium agreement", it instils reader's doubt on the report conclusions. Furthermore, nothing is detailed on the kinds of problems we can deal with: can we adapt to deltas salinization, flooding or coastal erosion at the same level/degree? At least, the list of people needed to propose adaptation (reserchers on sea level rise, decision sciences and decision makers) is too restricted. Adaptation measures should involved scientists, specialists of the different problem we would like to face (sediment transport, hydrogeology etc.). [Sylvain Ouillon, France]	Noted and taken into account.
2956	4	5	42	5	42	"decision sciences" -> "decision scientists" [Robert Kopp, USA]	Rejected. Refers to particular domain of scholarship.
21600	4	5	46	5	50	How much is this SLR versus other drivers of risk. I think most commumity based adaptation is focussed on fairly immediate problems, so as explained in the chapter multiple drivers are being addressed. [Robert Nicholls, UK]	Taken into account in revisions to SOD ES based on revisions to 4.4.4 and 4.4.5
23184	4	5	46	5	50	Community-based solutions are not exclusive to developing countries. In developed countries as well, adaptation mostly relies on local communities. It is not so much a question of development or available resources, but of political organisation. Examples and refs upon request. [Sebastian Weissenberger, Canada]	Accepted and taken into account in revisions to SOD ES
12242	4	5	46	5	56	Coastal resilience building for an array of climate change impacts will necessitate adopting and implementing RISK (Research - Innovation - Sensitization - Knowledge) in either a bottom-up or top-down mode as long as every "RISK" element is fully addressed in a participatory context. [Effiom Edem Antia (Prof), Nigeria]	Noted
21602	4	5	55	5	56	Isn't this sentence true for all responses, not just community-based adaptation? [Robert Nicholls, UK]	Accepted and taken into account in revisions to this section of the SOD ES
16080	4	6	1	6	1	"… and consensus building are essential parts of…" [Nathan Ross, New Zealand]	Noted and taken into account in revisions to this section of the SOD ES
24940	4	6	1	6	8	I am not sure that this point belongs in this chapter. It appears to be more a discussion of efficacy of different advocacy approaches, as opposed to a discussion of the science of oceans and the cryosphere. [Elizabeth Weatherhead, USA]	Rejected. The literature assessed shows that societal responses to SLR science involve making difficult choices about how to reconcile contending viewpoints, interests, etc.
21606	4	6	15	6	19	What is the biggest factor driver uncertainty in sea level rise to 2100. There are two factors -- emissions uncertainty and the climate response to a given degree of warning where there is deep uncertianty. Here you imply that emiissions is the biggest factor, but I wonder if we actually know at the present time. [Robert Nicholls, UK]	Noted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21608	4	6	15	6	19	In the main chapter the text says we can adapt to 21st century sea-level rise (see also Hinkel et al., 2018, NCC) and the problems of adaptation lie beyond 2100. This is not really consistent with this last bullet which could be reviewed in this light. It also draws out the point I have already made about lack of discussion of post 2100 changes. [Robert Nicholls, UK]	Noted and taken into account in revisions to SOD ES by among other things framing SLR responses in the more complex and nuanced way they are unfolding in practice; and will likely unfold in coming decades up to and beyond 2100.
21610	4	6	15	6	19	Post-2100 exposure, vulnerability , impacts and responses. We cannot know the world so far in the future, but if we keep responding without mitigation we have terrible challenges in the coastal zone. I think this should come across in the Executive Summary [Robert Nicholls, UK]	Accepted and taken into account in revisions to this section of the SOD ES
23946	4	6	15	6	19	Add section cross-reference [Hans-Otto Poertner and WGII TSU, Germany]	Accepted, change to be made
11918	4	6	16		18	provide evidence to this projection. [Chukwuma Anoruo, Nigeria]	Noted; evidence provided in revised ES re material related to 4.2
17358	4	6	18	6	18	See previous comment, and might want to add here something along the lines of, "..., and increasing impacts at higher coastline levels, which based on the paleo-climate record could be an additional 6-9 meters, up to 20 meters given past minimum SLR at similar temperatures, with increasing risk of irreversible SLR the longer these higher temperatures are maintained or the greater the degree of overshoot contributing to threshold-like responses." Also, consider adding population impact estimates to the ES, or (better) later in this Chapter a table showing estimates of numbers of people impacted at 1, 2, 3, 4 and beyond meters. For example, 1 meter has been estimated to impact around 180 million people; a comparison might be drawn to the current Syrian crisis where 20 million have been in movement. [Pamela Pearson, USA]	Noted; evidence provided in revised ES re material related to 4.2
23948	4	7	1			The introduction here should refer to the importance of the SDGs, which are new after AR5. [Hans-Otto Poertner and WGII TSU, Germany]	Noted; to be addressed post-SOD with development of 4.4.6
14438	4	7	3	7	4	The objective of this chapter is to assess the literature on sea level rise published since the Fifth Assessment Report (AR5) and its implications for low-lying islands, coasts, and communities. [Christophe Deissenberg, Luxembourg]	Noted.
1438	4	7	7	7	9	"..., the chapter's focus in on socio-ecological systems...". I found this passage (and section in general) a bit confusing, as it makes the reader think that this chapter almost entirely focusses on impacts/implications for low-lying regions. While this is an important aspect of the 4th chapter, the physical setting/basis forms an important part of this chapter as well (until p.56). The fact that the physical basis is also treated, only becomes clear in the next section (4.1.1). Maybe already mention in this section / make clearer? (i.e. more than mentioning "assess literature on sea level rise.." (p7, l.3)) [Harry Zekollari, Switzerland]	Noted
14440	4	7	8	7	8	and to the interlinked [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
14442	4	7	9	7	9	chapter focusses on [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
23950	4	7	13	7	14	Does the term 'human systems' not include groups and individuals, and 'ecosystems' species? [Hans-Otto Poertner and WGII TSU, Germany]	The purpose of the additional terms is to provide elaboration of the particular aspects of human systems and ecosystems that will receive attention in this report.
10730	4	7	13	7	19	This paragraph is really relevant! [Jacques Andre Ndione, Senegal]	Thank you.
23952	4	7	14	7	15	Refer also to Cross Chapter Box on risk. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted and implemented.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
3410	4	7	15	7	15	What is the rational for adopting SREX? [Mahmood Riyaz, Maldives]	While the literature has advanced since SREX, the risk framework developed for SREX continues to provide a useful approach for evaluating the literature. Elsewhere in this report (chapter 1 and CCB-Risk), controversies, critiques, and possible improvements to the SREX framework are discussed but, for the purposes of this chapter, do not justify a revision of the existing approach.
4052	4	7	21	7	21	Some general comments on the Chapter. There are some pivotal issues around sea level rise and time series analysis that have been published in the scientific literature since AR5 that have not been covered in the FOD. Whilst the sophistication of the science around sea level rise and projection modelling continues to gain pace, the application of overused, inappropriate and overly simplistic time series analysis techniques are undermining these efforts due to the coarseness of same. For example, the literature remains replate with the over use of linear regression techniques to determine mean sea level trends and the application of second order polynomials to estimate acceleration. Whilst convenient, neither are capable of accurately discerning the distinctly non-linear, time varying characteristic of mean sea level velocity, in turn reflective of associated acceleration varying over time also. The recent published works of Watson (2016, 2017, 2018a) advised above, have substantially advanced these issues with more robust determinations of mean sea level and associated time varying velocities and accelerations. These techniques improve the detection of genuine velocities and accelerations in long ocean water level records directly associated with the increasing mass of the ocean having removed biases associated with internal climate mode forcings and other dynamic (cyclical) influences. The application of these techniques to the data rich margins of the USA and Europe provide an improved understanding of the spatial variations in velocity and accelerations at the regional level than previously available. See key conclusions noted above in previous comment. The second key issue I would like to raise, relates to continued efforts to test the efficacy of the ensemble projection ocean model outputs for sea level at an increasingly localised scale. Whilst improved model outputs post AR5 are demonstrated (Slangen et al. 2017, Meyssignac et al. 2018) within error margins to accord with broad observational estimates over generally the 20th century, the work of Watson (2018b) provides improved techniques to compare observational records and AR5 projection model outputs at the regional scale over the recent decade (from 2007-2016). Importantly, this work concludes that whilst the observational and model-projected average velocity agree (95% confidence level), error margins are comparatively wide, masking the fact that the mean velocity for the model-projection products exceed observational records for nearly all 19 regional stations considered and Representative Concentration Pathway (RCP) experiments, and are likely in the range of 1.6–2.5 mm/year. The analysis might provide an early warning sign that the	We largey agree, most of the mentioned papers are addressed in the appropriate section on the observational record of sea level change
14444	4	7	23	7	24	including dynamical coastal morphology and the numerous processes contributing to sea level rise globally, regionally, and locally. [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
14446	4	7	27	7	27	Section 4.3 assesses the socio-economic [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
14448	4	7	35	7	35	Also important is how the risk perception affects the responses. [Christophe Deissenberg, Luxembourg]	Accepted and implemented.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14450	4	7	38	7	38	the literature on responses, particularly on options [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
14452	4	7	46	7	46	We anchorour presentation with specific examples of [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
16394	4	8	0	8	0	Figure 4.1 is still not clear enough to represent the overview and connection of the topics from Chapter 4. Improvement/rearrangement of the figure is needed. [Lee-Sim Lim, Malaysia]	Figure 4.1 has been replaced with an entriely difference approach.
14454	4	8	0	8	6	The Figure gathers different points addressed in the Chapter but does not put in evidence their links. It is somehwat misleading as it suggest that the Climate Resilient Development has no impact on the Drivers, that the Drivers influence the policies only through the risk, and so on. [Christophe Deissenberg, Luxembourg]	Figure 4.1 has been replaced with an entriely difference approach.
24592	4	8	0			Figure 4.1: not clear why the propeller diagram is changed so much, I am not convinced that is useful or needed.There should clearly be a consideration of adaptation capacity and its changes as well as ability to reduce risk by reducing exposure and or vulnerability. This would be a useful development of the propeller diagram (balancing with cross-chapter box on risk as well as chapter 1 needed). [Hans-Otto Poertner and WGII TSU, Germany]	Figure 4.1 has been replaced with an entriely difference approach.
10732	4	8	1	8	1	Regarding Figure 4.1, the area dedicated to "Risk" and colored in white, please make it a little bit bigger [Jacques Andre Ndione, Senegal]	Figure 4.1 has been replaced with an entriely difference approach.
23954	4	8	1	8	1	In Fig 4.1: The word "exposure" and the "risk" bubble are not centered, and therefore the whole figure looks odd. [Hans-Otto Poertner and WGII TSU, Germany]	Figure 4.1 has been replaced with an entriely difference approach.
2958	4	8	1	8	4	Why is there no link in Figure 4.1 from the drivers to hazards, exposure and vulnerability? [Robert Kopp, USA]	Figure 4.1 has been replaced with an entriely difference approach.
21626	4	8	1	8	4	Figure 4.1 -- "Development Impacts" or "Development Drivers"? [Robert Nicholls, UK]	Figure 4.1 has been replaced with an entriely difference approach.
22248	4	8	1	8	5	Figure 4.1 needs some work aesthetically: lines need to meet, symmetry is a bit off, there's no need to include shadow behind lines, arrows and boxes, etc. I also wonder if there should be arrows going back to the circles for hazards, exposure, and vulnerability from the box on the left? Adopting a design similar to Figure 6.1 would be beneficial. [Andra Garner, USA]	Figure 4.1 has been replaced with an entriely difference approach.
8268	4	8	1			The sidebars are unclear to me. Are they large arrows that point into the Venn diagram? [APECS Group Review, Germany]	Figure 4.1 has been replaced with an entriely difference approach.
8270	4	8	1			The interaction between the elliptical shapes is unclear to me. Is it a Venn diagram? Do the concepts intersect or are the shapes simply piled on top of one another? If the former, then I am confused about "Risk" and "Hazard" as it appears in the diagram that it is possible to have a risk-free hazard. [APECS Group Review, Germany]	Figure 4.1 has been replaced with an entriely difference approach.
1440	4	8	2	8	4	Figure 4.1 gives an overview of the chapter's various themes. The sections are mentioned in the caption, but I do not always immediately see which part belongs to which section from the figure. Would it maybe be worth adding section numbers directly in the figure? [Harry Zekollari, Switzerland]	Figure 4.1 has been replaced with an entriely difference approach.
18282	4	8	2	8	4	This Figure 4.1 has large overlaps but also differences with Figure 1.2 in Chapter 2 of the SROCC FOD. This should probably be coordinated between the chapters? [Laurens Bouwer, Netherlands]	Figure 4.1 has been replaced with an entriely difference approach.
300	4	8	14			add "of" after rise [Kerstin Jochumsen, Germany]	Accepted and implemented.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21616	4	8	16			"especially as they relate to flooding" -- this only refers to the "sources" of flooding and the flood system also comprises dynamic pathway and receptor components within the SPRC framework. The use of such language here and elsewhere in the report would be clearer. [Robert Nicholls, UK]	Accepted and implemented.
8272	4	8	19	8	21	Please cite a reference. [APECS Group Review, Germany]	Accepted and implemented.
22250	4	8	23	8	23	Change "address the tail" to "address the upper tail"--tails go both ways on distributions [Andra Garner, USA]	Accepted and implemented.
8274	4	8	27	8	27	Are is to be used before the word "assessed". [APECS Group Review, Germany]	This passage was edited so this is no longer an issue.
14456	4	8	27	8	27	The literature since AR5 assessed in this chapter leads us to reevaluate the AR5 projections of changes [Christophe Deissenberg, Luxembourg]	We think the wording "assessed in this chapter is unnecessary but have edited and shortened this passage in other ways.
17740	4	8	27	8	33	The comment is applicable more broadly to chapter 4. It is fully justified to state that local sea level changes may differ considerably from the globally observed changes. In fact, the reviewer is of the opinion that local effects are so important that a report with global coverage can never provide information in sufficient detail. Passages sketching local effects on sea level are therefore of limited use. [Hessel Voortman, Netherlands]	This chapter is more about regional and global than local effects, for precisely the reasons stated by the reviewer.
14458	4	8	28	8	33	(Section 4.2.3.4). The AR5 projections of regional sea level did not account for factors such as tectonics or subsidence associated with groundwater and hydrocarbon withdrawal. Doing so increases the uncertainty in the projected extremes. The AR5 projections of regional extremes of high water were also limited by the uncertainty on the projected characteristics of tropical and extratropical cyclones. Several of these uncertainties could not be removed and still limit the confidence in the updated projections of extremes for some regions and time periods. [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
8276	4	8	35	8	37	The authors need to discuss regarding the frequent flash floods and its associated changes such as landslides. eg. Kedarnath and Leh flash floods (India). [APECS Group Review, Germany]	This paragraph has been eliminated.
10734	4	8	35	8	37	Please, for these issue, you can consider the GEO SIDS published by UNEP… UNEP 2014. GEO Small Island Developing States Outlook. United Nations Environment Programme, Nairobi, Kenya. [Jacques Andre Ndione, Senegal]	This paragraph has been eliminated.
16410	4	9	1	56	16	Topic 4.2 was focus a lot on the polar regions and northern hemisphere areas, but there is no discussion on tropical regions and southern hemisphere, which are also major regions in the world an should not be overlooked. [Lee-Sim Lim, Malaysia]	We are puzzled by this comment. 4.2 is not focused on any particular region.
14460	4	9	3	9	4	AR5 comprehensively addressed coastal vulnerability and exposure to natural and human systems in many regional settings, finding [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
8278	4	9	8	9	10	The authors also need to discuss the coastal hypoxia and the expanding oxygen minimum zone too along with the global warming and ocean acidification since one phenomenon does not work in isolation. [APECS Group Review, Germany]	These impacts are important but their interaction with sea level rise in not apparent in the literature so we do not discuss them in this chapter.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14462	4	9	10	9	14	As noted in the previous subsection, the ability to perform a detailed assessment of future risk was constrained by the uncertainty in the regional projections of sea level change, of extreme sea levels, and of changes in storm frequency and intensity. An improved hazard assessment and a significant extension of the literature to more spatially and temporally explicit current and future coastal exposure and vulnerability, allows in this report an increased confidence in the assessment of coastal risk. [Christophe Deissenberg, Luxembourg]	We appreciate this suggestion but do not see the improvement it provides over the current version.
21618	4	9	16			Still unclear what responses means here -- does response include mitigation responses, for example? [Robert Nicholls, UK]	Emissions mitigation and policy toward emissions mitigation is not a central focus of this report. However, differences between RCPs are mentioned in this passage which inevitably reflects the effect of different emissions levels and this emissions mitigation. In other sections, emissions mitigation is mentioned explicitly.
14464	4	9	18	9	20	This chapter describes the different observed and available responses, the situations in which they have been applied, applied currently, their costs, benefits and co-benefits, frameworks for appraising and selecting appropriate options, and limits and barriers to their implementation. [Christophe Deissenberg, Luxembourg]	Accepted and implemented.
14466	4	9	22	9	28	We evaluate the implications for the responses of three key characteristics of future climate and sea level. These are 1) growing uncertainty in climate change arising from the increasing differences among the Representative Concentration Pathways (RCPs) beyond 2050; 2) growing uncertainty in global and regional sea level rise, due to uncertainty in the dynamical contribution from the Antarctic ice sheet, especially in the latter half of this century; 3) a resulting increase over time in uncertainty of estimated return periods for sea level extremes associated with storms and coastal flooding. Thus, the uncertainty in the projections increase with their futurity. This has implications for the effective response strategies (Section 4.4). [Christophe Deissenberg, Luxembourg]	Mostly accepted and wording modified accordingly, taking into account comment 8280 on same paragraph.
3412	4	9	22	9	29	Are these aspects discuss implication in the context of small islands specially? or generally? [Mahmood Riyaz, Maldives]	These aspects are covered in the specific case of small islands. Se for instance the case study box at end of section 4.2
8280	4	9	22	9	29	E1a It is unclear what "growing uncertainy" and "increasin over time in uncertainty" and "progressively greater uncertainty in risk with time" mean. Presumably you mean to refer to the "cone" phenomenon on e.g. emissions curves or climate response curves, where the uncertainty is greater at, say, 2090 than it is in 2050. However, the way it is currently stated, it could well be interpreted as our uncertainty next year will be more than it was this year (i.e., the science is not moving forward). Although that's clearly an incorrect interpretation, that was actually my first interpretation of how it's currently written. I might suggest "higher uncertainties farther in the future" or "higher uncertainties when looking farther ahead". [APECS Group Review, Germany]	Accepted and wording modified accordingly.
13562	4	9	26			Estimated return periods' This will probably get explained later, but at this stage of the chapter this term is not understood. [Debra Roberts and Durban Team, South Africa]	Accepted and implemented.
18284	4	9	27	9	27	Please use the term "cyclones" rather than "storms", as was done before in this chapter. [Laurens Bouwer, Netherlands]	Accepted and implemented.
2960	4	9	32	9	56	This secon should provide a definition, or definitions, of sea level. [Robert Kopp, USA]	we have included a definition of sea level in the beginning of the paragraph

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
11920	4	9	34		36	cite a source apparently [Chukwuma Anoruo, Nigeria]	rejected, this reflects where the chapter is about and does not require a reference
4180	4	9	36	9	36	rather than use "or," use "some combination of volume changes…and mass changes…" [Peter Clark, USA]	agreed, text rephrased
4142	4	9	36	9	37	Please insert some text to explain that global ocean volume changes are driven primarily from thermal expansion (mass changes are explained as originating from ice loss etc). [Matthew Palmer, UK]	accepted, text rephrased
20732	4	9	36	9	37	I think this sentence is weird to the non-expert reader. Both sources cause volume changes but by different means. Suggest 'caused by "density" changes of ocean water. Could also add something like ', which both change the volume of the oceans.' at the end. Or 'caused by the expansion of ocean waters as they warm, or by loss of land ice…' [Tamsin Edwards, UK]	solved by following previous comment 4180
2962	4	9	38	9	40	"Fingerprints" also include the effects of elastic deformation of the lithosphere. [Robert Kopp, USA]	accepted rephrased
17144	4	9	39	4	39	also earth deformation in addition to gravitational an rotation [Aimee Slangen, Netherlands]	as previous 2 comments changed accordingly
4182	4	9	39	9	39	and deformational changes [Peter Clark, USA]	as previous comment 4182 and changed accordingly
21620	4	9	40			The scales are clearly defined -- is there a scale in between regional and global -- basin scale -- North Atlantic, etc? [Robert Nicholls, UK]	we use local, regional and global basin scaled is not used as a horizontal scale
3414	4	9	41	9	41	HOw appropriate is this scale in the context of small islands to meaningfully address local scale? [Mahmood Riyaz, Maldives]	Local is used for spatial scales smaller than 10 km and hence capture small islands; no change needed
2964	4	9	42	9	42	Contrary to the sentence constructions, these are "long processes" not "changes in long processes". Tectonics, mantle dynamics, and "post-glacial rebound" need not change to cause changes in sea level; the processes themselves cause the change. [Robert Kopp, USA]	accepted rephrased together with 2964 2966 4184 14468
2966	4	9	42	9	42	"Post-glacial rebound" is a particular local expression of glacio-isostatic adjustment in the intermediate field; the more general term should be used. [Robert Kopp, USA]	accepted rephrased together with 2964 2966 4184 14468
4184	4	9	42	9	42	long as in long timescale? [Peter Clark, USA]	accepted rephrased together with 2964 2966 4184 14468
14468	4	9	42	9	42	both long-term processes [Christophe Deissenberg, Luxembourg]	accepted rephrased together with 2964 2966 4184 14468
23268	4	9	42	9	42	mantle dynamics is cited here and elsewhere in this report, but it is never defined. Should be more specific. There are many geological processes and controlling factors (dynamics) affecting the mantle and Earth. Perhaps some relevant to hydrosphere and ocean are mantle convection, mantle-crust exchange in oceanic ridges, magmatism and volcanic activities, etc. [Y. Jeffrey Yang, USA]	rejected, the term is barely used in the chapter not warranting the specification
18534	4	9	42			…dyanmics and post-glacial…' [Christopher Fogwill, UK]	accepted rephrased together with 2964 2966 4184 14468
14470	4	9	43	9	43	sediments, natural [Christophe Deissenberg, Luxembourg]	accepted rephrased together with 2964 2966 4184 14468
2968	4	9	44	9	44	"ocean surface" -> "sea-surface height"? [Robert Kopp, USA]	yes better
20864	4	9	44	9	44	RSLC acronym is not needed [Paolo Cipollini, UK]	rejected this abbreviation is used more often
2970	4	9	45	9	45	"relative sea-level change" must be defined as the CHANGE in the difference in elevation [Robert Kopp, USA]	agreed adjusted accordingly
14472	4	9	45	9	45	the land and the sea surface [Christophe Deissenberg, Luxembourg]	agreed adjusted accordingly

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3416	4	9	46	9	47	would it be possible to give a range for this xx-xx mm/y? [Mahmood Riyaz, Maldives]	No need for a change, an indication is given by a few mm/yr and reference to figure 4.4
14474	4	9	47	9	56	ividual events add further risks of flooding to this background of gradual change. Thus, the gradual changes in time and space need to be assessed together with processes have a broad spectrum of variability. These include storms, surges, waves, and tides or a combination of such events. In this section, newly emerging understanding of the different episodic and long-term aspects of sea level change are assessed, within the context of sea level changes measured directly over the last century and of those inferred for longer geological timescales. This longer-term perspective is important for contextualizing future projections of sea level and providing guidance for processes-based models of the individual components of sea level rise, including the polar ice sheets. [Christophe Deissenberg, Luxembourg]	partly accepted, the text is rephrased to express more clearly that the chapter is not only about gradual changes but also about the extreme events
4186	4	9	55	9	55	is there a reason to single out polar ice sheets? [Peter Clark, USA]	rephrased such that imprtance polar ice sheets is emphasized more
23278	4	10	0	56		These three subsections are the core of substances on coastal impacts and mechanisms (i.e., ice loss, RSL change and ESL occurrence). However, there appear to be a need for a significant revision, not only on substance, but editorial quality. Some obvious issues are listed below. [Y. Jeffrey Yang, USA]	we consider the comments below
23282	4	10	0	56		Editorial. This section starts to use "we" in describing the work - it appears to be taken from journal-type publications. The official IPCC report styles should be followed throughout this SR. Line 30. Term "the 5-95 percentile uncertainty range" needs clarification. Line 54. What "Supplemental Materials" refer to? Line 20. "RMSE" should be "(RMSE)" following the full term description. [Y. Jeffrey Yang, USA]	* "we" removed the word we where this was possible where it is used in the context of an assessment it is maintained and sometimes it is needed for an active phrasing * percentile is explained * supplementary information will be included for the SOD * RMSE as abbreviation is removed as it was only used once
23290	4	10	0	56		It appears the passages infer that coastal flooding is mainly resulted from SLR. This is not reflective of the literatures on this subject. [Y. Jeffrey Yang, USA]	We agree that this is much more nuanced as expressed by the reviewer, but we don't see to which section the reviewer refers. On page 10 there is no mention
12064	4	10	3	3	16	I really think one needs to also introduce a line about the speed in global average temerature change under the RCPs relative to the absolute change during the last glacial transition. [Michael Casey, Germany]	rate of changes of SLR are discussed in the paleo section. The advance in knowledge in this field is rather limited and does not warrant discussion in a general overview of history as 4.2.1 aims for.
14476	4	10	3	10	4	Sea level changes have been discussed throughout the various IPCC assessment reports as a key feature of climate change. [Christophe Deissenberg, Luxembourg]	yes we agree, the comment does not request for changes
23956	4	10	3	10	33	Suggest to cut part of this introduction which goes back to AR1. [Hans-Otto Poertner and WGII TSU, Germany]	we have reduced the introduction with a few lines, but feel it is importan to sketch the changing views on the crysophere in the IPCC context.
14478	4	10	5	10	5	the thermal expansion [Christophe Deissenberg, Luxembourg]	sentence is deleted hence comment obsolete
14480	4	10	6	10	9	It was also recognized that the slow response of the cryosphere and ocean together with ongoing warming in the near future implied a potentially substantial future sea level rise even if greenhouse gas (GHG) emissions were reduced (Warrick and Oerlemans, 1990). In the early 1990s, the observed changes [Christophe Deissenberg, Luxembourg]	sentence is deleted hence comment obsolete

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4188	4	10	10	10	10	there were other studies that reached similar conclusions, including Hindmarsh [Peter Clark, USA]	agreed add e.g.
11696	4	10	12	10	12	It is incorrect to state "it was assumed" - this response is based on well understood pysical process and their representation in climate models. [John Church, Australia]	agreed but the point is that at that we thought the clausius clapeyron effect was the dominant process for Antarctica in near future and that turned out to be not the case. The sentence is adjusted accordingly
2972	4	10	13	10	13	"Contribute a small drop" not "contribute to a small drop" [Robert Kopp, USA]	agreed and adjusetd
4190	4	10	20	10	20	more accurate to say that there was no published literature available to assess the process [Peter Clark, USA]	agreed
4192	4	10	20	10	22	purpose of this sentence? Otherwise, AR4 explicitly included atm. processes - surface mass balance is a contribution - so this is not "implied" [Peter Clark, USA]	the purpose of the sentence was to express that ice dynamical changes were not captured at that time but atmospheric processes effecting the surface mass balance were captured. This sentence is adjusted accordingly to express the key message better
14482	4	10	22	10	23	but not the adjustment of the ice flow to the changed environmental conditions. In AR5 [Christophe Deissenberg, Luxembourg]	agreed, sentence is rephrased
14484	4	10	22	10	33	In AR5 (Church et al., 2013) a first attempt was made to quantify the dynamic contribution of the ice sheets, although with limited physics, relying mainly on an extrapolation of existing observations. Furthermore, AR5 provided improved insight into local and regional patterns of sea level change. These two advances provide the basis for this chapter, where we focus on sea level changes around coastlines and low-lying islands, rather than on the global mean sea level rise. We explain the mechanisms driving past and contemporary sea level changes and episodic extremes of sea level and assess the current confidence in regional projections of future sea level over the 21st century and beyond. The emphasis is on progress since AR5 (published in 2013). Note that new climate model intercomparison results (CMIP6) providing updated guidance on specific components of sea level rise including ocean thermal expansion and circulation changes, ocean temperatures in contact with ice sheets, and evolving atmospheric temperatures above glaciers and ice sheets, are not yet available. [Christophe Deissenberg, Luxembourg]	largely rephrased according to the reviewers' suggestion
4194	4	10	24	10	24	I don'tthink this is an accurate description of what AR5 did (i.e., extrapolation of obsevations) [Peter Clark, USA]	accepted the statement is nuanced in line with comment 20734
20734	4	10	24	10	24	I would make it clear that this was not based on purely extrapolation, as in semi-empirical modelling methods - it used results from a numerical physical model for PIG. [Tamsin Edwards, UK]	accepted and rephrased
2974	4	10	35	11	12	This section is clearer than, but somewhat redundant with, the intro text to 4.2. [Robert Kopp, USA]	we removed most of this section and included a bit in the introduction of 4.2

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4146	4	10	35	11	12	Section 4.2.1.1: I think the explanation of what is meant by "global mean sea level", "regional sea level" and "relative sea level" should be made clearer. For example, the authors have already described processes that affect the Geiod (page 10 lines 42-44) before stating on page 11 that factors affecting the Geoid *may* be accounted for in RSL. My understanding is that RSL is simply the sea level height relative to the solid Earth (which is subject to vertical motions, as disucssed in the text). I don't understand why storms and high tides are mentioned in this context. In any case, please give a clear definition of all terms listed above. It may be helpful to focus on "geocentric sea level" and "relative sea level" when talking about regional sea level change ? [Matthew Palmer, UK]	accepted definitions are grouped in the introduction section of 4.2
21622	4	10	38			"decades to centuries" -- and longer -- this is truw at all scales and more important at longer scales. [Robert Nicholls, UK]	this text is removed
23114	4	10	39			" or its inverse, specific volume" : not clear ! [Jacques Beall, France]	this text is removed
14486	4	10	41	10	44	Tectonic and other dynamic Earth processes including the dynamic topography of the Earth's surface and glacial isostatic adjustment (GIA) caused by past changes in ice sheets also impact GMSL, through their effect on the geography and the mean depth of the underlying sea floor, and through the Earth's gravitational field (Tamisiea et al., 2010). [Christophe Deissenberg, Luxembourg]	this text is removed
2976	4	10	42	10	42	Mantle dynamic topography is not "dynamic topography of the Earth's surface" [Robert Kopp, USA]	this text is removed
2978	4	10	46	10	46	"Ancient" has no clear definition, and the paleo-sea level record is as used for the 17th and 18th centuries as for deeper time. If "paleo sea level" on its own isn't adequate, one could say "geologial sea level records" [Robert Kopp, USA]	agreed changed accordingly
3418	4	10	46	10	47	data Not used in in AR5 or is it a different data set or same? [Mahmood Riyaz, Maldives]	this text is removed
14488	4	10	46	10	53	Since the publication of AR5, a combination of approaches using information from ancient (paleo) sea level records, from the global network of tide gauges, and from satellite data, have substantially advanced our understanding of sea level change over the last century and beyond. Over the last century, about 50% of this change in sea level has been caused by thermal expansion of the global ocean. Since about 2005, however, the addition of ocean mass from the loss of land ice has begun to outpace thermal expansion as the dominant contributor (Table 4.1). Since 1993, a combination of tide gauge and satellite-based estimates consistently indicate a sharp increase in the rate of GMSL rise, with the rate of SLR accelerating within the satellite era WHAT DO YOU MEAN BY "WITHIN THE SATELLITE ERA"? (Nerem, 2018). [Christophe Deissenberg, Luxembourg]	satellite era is rephrased the rest is removed
3420	4	10	47	10	47	What new understanding ? [Mahmood Riyaz, Maldives]	added a reference to 4.2.2
2980	4	10	48	10	48	"this change in sea level" -> "the change in global-mean sea level" ("this" has no clear referent) [Robert Kopp, USA]	this text is removed
4576	4	10	48	10	48	Where does the number (50%) stem from? There is no data for thermal expansion available for that period... [Sönke Dangendorf, Germany]	this text is removed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1916	4	10	48	10	50	The statistic of 50% sea-level rise due to thermal expansion during the 1900-1999 period seems wrong. I am not sure how it is derived from Table 4.1, as the SLR budget (as presented) is not closed during these epoch (i.e. there is no total from which to compute a fraction attributable to one process). The corresponding statistic from Box and Colgan, 2017 (Chapter 9. Sea level rise contribution from Arctic land ice: 1850-2100. In: Snow, Water, Ice and Permafrost in teh Arctic (SWIPA) 2017. Arctic Monitoring Assessment Programme. Oslo Norway) is 3 cmSLR thermal expansion of 16 cm total SLR during 1900-1999, or 19% 20th Century sea level rise due to thermal expansion. [William Colgan, Denmark]	text is removed and topic discussed in section 4.2.2
11698	4	10	48	10	51	This statement needs a reference as justification. The 50% quoted would not be my assessment. In table 4.1, the mass contribution is larger than thermal expanion for earlier periods also. [John Church, Australia]	this text is removed
4578	4	10	51	10	52	The statement since 1993 is in my opinion misleading here as it reads a detected changepoint in the rates. This is not the case. The rates have started to accelerate already earlier. The period since 1993 is rather artificailly chosen (for the detection of a rate change) as it marks the start of the satellite record. [Sönke Dangendorf, Germany]	agreed text rephrased
17742	4	10	51	10	53	Although measuring the same basic process, tide gauge observations are radically different from satellite observations. Briefly formulated, tide gauges have a long history and limited coverage and satellites have a short history and a large spatial coverage. Combining the two indiscriminately would lead to the conclusion that sea level rise has accelarated while this might be not justified. A clear clarification how the two sources are combined is of extreme importance [Hessel Voortman, Netherlands]	section 4.2.2 treats this subject
4196	4	10	52	10	52	add reference to Hay et al. here [Peter Clark, USA]	agreed added
8282	4	10	52	10	52	"Rate of GMSL rise" and "SLR" is written but their rate is not mentioned. Also the "SLR" term is used for the first time so the full form should be written first. [APECS Group Review, Germany]	abbreviation removed
11700	4	10	52	10	52	The first papers identifying this acceleration were Watson et al. (NCC 2015), Chen et al. (NCC 2017) and Dieng et al. (2017, GRL?) [John Church, Australia]	agreed corrected
11818	4	10	52	10	53	Watson et al 2015 Nature CC were the first to identify this given the underlying issues in Topex. [King Matt, Australia]	agreed corrected
2982	4	10	55	10	56	Uness you are specifically focused on mean sea level, sea level also exhibits substantial seasonal and interannual variability. [Robert Kopp, USA]	at the start of 4.2 it is explained that short durat
4144	4	10	55	10	57	Note that these processes also bring about substantial changes in regional sea level through their affect on seawater density (it is not just ocean currents that matter). [Matthew Palmer, UK]	this text is removed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14490	4	10	55	11	3	The sea level does not rise uniformly but exhibits substantial regional variability at decadal to multi-decadal time scales. Changing winds, air-sea heat and freshwater fluxes, and the addition of riverine and glacial meltwater alter ocean currents and thus lead to regional and local changes in sea level. Contemporary and past changes in land ice cover perturb the gravitational field of the Earth, deform the Earth's 1 crust and change the orientation and rate of the Earth's rotation. In turn, these processes affect the sea level regionally, and can make it deviate substantially from the global mean sea level (Mitrovica et al., 2011). [Christophe Deissenberg, Luxembourg]	this text is removed
3422	4	10	56	10	57	Is there any evidence for this please provide the reference [Mahmood Riyaz, Maldives]	this text is removed
8284	4	10	57	11	2	E1a This sentence needs citations. I suggest Chen et al. (2013), https://doi.org/10.1002/grl.50552, which addresses primarily ice sheets, or Adhikari and Ivins (2016), https://doi.org/10.1126/sciadv.1501693, which is new since AR5. I don't have a reference suggestion for ice mass loss deforming the earth's crust. [APECS Group Review, Germany]	this text is removed
11816	4	11	1	11	3	The major focus on GIA is a disservice since may areas affected most by vertical land movement will be affected by local elastic effects due to dam building and earthquakes, post-seismic subsidence or uplift, sediment loading or groundwater pumping. These are partly much less predictable and create greater uncertainties [King Matt, Australia]	this text is removed
22252	4	11	3	11	3	Acronyms and abbreviations (such as GMSL for global mean sea level) should be used consistently. [Andra Garner, USA]	agreed abbreviations are introduced now in the first section of 4.2
2984	4	11	5	11	5	The concept of "regional sea level" is vague and not defined. [Robert Kopp, USA]	this is defined in the first section of 4.2
3310	4	11	5	11	6	This is a very good point. I think. See next comment. [Castor Muñoz Sobrino, Spain]	this text is removed
14492	4	11	5	11	12	Global mean and regional sea level changes are useful concepts for considering general trends. However, it is the relative sea level (RSL) that directly impacts coastal communities, cities and low-lying islands. An analysis of the RSL may consider regional to local changes in the Earth's geoid as water and ice mass move over the Earth's surface, vertical motions of the sea floor and coastal regions, and in some places the subsidence due to changes in the delivery and compaction of sediment and landfill and the extraction of subsurface freshwater and hydrocarbons. I.e., changes in RSL are caused by multiple, interacting, and sometimes compounding factors like storms and high tides. As a result, reliable projections of future RSL at specific times and locations remain difficult to make. [Christophe Deissenberg, Luxembourg]	this text is removed
18536	4	11	5			semicolon not comma after 'level' [Christopher Fogwill, UK]	this text is removed
2986	4	11	8	11	10	Repretitive of previous paragraph. [Robert Kopp, USA]	this text is removed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8286	4	11	10	11	12	Regional specific phenomenon needs to be discussed in a broader aspect in terms of RSL, as they are the key players in the RSL and its associated changes and its mitigation which would be significantly affecting the coastal communities. Please cite few examples of regional phenomemons which are affecting the RSL at different regions around the world. As RSL is completely area specific. [APECS Group Review, Germany]	this text is removed
21624	4	11	11	11	12	I am interested in the use of the term projections as opposed to sceanrios (plausible futures). Is the chapter providing projections or scenarios? [Robert Nicholls, UK]	the chapter uses projections and as such no change needed however the sentence disappeared
13564	4	11	12	11	12	Delete 'to make' [Debra Roberts and Durban Team, South Africa]	this text is removed
6260	4	11	14	39	35	Regardng glaciers and ice sheets there is much overlap with chapter 3 (and 2) which should be resolved. While the text is muh better organized and reads well in chapter 4, lots of it goes beyond just 'sea level' and may perhaps be beter dealt with in the previous chapters since they come first and chapter 4 can refer to those sections. [Regine Hock, USA]	The text on ice sheets and glaciers in Chapters 3 and 4 have been reorganized, with an emphasis on modern observations in Chapter 3 and an emphasis on future projections in Chapter 4.
23958	4	11	14			Too much text book style; this section can be reduced in length. [Hans-Otto Poertner and WGII TSU, Germany]	This text has been shortenend and is now in Chapter 3 (see comment 6260 above).
8288	4	11	16	11	16	Please use "the vast majority of fresh water in the cryosphere is stored" instead of "the vast majority of water in the cryosphere is stored". [APECS Group Review, Germany]	This is fixed. Note that most of this section of text has been moved to Chapter 3.
14494	4	11	16	11	31	The vast majority of water in the cryosphere is stored in the ice sheets of Antarctica and Greenland (see Chapter 3). Nevertheless, over the last century glaciers have contributed more to the GMSL rise than these ice sheets, due to their faster response time and location in relatively warmer climate zones (e.g., Gregory et al., 2013b). NEW PARAGRAPH The large ice sheets on Greenland and Antarctica are not expected to disappear on centennial timescales but, because of their volume, the loss of even a small fraction of their mass could dominate the sea level rise. It is therefore of utmost importance to understand their dynamics. Figure 4.2 illustrates the most important processes that drive the mass change of an ice sheet. The total mass of an ice sheet is partly controlled by the surface mass balance (SMB), i.e. the sum of ablation and accumulation controlled by atmospheric processes. But the ice sheets also lose mass through contact with warm ocean water below the ice shelves and by iceberg discharge at the ocean margin. Changes in the SMB, discharge, and melting forced by the ocean cause a dynamical adjustment of the ice sheets. This drive changes in sea level mainly through the loss or gain of land ice above flotation, which is the ice thickness above local sea level, corrected for density difference between water and ice. At present Greenland is contributing more to the sea level change than Antarctica, but significant parts of Antarctica are resting on bedrock below sea level, and thus has a large potential to contribute to sea level via a dynamical response to ocean melt, and a possible marine ice sheet instability (MISI, see Section 4.2.3.1 and Chapter 3). [Christophe Deissenberg, Luxembourg]	These are excellent editorial suggestions. Note that most of this block of text has been rewritten and moved to Chapter 3.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
6240	4	11	18	11	18	due to faster response times is incorrect. Response times have nothing to do with immediate reaction of an ice mass in terms of melt in response to a temperature increase. No matter response time (i.e. the time the ice mass approaches a new steady0-state, the mass loss response would be equal for two ice masses with the same climate. The reason for their large contribution to SLR is that an increase in temperature has a much larger effect on melt when temperatures are around the melting point than when temperatures are well below freezing most of the year. This also needs adjustment later (p.12, lines 16ff) [Regine Hock, USA]	While the Chapter Team agrees that the response of ice to a given warming in near freezing conditions will be greater than the response to the same incremental warming in very cold conditions, this doesn't change the essensce of the statement that glaciers are generally responding faster to climate change. Note that much of this block of text has been reworded and moved to Chapter 3.
8290	4	11	18	11	18	"Glaciers location in relatively warmer climate zones" what does this signify, its little vague. Please reframe it. [APECS Group Review, Germany]	This has been reworded and moved to Chapter 3.
17600	4	11	18	11	18	I know what you're trying to say by "faster response time" but it's not really correct as stated. The SMB of GrIS has the same response time as the SMB of GIC and the AIS. Ice dynamics can respond to foricing on a range of time scales from diurnal cycling by tides to millennial. Would benefit from rephrasing. [Jonathan Bamber, UK]	This has been reworded and moved to Chapter 3.
18538	4	11	19			...on centeniial, or even millenial, timescales...' [Christopher Fogwill, UK]	WAIS retreat could have a retreat rate measured with ean e-folding timescale measured in centuries.
18540	4	11	21			the mass of ice sheets' instead of 'ice sheets' mass' [Christopher Fogwill, UK]	Agreed. Fixed. This body of text has mostly been moved to Chapter 3.
20736	4	11	22	11	24	It's incorrect to say that total mass is controlled by SMB, and then put the melting and discharge as a separate sentence - these two sentences should be merged. [Tamsin Edwards, UK]	Agreed. See comment 8292 above.
8292	4	11	22	11	25	E1a Combine these two sentences, since on their own each is not strictly true. "The total mass of an ice sheet is controlled by the surface mass balance and through contact with the ocean." (remove "warm") Next sentence, describe SMB and sub-shelf melting/calving. [APECS Group Review, Germany]	Agreed. Fixed. This body of text has mostly been moved to Chapter 3.
21552	4	11	22	11	25	The total mass of an ice sheet is controlled by SMB...'. This is confusing. The mass (balance) of the ice sheet is determined by the SMB minus the ice discharge at its marine margins (if the ice sheet is in contact with the ocean) which includes submarine melting and calving. [Fiamma Straneo, USA]	Agreed. Fixed. See comment 8292 above.
2988	4	11	23	11	23	"Ablation" is called "surface melt" in the figure [Robert Kopp, USA]	Agreed. An attempt has been made to improve consistency. Much of this text has been moved to Chapter 3.
4198	4	11	25	11	25	and at grounding lines of calving margins [Peter Clark, USA]	The chapter team thinks "below ice shelves" is adequate. Note that this text has been moved to Chapter 3.
17744	4	11	26	11	28	Appears to be incorrect. Following Archimedes a floating body displaces water equal to its mass. Therefore, once an ice sheet is floating, its contribution to sea level rise is fully established and does not increase further if melting takes place [Hessel Voortman, Netherlands]	The SROCC is aware of this and explicitly differentiates between floating ice and grounded ice "above floatation"

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21554	4	11	28	12	10	Much of the future ice sheet contribution to sea level rise in this chapter is dedicated to Antarctica. While Antarctica has a larger potential for SLR increase than Greenland (because of size and because of the portion grounded below sea level), the relative importance of Greenland or Antarctica may depend on the timescale. Currently, as indicated in the text, Greenland's ice loss accounts for a significantly larger fraction of SLR than Antarctica (about twice). Greenland, because of its location, may continue to shrink at a faster rate than Antarctica for the next ? 100 years? more? This is not reflected in the discussion presented in this chapter. [Fiamma Straneo, USA]	This is issue of timescales is a good point. This block of text has been reworded and moved to Chapter 3.
4200	4	11	29	11	29	add reference [Peter Clark, USA]	Sea level changes are cited, and updated here in a new Table. Note that most of this block of text has been moved to Chapter 3.
17746	4	11	29	11	31	Passage unclear and possibly incorrect. Ice between current sea level and a submerged rock surface does not contribute to sea level if that ice melts. Ice above current sea level contributes if the ice level is higher than would have been the case had the ice sheet been floating. [Hessel Voortman, Netherlands]	The chapter team agrees. This should be reworded. Note that most of this text has been moved to Chapter 3.
18392	4	11	30	11	30	'which' is incorrect here, it implies that the bedrock is the thing that has potential to contribute to SL. Reword. [Nicholas Golledge, New Zealand]	Agreed. See comment 17746 above.
11820	4	11	33	11	33	"do not" is not correct. There is a small thermal effect discussed originally by Jenkins and then by Shepherd et al in GRL [King Matt, Australia]	The chapter team agrees. This text has been reworded and moved to Chapter 3.
3990	4	11	33	12	10	I appreciate that it is difficult to avoid some repetition on ice sheets between chapters 3 and 4 but perhaps worth reviewing this section again [Helene Hewitt, UK]	The chapter team agrees. Chapters 3 and 4 have been restructured.
11922	4	11	33		34	provide evidence with literature [Chukwuma Anoruo, Nigeria]	References are provided.
8294	4	11	34	11	34	E1a Replace "They" with "Ice shelves" [APECS Group Review, Germany]	Agreed.
8296	4	11	34	11	34	E1a "The surface MASS balance..." [APECS Group Review, Germany]	Agreed.
6242	4	11	35	11	37	ablation includes all processes that remove mass, i.e. also calving, submarine melt. It looks like what is meant here is surface melt and not ablation? [Regine Hock, USA]	Agreed.
8298	4	11	35	11	40	"If the ablation is substantial, the shelves are not only losing mass, but penetration and movement of the surface meltwater can deepen crevasses at the surface, and cause stresses that can lead to hydrofracturing and ice shelf collapse. This has been witnessed on the Larsen A, Larsen B, and Wilkens ice shelves on Antarctica" the authors need to discuss the same process of ablation and accumulation in Greenland too. [APECS Group Review, Germany]	Agreed. These processes (on Greenland) are discussed in Chapter 3. Note that this block of text has mostly been moved to Chapter 3.
14496	4	11	36	11	37	but changes in ablation usually affect the climate more than changes in accumulation. [Christophe Deissenberg, Luxembourg]	This text has been resturctured and moved to Chapter 3.
14498	4	11	38	11	38	at the surface and cause stresses [Christophe Deissenberg, Luxembourg]	This text has been changed and moved to a cross-chapter box in Chapter 3.
8300	4	11	38	11	39	E1a "that can lead to FULL-THICKNESS hydrofracturing" -- as hydrofracturing alone doesn't imply reaching all the way to the bed, which you intend here [APECS Group Review, Germany]	The chapter team agrees, although "hydrofracturing" is usually associated with ice-shelf break up (implying full-thickness fracturing). Hydrofracturing is now described in a new cross-chapter box in Chapter 3.
14500	4	11	42	11	42	melt [Christophe Deissenberg, Luxembourg]	Agreed. "In addition" is not needed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8302	4	11	43	11	43	Please add "and" after the word "pressure". [APECS Group Review, Germany]	The chapter team is refering to the "pressure melting point" of ice- the temperture ice melts at a given pressure. This is fine, as is.
8304	4	11	43	11	43	E1a "pressure melting point of ice DECREASES with water depth" [APECS Group Review, Germany]	Agreed. The typo has been fixed.
20738	4	11	44	11	44	Grounding line definition rather hidden in this sentence - separate out and put earlier in section. [Tamsin Edwards, UK]	This is exlained in a new cross-chapter box in Chapter 3.
4202	4	11	44	11	45	differnce between these two situations? [Peter Clark, USA]	Agreed. This is repetitive.
11824	4	11	47	11	47	the role of sea ice has been suggested most recently by Massom et al in Nature 2018 [King Matt, Australia]	Agreed. Mossom is now cited. See Cross-Chapter Box in Chapter 3.
1442	4	11	49	11	51	Ice shelves acting as buffers for the ice sheet behind the shelf. Would recommend also adding a reference to the relatively recent study of Fürst et al. (2016, Nature Climate Change, doi: □10.1038/NCLIMATE2912) here [Harry Zekollari, Switzerland]	Agreed. Furst et al. is now cited in a new cross-chapter box in Chapter 3, describing these processes.
8306	4	11	50	11	50	E1a Need a qualifier -- "Their thinning or disappearance CAN lead to... " since unconfined ice shelves do almost nothing to stabilize ice upstream [APECS Group Review, Germany]	Agreed. See the new cross-chapter box in chapter 3. However, also note that almost all ice shelves provide some buttressing (Furst et al., 2018).
4204	4	11	52	11	52	do not need complete collapse of ice shelf to trigger MISI [Peter Clark, USA]	Agreed. A previous sentence also refers to the importance of "thinning".
13566	4	11	52	11	52	Total discharge or time-bound? If the latter, what is the time period for the quantified loss? [Debra Roberts and Durban Team, South Africa]	Agreed. This can be very fast (loss of the Larsen B ice shelf occurred over days-weeks.
20740	4	11	52	11	52	triggering MISI' - this should be more caveated - 'potentially triggering MISI' - as MISI is still really a hypothesis, and certainly the vulnerability and duration would vary by location. [Tamsin Edwards, UK]	MISI and MICI are "caveated" in their description, now in Chapter 3.
18828	4	11	52	11	54	The reference to Bamber et al (2009) is outdated and based on pushing grounding lines onto the onset of a downard sloping bed. This was before models could cope with MISI. Recent model simulations (Golledge et al, 2015; DeConto and Pollard, 2016; Pattyn, 2017) show a higher potential for WAIS and the whole Antarctic ice sheet based on ice dynamics and interaction with subglacial environment. [Frank Pattyn, Belgium]	Agreed. This reference should be updated, although we note that other modeling studies find similar total equivalent sea level rise (based on loss of ice above floatation) when most marine-based ice in West Antarctica is lost.
20742	4	11	53	11	53	Replace "3.5 meter" with "3.3 m" [Tamsin Edwards, UK]	Agreed.
3424	4	11	53	11	54	What is recorded elsewhere? and what is the average?? [Mahmood Riyaz, Maldives]	This text is meant to be descriptive and isn't central to the storyline or projections. It simply demonstrates the amount of ice above floatation in WAIS alone. The updated projections provided later in the chapter include all sources of ice loss. We also note that this text has been moved to a cross-chapter box in Chapter 3.
13082	4	11	53	11	55	Only one study? Need assessment of confidence here. [Gerhard Krinner, France]	More recent studies demonstrate similar values. See comment 3424 above.
18542	4	11	53			meters' or 'm' not 'meter' [Christopher Fogwill, UK]	Agreed.
4206	4	11	54	11	55	this sentence should go before preceding one [Peter Clark, USA]	Agreed.
18544	4	11	54	11	55	West Antarctica doesn't need hyphen [Christopher Fogwill, UK]	Agreed.
11822	4	11	55	11	55	please do not propagate the myth this is really about West Antarctica. Marine basins in East Antarctica hold more ice. [King Matt, Australia]	The chapter team agrees. EAIS submarine basins contain ~20 m of sea level equivalent.
8308	4	11	57	12	1	E1a I wouldn't say that it is merely postuated that ice shelf disappearance allows formation of ice cliffs -- that is pretty simple physical logic. Only the second clause is what is postulated. Replace "which" with "that" and it is fixed. [APECS Group Review, Germany]	The description of MICI has been reworded and moved to a cross-chapter box in Chapter 3

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
20744	4	12	1	12	2	"Their collapse can lead to ice sheet retreat through a process called ..." - this statement is too strong - MICI is still a hypothesis. 'could potentially' or 'has been hypothesised' etc. [Tamsin Edwards, UK]	The description of MICI has been reworded and moved to a cross-chapter box in Chapter 3
14502	4	12	4	12	6	However, few direct observations are available to quantify the importance of ice-cliff failure, and these observations pertain to relatively narrow outlet glaciers on Greenland and may not provide appropriate analogues for the larger spatial scales of many Antarctic glaciers. [Christophe Deissenberg, Luxembourg]	Agreed. The description of MICI has been reworded and moved to a cross-chapter box in Chapter 3.
2892	4	12	10	12	10	Bamber and Aspinall (2013) report a formalized pooling of expert views on uncertainties in future ice-sheet contributions. Bamber J.L., W.P. Aspinall, 2013: An expert judgement assessment of future sea level rise from the ice sheets. Nature Climate Change, 3, 424–427. [M. Dolores Garza-Gil, Spain]	The chapter team is aware of this work, but concentrates its assessment on literature published since AR5.
3426	4	12	10	12	10	Reference at the end of the scentence would be useful [Mahmood Riyaz, Maldives]	Agreed. Kopp et al., (2017) has been added.
4208	4	12	10	12	10	or only true for Antarctica? [Peter Clark, USA]	We prefer the existing wording. Greenland uncertainty due to unknown climate forcing is also considerable, as discussed extensively in SROCC.
1444	4	12	14	12	14	"... also contribute to sea level change": the "also" is rather misleading. As this is mentioned after the ice sheets, it sounds at this point in the chapter, as if glaciers only have a relatively limited contribution. It only becomes clear later that glaciers (together with thermal expansion) were the main contributors to SLR over the past century / decades (e.g. section 4.2.2.3.6 & Table 4.1). Would suggest rephrasing this: e.g. "Glaciers not associated with Antarctica and Antarctica are important sea-level contributors" (notice that I also omitted "and ice caps", cf. a few comments earlier concerning the nomenclature) [Harry Zekollari, Switzerland]	accepted: text revised
4210	4	12	14	12	14	not sure what "not associated with" means - don't those associted with the ice sheets (I'm assuming you mean peripheral glaciers) also contribute? [Peter Clark, USA]	accepted: text revised to "outside of the Greenland and Antarctic ice sheets"
8310	4	12	15	12	15	Do not use "mass" twice, It can be written as "They gain mass by accumulation (mainly snowfall) and lose by ablation". [APECS Group Review, Germany]	copyedit to be completed prior to publication
4212	4	12	16	12	16	this sentence about glaciers ("on average") with respect to ice sheets is inaccurate [Peter Clark, USA]	accepted: text revised to "specific accumulation and ablation rates, which are often high compared to those of the ice sheets"
6244	4	12	16	12	16	this depends on the units. The acc and abl rates are much higher for the ice sheets if expressed in Gt. I assume you mean 'specific acc and abl rates' (i.e. per unit are)? [Regine Hock, USA]	accepted: text revised
8312	4	12	16	12	18	Please cite a reference. [APECS Group Review, Germany]	accepted: reference added
14504	4	12	16	12	23	They have, on average, high accumulation and ablation rates compared to the ice sheets. Thus, they respond fast (within decades or so) to and are sensitive indicators of climate changes. Indeed, during the past century, glaciers added more mass to the ocean than the Greenland and Antarctic ice sheets (e.g., Gregory et al., 2013b). However, the total volume of ice in these land reservoirs is comparatively small, equivalent to only ~0.4 m sea level rise (Arendt et al., 2012). In some areas, the loss of glaciers has been thwarted by periods of increased precipitation or regional cooling (Mackintosh et al., 2017), but on longer time scales there is a net glacial melt. [Christophe Deissenberg, Luxembourg]	taken into account: combined with comments 4210, 4212, 6244.
11924	4	12	16		18	provide evidence with literature [Chukwuma Anoruo, Nigeria]	accepted: reference added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8314	4	12	18	12	18	Please add "have" after "glaciers" Please mention glaciers of which region in general, apart from Greenland and Anatrctica. [APECS Group Review, Germany]	accepted: text revised. Regional discussions of glacier mass loss can be found in Ch. 2 and 3.
2894	4	12	19	12	19	Berthier et al., 2010 may be added to reference about glaciers contribution to SLR. Berthier E., E. Schiefer, G. K. C. Clarke, B. Menounos, F. Rémy, 2010: Contribution of Alaskan glaciers to sea-level rise derived from satellite imagery. Nature Geoscience, 3, 92-95. [M. Dolores Garza-Gil, Spain]	rejected: regional glacier mass change is addressed in Ch. 2 and 3. Furthermore, this specific reference was already addressed in Ch.4 of AR5.
2990	4	12	20	12	20	equivalent to 0.4 m GLOBAL-MEAN sea level rise [Robert Kopp, USA]	accepted: text revised
3428	4	12	21	12	21	Is it possible to give range "loss og glacier" xx-xx rather than one single figure [Mahmood Riyaz, Maldives]	accpeted: text revised to specific the AR5 assessment (which is still up-to-date)
4214	4	12	21	12	21	should also reference AR5 [Peter Clark, USA]	accepted: text revised
6246	4	12	21	12	21	Wrong reference - this information is not from Arendt. The best compilation so far is the one by IPCC 2013 which compiled all published values. [Regine Hock, USA]	accepted: text revised and AR5 assessment included
4216	4	12	23	12	23	"longer timescale" - such as? [Peter Clark, USA]	accepted: text revised to specify "multidecadal and longer"
4218	4	12	23	12	23	reference for this statement? [Peter Clark, USA]	acctepted: reference to Marzeion et al., 2015, added
8316	4	12	23	12	23	Please cite a reference. [APECS Group Review, Germany]	acctepted: reference to Marzeion et al., 2015, added
22254	4	12	23	12	23	Reference for the second half of this sentence? [Andra Garner, USA]	acctepted: reference to Marzeion et al., 2015, added
14506	4	12	27	12	32	The warmer the ocean water, the lower its density and therefore the larger its volume per unit of mass (thermal expansion). Thus, warming leads to a higher sea level even when the ocean mass is constant. Over at least the last 1500 years the sea level was tightly coupled to the global mean temperatures (Kopp et al., 2016), partly because of ice melting, and partly because of thermal expansion. In the past century, thermal expansion was the single greatest contributor to the GMSL rise, although increased ocean mass mainly through ice loss is the dominant contributor since ~2005; (Shepherd et al., 2012; Church et al., 2013; A. Cazenave, 2018). [Christophe Deissenberg, Luxembourg]	accepted and rephrased
14510	4	12	27	12	32	The warmer the ocean water, the lower its density and therefore the larger its volume per unit of mass (“thermal expansion”). Thus, warming leads to a higher sea level even when the ocean mass is constant. Over at least the last 1500 years the sea level was tightly coupled to the global mean temperatures (Kopp et al., 2016), partly because of ice melting, and partly because of thermal expansion. In the past century, thermal expansion was the single greatest contributor to the GMSL rise, although increased ocean mass mainly through ice loss is the dominant contributor since ~2005; (Shepherd et al., 2012; Church et al., 2013; A. Cazenave, 2018). In addition to thermal expansion changes in the ocean dynamics and salinity also play a role in regional sea level changes. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
2992	4	12	29	12	29	"Tightly" coupled may be too strong; Kopp et al. 2016 showed that, for global-mean sea level, temperature had predictive power. [Robert Kopp, USA]	accepted and rephrased
3430	4	12	29	12	30	IS ti possible to broadly quantify how much is from thermal expansion and others factors? This is very vague [Mahmood Riyaz, Maldives]	This is treated in section 4.2.2
4220	4	12	29	12	30	how is this known? [Peter Clark, USA]	unclear what the reviewer means reference for the explanation is given

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
11702	4	12	30	12	31	What is the basis for this statement. I do not believe the AR5 stated this and I do not think the references quoted stated this. Glaciers are a major contributor and the statement is misleading. [John Church, Australia]	This is treated in section 4.2.2
11826	4	12	30	12	32	the Nature CC paper of Chen et al 2017 makes this point clearly [King Matt, Australia]	the text is removed
17436	4	12	30	12	32	Repetition of the content of the sentences on page 10, lines 48-53: I suggest deleting this. [Sonya Legg, USA]	accepted and removed
3764	4	12	32	12	32	Include reference Shepherd, A., et al. "Mass balance of the Antarctic Ice Sheet from 1992 to 2017." Nature 556 (2018): pages219-222. [Ola Kalen, Sweden]	the text is removed
11926	4	12	32			check reference. You can refer to line 48 [Chukwuma Anoruo, Nigeria]	references are checked
17438	4	12	34	12	34	It should be noted that the thermal expansion coefficient is highly temperature dependent, and heat uptake by a warm region of the ocean therefore has a different impact on sealevel rise than heat uptake by a cold region. [Sonya Legg, USA]	accepted and included
14512	4	12	34	12	38	Over the last decades, more than 90% of the increase in energy in the climate system has been stored in the ocean. Climate change and sea level are thus intimately related and thermal expansion provides insight in our understanding of the climate system and climate sensitivity. Findings from these two viewpoints are consistent (Otto et al., 2013). [Christophe Deissenberg, Luxembourg]	accepted and rephrased accordingly
17748	4	12	34	12	38	Indicated whether this is an observed or calculated result. [Hessel Voortman, Netherlands]	accepted and included
4222	4	12	36	12	36	reference to Church (including AR5) here [Peter Clark, USA]	accepted and included
4224	4	12	36	12	36	which two fields? [Peter Clark, USA]	text rephrased to clarify
2140	4	12	37	12	38	We need to refer some papers here. The dynamical and steric ocean responses to global warming are well described by following papers. (e.g. Lowe and Gregory, 2006; Suzuki and Ishii, 2011) Lowe, J. A., and J. M. Gregory, 2006: Understanding projections of sea level rise in a Hadley Centre coupled climate model, J. Geophys. Res., 111, C11014, doi:10.1029/2005JC003421. Suzuki, T., and M. Ishii, 2011: Regional distribution of sea level changes resulting from enhanced greenhouse warming in the Model for Interdisciplinary Research on Climate version 3.2. Geophys. Res. Lett.,38, L02601, doi:10.1029/2010GL045693. [Michio Kawamiya, Japan]	references are added
18546	4	12	37			comma after 'changes' [Christopher Fogwill, UK]	sentence is changed comment obsolete

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
23186	4	12	40	12	48	There is a lack of references in that section. Regarding the role of man-made reservoirs in contributing to absolute and relative sea level changes, good references are: Wada, Y., J. T. Reager, B. F. Chao, J. Wang, M.-H. Lo, C. Song, Y. Li, A. S. Gardner, 2017. Recent Changes in Land Water Storage and its Contribution to Sea Level Variations. Surveys in Geophysics, Volume 38, Issue 1, p. 131–152. I SAW SOME OF THOSE REFERENCES WERE USED IN SECTION 4.2.2.3.5 Fiedler, J. W., C. P. Conrad, 2010. Spatial variability of sea level rise due to water impoundment behind dams, Geophysical Research Letters, Volume37, Issue12, doi.org/10.1029/2010GL043462. Chao, B. F., Y. H. Wu, Y. S. Li, 2008. Impact of artificial reservoir water impoundment on global sea level. Science, 320 (5873), 212-214. doi: 10.1126/science.1154580 [Sebastian Weissenberger, Canada]	references are added
1786	4	12	42	12	43	Please be more specific that withdrawal of ground water decreases the sea level. [Meer Ali, India]	rejected withdrawal of ground water increases sea level
1788	4	12	42	12	43	The reference is Narem et al. 2018 [Meer Ali, India]	Unfortunately the reference suggestion is incomplete and unknown to us
11704	4	12	42	12	46	References required. [John Church, Australia]	references are added
2678	4	12	42	12	48	What is the implication of ENSO on temporary storage of water? [Mohammad Javad Zareian, Iran]	is has impact on precipitation distribution and temporary storage of water on continents, see text.
18548	4	12	42			Get rid of 'finally' - there's another section beneath this one! [Christopher Fogwill, UK]	accepted and rephrased
8318	4	12	43	12	43	Please remove "behind" instead use "through". [APECS Group Review, Germany]	accepted and rephrased
8320	4	12	43	12	43	please specify sea level change as RSL or GMSL [APECS Group Review, Germany]	accepted and rephrased
8322	4	12	45	12	45	Please use "agricultural and indusrial usage" instead of "agricultural and industrial processes". [APECS Group Review, Germany]	accepted and rephrased
14514	4	12	45	12	45	anivorano [Christophe Deissenberg, Luxembourg]	unknown what is meant by the reviewer
14518	4	12	45	12	45	agricultural, and industrial processes, has [Christophe Deissenberg, Luxembourg]	accepted and rephrased
2994	4	12	50	13	13	This section is really disorganized. [Robert Kopp, USA]	An attempt has been made to streamline this section.
4226	4	12	52	12	52	glaciers are part of land ice [Peter Clark, USA]	Agreed. Glaciers has been deleted.
2996	4	12	53	12	53	It's not clear why fingerprints would dominate local changes on a different timescale than the land ice and glacier changes with which they are associated. GIA is important on long timescales (though it is in some regions a dominat contributor to regional variability today). [Robert Kopp, USA]	Agreed. This is a matter of wording, which has been improved.
2998	4	12	53	12	55	If the term "geodynamic" includes changes in ocean, salinity, and atmospheric pressure, then the term is meaningless. [Robert Kopp, USA]	Agreed. "Geodynamic" is meant here to convey processes primarily associated with the "solid" Earth.
4228	4	12	55	12	55	add references [Peter Clark, USA]	Agreed.
8324	4	12	56	12	57	Please cite a reference. [APECS Group Review, Germany]	Agreed. See comment 4228 (above).
4230	4	12	57	12	57	do you mean mass unloading? [Peter Clark, USA]	Either loading or unloading are appropriate. They both have visco-elestic effects on the Earth.
14522	4	12	57	13	1	Even far away from the places where the redistribution occurs, the sea level changes can exceed the global average by up to 25%. [Christophe Deissenberg, Luxembourg]	Agreed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24594	4	13	0			FIGURE 4.2: great figure, caption needs to explain abbreviations. SLE potential could be listed for other processes as well. [Hans-Otto Poertner and WGII TSU, Germany]	Agreed. This figure has been redrafted.
14524	4	13	1	13	6	Near the retreating ice sheets, the sea level drops although, on the average, the global sea level rises. The gravitational and rotational influences of an ice loss are instantaneous, but additional long-term effects arise due to the visco-elastic deformation of the Earth around the location of mass change. Such a deformation is observed in regions previously covered by ice during the Last Glacial Maximum (LGM), including much of Scandinavia and parts of North America (Lambeck et al., 1998; Peltier, 2004). [Christophe Deissenberg, Luxembourg]	Agreed. This wording has been changed.
3000	4	13	2	13	2	"globally averaged sea level" -> "global mean sea level" [Robert Kopp, USA]	Agreed.
3002	4	13	4	13	6	GIA affect RSL everywhere, even in the far field (e.g., via ocean syphoning) [Robert Kopp, USA]	Agreed.
4232	4	13	8	13	9	this sentence does not seem relevant to rest of paragraph [Peter Clark, USA]	Ageed. Some of these concepts are coverded elsewhere in the chapter.
3004	4	13	17	13	17	What is the distinction being drawn between "waves" and "wave run-up"? [Robert Kopp, USA]	run-up is cloase to the coast as the figure shows
8326	4	13	17	13	18	Authors need to mention and discuss regarding the Cyclonic anticyclonic winds too. [APECS Group Review, Germany]	that is captured under surges
8328	4	13	18	13	18	"E1a high-frequency" -- Unclear whether this means "often" or "short-period" [APECS Group Review, Germany]	it means short period as defined in the beginning of section 4.2
3006	4	13	20	13	20	"but also" -> "as well as" [Robert Kopp, USA]	accepted and rephrased
14526	4	13	20	13	20	variability of the wind fields [Christophe Deissenberg, Luxembourg]	accepted and rephrased
21628	4	13	25	13	30	Figure 4.2 -- where do tsunamis fit as they certainly influence short-term extreme sea level [Robert Nicholls, UK]	rejected tsunamis indeed influence short-term extreme sea level, but they are not predictable or climate driven so in fact ignored here
8330	4	13	26	13	26	Color the sub-marine, slice-away component of the ice sheet a lighter blue, it currently blends into the ocean [APECS Group Review, Germany]	accepted figure adjusted
8332	4	13	26	13	26	What are the gray bars above the subsidence arrow? [APECS Group Review, Germany]	they express cities, a different icon is taken to improve this
1446	4	13	28	13	30	Figure 4.2: very nice figure. Minor comment: on my screen, and even more when printed, it seems like the palm trees are also located on ice (the beach/sand looks rather white). Maybe make color of the sand a bit darker, in order to make it clearer that this is not a polar feature? [Harry Zekollari, Switzerland]	color of sand is adjusted to circumvent this misunderstanding
3008	4	13	28	13	30	Figure 4.2 has several issues. GIA causes both land motion and a change in SSH, and these can go in either direction depending on location. "Subsidence" includes subsidence caused by GIA. Groundwater is shown as an entity, but is not related to subsidence via groundwater withdrawal. [Robert Kopp, USA]	accepted the arrow for GIA is presented in two directions and an additional arrow is added from groundwater to subsidence to express the linkage
8334	4	13	28	13	30	Please mention Cyclonic anticyclonic winds in figure and its description as well. [APECS Group Review, Germany]	accepted and included in the figure
10736	4	13	28	13	30	Please, in dicate the source of the Figure 4.2 [Jacques Andre Ndione, Senegal]	the source is this report it is not from existing literature
2726	4	13	29	13	30	Figure 4.2. Colours should be very contrasting for major ice processes, e.g. orange and black. [Poh Poh Wong, Singapore]	accepted and included

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
3454	4	13	33	56	16	section 4.2.2. very broad and vague statements and very little seems to explained in the context of small island, would it be possible to focus more on status of the small islands (after all this is a chaptor on small island) [Mahmood Riyaz, Maldives]	Small islands with tide gauges are captured in the observational parts and by using figure 4.4 we have attempted to put focus on individual locations
3010	4	13	36	13	36	delete comma after "today" [Robert Kopp, USA]	accepted and included
14528	4	13	36	13	36	Past climate intervals warmer than today are of particular interest because [Christophe Deissenberg, Luxembourg]	solved by including the previous comment 3010
3312	4	13	37	14	12	Other interesting period may be the Lateglacial. Although it is not used as a reference in this document, this quite well-known period teaches us that global warmer conditions may be unequally characterized at continental/regional scales. See for example discussion on Heiri et al. 2014 (Nature Communications, 5: 4914) and the regional reconstructions included in it. [Castor Muñoz Sobrino, Spain]	the interest in this chapter is what we can learn from the geological archives for near future sea level, The Heiri paper does not provide this information
4234	4	13	38	13	38	I suggest that since these details on temperature are covered later, they are no needed here and this paragraph be shortened [Peter Clark, USA]	good suggestion we removed this information
17254	4	13	38	13	38	it is not clear the definition of the BP in the amount "3 Myrs BP", please clarify [Iulian Florin Vladu, Germany]	accepted BP has been removed
8336	4	14	1	14	1	Please mention the relative sea level changes (in numerical values) along with GMSL at MPWP along with global mean temperature as it is more relevant to discuss about sea level changes due to increased temperatures at MPWP. [APECS Group Review, Germany]	this level of detail is removed from the introduction and comes back in 4.2.2.1.1.
3012	4	14	1	14	3	Hoffman et al don't make a direct claim about GMST; their analysis is of global mean SST. They found global average sea surface temperature was about 0.5° ± 0.3°C (0.9° ± 0.5°F) above the preindustrial level [that is, comparable to the average over 1995–2014, when global mean temperature was about 0.8°C (1.4°F) above the preindustrial levels]. Importantly, ice cores indicate that (presumably due to differences in insolation) peak polar temperatures were substantially warmer than would be associated with a GMST increase of this level, and are more comparable to those projected for 1°–2°C (1.8°–3.6°F) of global mean warming above the preindustrial level [Robert Kopp, USA]	accepted, this part of the sentence has been removed given comment 4234
4236	4	14	2	14	2	avoid use of Eemian - a Euro-centric word, not global [Peter Clark, USA]	We use both Eemian, Last interglaciaal and the period 130-115 kyr BP to express the same.
8338	4	14	3	14	3	Please mention the GMSL at last interglacial and its relevance to present day conditions. [APECS Group Review, Germany]	this is discussed in detail in 4.2.2.1.2
23960	4	14	3	14	3	please be consistent with the style of writing 1°C/1.0°C (in other parts of the text and in other Chapters it is 1°C) [Hans-Otto Poertner and WGII TSU, Germany]	0.5 needs to be followed by 1.0 not 1
12538	4	14	3			refer here also to Fischer et al (2018, NGS), which takes the Hoffman SST value of 0.5+/-0.3 and converts that to a GMT of 0.8+/-0.5. by itself Hoffmann can't give you GMT, and the data shown in Otto-Bliesner (Fig 2) from McKay and Turney do not represent a time slice and are therefore biassed. You need to use the new paper tio justify a new value. [Eric Wolff, UK]	point taken updated with new references
4238	4	14	5	14	5	add reference to Hoffman et al. (2017) [Peter Clark, USA]	reference is given to Hoffman et al.
8340	4	14	6	14	7	Please justify the statement by a number (quantification). Meagerly stating higher, would not serve the purpose. As these paleo events are windows of the past through which the present is compared and it helps us in understanding the future variability too. [APECS Group Review, Germany]	this is specified in section 4.2.2.1.2

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13568	4	14	7	14	7	Change 'considerably' to 'considerable' [Debra Roberts and Durban Team, South Africa]	accepted and rephrased
14530	4	14	10	14	10	and of implications for the future evolution [Christophe Deissenberg, Luxembourg]	accepted and rephrased
8344	4	14	18	14	18	E1b It's too extreme to say the MPWP is "far" beyond the timescale of ice cores. For instance, Kehrl et al. (2018) https://doi.org/10.1029/2018GL077511 find (disturbed) ice they estimate to ~2.7 Ma in the Allan Hills; this is not quite MPWP but also not so far off. [APECS Group Review, Germany]	The chapter team is referring to standard, time-continuous, undisturbed ice cores and prefers the original wording.
8342	4	14	18	15	5	In the entire sub section of Mid-Pliocene/Mid-Piacenzian warm period different studies mentioned does not conclude on one single stand. As to how much of sea level was higher in this particular period comapred to present and what was the sea ice extent at Greenland and Antarctica. How can we learn from the past climatic fluctutions in general which is very relevant to our present. (This section is not very clearly written, it would be better if its reframed). [APECS Group Review, Germany]	The chapter team agrees. This section has been largely rewritten.
4240	4	14	20	14	20	see Foster et al. (2017, Nat. Comm.) for updated estimates/compilation [Peter Clark, USA]	Agreed. Updated.
3432	4	14	21	14	21	what is ppmv? [Mahmood Riyaz, Maldives]	Defined. Parts per million by volume.
4242	4	14	21	14	21	wider range (and higher values) in Foster 2017 [Peter Clark, USA]	Agreed.
17750	4	14	22	14	24	Appears to be a circular argument. What alternative explanations are available for the high temperature in the Mid Pliocene? [Hessel Voortman, Netherlands]	The point here is that potentially large sea level rise occurred with relatively modest greenhouse gas forcing.
12540	4	14	23	14	24	"implying high climate sensitivity (Pagani)". While this may be correct it was not the conclusion of the later Martinez-Boti paper that contains the best CO2 data; they said "predictions of equilibrium climate sensitivity (excluding long-term ice-albedofeedbacks) for our Pliocene-like future (with CO2 levels up tomaximum Pliocene levels of 450 parts permillion) are well described by the currently accepted range of an increase of 1.5K to 4.5K per doubling of CO2. You should look into this before settling on this statement. [Eric Wolff, UK]	This has bee rephrased. Also see comment 17750 above.
18550	4	14	25			little constraint' or 'few constraints' [Christopher Fogwill, UK]	Agreed. Few constraints.
4244	4	14	27	14	27	see Rohling et al. (2014, Nature) [Peter Clark, USA]	Agreed. Rohling et al., (2014) is broadly consistent with other Pliocene sea level estimates(9-32 m), although we note the ongoing uncertainty with the Red Sea record among others.
17752	4	14	27	14	30	Considering the large uncertainties, it is questionable whether this is a confirmation of high climate sensitivity. Reviewer suggests to expand the explanation [Hessel Voortman, Netherlands]	Agreed. The focus is shifted to the ice sheet response to a relatively modest temperature change.
14532	4	14	29	14	29	questioned the interpretations [Christophe Deissenberg, Luxembourg]	Agreed.
8346	4	14	32	14	33	E1a Redundant that "obliquity-paced variations" ... "are probably related to obliquity changes". [APECS Group Review, Germany]	Agreed.
8348	4	14	37	14	37	E1a Specify here that WAIS and Greenland are fully melted, and/or clarify if it's the entire WAIS or just its marine parts [APECS Group Review, Germany]	Fixed.
13570	4	14	37	14	37	First use of acronym 'WAIS'. Please spell out in full. [Debra Roberts and Durban Team, South Africa]	WAIS is now defined.
4246	4	14	38	14	38	is relying on multiple records a problem? Increases signal/noise [Peter Clark, USA]	This has been reworded.
18552	4	14	40			isotope-enabled [Christopher Fogwill, UK]	Agreed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14534	4	14	41	14	41	concluded that a Mid Pliocene Antarctic [Christophe Deissenberg, Luxembourg]	Agreed.
8350	4	14	50	14	50	C2 "little work has been done to date" -- rephrase as something like "as evidence by the few currently available studies on this topic" [APECS Group Review, Germany]	Agreed.
302	4	14	50			delete "a" after during [Kerstin Jochumsen, Germany]	Agreed.
14536	4	14	51	14	51	stand would require a larger [Christophe Deissenberg, Luxembourg]	Agreed.
3434	4	14	53	14	53	How is 17.8 m came it has not been mentioned earlier? [Mahmood Riyaz, Maldives]	This is a range of modeling studies of Pliocene, Antarctic ice loss. The chapter team prefers the current wording.
3014	4	14	54	14	54	meters sea-level equivalent [Robert Kopp, USA]	Agreed.
8352	4	14	56	14	56	E1a I'd suggest rephrasing "new and uncertain" as "newly incorporated, coarsely parameterized" [APECS Group Review, Germany]	This text has been reworded.
8354	4	14	56	14	57	E1a Replace "crevassing" with "ice-shelf breakup". Meltwater doesn't influence crevassing (the binary presence/absence of a crevasse), but it does affect their depth / potential to penetrate all the way through an ice shelf. [APECS Group Review, Germany]	Agreed.
14538	4	14	57	14	57	(MICI), is [Christophe Deissenberg, Luxembourg]	Marine Ice Cliff Instability (see comment 3572 above).
13572	4	14	57			Marine terminating ice cliff failure (MICI) - how is MICI an acronym for this? Later you introduce MISI. Please just spell these terms out. Be creative, no need to write the whole term each time it comes up. [Debra Roberts and Durban Team, South Africa]	These Processes are now described in a cross-chapter box in Chapter 3 and they are defined in both Chapter 3 and earlier in Chapter 4.
8366	4	15	0	15		In general, the projected temperatures are 1-1.5º C higher by the next century compared to present day. This increase in temperature is more comparable to the LIG temperatures, but can we comapre the sea level of present day to LIG sea level. It seems uncomparable since our projections go upto till 18-59 cm by 2100. Like wise, the CO2 concentrations in atmosphere at present (405 ppm) also show us it cannot be compared with the 300 ppm of MPWP along with sea level changes. so is it justifiable to compare the paleo events in general or the regional phenomenon were prominent at that times comapred to present or vice versa, which needs to be studied and discussed quite extensively. In this regard few lines should be mentioned after the LIG or in this section. [APECS Group Review, Germany]	The chapter team is uncertain of the comments meaning. Projected temperatures of 1-1.5º mentioned by the reviewer are in line with RCP2.6 but certainly not the current INDCs or RCP8.5. The main point of the LIG assesment is the conclusion that the magnitude of sea level rise at that time demonstrates substantial ice sheet sensitivity to relativelt modest warming, with the caveat that the response time is uncertain.
8356	4	15	3	15	3	E1a "sea level IN THE MWPW is estimated..." [APECS Group Review, Germany]	Agreed.
14540	4	15	3	15	3	evidence, the sea [Christophe Deissenberg, Luxembourg]	The original wording is preferred, although we note that this section of text has been been reworded.
16396	4	15	3	15	5	This phrase is conclusive and repoted evidences should be cited to vaidate the phrase [Lee-Sim Lim, Malaysia]	Agreed.
3436	4	15	4	15	4	Where is the reference after 6-9m [Mahmood Riyaz, Maldives]	Agreed. See comment 16396 above.
17754	4	15	7	15	50	CO2 levels and temperatures in this period are not stated in the report but are crucial to judge what can be concluded from this, also in comparison with the Mid Pliocene and with the present. To the reviewer's knowledge, geological research indicates that CO2 levels were comparable with current levels and that temperatures were higher. If correct, this would imply that the previous inter-glacial is a counter-example for the assumption of high climate sensitivity [Hessel Voortman, Netherlands]	There is an orbital component to polar warming (and possible sub-surface oceanic warming) during the LIG, that precludes a direct comparison between the paleo and modern/future situation.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
18554	4	15	7			Capital 'I' on 'interglacial' [Christopher Fogwill, UK]	Agreed.
4248	4	15	11	15	11	thius is not a fair statement. They gave a central estimate but their range is comparable to Dutton et al. [Peter Clark, USA]	This has been reworded to reflect this point.
4250	4	15	13	15	13	also Dendy et al. (2017, QSR). Should elaborate on what these consequences are [Peter Clark, USA]	Agreed.
4252	4	15	14	15	14	should also mention uncertainty from dynamic topography (Austermann et al., 2017) [Peter Clark, USA]	Agreed.
4254	4	15	14	15	15	This assessment does not follow from previous discussion in this paragraph [Peter Clark, USA]	This section of text has largely been reworded.
8358	4	15	17	15	17	The term "GIA" has been used for the first time so the full is required here. [APECS Group Review, Germany]	GIA is now defined.
4256	4	15	17	15	18	these uncertainties do not influence uncertainties in contributions from Greenland and Antarctica ice sheets [Peter Clark, USA]	If there were zero uncertainty in GIA corrections and the raw sea level data, the relative contributions of the ice sheets could be better figerprinted. With that said, this text has been reworded to improve clarity.
14542	4	15	18	15	18	the relative contributions of the Greenland and Antarctic Ice Sheets to the LIG sea level remain uncertain [Christophe Deissenberg, Luxembourg]	Reworded.
3016	4	15	20	15	22	Kopp et al. (2013) is more recent than Dutton and Lambeck (2012). There is more recent relevant work, eg Vyverberg, K., Dechnik, B., Dutton, A., Webster, J. M., Zwartz, D., & Portell, R. W. (2018). Episodic reef growth in the granitic Seychelles during the Last Interglacial: Implications for polar ice sheet dynamics. Marine Geology, 399, 170-187. [Robert Kopp, USA]	Kopp et al., (2013) provides a reinforcing reference, although Vyverberg is focused on RSL in the Seychelles, and fluctuations within the LIG, rather than GMSL.
18394	4	15	20	15	22	This sentence quotes a 2013 paper as ‘early’ and a 2012 paper as ‘more recent’. This makes no sense. [Nicholas Golledge, New Zealand]	Agreed.
23270	4	15	20	15	23	Kopp et al (2013) as early study, while Dutton and Lambeck (2012) as more recent work? [Y. Jeffrey Yang, USA]	Agreed. See comment 18394 above.
8360	4	15	21	15	22	E1a The 2012 study is not more recent than the 2013 study. [APECS Group Review, Germany]	Agreed. See comment 18394 above.
12542	4	15	24			No hyphen in West Antarctica [Eric Wolff, UK]	Agreed.
4258	4	15	28	15	28	define SLC [Peter Clark, USA]	Agreed.
304	4	15	29			change "at" to "as" [Kerstin Jochumsen, Germany]	Agreed.
3018	4	15	32	15	34	The "limited retreat" downplays the analysis of Yau et al., which suggests a more substantial loss from Greenland (4-6 m sea-level equivalent) [Robert Kopp, USA]	Yau is considered an outlier and less robust than other LIG modeling references here. The chapter team prefers the current wording.
17604	4	15	33	15	33	From memory, Dahl Jensen estimate 1 m SLE with a max of 2 m. That is not limited and is consistent with low end model simulations, which have a large spread, as do summer time temperature anomalies for the Eemian. [Jonathan Bamber, UK]	This wording has been changed. We note that recent modeling by Goelzer et al., and pro work by Helsen et al., (2013) and Quiquet et al., (2013) indicate 0.5m-1m early in the LIG, with the maximum Greenland retreat occuring ~122 ka. Relative timing between Greenland and Antarctica is clearly important, but not yet known.
3020	4	15	34	15	34	Clarify that the large increase is in Arctic temperatures since the increase in GMST is not large [Robert Kopp, USA]	Agreed. Fixed.
4260	4	15	35	15	35	This assessment is incomplete. Yau et al. report a contribution from Greenland of 5.1 m (4.1-6.2 credible interval, 95%). Including this suggess that the Greenland contribution, and thus the Antarctic contribution, remains highly uncertain. [Peter Clark, USA]	Yau et al., is included. See comment 3018 above.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14544	4	15	35	15	35	either a low sensitivity of the Greenland ice sheet [Christophe Deissenberg, Luxembourg]	Agreed.
4262	4	15	37	15	37	add Hoffman et l. (2017) [Peter Clark, USA]	Agreed. Hoffman has been added.
12544	4	15	37	15	39	It would be worthwhile to add a sentence saying that there is so far little direct evidence about losses of ice in the LIG in Antarctica and then cite Steig et al (2015, GRL) and Holloway et al (2016, Nature Comms) as giving contrasting interpretations of what the water isotope record in ice cores suggests about reductions in WAIS in the LIG. [Eric Wolff, UK]	Agreed.
8362	4	15	39	15	39	E2 Wrong Bierman reference; I think you wanted Bierman et al., 2016 doi:10.1038/nature20147 [APECS Group Review, Germany]	Agreed.
4264	4	15	39	15	45	The latter part of this paragraph is disjointed and unclear as to what main point is - needs major rewrite [Peter Clark, USA]	This section of text has been reworded.
3022	4	15	40	15	40	Be more specific about how the last 7.5 Ma relates to the Last Interglacial [Robert Kopp, USA]	This has been improved.
3030	4	15	42	15	42	Hay et al. (2015) introduced two methods, the Kalman smoother and Gaussian process regression [Robert Kopp, USA]	This comment appears to be refering to a different part of the text.
3024	4	15	47	15	47	I would suggest medium confidence, or high confidence in a likely change of 6-9 m. [Robert Kopp, USA]	The chapter team adopted the former recommendation.
3438	4	15	47	15	47	The confidence level given here for the same value 6-9m is different from line 4 of this page which says medium confidence? is it medium confidence or high confidence ? [Mahmood Riyaz, Maldives]	Agreed. This is now consistent. See comment 3024 above.
4266	4	15	47	15	47	This assessment is not well supported by prevous discussion in this section. [Peter Clark, USA]	The chapter team's assessment is now more consistent with line 4 of the same page, other reviewer's comments, and the general spirit of the discussion.
4268	4	15	52	15	52	I don't see this section as adding anything useful and suggest delete [Peter Clark, USA]	new information has been added
11106	4	15	52	16	7	This section does not discuss the literature on late Holocene sea level change (e.g. Kopp et al 2016 and references therein). [Robert Kopp, USA]	A subsection about Late holocene sea-level changes was added
17756	4	15	52	16	7	CO2 levels and temperatures in this period are not stated in the report but are crucial to judge what can be concluded from this, also in comparison with the Mid Pliocene and with the present. According to reviewer's knowledge, geological research indicates that CO2 levels were comparable and temperatures were rising due to astromical cycles. If correct, this would imply that this is a counter-example for the assumption of high climate sensitivity [Hessel Voortman, Netherlands]	It is unclear what the reviewer wants. There is no information in the text on the climate sensitivity during the deglaciation
3314	4	15	54	16	7	The 8.2 event may be also considered here because it had visible effects in coastal and mountains ecosystems from SW Europe (e.g. Iriarte-Chiapusso et al., 2016, Quaternary International 403: 211-236). [Castor Muñoz Sobrino, Spain]	This reference is about climatic changes derived from chironomid variations in Northern Spain. Instead, references about climatic variations assigned to sea-ice fields were introduced for the Pleistocene-Holocene transition.
8364	4	15	54	16	7	The authors have not discussed the sea level variations of the last deglaciation. How much high was sea level compared to present. [APECS Group Review, Germany]	we have added information on the variations of the sea level during OIS3, during the deglaciation (melt water pulses) and the Holocene sea-level fluctuation
18396	4	16	1	16	2	'recent evidence... support evidence...' - clumsy wording, and actually not true. The keel marks indicate large scale calving, which MAY be consistent with MICI, but it is not unequivocal support. [Nicholas Golledge, New Zealand]	This has been reworded in line with the suggestion by the reviewer

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
20746	4	16	2	16	2	Wise et al. provides evidence for cliff failure, but not for MICI (i.e. whether and how a cliff failure could lead to a widespread, sustained positive feedback) [Tamsin Edwards, UK]	as previous comment 18396
12636	4	16	5	16	7	Within section 4.2.2.1.3 Last deglaciation, last sentence says "However, it is important to note that the retreat of ice sheets during the Holocene occurred during climatic conditions generally colder than today, so the mechanisms of retreat maybe be different from those that will dominate the future.". I believe it is important also to add here the following idea: "Also, during the Holocene, the lag between maximal insolation (i.e. warming) about 11.7 ka BP and maximum sea level rise was of 4500 years. This lag between the rise of temperature in the atmosphere and the rise in sea level occurs because the latter depends on heat penetrating deep within the ocean, which inevitably takes considerable time (Clark et al., 2016)." (this reference is already included in the References section of this chapter) [Alejandro Cearreta, Spain]	we have added a remark on the radiation conditions
17816	4	16	11	16	21	Include results from The IMBIE team (Nature, 2018) [Robert Arthern, UK]	accepted and included
3026	4	16	12	16	12	"extending to" -> "beginning" [Robert Kopp, USA]	accepted and corrected
8368	4	16	12	16	12	E1a Suggest "This record, dating from around 1700 in some locations…" to better show that now we are moving forward in time from 1700 (as opposed to backwards in time in the previous section) [APECS Group Review, Germany]	accepted and corrected
14546	4	16	12	16	12	sea level trends. Moreover, since [Christophe Deissenberg, Luxembourg]	accepted and corrected
14548	4	16	17	16	17	Velicogna and Wahr, 2006), confirming [Christophe Deissenberg, Luxembourg]	accepted and corrected
11828	4	16	19	16	19	"as a result" - the sentence does not follow on from the prior argument. GRACE is not the only thing that has led to this - improved altimetry, more ARGO etc. [King Matt, Australia]	accepted and corrected
16398	4	16	19	16	19	Full spelling of SMB should be included [Lee-Sim Lim, Malaysia]	accepted and corrected
14550	4	16	20	16	21	and relative contributions of the different processes causing sea level change (and in particular of the increasing contribution of the ice sheets) has emerged since AR5, [Christophe Deissenberg, Luxembourg]	accepted and corrected
23962	4	16	26	16	28	It should be added that many tide gauges lack long term records or have significant gaps in their records. [Hans-Otto Poertner and WGII TSU, Germany]	accepted and corrected
306	4	16	28	16	30	the sentence beginning with "Tide gauges are grounded …" is doubled, remove [Kerstin Jochumsen, Germany]	accepted and corrected
5144	4	16	28	16	30	The sentence 'Tide gauges are grounded on land.....sediment compaction' is duplicated in Line 30 to Line 33. [Sai Ming Lee, China]	accepted and corrected
8370	4	16	28	16	30	The sentence is written twice. Please delete from "Tide gauges.....sediment compaction", Sentence then can start from "Tide gauges"..... line no. 30 [APECS Group Review, Germany]	accepted and corrected
4270	4	16	28	16	32	Something went wrong with this sentence [Peter Clark, USA]	accepted and corrected
13984	4	16	28	16	33	Sentence repeated [Debra Roberts and Durban Team, South Africa]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15958	4	16	28	16	33	Sentence partially repeated. Should read: "Tide gauges are grounded on land and are affected by the vertical motion of Earth's crust caused by both natural processes (e.g., GIA, tectonics and sediment compaction; Wöppelmann and Marcos, 2016) and anthropogenic activities (e.g., groundwater depletion, dam building or settling of landfill in urban areas; (e.g., Raucoules et al., 2010)." [Tim Riding, New Zealand]	accepted and corrected
23964	4	16	28	16	33	Sentence needs revision [Hans-Otto Poertner and WGII TSU, Germany]	accepted and corrected
18556	4	16	30			no capital 't' needed on 'tide' [Christopher Fogwill, UK]	accepted and corrected
23116	4	16	30			this sentence is a duplicate of previous one [Jacques Beall, France]	accepted and corrected
3028	4	16	33	16	33	Tie back to prior discussion by noting that tide gauges thus measure RSL change. It's not clear why a "correction" needs to be applied unless you are trying to extract SSH change from a record of RSL change; this needs to be made explicit. [Robert Kopp, USA]	accepted and corrected
11832	4	16	33	16	35	this a superficial statement. This approach assumes that the GPS record, which could be distant to the tide gauges, and spans maybe up to 20 years represents the land motion over the full period. Far-field effects mean that this is not the case and the work of Riva et al TC 2017 should be noted here since these effects (like melting greenland deforms Australia through 20th century) should be discussed [King Matt, Australia]	accepted and corrected
11830	4	16	34	16	34	co-located not collocated [King Matt, Australia]	accepted and corrected
23272	4	16	36	16	38	-0.002 mm/yr^2 is deceleration, contradicting to the statement. Please check if this is a typo (when compared to the data stated in later sections) [Y. Jeffrey Yang, USA]	rejected. It is not a typo. Here we use the term "acceleration" in the physical sense and not the literary sense. In the physical sense the "acceleration" can be either positive or negative.
3440	4	16	38	16	38	Whos is "They" ((Church et al. (2013)?) [Mahmood Riyaz, Maldives]	accepted and corrected
3442	4	16	40	17	4	The range in page 16 line 40 is (–0.002 to 0.019 mm yr–2). and in lpage 17 line 4 is (0.002–0.019 mm yr–1).Why?? [Mahmood Riyaz, Maldives]	accepted and corrected
3032	4	16	42	17	5	I'm not sure the numbers in this paragraph account for different time period accurately. Hay et al. 2015 estimate 1.59 ± 0.14 mm/yr (2σ) over 1901-2010. Dangendorf et al. (2017) is 1.1 ± 0.3 mm/yr (1s) over 1902-1990 and 3.1 ± 1.4 mm/yr over 1993-2010, suggesting a rate over 1900-2012 of about 1.5 mm/yr. [Robert Kopp, USA]	accepted and corrected
17146	4	16	42	17	5	The reconstructed rates are more (or less) different, dependent on the period that is considered, and most reconstructions agree for the second half of the 20th century, but this doesn't quite come across in the text. [Aimee Slangen, Netherlands]	accepted and corrected
17758	4	16	45	16	47	Explain the "ad hoc" procedure, as it may be important for the conclusions derived [Hessel Voortman, Netherlands]	rejected. The had oc correction is not explained because it is not important for the conclusions derived. The reviewer can refer to the publication cited in the report to get more details on the ad hoc correction
11838	4	16	48	16	48	It should be noted that it is a technique using Tide Gauges with altimetry. So tide gauges are used twice… [King Matt, Australia]	accepted and corrected
11836	4	16	49	16	49	the statement "before 1990" is more correctly put that it is the 1940s and 1950s where the differences are seen in terms of rates. This is not a whole-of-century effect [King Matt, Australia]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17606	4	16	53	16	54	They are not AR5 estimates as everything in the AR5 must come from published results. This is an important point and not just semantics. Suggest rephrase here. [Jonathan Bamber, UK]	accepted and corrected
11840	4	16	54	16	54	I think the new estimates of lower values overlap within uncertainties with AR5 - please check - if so then this statement is not robust. [King Matt, Australia]	rejected. New estimates are 1.3 mm/yr ±0.2 which is not in the 90% CL range of AR5 which is 1.5-1.9 mm/yr
14552	4	17	1	17	1	that the sea level increase has [Christophe Deissenberg, Luxembourg]	rejected. This sentence refers to the acceleration in sea level which is the second derivative of the sea level. The" sea level increase acceleration" is the third derivative in sea level which we don't adress here
23966	4	17	1	17	1	'Sea level rise' has accelerated, not 'sea level' [Hans-Otto Poertner and WGII TSU, Germany]	rejected. This sentence refers to the acceleration in sea level which is the second derivative of the sea level. The" sea level rise acceleration" is the third derivative in sea level which we don't adress here
13084	4	17	3	17	4	acceleration of SLR should be measure in mm/yr/yr, not mm/yr [Gerhard Krinner, France]	accepted and corrected
2736	4	17	3	17	5	The acceleration should probably be in mm/yr^2 not mm/yr. [Nicolas Jourdain, France]	accepted and corrected
17760	4	17	4	17	4	Unit of accelaration is mm/yr^2, not mm/yr. Found in other places as well, but not all instances have been noted in this form [Hessel Voortman, Netherlands]	accepted and corrected
11844	4	17	7	17	24	it should noted that the altimetry estimates rely on an uncertain GIA correction associated with enlarging ocean basins - typically taking 0.3mm/yr but Tamisiea 2012 gives this as 0.15-0.40 - no progress has been made in reducing this uncertainty beyond the suggestion of Rietbroek et al PNAS that the term may be at the smaller end due to more rapid adjustment in West Antarctica meaning that the ocean basins have adjusted more rapidly [King Matt, Australia]	rejected.Concerning the result from Tamisiea, it is not new since the IPCC AR5. Here we focus on new results since the IPCC AR5. Concerning the result from Rietbroek suggesting the lower end for the GIA correction, it is supported only by one publication so far. This result needs to be consolidated by other publications or other line of evidences to be reported here
16400	4	17	13	17	13	Full spelling of VLM should be included [Lee-Sim Lim, Malaysia]	accepted and corrected
11842	4	17	14	17	16	this sentence confuses the various studies. Watson et al identify an instrumental issue with TOPEX and show that correcting it leads to *data* which suggests an acceleration. Dieng take models and adjust the data from TOPEX to fit the models! Chen et al take the Watson data, treat it to compute rates, and show that models reproduce it within uncertainty [King Matt, Australia]	accepted. It is true that the Chen et al study does not use the sea level closure approach with independent estimates of the contributions to sea level (as Dieng et al. did) to estimate the bias in satellite altimetry. The reference to this study has been removed.
308	4	17	15			add "and" after (Watsen et al 2015) [Kerstin Jochumsen, Germany]	accepted and corrected
3444	4	17	18	17	19	This is a significant change in the context of Small islands and should be addressed [Mahmood Riyaz, Maldives]	noted
3034	4	17	20	17	24	What about evidence for Antarctic acceleration? Also, is the 'satellite altimetry' section the right place for this discussion? [Robert Kopp, USA]	rejected. The Antarctic acceleration is small compared to the Greenland acceleration. This is the reason why we write "mostly explained by Greenland acceleration".
23968	4	17	26			The estimation of the observed contributions could be moved one level up, i.e. as 4.3, do be more distinct from the previous one. [Hans-Otto Poertner and WGII TSU, Germany]	rejected. The section on the estimation of contributions to sea level is at the level 4.2.2.3. Moving it a level up would bring it at level 4.2.3 with sections like "4.2.2 observed changes in sea level" or "4.2.1 processes of sea level change". The upper level seems too general a level for just adressing the observed contributions to present sea level change
3446	4	17	31	17	31	AOGCM need to write the long form [Mahmood Riyaz, Maldives]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17256	4	17	31	17	31	it would be worth it to include reference to the AOGCM experiments as they are relevant for the instrumental period [Iulian Florin Vladu, Germany]	accepted and corrected
2354	4	17	38	17	41	Additional citations to add: Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Dutton A., et al. (2015) Sea-level rise due to polar ice-sheet mass loss during past warm periods, SCIENCE 349(6244):153. [Kristin Campbell, USA]	rejected. In this paragrpah we adress the thermal expansion of the ocean at global scale. In addition, in this report our objective is to review new material since the IPCC AR5. SWIPA 2017 uses the IPCC AR5 estimate for thermal expansion. there is nothing new from SWIPA 2017 to report here.
2480	4	17	38	17	41	Additional citations to add: Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Dutton A., et al. (2015) Sea-level rise due to polar ice-sheet mass loss during past warm periods, SCIENCE 349(6244):153. [Durwood Zaelke, USA]	rejected. In this paragrpah we adress the thermal expansion of the ocean at global scale. In addition, in this report our objective is to review new material since the IPCC AR5. SWIPA 2017 uses the IPCC AR5 estimate for thermal expansion. there is nothing new from SWIPA 2017 to report here.
12978	4	17	38	17	41	Additional citations to add: Arctic Monitoring and Assessment Programme (AMAP) (2017) SNOW, WATER, ICE, AND PERMAFROST IN THE ARCTIC: SUMMARY FOR POLICYMAKERS; Dutton A., et al. (2015) Sea-level rise due to polar ice-sheet mass loss during past warm periods, SCIENCE 349(6244):153. [Gabrielle Dreyfus, USA]	rejected. In this paragrpah we adress the thermal expansion of the ocean at global scale. In this report our objective is to review new material since AR5. SWIPA 2017 uses the IPCC AR5 estimate for thermal expansion. There is nothing new from SWIPA 2017 to report here
14554	4	17	39	17	40	(Church et al., 2013; Rhein et al., 2013). It is very likely due (ITALICS MISSING) [Christophe Deissenberg, Luxembourg]	accepted and corrected
4148	4	17	43	17	49	I wonder if it would be clearer to state that estimates of global thermal expansion are fundametnally limited by the availability of in situ ocean temperature measurements? While sampling of the upper 700m is reasonable from the late 1960s, the deeper layers are only observed through a sparse network of hydrographic sections, prior to Argo observations becoming available in the early 2000s? [Matthew Palmer, UK]	accepted and corrected
14556	4	17	46	17	46	causes. However, the global observing system is not ideal [Christophe Deissenberg, Luxembourg]	accepted and corrected
4152	4	17	47	17	48	I wonder whether it would be helpful to the reader to briefly list what these sources of uncertainty are? I would then tend to call out mapping method and XBT bias corrections as two of the key sources (which are discussed in more detail later). [Matthew Palmer, UK]	accepted and corrected
17148	4	17	51	17	51	what does 'those' refer to? [Aimee Slangen, Netherlands]	accepted and corrected
3036	4	17	52	17	52	Define XBT correction. [Robert Kopp, USA]	accepted and corrected
15960	4	17	52	17	52	Update reference when available [Tim Riding, New Zealand]	We expect this paper to be published on time for the SROCC final draft. If not we will remove this reference
17258	4	17	52	17	52	The XBT correction for thermal expansion could be briefly discribed as key for bias-correction [Iulian Florin Vladu, Germany]	accepted. We specify now that it is a correction of the bias in the XBT instruments. We don't have room enough to go further into details and we refer the reader to several publications for more details
23970	4	17	52	17	52	Remove ‘in prep.’ citations. [Hans-Otto Poertner and WGII TSU, Germany]	We expect this paper to be published on time for the SROCC final draft. If not we will remove this reference
14558	4	17	54	17	54	the results lie [Christophe Deissenberg, Luxembourg]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4150	4	18	1	18	8	The sudden mention of heat gain (in Wm-2) in this section comes a bit "out of the blue" and it is not immediately obvious how this is relevant to the section heading. Perhaps you need to include some sentences earlier that state the relationship between global thermal expansion and global ocean heat uptake? [Matthew Palmer, UK]	accepted and corrected
14560	4	18	3	18	3	Wijffels et al., 2016). For 2006–2015, the data indicates a global ocean [Christophe Deissenberg, Luxembourg]	accepted and corrected
14562	4	18	6	18	8	I DO NOT UNDERSTAND HOW THERMAL EXPANSION CAN CAUSE WARMING …. [Christophe Deissenberg, Luxembourg]	accepted and corrected
3038	4	18	10	18	10	Why no uncertainty in 1970-2015 rate? [Robert Kopp, USA]	accepted and added
4272	4	18	10	18	10	will these be reported as 5-95% range as in AR5? [Peter Clark, USA]	accepted. Yes uncertainties are reported now as 5-95% range
8372	4	18	10	18	10	E1a It's unclear why "Since AR5" is specified. Have these estimates changed since AR5? Are they still consistent within errors? [APECS Group Review, Germany]	accepted and corrected. Yes these estimate are still consistent with AR5
17762	4	18	14	18	20	Specific example but the comment is valid in a wider context in the report. A large number of historical reconstructions and (obviously) all projections rest on studies using models. Considering the importance of models, showing that these models have been properly calibrated and validated is of extreme importance. This aspect of proper use of model appears to be lacking completely from the report and is badly missed. [Hessel Voortman, Netherlands]	rejected. In this chapter we address specifically the question of the validation of climate models. We review all the litterature on the comparison between sea level observations and climate model simulations of these observations . We do it for each contribution to past sea level (see sections 4.2.2.3.1 to 4.2.2.3.5), for the total global man sea level (see section 4.2.2.3.6) and for regional sea level (see section 4.2.2.4). We don't adress the question of the calibration of climate models as climate models are not calibrated against observations of sea level or observations of contributions to sea level. For more details on the calibration of climate models please refer to the IPCC AR5 chapter 9
13574	4	18	15	18	17	is this a sentence? [Debra Roberts and Durban Team, South Africa]	accepted and corrected
4154	4	18	21	18	23	The different expansion efficiencies of the models must play some part in this overall spread. If these differences are negligible compared to the other factors mentioned, then this should be stated. It may be worth mentioning expansion efficiency earlier in the chapter, to help explain the relationship between ocean heat uptake and globall thermal expansion? On line 22 I suggest replacing "modelled" with "model". In general, when referring to model simulations, I think it is better to use the term "model simulated" rather than "modelled", but this is a stylistic point. [Matthew Palmer, UK]	accepted. The expansion efficiency of heat plays a minor role in the spread (see Melet and Meyssignac 2015). We corrected the text to specify that the spread was "essentially" due to uncertainty in the forcing and the climate sensitivity and ocean heat uptake efficiency. We prefer not to introduce the expansion efficiency of heat to avoid too many technical details. We refer the reader to Melet and Meyssignac 2015 for more details on this.
3040	4	18	44	18	46	Need more context to understand whether 5 mm is a lot or a little. [Robert Kopp, USA]	accepted and corrected
3766	4	18	45	18	45	"…AR5, the…" [Ola Kalen, Sweden]	accepted and corrected
14564	4	18	45	18	45	since AR5, the biggest [Christophe Deissenberg, Luxembourg]	accepted and corrected
4274	4	18	48	18	48	I don't see any mention as to whether you include (or don't) glaciers peripheral to the ice sheets - needs to be clarfied. [Peter Clark, USA]	Taken into account: much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, text has substantially changed. We now point out at Table 4.1 which glaciers are included, and which are excluded.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8374	4	18	55	18	55	E1a Reference Zemp et al. (2015) is missing some authors (p. 139 l. 6) [APECS Group Review, Germany]	copyedit to be completed prior to publication
14566	4	18	57	18	57	series are presented in Marzeion [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
1448	4	18	57	20	5	I found this section to be a bit confusing and difficult to follow. First the study by Marzeion et al. (2015) is introduced, in which observations/modelling is used to estimate contributions from glaciers. Subsequently a section is dedicated to gravimetric data (p.19, l.25-34) and the formation of lakes (p.19, l.36-42), after which modelling results are mentioned and a number from Marzeion et al. (2015) is stated again. How does this number compare to the numbers previously mentioned (p.18, l.16-23) and should this not be put together? Furthermore, some of the studies mentioned on p.19, l.46-47 are glacier modelling studies, but they do not consider the evolution of glaciers over the past century. [Harry Zekollari, Switzerland]	Taken into account: much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, text has substantially changed.
17764	4	19	1	19	23	Message of this section appears to be that the rate of acceleration of sea level is highly uncertain with the apparent possibility that there is no accelaration after all. Either this message is unintentional and the section should be re-phrased or this section is in contradiction with earlier conclusions in the report in which case these conclusions need to be modified [Hessel Voortman, Netherlands]	rejected: This section only refers to glacier mass change. Uncertainty in acceleration of glacier mass loss does not imply uncertainty in acceleration of sea level rise, since other contributors than glaciers can still cause acceleration. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
14568	4	19	4	19	4	and leads to [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
1450	4	19	5	19	52	Several references to Leclercq: should in all cases be Leclercq et al. (both for the 2011 and the 2014 study) [Harry Zekollari, Switzerland]	copyedit to be completed prior to publication
17608	4	19	13	19	23	Marzeion updated has been further updated in Bamber et al, 2018 ERL by substituting observations for the HMA and some areas with significant marine terminating sectors especially in the Arctic. Revised estimates agree better with GRACE data. [Jonathan Bamber, UK]	taken into account: we agree with the reviewer's statement. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
6248	4	19	16	19	23	hard to follow all those numbers. Better in a table? [Regine Hock, USA]	taken into account: we agree with the reviewer's statement. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text no longer exists here.
11846	4	19	25	19	34	A clarfication is needed that moutnain glacier mass change may not mean the mass leaves the region and enters the oceans. GRACE only senses the net mass change and water stored in dams remains in the region. Is this potential distinction included? [King Matt, Australia]	Taken into account: this issue was covered page 19, lines 36 to 42. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14570	4	19	26	19	26	changes to glaciers among other sources [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
4276	4	19	33	19	33	These numbers should be tabulated in a more comprehensive way [Peter Clark, USA]	taken into account: we agree with the reviewer's statement. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
17610	4	19	36	19	42	Discussed and analysed in Brun et al, 2017 Nat Geo which should be cited and discussed. Does not make a big difference for HMA.... [Jonathan Bamber, UK]	taken into account: Note that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion - particularly of regional changes - was moved to these chapters. Therefore, this text has substantially changed.
8376	4	19	39	19	39	E1a "... potential of meltwater retention ON LAND is only ..." [APECS Group Review, Germany]	copyedit to be completed prior to publication
11928	4	19	39			check reference for consistent [Chukwuma Anoruo, Nigeria]	copyedit to be completed prior to publication
13086	4	19	40	19	42	Might add that this would in most cases be a transient problem [Gerhard Krinner, France]	Rejected: often, this is not a transient problem, since new pro-glacial lakes in overdeepenings can be expected to be permanent.
17766	4	19	44	19	50	Obviously, models have their limitations and limitations should be reported as is done here. However, it is important to explain how this aspect influences the uncertainty of the model results and consequently the confidence level of the conclusions [Hessel Voortman, Netherlands]	taken into account: model uncertainty is discussed in Sect. 4.2.3
8378	4	19	45	19	45	E1a Glacier valley width is too generous here -- the largest valley glaciers I could think of (Aletsch Glacier, Baltoro Glacier) are each only 2km wide. On ice sheets, Petermann Glacier (Greenland) is about 20 km wide, but I don't think that is relevant in the glaciers section. [APECS Group Review, Germany]	taken into account: we agree that "a few km or less" is more appropriate. Note, however, that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
4278	4	19	47	19	47	Is this statement consistent with lines 9-10 on this page? [Peter Clark, USA]	taken into account: lines 9-10 refer to the reconstruction of glacier mass change based on observations of glacier length change, the section here refers to reconstructions based on climate and glacier model. The increase of the estimate based on length change combined with a decreased model-based estimate generally increases the agreement between the two.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1452	4	19	52	19	54	As a reader, I am a bit confused here. To which studies does "the modelled contribution from climate models" refer? Do I understand it correctly that this is the 'CMIP5 ensemble mean' in Figure 4.3? The observed series by Leclercq et al. (2011) and Marzeion et al. (2015) are mentioned: should these be in Figure 4.3 / what is their relationship (if any) to the other studies shown in Figure 4.3 (e.g. Church et al., 2011; Ray and Douglas, 2011,...etc)? How does this section relate to the section in which the Marzeion et al. (2015) study is explained (p.18, l.57 - p.19,l.23). Maybe worth reorganising this section a bit? [Harry Zekollari, Switzerland]	taken into account: Fig. 4.3 focuses on the validation of CMIP5-based reconstructions of sea-level rise; thus the glacier reconstructions of Leclercq et al. (2011) and Marzeion et al. (2015) are not shown. Instead, the glacier contribution shown here is based on the (updated) model of Marzeion et al. (2012), forced not by climate observations (which is done in Marzeion et al., 2015), but by CMIP5 results. However, Marzeion et al. (2015) was used to illustrate the impact of the Greenland peripheral glaciers' contribution in the first half of the 20th century (blue dashed line "CMIP5 ensemble mean corrected"). Note that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
17152	4	19	52	19	54	the time series of marzeion is not an observed time series, it is a model driven by a reanalysis. Also the observations of Leclerq are actually a reconstruction based on a limited set of obervations. [Aimee Slangen, Netherlands]	Taken into account: A glacier model is involved in producing the time series of Marzeion et al. (2015). However, this model is calibrated using glacier observations, and forced using climate observations (not reanalysis data), such that we think it is more justified to consider the model as an elaborate way of interpolating glacier observations than as a modeled glacier reconstruction, to which we refer here: in this case the model is forced with CMIP5 results, and the model results are thus largely independent of observations. Note that much of section 4.2.2.3.3 was a duplication of material either to be covered in Ch.2 (low- and mid-latitude glaciers) or Ch. 3 (high-latitude glaciers). The more detailed discussion of glaciers was moved to these chapters. Therefore, this text has substantially changed.
15192	4	20	0	80		ALMOST NOTHING IS SAID ABOUT POSSIBLE CHANGES IN OCEANIC CIRCULATION AND THEIR POTENTIAL AGGRAVATING IMPACT [Christophe Deissenberg, Luxembourg]	taken into account: this is addressed in section 4.2.3
14572	4	20	3	20	3	century, for which the Cogley [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
17150	4	20	5	20	5	strange use of 'rather' [Aimee Slangen, Netherlands]	copyedit to be completed prior to publication
1454	4	20	10	20	10	limited' and 'medium' in italic? [Harry Zekollari, Switzerland]	copyedit to be completed prior to publication

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4280	4	20	10	20	10	I don't think you can say that confidence has increased but remains medium. If it remains medium, then all you can say is that "there hae been improvements in X,Y, and Z, but because of … confidence remains medium." [Peter Clark, USA]	accepted: text change to: While the agreement between observational estimates of glaciers mass changes (in particular in the first half of the 20th century) has increased, and while historical model results are more consistent with observational estimates, the confidence in the use of glacier models to reconstruct sea level change is limited and the overall confidence remains medium because of the still limited number of well-observed glaciers to validate models on long time scales, because of the unexplained bias in models over the first part of the 20th century, and because of the small number of model-based global glacier reconstructions.
17156	4	20	15	20	15	IMBIE2 results need to be added to this section [Aimee Slangen, Netherlands]	accepted and corrected
4282	4	20	17	20	17	Delete thi first clause and start with "Reliable observations…" [Peter Clark, USA]	accepted and corrected
8380	4	20	17	20	17	E1a Replace "reliable" with either "frequent" or "regularly occurring" -- there is nothing inherently unreliable about ground- or air-based measurements! [APECS Group Review, Germany]	accepted and corrected
17154	4	20	17	20	17	even though the coverage of the satellite observations is larger does not automatically imply that the observations are reliable: as mentioned in the following sentences, models or additional data are still needed to estimate mass changes [Aimee Slangen, Netherlands]	accepted and corrected
1918	4	20	17	20	19	Worth noting here that reliable pre-satellite era mass balance has been geodetically reconstructed for the Greenland ice sheet. This empirically constrains level rise contribution between 1900 and 1983 to 17.2 +/- 6.5 mmSLE (Kjeldsen et al., 2015, Nature, Spatial adn Termporal Distribution of Mass Loss from the Greenland Ice Sheet). This longer term context also highlights that post-2010 ice sheet mass loss is about 3x greater than the 20th Century mean. [William Colgan, Denmark]	accepted and added
4284	4	20	18	20	24	long sentence - suggest break it up [Peter Clark, USA]	accepted and corrected
8382	4	20	21	20	21	E1a The input-output method does not necessarily require SAR: any reliable velocity dataset with good spatial coverage will do. That has historically meant SAR, but new optical techniques (Landsat- or Sentinel-based) are fair game too. Suggest replace "measurements of flow velocities estimated with SAR data along the margin" with "ice velocities measured, often by satellite, across key outlets" [APECS Group Review, Germany]	accepted and corrected
3768	4	20	24	20	24	add "et al.," to reference Vaughan [Ola Kalen, Sweden]	accepted and added
16406	4	20	24	22	25	Related references should be included/cited in between this section. [Lee-Sim Lim, Malaysia]	rejected. For the ease of the reader we prefer to gather all references at the end of the chapter. We refer the reviewer to the end of the chapter where he will find all the cited references.
17818	4	20	25	20	27	The satellite methods do not attribute cause. Have the increased losses been unambiguously attributed to warming of the immediately adjacent ocean? If so, a citation to that study is needed. Circulation changes are also important to heat delivery from ocean to ice, and Paulo et al. (2015) also mention atmospheric forcing. [Robert Arthern, UK]	accepted and corrected
4286	4	20	27	20	27	add Bamber et al. 2018 [Peter Clark, USA]	accepted and added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13088	4	20	27	20	28	IMBIE (2018) might be cited here [Gerhard Krinner, France]	accepted and added
11848	4	20	28	20	30	while uncertainties are given they are often not robust and assume often noise properties (e.g., no serial correlations in the data). Williams et al EPSL 2014 show that for GRACE uncertainties could be underestimated by factor 4-6 [King Matt, Australia]	noted. We want to highlight that the numbers we compiled here are derived from the three different methods for estimating ice sheet mass loss, not only GRACE
8384	4	20	30	20	30	E1b When did the accelerations begin? [APECS Group Review, Germany]	This information is not stated in the papers that we reviewed and thus we don't provide it here
4288	4	20	31	20	31	new IMBIE paper in Nature [Peter Clark, USA]	accepted and added
8386	4	20	34	20	34	E1a "In Greenland, the acceleratED MASS LOSS is caused..." [APECS Group Review, Germany]	accepted and corrected
17820	4	20	38	20	40	Similar to above. Check whether Paolo et al. 2015 directly attribute the thinning that they observe to ocean warming. [Robert Arthern, UK]	accepted and changed
2738	4	20	42	21	9	This section will need to be updated based on the new paper from the IMBIE group (Shepherd et al. Nature 2018). [Nicolas Jourdain, France]	accepted and changed
17768	4	20	42	21	9	Compliments to the authors for explaining a number of difficulties and uncertainties related to an undoubtedly difficult topic. However, as an outsider one could conclude that efforts related to ice sheet response are not sufficiently mature to draw firm conclusions. Hence, uncertainties are large and confidence levels of a number of conclusions appear to be too high [Hessel Voortman, Netherlands]	rejected. The confidence level is not related to the width of the uncertainty range. Even if the uncertainty is large we can have high confidence in their estimation and vice versa. We refer the reviewer to the "Uncertaint Guidance Note" of the IPCC report AR5. This report will be updated for the SROCC but will be along the same lines.
21550	4	20	42	21	9	This section first presents estimates for the contribution of both polar ice sheets to sea level rise. These contributions, in turn, are a result of both changes in SMB and of dynamic changes. Yet the bulk of the discussion presented is about the SMB changes. Dynamic changes, for both ice sheets, should have a similar amount of space - or at least be brought more to the fore front. [Fiamma Straneo, USA]	rejected. This paragraph is devoted to the estimate of ice sheet mass loss from climate models over the 20th century and how they compare with observations. To our knowledge there are no results since 1R5 on ice sheet dynamic changes over the 20th century from climate models. For this reason we can not present any results on this in this paragraph. This is the reason why this paragraph is dominated by results on SMB. We acknowledge that we should be clear on that. We add now a sentence at the beginning of the pargarph to recall this.
18558	4	20	42			modeled [Christopher Fogwill, UK]	accepted and corrected
14574	4	20	57	21	1	For Antarctica, reanalyses-based estimates are reliable only since 1979 (Favier et al., 2017). Over 1979–2015 climate model based [Christophe Deissenberg, Luxembourg]	accepted and corrected
13576	4	21	1	21	1	Before 1979? [Debra Roberts and Durban Team, South Africa]	Before 1979 results are not reliable as stated in the previous sentence. So we can not say anything for now.
1806	4	21	1	21	3	If reanalysis based estimates are available, why not showing this for Antarctica SMB in Table 4.1? Does the reanalysis agree with the negative contribution the models produce? Also, change small contribution to negative contribution in the text. [Sybren Drijfhout, Netherlands]	accepted and corrected. Actually, reanalyses and observations agree with climate models in indicating a non significant contribution to sea level rise. We don't show the partition into SMB and ice discharge for Antarctica in table 4.1 because the uncertainty in SMB is very large while the uncertainty in the sum is much smaller. We may change of strategy in the final order draft.
17822	4	21	1	21	9	Include results from the IMBIE team (Nature, 2018) [Robert Arthern, UK]	accepted and corrected
11930	4	21	6			there is need to list such studies [Chukwuma Anoruo, Nigeria]	accepted and corrected
14576	4	21	7	21	7	West Antarctica. For East Antarctica [Christophe Deissenberg, Luxembourg]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17158	4	21	13	21	13	this sentence reads like the majority of groundwater storage changes are natural changes, but the rferences cited partly deal with anthropogenic driven groundwater changes [Aimee Slangen, Netherlands]	accepted and corrected
4294	4	21	13	21	39	This paragraph needs to be better organized with respect to parsing out natural from anthro changes in land watr. I think the reason AR5 did not assess natural (due to poor understanding) is still valid. The anthro contribution (given different time periods) remains consistent, so the discrepency is because of poor knowledge of natural changes. [Peter Clark, USA]	accepted and corrected
4290	4	21	30	21	30	unclear whether net refers to anthro and natural [Peter Clark, USA]	accepted and corrected
14578	4	21	31	21	31	range from -0.33 to 0.23 mm [Christophe Deissenberg, Luxembourg]	accepted and corrected
14580	4	21	34	21	34	respectively. However, hydrological models estimate slightly positive trends over the same period. [Christophe Deissenberg, Luxembourg]	accepted and corrected
4292	4	21	35	21	35	how can you have medium confidence if of opposite sign? [Peter Clark, USA]	accepted and corrected
8388	4	21	35	21	35	C4 I'm confused that we have medium confidence in TWS contribution to SLR when the magnitude is up to ~10% of SLR rates, and we don't even know its sign! Some elaboration is necessary here to help readers see what I surely am missing. [APECS Group Review, Germany]	accepted and corrected
4580	4	21	44	22	56	The set-up for the modelled budget is unclear here. It is mentioned that historical runs are used, which are only available until 2005. Where do the values between 2005 and 2015 stem from? RCP8.5? [Sönke Dangendorf, Germany]	accepted and corrected
1456	4	21	47	21	47	"since 1993-2015" --> "1993-2015" [Harry Zekollari, Switzerland]	accepted and corrected
14582	4	21	47	21	47	WAHAT DO YOU MEAN WITH since 1993–2015 ??? [Christophe Deissenberg, Luxembourg]	accepted and changed
3042	4	21	48	21	50	Internal climate variability might also be expected to be significant over 1993-2015. [Robert Kopp, USA]	accepted and corrected
13578	4	21	50	21	52	First use of acronym 'AOGCM'. Please spell out in full. [Debra Roberts and Durban Team, South Africa]	accepted and changed
4296	4	21	54	21	54	and exclude hydrological models (with opposite sign) because? [Peter Clark, USA]	Hydrological models are excluded because there is no community consensus on land water changes (natural or including human impact) before 2002. .
4298	4	21	54	21	54	reference to support this statement about climate related variations? [Peter Clark, USA]	The statement has been removed
3044	4	21	54	21	55	This sentence doesn't make sense. I think it is confusing point in time ("before 2005") with duration. [Robert Kopp, USA]	accepted and change.
13580	4	22	0			There is a sense of repetition from one section to the next. Text keeps going back and forth between ice sheets, glaciers, groundwater etc. Is there a way to avoid this? Perhaps by making sure that there is a logical flow of information, going topic by topic? For instance, it would be more natural to create sub-headings by components (eg ice sheets, glaciers, etc) and discuss for each observations, processes, contribution to sea level etc. than the other way round. It's easier to follow if the visual, physical realities have a higher heading level than the more abstract concepts, measurements and processes (lower heading level). [Debra Roberts and Durban Team, South Africa]	rejected. In this subsection on GMSL budget we don't adress the different contributions to sea level. Instead we look at all contributions together and we compare their respective amplitude. This general analysis of the relatve amplitude of each contribution can not be moved up and spread out into subsections adressing a single contribution . It has to be adressed in a general subsection like this one on GMSL budget

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1458	4	22	6	22	8	Are the numbers given here based on Table 4.1 or do they come from Cazenave et al. (2018) + what is reference for Cazenave et al. (not in reference list) [Harry Zekollari, Switzerland]	The numbers are based on Table 4.1. These numbers are consistent within error bars with Cazenave et al. 2018.
3770	4	22	8	22	8	add "et al.," to reference Cazenave [Ola Kalen, Sweden]	accepted and changed
4300	4	22	11	22	13	This sentence is almost verbatim from AR5 re: AR4 (p. 1157), but in this case, it is not clear why your finding is a significant advane over AR5 in terms of physical understanding. [Peter Clark, USA]	The significant advance resides in the fact that now the sea level budget is closed at monthly to multidecadal time scales
11706	4	22	13	22	14	I think this sentence overstates the confidence in the observing system. We still do not have comprehensive observations of ocean thermal expansion below 2000 m, contributions from underice regionas and marginal seas, or of glacier contributions. [John Church, Australia]	accepted and changed
17770	4	22	16	22	19	With an observation record of less than 30 years, it appears that records are too short to draw firm conclusions on how things might develop. Currently we have no way of knowing whether we have observed one side of a cycle or indeed a trend. See general comment above related to uncertainties stemming from short observation records [Hessel Voortman, Netherlands]	Rejected. Our observations are not derived only from satellites. We have many other observations over the whole 20th century from tide gauge records and on longer time scales from proxies (see AR5 for more details on this. Climate models are validated against these long term observations. They are also validated against tide gauge records of the 20th century (see for example Slangen et al. 2017 and Meyssignac et al. 2017). These lines of evidence clearly show that we are observing a trend in sea level over more than 120 years.
1460	4	22	18	22	18	1900-1990 --> 1901-1990 [Harry Zekollari, Switzerland]	accepted and corrected
4302	4	22	18	22	19	Gregory et al. (2013) and AR5 tried accounting for these issues. [Peter Clark, USA]	accepted and added
11932	4	22	21		22	provide evidence with literature [Chukwuma Anoruo, Nigeria]	see Table 4.1
4304	4	22	23	22	23	AR5 period was 1971-2010 [Peter Clark, USA]	accepted and corrected
8390	4	22	33	22	33	E1a Instead of "regional changes in Greenland SMB" I think you mean "regional VARIATIONS" [APECS Group Review, Germany]	accepted and changed
8392	4	22	40	22	46	E1b I suggest being more specific about what the bias correction is (as I understand it from Section 4.2.2.3.4, it is using reanalysis-based SMB instead of climate model -based SMB). I would replace the word "bias" with "difference between" and resist saying "when this bias is corrected"; instead saying "when reanalyses are used instead of climate models" or something like that. [APECS Group Review, Germany]	rejected. The bias is detailed in section 4.2.2.3.3 and 4.2.2.3.4. We don't have room enough to come back on the definition of the "bias" here. Note that the "bias correction" we are using here is actually a time series derived from climate model runs that is corrected before the mid 20th century with a correction calibrated against reanalyses. As such this time series is different from the reanalyses time series. In this sense, we can not say we use "reanalyses instead of climate models. We believe that the term "Bias corrected" is more appropriate
14584	4	22	41	22	41	Greenland SMB and glacier ice loss [Christophe Deissenberg, Luxembourg]	accepted and corrected
8394	4	22	48	22	48	E1a specify: "... the percentage OF OBSERVED SLR explained by the simulations..." [APECS Group Review, Germany]	accepted and changed
11850	4	22	48	22	50	which altimeter record is being used here - the Watson et al one (supported by Dieng et al) or the standard one without correction for bias drift in TOPEX? I think there is agreement now that TOPEX record is suspect and needs some adjustment. [King Matt, Australia]	noted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
310	4	22	48			How can be the percentage explained be > 100% ? [Kerstin Jochumsen, Germany]	When the sum of the trends of the different contributions to sea level is higher than the trend in sea level we say that the "percentage explained is higher than 100%"
8396	4	22	51	22	54	E1a You say "compared to AR5" here, but no such comparison is made [APECS Group Review, Germany]	accepted and changed
13582	4	23	0			Fig 4.3 what is the grey area? what is the meaning of the blue shading ? What exactly is the zero on the Y axis? - Mean of observed or modelled or what? Text does not expain this clearly. And why is that used as zero, as opposed to say the mean observed in 1900? Or today's mean for that matter? Why 1980-2000? [Debra Roberts and Durban Team, South Africa]	accepted and added
23274	4	23	0			Unit for the numbers is needed. [Y. Jeffrey Yang, USA]	accepted and corrected
1914	4	23	1	23	10	I am not sure why these are so many blank terms in the 1901-1990 period. For example, Box and Colgan, 2017 (Chapter 9. Sea level rise contribution from Arctic land ice: 1850-2100. In: Snow, Water, Ice and Permafrost in teh Arctic (SWIPA) 2017. Arctic Monitoring Assessment Programme. Oslo Norway) provide first-order closure of the global budget back to 1850. In terms of specific missing pre-1990 terms, since AR5 the Greenland ice sheet sea level rise contribution between 1900 and 1983 has now been empirically constrained to 17.2 +/- 6.5 mmSLE by Kjeldsen et al., 2015 (Nature, Spatial adn Termporal Distribution of Mass Loss from the Greenland Ice Sheet). I seems the state of knowledge of teh 1901-1990 sea-level budget is substantially more advanced than presented in this table. [William Colgan, Denmark]	partially accepted. Box and Colgan 2017 use estimates of the contributions to the 1901-1990 sea level rise computed from the AR5. This is true for all contributions except the Greenland ice sheet contribution. The Greenland contribution is based on the Kjeldsen et al. 2015 estimate. This is the only new material since AR5. In this report the objective is to review new material since AR5. We add now the Kjeldsen estimate for Grenland ice sheet for the period 1901-1990.
12576	4	23	1			the numbers presented in Table 4.1 lack units (mm yr-1 ??). These should be stated at least in the caption. [Thomas Vikhamar Schuler, Norway]	accepted and corrected
1462	4	23	2	23	2	Table 4.1: 'Observed contribution to GMSL': Not entirely sure I get this? As 'Total contribution' is the sum of the individual 'observed' parts, would it make sense to rename 'Observed contribution to GMSL' as 'Observed GMSL rise'? (I may also be misunderstanding this) [Harry Zekollari, Switzerland]	accepted and changed
1464	4	23	2	23	2	In the modelled part, to what does the 'Residual' correspond? Difference between the total modelled (row above) and the observed? If that's the case, the numbers do not entirely match (independent of whether the 'Observed contribution to GMSL' or the 'Total Contributions' is used for the observations) [Harry Zekollari, Switzerland]	accepted and changed
6250	4	23	2	23	2	Ice sheet dynamic is a confusing and unclear term here: I assume what is meant here is mass losses at the ice-ocan interface through calving and submarine melt, often referred to as ice discharge [Regine Hock, USA]	accepted and corrected
16156	4	23	2	23	2	Missing standard deviation values xx in the table [Adi Nugraha, USA]	accepted and corrected
16404	4	23	2	23	2	A short explaination is needed to remind the readers on the information presented by the table. Furthermore, the source presented in the four columns (column 2 to column 5) is not cited. [Lee-Sim Lim, Malaysia]	accepted and corrected
2098	4	23	2	23	3	Please, indicate the unit of GMSL change (mm/y?, cm/y?). [Josep Medina, Spain]	accepted and added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
3046	4	23	2	23	3	Slangen et al 2017 seems to close the 20th century budget better than reflected here. [Robert Kopp, USA]	That is true but it is not sure that Slangen et al. 2017 close better the sea level budget for a good reason. Slangen et al. 2017 uses only the land water storage changes due to human intervention (groudwater pumping and Dam building). Here we take into account the land water storage changes due to the climate variability. There is large uncertainty on this term. It is this term that makes a difference with Slangen et al. 2017. It i not clear why considering this term results in a worst closure of the sea level budget. Is it because of errors in the land water storage term or because of errors in other contributions? This remains an open question
1808	4	23	2	23	10	It was unclear, and could not be reproduced by me, how “Total including water storage and ice sheet dynamics” was calculated, nor how the “residual” was calculated, nor how the uncertainties (standard deviations) were dealt with when adding up single contributions. Also, residuals can be positive or negative and I wasn’t sure this was accounted for correctly as the modelled total was sometimes larger, sometimes lower than “Observed total contributions” although always lower than “Observed total”, but then the numbers of the residual in the last 2 columns do not make any sense. Also, explain better that the terms included in the Total for models explicitly mentioned are observed estimates. It would be nice if some sources were given for the observed estimates of GMSL contributions. [Sybren Drijfhout, Netherlands]	accepted and changed
8398	4	23	2	23	10	E1a I could not find where the units of these numbers (presumably mm/yr) were specified [APECS Group Review, Germany]	accepted and added
17900	4	23	2	23	10	Very important table, and it is good that a comparable format was used as in the AR5. Further information is needed: e.g. 1) definition of uncertainties and model ranges, and 2) indication of which rows (e.g. glaciers) are subsets of information in other rows (e.g. ocean mass). [Haroon Kheshgi, USA]	Accepted for the uncertainties. When rows are subset of other rows this is indicated in the footnotes
17824	4	23	2	23	12	Include units on Table 4.1. [Robert Arthern, UK]	accepted and corrected
312	4	23	2			units are missing for the numbers in the table [Kerstin Jochumsen, Germany]	accepted and corrected
6252	4	23	4	23	4	if those glaciers are excluded (more than 130,000 km2), where are they in the budget? [Regine Hock, USA]	Those glaciers are not included in the budget because we don't have any reliable estimates of their changes. They appear for the time being in the uncertainty range. We may change our strategy in the final order draft
8400	4	23	4	23	10	E1a The footnotes do not appear in order (a-b-c-d-e) on the table [APECS Group Review, Germany]	accepted and corrected
11708	4	23	9	23	9	this contribution looks to be in the incorrect column. [John Church, Australia]	accepted and changed
18560	4	24	1			modeled [Christopher Fogwill, UK]	accepted and corrected
21284	4	24	9	25	37	The report discuss the differences in regional sea level changes and how this is different from the oceanic point ov view. To start with the changes on the coastal areas nd shelf seas is not onlysteric but also due to mass effects. It discusses the complication of changes due to regional variabilities like ENSO, PDO, NAO, etc. [Alejandro Souza, Mexico]	noted
23972	4	24	9			Refer to new knowledge since AR5 [Hans-Otto Poertner and WGII TSU, Germany]	noted
14586	4	24	11	24	11	The sea level does not [Christophe Deissenberg, Luxembourg]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14588	4	24	12	24	12	that the sea level [Christophe Deissenberg, Luxembourg]	accepted and corrected
4306	4	24	14	24	14	delete "Observations with" [Peter Clark, USA]	accepted and corrected
11710	4	24	17	24	18	Zhang and Church (GRL2012) should be added to these references. [John Church, Australia]	rejected. In this report we intend to present new knowledge sincethe IPCC AR5. Zhang and Church 2012 is anterior to AR5
4308	4	24	20	24	34	references needed throughout this paragraph [Peter Clark, USA]	accepted and corrected
4586	4	24	20	24	34	the whole paragraph comes without any reference… [Sönke Dangendorf, Germany]	accepted and corrected
8402	4	24	20	24	51	C1 These paragraphs are missing references [APECS Group Review, Germany]	accepted and corrected
23974	4	24	20	24	51	Need to include specific references. [Hans-Otto Poertner and WGII TSU, Germany]	accepted and corrected
4582	4	24	22	24	23	On shallow shelfs the steric sea level signal is almost zero and the mass signal is by far the dominating source of dynamic sea level changes (e.g. in the North Sea it explains ~95% of the variability). [Sönke Dangendorf, Germany]	accepted and changed
11712	4	24	23	24	25	There is a paper by Wu et al. (JGRC, 2018) that quantifies these terms for the Pacific and indian ocean basins. [John Church, Australia]	accepted and added
4584	4	24	27	24	27	I doubt that the common reader knows the term bouyancy forcing. I think it would help to intriduce the meaning of this process [Sönke Dangendorf, Germany]	accepted and corrected
13584	4	24	27	24	27	First use of acronym 'IPO'. Please spell out in full. [Debra Roberts and Durban Team, South Africa]	accepted and corrected
13586	4	24	27	24	27	What does 'NGPO' mean? It is used only once in this chapter [Debra Roberts and Durban Team, South Africa]	accepted and corrected
4310	4	24	36	24	51	references needed throughout this paragraph [Peter Clark, USA]	accepted and corrected
4588	4	24	36	24	51	Again as in the paragraph before there is no reference mentioned! Where does this knowledge stem from? Just to mention a few from my own major research topics: Along the eastern boundary there has been a lot effort on explaining coherent decadal scale variability along the continental slope (Calafat et al., 2012: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2012JC008285; Dangendorf et al., 2014: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2014JC009901; Frederikse et al., 2017: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL070750; etc.). Along the western boundary of the Atlantic wind forcing (Woodworth et al., 2015: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2014JC010520 ;Piecuch et al., 2016: https://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-16-0048.1) and the inverse barometer effect (Piecuch et al., 2015: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL064580) have been identified as dominant sources of variability on top of AMOC induced variability (e.g. Little et al., 2016 and references therein: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JC012713). This can be extended sentence by sentence in the two paragraphs… [Sönke Dangendorf, Germany]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
10620	4	24	36	24	51	There are no citations at all in this paragraph so I do not know the origin of the statement that “in the basin interior, surface heat fluxes are suggested to be the major force for the decadal sea level patterns due to AMOC variations.” , but I am not sure that it is correct. I think it coudl be said that "ocean heat transport variability due to changes in the AMOC results in decadal sea-level variability (with citation to McCarthy, G. D., et al., (2015). Ocean impact on decadal Atlantic climate variability revealed by sea-level observations. Nature, 521(7553), 508–510. http://doi.org/10.1038/nature14491). See also citations on p37 of chapter 6. [David Smeed, UK]	accepted and corrected
4590	4	24	53	25	22	What I am missing here is first of all one of the most challenging research questions on regional sea level, that is: How to estimate steric sea level along the coast? Steric sea level along the coast is largely dominated by an ocean bottom pressure signal stemming from steric sea level variations in the nearby deep ocean. Estimating this contribution is extremely difficult (Bingham and Hughes, 2012: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2011JC007276), but there has been some important progress on that (e.g. Frederikse et al., 2017: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL070750). Answering this question is critical to assessing the regional/local sea level budget from observations. This also brings me two my second major comment in this section. I personally think that the section is highly biased towards the (by no matter important) progress made in the ISSI climate model team. Where are, however, the studies from the recent years dealing with real observations (for instance the studies from Frederikse et al. in the North Sea, the Northeast American Shelf and the already cited assessment on basin scales in the global section)? Third I believe that the statement in Line 6 of page 25 is simply wrong. The inter-annual variability is certainly not well reproduced by climate models, which lack a proper resolution along the coast (and therefore the involved physics explained in the paragraphs before). Last but not least I think that there is over-confidence in the climate model simulations (high confidence as stated in the current version of the report), which are highly dependent on the numerous and partly very suggestive corrections of the models in the first half of the century. Giving high confidence to these simulations is in my opinion much to early at this stage given the serious limitations many of the models have. Therefore I would recommend rewording this to medium confidence... [Sönke Dangendorf, Germany]	partially accepted:As for the steric signal at the coast: we agree it is a difficult question and it is important for assessing the sea level budget locally on shelfs. Bingham and Hugues 2012 identified this question indeed. However they showed that in general the steric sea level off the shelf, in the depth range 500-1000m is also a good proxy of the steric sea level on the shelf and that the difference between both is small (except in boundary current regions). The difference in any case is small compared to other contributions to sea level at the coast that are still poorly known like the wave contribution or the circulation contribution. For this reason we did not put the emphasis on this. We prefer to focus on other terms that play a more important role on coastal sea level. Concerning Frederikse 2017, we don't see it as a progress on the question identified by Bingham and Hugues 2012 but rather an application on the North sea. As for the bias toward the progress made in the ISSI group, we acknowledge that the section on observations use lots of the results of the ISSI group on comparison between climate models and observations. It is not a personal bias of the authors here. The reason is structural. The primary objective of the IPCC chapter on sea level (as defined by the IPCC WGI and WG II chairs) is to provide information to people, stakeholders and policy makers on the risk, exposure and vulnerabilty to futur sea level rise. This information is build out from climate model projections. So the primary goal of the physical scientist in the IPCC sea level chapter is to provide reliable projections of future sea level from the literature. Most of the projections are based on climate models . The only way to get reliable projections from climate models is to validate climate models against observations. The consequence of this is that a large part of the "observation" section in the sea level chapter has to be devoted to the validation of climate models. To our knowledge there are only 3 publications that validated climate model simulations of the 20th century sea level rise against observations: Church et al. 2013 and the two publications from the ISSI group on climate models, i.e. Slangen et al; 2017 and
23276	4	25	0			The diagrams are not legible. [Y. Jeffrey Yang, USA]	accepted and changed
24632	4	25	0			small inset figures have no axes legends. [Hans-Otto Poertner and WGII TSU, Germany]	accepted and corrected

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17772	4	25	5	25	8	An alternative explanation for the too low sea level trends before 1950 is a lower climate sensitivity than assumed in the models. That would imply that natural forcings are stronger than assumed which would then also imply a higher level of natural forcing in the present. The character of the "correction" should be explained. [Hessel Voortman, Netherlands]	Partially accepted. The correction is explained in details in sections 4.2.2.3.3, 4.2.2.3.4 and 4.2.2.3.6. We clarify this in the regional sea level section now
8404	4	25	5	25	22	C1 These paragraphs are missing references [APECS Group Review, Germany]	accepted and corrected
23976	4	25	5	25	22	Need to include specific references [Hans-Otto Poertner and WGII TSU, Germany]	accepted and corrected
4312	4	25	6	25	8	where is this shown? [Peter Clark, USA]	accepted and corrected
8406	4	25	8	25	11	E1b This sentence is repeated from page 22 line 40 and I have the same suggestsions for it (see row 42 of this spreadsheet). [APECS Group Review, Germany]	rejected. The bias is detailed in section 4.2.2.3.3 and 4.2.2.3.4. We don't have room enough to come back on the definition of the "bias" here. Note that the "bias correction" we are using here is actually a time series derived from climate model runs that is corrected before the mid 20th century with a correction calibrated against reanalyses. As such this time series is different from the reanalyses time series. In this sense, we can not say we use "reanalyses instead of climate models. We believe that the term "Bias corrected" is more appropriate
4592	4	25	8			can essentially be explained (rather than is) [Sönke Dangendorf, Germany]	accepted and corrected
4314	4	25	10	25	10	is this what is shown on Fig. 4.4? [Peter Clark, USA]	yes. This is indicated now in the text
4594	4	25	10			leads [Sönke Dangendorf, Germany]	accepted and corrected
20858	4	25	14	25	15	groundwater depletion increases sea level rise so the sentence appears to be wrong [Paolo Cipollini, UK]	rejected.Groundwater depletion causes sea level rise in the far field but sea level drop in the near field because of the associated local decrease in geoid
17774	4	25	17	25	19	In view of previous comment, "high confidence" appears not to be justified [Hessel Voortman, Netherlands]	accepted and corrected
11852	4	25	17	25	22	the cautionary notes of Frederiksee et al GRL 2018 and Lickley et al J Climate 2018 should be included here given the prevaleence of comparing numerical model outputs to sea level reconstructions [King Matt, Australia]	accepted and corrected
11714	4	25	22	25	22	suggest "been fully tested so far." [John Church, Australia]	accepted and corrected
2100	4	25	23	25	27	The font-size is too small. [Josep Medina, Spain]	accepted and corrected
21630	4	25	23	25	35	Figure 4.3 -- this compares a local estimate of sea level (from a tide gauge) with a model estimate which I interpret as regional sea level. What are the implications of these different scales? [Robert Nicholls, UK]	At interannual to longer time scales the point wise measurement of tide gauge records represents actually the regional (regions of 50x50 to 500x500 km) sea level change around the tide gauge record. See for example Chepurin et al. 2014
21632	4	25	23	25	35	Caption should be comprehensive on sites corrected for subsidence -- Venice certainly should be added. [Robert Nicholls, UK]	accepted and corrected
314	4	25	24			units are missing on the y-axis of the time series plots (mm I suppose) [Kerstin Jochumsen, Germany]	accepted and corrected
13588	4	25	30	25	30	First use of acronym 'AOGCMs'. Please spell out in full. [Debra Roberts and Durban Team, South Africa]	accepted and corrected
316	4	25	34			The reference to Meyssignac et al. Does not fit. There are no tide gauges in this paper. [Kerstin Jochumsen, Germany]	accepted and corrected
3450	4	26	1	26	57	This is very interesting but not specific to small island, what would be the consequence for small island? [Mahmood Riyaz, Maldives]	consequences of individual cases are discussed in figure 4.9 and the case study box

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3048	4	26	1	27	49	The distinction between "regional" and "local" sea level is never clearly defined. [Robert Kopp, USA]	rejected this is explain at the start of 4.2
17776	4	26	1	27	49	As mentioned earlier. Considering the dominance of local circumstances on local sea level, a report with a global coverage such as this one can never provide sufficient detail. [Hessel Voortman, Netherlands]	we realize that local circumstance are important but we do offer as much information as possible to make the step from global climate models to local sea level. We spend quite some text to combining RSL and ESL for instance.
14592	4	26	3	26	3	Since the local coastal sea level [Christophe Deissenberg, Luxembourg]	accepted and rephrased
14594	4	26	4	26	4	At the coast, the sea level [Christophe Deissenberg, Luxembourg]	accepted and rephrased
16138	4	26	4	26	5	Missing wave setup in "At the coast, sea level change is additionally affected by wave 5 run up, tidal level, sea level pressure (SLP), ..." [Adi Nugraha, USA]	we defined in 4.2.1.7 wave run-up as the sum of wave set-up and swash hence by mentioning wave run-up we implicitly also mean wave set-up
20860	4	26	5	26	5	"Section 4.2.2.5" should be "Section 4.2.2.4" [Paolo Cipollini, UK]	as comment 318 wrong reference is corrected
23978	4	26	5	26	5	Cross-reference probably referring 4.2.2.4? [Hans-Otto Poertner and WGII TSU, Germany]	as comment 318 wrong reference is corrected
318	4	26	5			climate variability is not targeted in section 4.2.2.5 -> update reference [Kerstin Jochumsen, Germany]	accepted wrong reference corrected
4596	4	26	5			wind forcing is missing [Sönke Dangendorf, Germany]	accepted and added
16140	4	26	6	26	6	Need reference for mesoscale eddies causing sea level change [Adi Nugraha, USA]	references are provided in the section where they are discussed
21634	4	26	6			Emphasises subsidence which is correct but what about tectonic uplift, which is nothing to do with GIA -- as most textbooks on the subject will stress land can go up and down for several reasons. Important to acknowledge uplift on for example US West Coast where observed RSLR tends to be below global changes -- as far as I am aware for this reason. [Robert Nicholls, UK]	accepted and added
21636	4	26	7	25	8	as well as positive ectreme sea level, components can add up to negative extreme sea levels when we have spring low tides and high pressure for example and shipping sometimes runs aground as water depths fall below chart datum. It is right to focus on positive extreme sea level, but worth rembering it is part of a spectrum of water levles. [Robert Nicholls, UK]	rejected, although correct we think this caveat will interrupt the story line too much where we want to explain that RSL and ESL need to be combined.
3050	4	26	9	26	9	RSL is RELATIVE sea level [Robert Kopp, USA]	accepted and rephrased
20862	4	26	9	26	9	"trend in sea level (RSL)" is confusing - should repeat "relative sea level" [Paolo Cipollini, UK]	accepted as comment 3050
14596	4	26	12	26	12	(e.g., Section 4.2.2.3.6). Here [Christophe Deissenberg, Luxembourg]	accepted and changed
3052	4	26	14	26	14	It seems virtually certain that RSL increase will cause an increase in ESL. [Robert Kopp, USA]	rejected the sentence refers to a finding in AR5
3054	4	26	22	26	22	Delete "[quasi-]" -- no need to comment on their name [Robert Kopp, USA]	accepted and changed
3056	4	26	32	26	32	GCMs [Robert Kopp, USA]	accepted and changed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21644	4	26	36	27	1	This is a point here, which applies elsewhere in the chapter. Changes in waves and surges versus climate-induced sea-level rise. We are confident that sea-level rise is happening as a slow and accumulating process so while the change in any year is almost negligible if we look over a century the implications are profound. In contrast waves and surges are a product of the weather and hence have much more variability and even if we observe big changes, it is harder to be confident if we are seeing systematic change or just an expression of variability. A now rather old paper by Zhang et al (2000) showed the long timescales of surge climate variability based on the US East Coast sea-level dataset: you need more than 60 years of surge climate data to obtain stable statistics. Reference: Zhang et al. (2000) Twentieth-Century Storm Activity along the U.S. East Coast. Journal of Climate, 13, 1748-1761 [Robert Nicholls, UK]	rejected, although we agree with the reviewer we do not see an evident place to accommodate the comment
21638	4	26	36			Suggest adding "by definition". [Robert Nicholls, UK]	accepted and changed
3058	4	26	40	26	43	over what period? One during which RSL wasn't rising substantially in the Pacific Northwest? [Robert Kopp, USA]	accepted time indication added
21640	4	26	41	26	43	Unclear that the Ruggiero (2012) based sentence is based on observations. What time period are these observations based upon? [Robert Nicholls, UK]	accepted time indication added
14598	4	26	42	26	42	erosion than RSL (Ruggiero [Christophe Deissenberg, Luxembourg]	accepted and rephrased
3448	4	26	43	26	43	"all the other regions" please specify [Mahmood Riyaz, Maldives]	rejected the sentence reads as confirmation of the previous sentence in a genera sense in other regions
16142	4	26	43	26	43	This is also true in other regions (Melet et al., 2016; Melet et al., 2018). Which region disccused in that sentence? [Adi Nugraha, USA]	accepted and rephrased
21642	4	26	43			other regions -- give one or two examples. [Robert Nicholls, UK]	accepted and rephrased
21654	4	26	46			"flooding risk" -- did Wahl et al (2015) assess the return periods of water levels or did he consider the societal consequences? [Robert Nicholls, UK]	accepted and replaces flooding risk once to clarify that high water level is meant
8408	4	26	55	26	57	C2 Need a citation for why these findings are typical for the island type [APECS Group Review, Germany]	rejected the citation is already mentioned Hoeke et al. 2015
14600	4	26	56	26	56	that are common in the [Christophe Deissenberg, Luxembourg]	accepted and rephrased
21656	4	27	1			New relevant reference on flooding in the Maldives due to energetic swell waves -- M. Wadey et al (2017). Reference: Wadey, M., Brown, S., Nicholls, R., & Haigh, I. (2017). Coastal flooding in the Maldives: an assessment of historic events and their implications. Natural Hazards, 89(1), 131–159. DOI: 10.1007/s11069-017-2957-5 [Robert Nicholls, UK]	accepted and added
10792	4	27	9	27	9	the term 'subsidence' includes both long-term isostatic, geological subsidence from the weight of overlying sediments and shorter-term accelearted subsidence both from the compaction of sediments (both natural and anthropogenically-accelerated) but also the anthropogenic removal of fluids. Both sets of processes occur in all deltas but to differeing degrees. This distinction should be made in the text. [Thomas Spencer, UK]	accepted and expanded in the text
14602	4	27	9	27	9	In deltas, the local sea level can be dominated by subsidence more than by above processes. [Christophe Deissenberg, Luxembourg]	accepted and added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21658	4	27	9	27	15	This paragraph needs to refer to the Syvitski et al (2009) paper referenced in the next paragraph as this paper established the issues being discussed. [Robert Nicholls, UK]	accepted and added
14604	4	27	13	27	15	It is estimated, for example, that globally one in fourteen human resides in mid to-low latitude deltas (Day et al., 2016). Although in these areas RSL is dominated by subsidence, climate effects need to be included for estimating risks. [Christophe Deissenberg, Luxembourg]	accepted and added
10794	4	27	17	27	27	see point above. This paragraph could be improved by better identification of the different subsidence terms and how they are affected by human interventions [Thomas Spencer, UK]	accepted and solved with comment 10792
14606	4	27	18	27	18	It is the compaction which causes a drop in land [Christophe Deissenberg, Luxembourg]	accepted and rephrased
8410	4	27	27	27	27	Authors need to mention and discuss regarding rate of sedimentation as more sediment influx over burden the deltaic system and the land subsidence increases. (Karpytchev et al., 2018) [APECS Group Review, Germany]	accepted and included
2102	4	27	29	27	37	Esteban et al. (2018) indicated subsidence ratios in Yakarta (along the coast) from 95 to 215 mm/year. [Josep Medina, Spain]	willing to include but seems conference paper
2104	4	27	29	27	37	Esteban, M., Takagi, H., Jamero, L., Thao, N.D., Mikami, T., Onuhi, M., Yamamoto, L. and Chadwick, C. (2018). Adaptation to sea level rise in cities: Lessons from present examples of land subsidence. Proc. Coasts, Marine Structures and Breakwaters 2017, Liverpool, 5-7 September 2017, ICE Publishing (in press). [Josep Medina, Spain]	We could not locate this publication.
14082	4	27	29	27	41	Addressing ground subsidence with its connection to risk of flooding in Dakha (population about 7 million) and Jakarta with population almost 12 million people would also be valuable to be more explored in this chapter. It is reported that the Jakarta floods may have been compounded by land-use change via urban development and associated land subsidence. These types of mechanical factors reemphasize the various pathways beyond climate change by which human activity can increase regional risk of extreme events. (see Siswanto et al, 2015 BAMS https://doi.org/10.1175/BAMS-D-15-00128.1) [Siswanto Siswanto, Indonesia]	rejected, although we are willing to add Jakarta as an example of subsidence the suggested reference addresses flood risk due to rainfall events which is not the topic of this paragraph
3060	4	27	30	27	32	This sentence seems to be confusing subsidence and net accumulation. [Robert Kopp, USA]	accepted should be decrease rather than increase
21660	4	27	30	27	32	This statement supported by the reference to Tessler et al (2018) must be wrong. The cities are not experiencing sedimenation today so less sediment will have no effect. Note that a city that was experiencing sedimenation would be progressively submerged in sediment. Increases subsidence reflects dewatering, drainage and groundwater withdrawal. [Robert Nicholls, UK]	the sentence is rephrased to explain the Tessler refeference better
3062	4	27	35	27	35	I wouldn't expect New Orleans to quaify as a mega-city. [Robert Kopp, USA]	rejected the authors of the reference do so
14608	4	27	46	27	47	level changes, as in some cases they dominate the large-scale sea level rise patterns. Although erosion, sedimentation and compaction may be very large locally, they are not accounted for in the projection [Christophe Deissenberg, Luxembourg]	accepted and rephrased
8412	4	27	46	27	49	E1a This sentence is awkward and should be rewritten, probably split into two sentences: 1. Erosion etc. can be locally large, but are not currently included. 2. Their omission is due to the current lack of a global dataset for these local variables. [APECS Group Review, Germany]	accepted and rephrased

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
320	4	27	48			global data sets are [Kerstin Jochumsen, Germany]	accepted and changed
4598	4	27	51	30	4	I think this entire section needs to be slightly reworded. It is called "Detection and Attribution" but the word detection only appears in two of the subheadings but never in the main text. The two terms "detection" and "attriibution" are in my opinion not well seperated here (although there is a very clear definition in the literature including the IPCC reports) and often mixed. Essentially everything stated in these paragraphs (also pure detection studies) is termed attribution.... [Sönke Dangendorf, Germany]	Taken into account: we changed the heading of sect. 4.2.2.6.2 and revised the text of the entire sect. 4.2.2.6 to more carefully distinguish between detection and attribution. We also added the definition of detection to the introduction, in order to emphasize that there is a difference.
1400	4	28	1			is possible in numerical and statistical models [Ali Mahmood, Iraq]	rejected: also statistical models are numerical
1402	4	28	3			Statistically, Mahmood (2016) detect the contribution of the North Atlantic Ocean to the related configured linear trend in the river discharges of Northern Europe and coastal mean sea levels of the Baltic Sea and North Sea in winter condition by using the standard ordinary univariate linear regression method. The formal regressions could be estimated by using the first-degree autoregressive AR (1) model, which includes calculations of the GLS-Generalized Least Square Error Minimization regression method, for obtaining more stable solutions in the context of global warming. Where, the autocorrelation error in this case arose from natural climate variability. Furthermore, the nonparametric Theil-Sen method could be applied for satisfying the exact trend fitting in the case of full probability distribution. Thus, autocorrelation error in this case represents the more extreme climate variability. [Ali Mahmood, Iraq]	rejected: outside of scope of the section, which focusses on the detection of a change, and/or the attribution of that change to specific climate forcing. The introductory text was revised to clarify.
24634	4	28	9			detection and attribution should distiguish WGI and WGII approaches as well as their potential integration. Th term impact should be exclusively used for effects on vulnerable systems in order avoid confusion between WGI and WGII uses. [Hans-Otto Poertner and WGII TSU, Germany]	taken into account: in the introduction to the section, we now define our use of both "detection" and attribution". We changed the word "impact" to effect.
3064	4	28	18	28	20	This sentence is unintelligible. [Robert Kopp, USA]	accepted: sentence shortened
14610	4	28	19	28	19	and found that during [Christophe Deissenberg, Luxembourg]	accepted: text revised
3772	4	28	20	28	20	remove "(" before Slangen [Ola Kalen, Sweden]	copyedit to be completed prior to publication
14612	4	28	20	28	21	(Slangen et al., 2014c) included the full ocean depth in their analysis and quantified the impact [Christophe Deissenberg, Luxembourg]	accepted
1466	4	28	34	28	42	Attribution of glacier changes: overlap with sections from chapter 2 (2.2.3.3) and chapter 3 (3.2.2.4): make sure this is consistent and consider ommiting one of these and refer to other chapters. For consistency, in the two other chapters the important study by Roe et al. (2017, Nature Geoscience, doi: □10.1038/ngeo2863) is mentioned, which is not the case here. [Harry Zekollari, Switzerland]	taken into account: we refer to Ch 2 and 3,we ensured consistency and reduced overlap. Specifically, studies addressing a small fraction of all glaciers (like Roe et al. 2017) are more appropriately addressed in Ch 2 and 3, while Ch 4 needs to take a more integrative approach.
17778	4	28	36	28	39	Conclusion is valid only under the assumption that natural forcings are small in the first place. Conclusion appears to lack general validity [Hessel Voortman, Netherlands]	rejected: natural forcings are considered specifically in the cited reference, their effect is thus not based on an assumption. Furthermore, since a large fraction of the total mass loss during the 20th century was attributed to variability preceding substantial anthropogenic forcing, they cannot be considered small.
8414	4	28	41	28	42	Please cite a reference. [APECS Group Review, Germany]	Rejected: the "they concluded" at the beginning of the sentence refers to the correct reference mentioned in the previous sentence, which is Marzeion et al. (2014).

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1468	4	28	43	28	43	"For ice sheet mass loss..": would suggest starting a new paragraph here, as this does not directly relate to previous part. This would also nicely split up section 4.2.2.6.1 into: (1) Thermosteric sea level rise, (2) glaciers, (3) Ice sheets + rest (groundwater) [Harry Zekollari, Switzerland]	accepted
23980	4	28	55	28	55	Likelihood statements in italics [Hans-Otto Poertner and WGII TSU, Germany]	accepted
3066	4	29	5	29	8	See the correction to Kopp et al. (2017). It is extremely that at least 41% (and most likely 56-97%) of observed GMSL rise is attributable to 20th century warming. Also, it is likely that GMSL emerged from the natural background by 1940, and extremely likely that it had done so by 1970. [Robert Kopp, USA]	accepted: text revised to the corrected number, and statement about emergence of the sea level from background variability added
8416	4	29	6	29	6	E1a Big red flag on the phrase "counter-factual" -- I would strongly caution against stating that any scientific work incorporated "counter-factual" information. Instead perhaps "test scenarios" or something more benign. [APECS Group Review, Germany]	accepted: this part of the sentence was removed while addressing comment 3066
11934	4	29	8		9	check staement for proper wording. Rephrase sentence. [Chukwuma Anoruo, Nigeria]	taken into account: this sentence was revised addressing comment 3066
8418	4	29	10	29	13	E1a This description sounds very much like that of Slangen et al., 2014 (p. 28 line 20). I suggest rewriting / shortening one or the other to make it read less repetitively. [APECS Group Review, Germany]	accepted: the sentences was rephrased and shortened.
17160	4	29	17	29	20	Might be wort menstioning that the explained percentage increases for individual reconstructions [Aimee Slangen, Netherlands]	rejected: while we agree that this is the case, we instead point to a new estimate of sea level contribution from glaciers that have either melted within the 20th century or are missing from the global glacier inventory (Parkes & Marzeion, accepted in Nature). This contribution has the potential to close the budget for any of the sea level time series considered.
3068	4	29	19	29	20	Is 24% the "minor fraction" referred to? [Robert Kopp, USA]	taken into account: This would have been the 26 % "missing" when 74 % are attributable. However, we deleted this sentence and instead point to a new estimate of sea level contribution from glaciers that have either melted within the 20th century or are missing from the global glacier inventory (Parkes & Marzeion, accepted in Nature). This contribution has the potential to close the budget for any of the sea level time series considered.
3070	4	29	27	29	28	I don't think this is a justifiable conclusion, because sea-level rise did not start in 1990. The onset of accelerated rates of GMSL rise is apparent in comparing 20th century rates to earlier Common Era rates (e.g., Kopp et al, 2016; Gehrels and Woodworth, 2013) and given the identification of a global mean sea level signal, attribution of a portion seems justified. Certainly, the detection task for GMSL has been accomplished by Kopp et al 2016, and would not be a great leap to extend that work to regional and local mean sea level change. [Robert Kopp, USA]	taken into account: the statement does not refer to the question whether a fraction of regional sea level rise is attributtable to global sea level rise (in that case, we agree with the reviewer), but whether a fraction of regional sea level rise is attributable to specific radiative forcing of the climate system. This attribution is complicated by (mostly, but not exclusively) internal variability - not just of the global mean signal, but also of dynamic effects. We also agree that detection of a regional change in sea level is a simpler task, but not its causal attribution. To clarify the difference between detection and attribution, we added a definition of detection to the introductory text of the section.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14614	4	29	27	29	31	Since the sea level variability is larger at the regional scale than at the global scale, the attribution of observed regional sea level change to specific climate forcings is most challenging. For example, Palanisamy et al. (2015b) show explicitly that the anthropogenic fingerprint of sea level rise predicted by CMIP5 models for the Pacific Ocean is too small, compared to the level of internal variability, to be detected in altimetric observations in the considered region mostly associated with the Pacific Decadal. [Christophe Deissenberg, Luxembourg]	accepted: text revised closely along the suggested lines.
8420	4	29	31	29	31	E1a Missing word "Pacific Decadal OSCILLATION" [APECS Group Review, Germany]	accepted
13590	4	29	40	29	40	Where also Bilbao et al. (2015) predict the earliest detectability of an anthropogenic trend' The point is not clear. Earlier than 2020? It will be more helpful to specify the year as were the case for the preceding projections. [Debra Roberts and Durban Team, South Africa]	accepted: text revised to clarify that we refer to the same region, not the same (or an earlier) time.
14616	4	29	40	29	40	which according to Bilbao et al. (2015) is the place where one should be first able to detect an anthropogenic trend. [Christophe Deissenberg, Luxembourg]	taken into account: text was revised addressing comment 13590
3316	4	29	47	29	48	Of course it is true but it is a pity because those are the scales of greatest interest. [Castor Muñoz Sobrino, Spain]	accepted, no text revision
11716	4	29	47	29	48	I think this statemen is incorrect. The regional sea level change is dominated by the global mean signal over multidecadal periods. And thus I think it is incorrect to state that regional attribution is not yet possible (although it might not have been done specifically). What I suspect is meant here is that it is not yet possible to attribute the regional departutures of sea level from the global mean sea level change. [John Church, Australia]	taken into account: There are numerous studies (cited in the preceding two paragraphs) that investigate when the anthropogenic signal will become detectable on regional scales. Attributing such change to specific anthropogenic forcing is even more demanding, since it requires, additionally to observations adequate to the considered spatial scale, climate model reconstructions that are reliable in representing regional scale sea level variability. We don't think it is possible to do this (but would welcome studies that prove us wrong). We revised the sentence to clarify that we mean attribution to anthropogenic forcing, and added a definition of detection to the introductory text of the section to clarify the difference to attribution.
3072	4	29	50	30	4	Again, what about approaches based on attribution of sea-level change? [Robert Kopp, USA]	taken into account: if we understand correctly, the reviewer suggests attribution of extreme events to sea level change. In this case, we agree that this is possible, and we introduced a sentence to the beginning of the section to point this out. It does, however, not transfer to the attribution to anthropogenic forcing, which is the issue here. We also revised the section title to put more emphasis on the attribution issue.
23982	4	29	50			Suggest to add cross-reference chapter 6 [Hans-Otto Poertner and WGII TSU, Germany]	accepted
8422	4	30	1	30	1	E1a "the atmosphere WOULD LEAD to..." [APECS Group Review, Germany]	accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
19088	4	30	6			The focus of projections of future sea level is still on 2100 even though it is long clear that this time frame is insufficient to evaluate the imprint of emission pathways on SLR trajectories. The "Long term" projection section is combining information from century to multi-millennial time scales, thereby obscuring rather than clarifying the picture. However, 2300 represents a meaningful time scale to inform on SLR as a result on mitigation pathways (e.g. Schaeffer et al 2010, Horton et al. 2014, Yin 2012) and allows to clearly discriminate e.g. 1.5°C and 2°C pathways (Mengel et al. 2018). In fact, Mengel et al. 2018 have shown that even small near term differences in emission pathways (i.e. 5 year delay in peaking of global CO2 emissions) will contribute significantly to SLR on centennial time scales. They have also quantified the effects of a potential temperature overshoot above 1.5°C for SLR, a highly policy relevant information. There is more literature coming available looking beyond 2100 (e.g. Rasmussen et al. 2018) and it seems appropriate to reflect this in this chapter more prominently. [Carl-Friedrich Schleussner, Germany]	We address the long-term in the synthesis section of 4.2
4316	4	30	8	30	8	move "mean" to after "global" [Peter Clark, USA]	taken into account: revised to "As a consequence of climate change the global and the regional mean sea level will change" since also the temporal mean is implied here.
8424	4	30	8	30	8	E1a remove "we use" at the end of this line [APECS Group Review, Germany]	accepted
14618	4	30	8	30	8	As a consequence of climate change the global and the regional mean sea level will change. [Christophe Deissenberg, Luxembourg]	accepted
17162	4	30	8	30	9	repetition of 'use' and 'are used', and I don't understand the use of 'hence'. Sea level projections cannot be made wih AOGCMs only [Aimee Slangen, Netherlands]	taken into account: "Hence" was deleted, and the subsequent sentences address that AOGCMs alone cannot be used to produce complete projections.
17440	4	30	8	30	9	This sentence, beginning "Hence,we use..." seems out of place, and repeats information written more carefully later. I suggest deleting this sentence. [Sonya Legg, USA]	the abbreviation is now introduced in para 4.2.2.3
14620	4	30	8	30	12	Hence, Atmosphere Ocean General Circulation Models (AOGCMs) are used to make projections of the climate changes and the associated sea level rise. AOGCMs can be applied on century time scales to provide estimates of the steric (temperature and salinity effects on sea water density) and ocean dynamical (ocean circulation) components of sea level change, both globally and regionally. [Christophe Deissenberg, Luxembourg]	taken into account: the abbreviation AOGCM was previously used, the definition is therefore not repeated here.
8426	4	30	9	30	9	Please add "which" before "are used" [APECS Group Review, Germany]	are used has been removed
13592	4	30	9	30	9	Delete 'are used' [Debra Roberts and Durban Team, South Africa]	accepted and added
3774	4	30	9	30	10	Explanation to abbreviation AOGCM should move to line 9 [Ola Kalen, Sweden]	the abbreviation is now introduced in para 4.2.2.3
22256	4	30	9	30	10	AOGCM is used on line 9 before it is defined in Line 10 [Andra Garner, USA]	the abbreviation is now introduced in para 4.2.2.3
23984	4	30	9	30	10	Spell out acronym AOGCM the first time used on the section, then abbreviate [Hans-Otto Poertner and WGII TSU, Germany]	the abbreviation is now introduced in para 4.2.2.3
14622	4	30	18	30	19	produced for AR5 are used to provide information on expected changes in the oceans and on the evolution of glaciers and ice sheets. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
17442	4	30	20	30	20	Insert the word "necessary" after "small scales" [Sonya Legg, USA]	accepted and added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8428	4	30	21	30	22	C3 The description of MICI and other ice dynamics processes as "poorly understood" is consistently too strong within this section. I haven't asked for access to Ch. 3, but I would wager that our understanding of glacier flow and our ability to predit it is rated with "medium confidence" or better. Here, I could suggest "incompletely implemented" instead of poorly understood. [APECS Group Review, Germany]	There are still serious issues in the physics prohibiting ice sheets to be part of AOGCM models. So we believe that this wording is justified. Chapter 3 and section on ice physics in chapter 4 elaborate in more detail on this.
22258	4	30	22	30	26	Reference? [Andra Garner, USA]	references are provided in line 29
2740	4	30	22	30	29	Donat-Magnin et al. (2017) have brought evidence that omitting ocean/ice-sheet coupling can significantly affect projections of coastal warming in the Amundsen Sea, Antarctica. The important ascpects are (1) the ocean circulation induced by ice-shelf melt affects the response of ocean circulation and temperatures to changing winds, and (2) changes in the ocean circulation are affected by the changing shape of ice shelves. Donat-Magnin, M., Jourdain, N. C., Spence, P., Le Sommer, J., Gallée, H., & Durand, G. (2017). Ice-Shelf Melt Response to Changing Winds and Glacier Dynamics in the Amundsen Sea Sector, Antarctica. Journal of Geophysical Research: Oceans. [Nicolas Jourdain, France]	The reference is added to the general statement that ice shelf and ocean circulation need to be coupled
4318	4	30	24	30	24	temporal should be spatial? [Peter Clark, USA]	accepted and corrected
8430	4	30	25	30	26	E1a I suggest replacing "Particularly problematic" with "Notably lacking" and add "in most models" to the end of the sentence [APECS Group Review, Germany]	accepted and rephrased
8432	4	30	26	30	26	E1a I suggest "This deficiency LIMITS adequate representation..." -- not prohibits, that is too strong, as some models are weakly coupled [APECS Group Review, Germany]	accepted and rephrased
8434	4	30	29	30	30	C1 Need citation here. I'd also suggest specifying "lack of DIRECT observations" as we have a number of indirect (e.g., seismic surveys) observations of till characteristics in fast-flowing areas -- e.g., Kulessa et al. (2017) doi:10.1126/sciadv.1603071 [APECS Group Review, Germany]	accepted and rephrased
12582	4	30	30	8	9	It is true that AOGCM are used to make projections but this sentence is misleading because land ice which Is the main contributor to sea level rise are not in AOGCM. [Dewi Le Bars, Netherlands]	We fully agree and have strengthened this point at the start of the paragraph
8436	4	30	30	30	32	E1a Do you mean that new ice-sheet model RUNS have been recently generated, or that many models have improved by adding processes or resolution? I am less aware of "many new" ice-sheet models that have come online since AR5 (I can maybe think of one). [APECS Group Review, Germany]	maybe many was too strong it has been changed to several
22260	4	30	32	30	33	Additional references seem like they might be relevant here, including Kopp et al., 2014 and 2017, Bakker et al., 2017 [Andra Garner, USA]	we have added Kopp et al. 2014, we agree that there are many more
11936	4	30	36		37	provide evidence with literatute [Chukwuma Anoruo, Nigeria]	this is a general description of the process of calculating RSL not referring to any GIA model in particular so a reference is not needed here in the introduction
3074	4	30	37	30	38	Terminology needs to be cleaned up here. The short-term "fingerprint" effects include an elastic lithospheric deformation component, not just gravitational and rotation. GIA also incorporates a viscoelastic mantle response. [Robert Kopp, USA]	We added short-term and long-term to clarify

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14624	4	30	43	30	45	At the local spatial scales of specific cities, islands, and stretches of coastlines, hydrodynamical models (Section 4.2.3.3) and knowledge on sedimentation and erosion are necessary to analyse the impacts of highly variable processes leading to ESL, such as tropical cyclone-driven storm surges. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
8438	4	30	45	30	45	E1a Replae "These models" with "Hydrodynamical models" [APECS Group Review, Germany]	accepted
13594	4	30	48	30	48	Delete 'and' before 'wind regime' [Debra Roberts and Durban Team, South Africa]	accepted
4320	4	30	51	30	54	paragraph is awkwardly written [Peter Clark, USA]	accepted and rephrased along the line of comment 14526
14626	4	30	51	30	54	In summary, climate models play an important role at the various stages of projections in providing, together with emission scenarios, geodynamic, ice dynamic, and hydrodynamic models, the required information for hazard estimation for coasts and low-lying islands. In this report we rely on results of the CMIP5 model runs. [Christophe Deissenberg, Luxembourg]	accepted
17444	4	30	53	30	54	This sentence, beginning " In this report" repeats information given earlier: delete. [Sonya Legg, USA]	true, but we believe a re-emphasizing this in a summary statement is important to set the scene for what follows.
14632	4	31	0	31		FIGURE: THE THIN BLACK LINE IS MORE OR LESS INVISIBLE! [Christophe Deissenberg, Luxembourg]	This figure has been redrafted for clarity.
13596	4	31	0			Fig 4.5 please provide lat/long on grid [Debra Roberts and Durban Team, South Africa]	This figure has been redrafted for clarity.
2742	4	31	1	32	28	There are other important sources of uncertainties for the dynamics of the Greenland ice sheet. While it is possible to obtain basal friction parameters that reproduce the present-day observed dynamics, it is currently not or poorly linked to surface melting conditions. In a future World with extensive surface melting in Greenland, basal friction may significantly change. The ice-sheet models therefore need to incorporate hydrology models to provide better projections. See ,e.g., Koziol, C. P., and Arnold, N. (2018). Modelling seasonal meltwater forcing of the velocity of land-terminating margins of the Greenland Ice Sheet. The Cryosphere, 12(3), 971. Another strong limitation is that CMIP5 models do not resolve the relatively fjords in which Greenland glaciers terminate, which makes it difficult to estimate the ocean forcing for these ice sheet models. Finally, calving is heavily parameterized (when not simply constrained to keep the present-day ice front), and it is unclear how this will be affected by changing ocean and atmosphere. See review in Goelzer, Heiko, et al. "Design and results of the ice sheet model initialisation experiments initMIP-Greenland: an ISMIP6 intercomparison." The Cryosphere 12.4 (2018): 1433. [Nicolas Jourdain, France]	The chapter team acknowledges these additional sources of uncertainty in Greenland's future response to warming, in addition to the SMB uncertainty empasized in the FOD. The text has been modified to reflect the importance of basal hydrology, particularly in short term variability, and fjord-scale ocean forcing, which remains under-resolved in GCMs. References have been added where appropriate, mainly in later sections of text. Note that most text associated with observed changes in the Greenland and Antarctic Ice Sheets.
10716	4	31	6	31	6	The Greenland Ice Sheet (GIS) term is very confusing, because is similar to Geographic Information Systems (GIS). Please find other abbreviation. [Oxana Lipka, Russian Federation]	The chapter 4 and chapter 3 author teams prefer to use GIS consistently throughout SROCC.
1470	4	31	6	31	7	Additional reference to Table 4.1 here? [Harry Zekollari, Switzerland]	This is a good suggestion. Table 1 is now referenced at the appropriate loctation in the text. Note that most test related to current trends in Greenland and Antarctic ice mass are now in Chapter 3.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
17826	4	31	6	31	7	Check this in the light of Antarctic results from the IMBIE team (Nature, 2018) and update if necessary. [Robert Arthern, UK]	The Antarctic compoent in the revised sea-level budet is consistent with IMBIE2 as is the comparison between Greenland and Antarctica.
2356	4	31	6	31	14	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Kristin Campbell, USA]	Feedbacks associated with changes in the physical characteristics of firn are now mentioned, as recommended by the reviewer. References to Tedesco et al. (2016), Noel et al. (2017), and Machguth (2016) have been added. Note that an assessment of recent Greenland mass change is provided in Chapter 3.
2482	4	31	6	31	14	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Durwood Zaelke, USA]	Same as comment 2356
12980	4	31	6	31	14	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Gabrielle Dreyfus, USA]	same as comment 2356
4322	4	31	7	31	7	add reference to Bamber et al. (2018, ERL) [Peter Clark, USA]	The chapter team agrees. This reference has been added. Note that the assessment of observed GIS mass change has been moved to chapter 3.
8440	4	31	8	31	11	E1a The 60% figure is from van den Broeke; the other two references are not quite right for this sentence. You could add a following sentence along the lines of "Ice dynamics, which currently comprises the remaining 40% of the mass imbalance, is highly variable in space and time (Csatho, Enderlin)" if you wanted to retain those references. [APECS Group Review, Germany]	This is a good suggestion. The text has been reworded.
8442	4	31	9	31	9	E1a The word "dominate" is a bit strong for something that is split 60-40%. Perhaps "is primarily driven by" [APECS Group Review, Germany]	This is a good suggestion. The text has been reworded.
14628	4	31	9	31	10	dominated (60%) by an increasingly negative surface mass balance (SMB) caused by surface melt and runoff on the lower elevations of the ice sheet's margins, rather than by ice dynamical changes (Csatho et al., 2014; [Christophe Deissenberg, Luxembourg]	This text has been slightly modified (see comments 8440. 8442)
18562	4	31	11			modeling. Need to check throughout report for consistency between modelled/modeled and modelling/modeling, as both are used frequently [Christopher Fogwill, UK]	The author team will check for consistancy.
8444	4	31	14	31	14	E1a Less important than what? (than in Antarctica or than SMB -- which do you mean here? If the latter, then this sentence would be redundant and unnecessary.) [APECS Group Review, Germany]	The author team meant the latter. This sentence has is indeed redundant and has been removed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8446	4	31	14	31	16	C1 It's simply not true that only a "limited volume of ice" has "direct access to the ocean" in Greenland -- the marine-based NEGIS (16% of ice sheet area), Jakobshavn (8%), and Petermann (4%) glacier catchments all hold non-trivial fractions of Greenland's ice. (Note that these are areal fractions, not volume, which I don't have readily available.). It is true that Antarctica is comparatively more marine-based, so I take no issue with Figure 4.5. You might say instead of "limited" here, "lesser volume compared to WAIS". [APECS Group Review, Germany]	The intentaion was to compare the ice (cross-sectional gates) on Greenland versus Antarctica in direct contact with the ocean, not the fractional area of drainage basins with outlets reaching the ocean. This wording has been modified to improve clarity.
8448	4	31	16	31	17	C4 The fraction of the coast that is above sea level does not matter -- it's the fraction of the marine interior that is accessible to rapid retreat through a submarine flux gate (e.g., Jakobshavn, Petermann, or NEGIS outlet glaciers). [APECS Group Review, Germany]	See comment 8446. The text has been modified to add claritiy.
14630	4	31	17	31	19	The opposite condition exists in Antarctica, where the subglacial bedrock slopes downward away from the coast (reverse-sloped). The glacial ice is then susceptible to dynamical instabilities that can contribute rapid ice loss to the ocean. [Christophe Deissenberg, Luxembourg]	Good suggestion. This text has been slightly modified for clarity.
20748	4	31	18	31	18	the glacial ice 'may be' susceptible [Tamsin Edwards, UK]	Agreed. This text has been changed.
23986	4	31	21	31	25	The black line in the figure is difficult to recognize. Suggest to increase thickness or change colour. [Hans-Otto Poertner and WGII TSU, Germany]	This figure has been redrafted for clarity.
8450	4	31	22	31	22	Scale bars in each panel would be helpful and straightforward to include and would better highlight the "Mind that" note in the caption (p. 31 l. 26) [APECS Group Review, Germany]	Agreed. This figure has been redrafted.
8452	4	31	22	31	22	Suggest changing colorbar label to "Bedrock elevation (meters above sea level)" or m a.s.l. [APECS Group Review, Germany]	This figure has been redrafted for clarity.
3776	4	31	30	31	33	In is not described in the text for what timeframe the results from Furst are valid [Ola Kalen, Sweden]	The timeframe has been added.
8454	4	31	30	31	33	E1a It should be explicitly stated somewhere in this summary that variations in the SMB (AOGCM output), not uncertainties in ice dynamics or rapid variations in ice dynamics, that control the range of sea-level contributions reported by Furst. [APECS Group Review, Germany]	Agreed. This point is made in the summary paragraph of this section, with reference to Furst et al., 2016) in addition to Edwards et al., (2014).
17614	4	31	30	32	27	The section on GrIS projections is OK but it does not discuss the fact that none of the projections reproduce the trends seen over the last ~15 years, including the increase in SAT, such that the contribution from the GrIS averaged over the last decade is 0.8 mm/yr. With no increase in temperature at all this would imply 8 cm for 2000-2100. All climate model simulations produce an increase in temp over Greenland that should increase mass loss. There is a debate about whether recent (last ~2 decade) increase in SAT is due to external forcing or internal variability but it would be useful to discuss these issues somewhere. [Jonathan Bamber, UK]	This is an excellent, substantive recommendation. The discusion of recent behaviour of Antarctic and Greenland ice sheets has been moved to Chapter 3. The point that models generally fail to reproduce current trends is important for the dicussion on projections and some text has been added to reflect this.
12584	4	31	31	39	39	This range of 7 to 21 cm is for the period 2080-2100 not for 2100. See AR5 13.SM.1, the range for 2100 is 9-28. [Dewi Le Bars, Netherlands]	The referenced time period has been added.
17612	4	31	33	31	33	numbers in brackets not defined [Jonathan Bamber, UK]	Agreed. This has been fixed.
11938	4	31	33			add cm to 5.1 for clear comparison and subsequently. See line 39,43 etc [Chukwuma Anoruo, Nigeria]	Agreed. This has been fixed.
8456	4	31	35	31	36	E1b Shannon et al (2013) is a bit of a non sequitur here and is not new since AR5; suggest removing this sentence [APECS Group Review, Germany]	Agreed. This has been fixed.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8458	4	31	41	31	42	E1a Sentence structure -- "Modeling... reported a [result]" is incorrect; modeling cannot report anything, only a human or a study can. [APECS Group Review, Germany]	Agreed. This has been fixed.
3778	4	31	41	32	1	In is not described in the text for what timeframe the results from Nick are valid [Ola Kalen, Sweden]	This test has been modified to inprove clarity in the referenced time periods.
8460	4	31	41	32	13	C3 This paragraph (and consequently the entire section) buries the uncertainties associated with Greenland's marine-based glaciers. The studies that found relatively small ice dynamical contributions (Nick, Furst, etc.) used a coarser basal topography map (Bamber 2013) than is currently standard (Morlighem). As highlighted here on p. 32 lines 6-10, use of the Morlighem bed in similar model experiments could well change the conclusions. This uncertainty needs to be brought to the forefront. [APECS Group Review, Germany]	The author team agrees with the reviewer here. This issue is highlighted later in the document, and does not need to be repeated here.
14634	4	32	4	32	4	suggests that Greenland's [Christophe Deissenberg, Luxembourg]	Agreed. This text has been modified.
20750	4	32	5	32	5	I think 'topographically' is not quite right. As I understand it, part of the decrease is due to SMB changes removing the ice 'before' it can be discharged. You somewhat imply this in the thinnning described earlier in this sentence, but I think adding 'topographically' Is confusing. [Tamsin Edwards, UK]	Good suggestion. This text has been slightly modified for clarity. "topographically" was indeed confusing.
4324	4	32	15	32	15	"guidance" not best word - how about "results"? [Peter Clark, USA]	This wording has been changed.
2358	4	32	15	32	27	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Kristin Campbell, USA]	Same as comment 2356
2484	4	32	15	32	27	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Durwood Zaelke, USA]	Same as comment 2356
12982	4	32	15	32	27	Surface runoff can also hinder firn's ability to store water, which can lead to additional runoff reaching the ocean. (Tedesco M., et al. (2016) Arctic cut-off high drives the poleward shift of a new Greenland melting record, NATURE COMMUNICATIONS 7(11723):1–6; Noël B., et al. (2017) A tipping point in refreezing accelerates mass loss of Greenland's glaciers and ice caps, NATURE COMMUNICATIONS 8(14730):1–8; Machguth H., et al. (2016) Greenland meltwater storage in firn limited by near-surface ice formation, NATURE CLIMATE CHANGE 6:390–393.) [Gabrielle Dreyfus, USA]	Same as comment 2356
14636	4	32	22	32	22	regions that is inexistant or negligible in the much [Christophe Deissenberg, Luxembourg]	The chapter team does not understand the intended meaning of the comment.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
20752	4	32	22	32	22	Delete reference to Antarctica - poorly worded and confusing. In fact the whole sentence is unclear.s [Tamsin Edwards, UK]	Agreed. Reference to Antarctica has been removed.
20754	4	32	24	32	24	Cite Furst et al. (2015) alongside Edwards et al. (2014) [Tamsin Edwards, UK]	Agreed. Furst et al., (2015) has been added.
8462	4	32	29	39	27	The section "4.2.3.1.2 Antarctica" has a high scientific and technical quality. But a general review should be carried out to improve the links between the paragraphs (maybe change some paragraph order), to make the central idea of this section clearer. [APECS Group Review, Germany]	This section of text has been modified, with an eye toward improving clarity and readability. Note that much of this text has been moved to chapter 3.
11720	4	32	29	39	35	I am surprised that this section ends up on using only the Golledge estimates, rather than the range of estimates including Ritz et al. and Levermann et al. (2014). Compared with the AR5, I would have expected the bottom of the range of possible Antarctic contribution to have only increased slightly (if at all) but the mean and top of the range to have increased, perhaps by the amount included here. It is also unclear whether there has been sufficient attention paid to changes in surface mass balance, particularly the projections of increased snowfall. Finally, it is important to evaluate the temperatures and timing at which significant surface mealting of ice shelves occurs. De Conto and pollard have considered this for only one CMIP5 model and one downscaling scheme as I understand, rather than a range of models and techniques. [John Church, Australia]	SROCC sea level projections have evolved substantially since the FOD. Ritz et al., (2015) is discussed, but not included explicitly, because it follows SRES vs RCP forcing scenarios. Levermann et al., (2014) is included in an expanded discussion section. We more clearly acknowledge the climate forcing limitations of both the 2016 and updated DeConto and Pollard ice sheet modeling studies.
18832	4	32	31	32	38	Contrary to the Greenland section above, current mass loss numbers are not given. This should be done in order to have an equilibrium between both sections. Would it also be possible to include the recent IMBIE2 results? [Frank Pattyn, Belgium]	Most text involving modern observations of ice sheet changes has been moved to chapter 3. Both Greenland and Antarctic mass loss changes are discussed there and in the section on the sea-level budget. The text now includes reference to IMBIE2.
20756	4	32	34	32	34	These geographic features "are thought to" make the overlying ice sheet vulnerable to dynamical instabilities..." - we are still unsure the degree to which these potential instabilities might be slowed or stopped by negative feedbacks. [Tamsin Edwards, UK]	The chapter team agrees. Text is included that highlights the ongoing uncertainty in the persistence of these processes if and when they do commence.
4326	4	32	35	32	35	add reference to Hughes (1981) "weak underbelly" [Peter Clark, USA]	References to foundational (pre AR5) literature is limited unless it is absolutely critical to the discussion. The author team feels Weertman (1974) provides sufficient theoretical background.
8464	4	32	35	32	35	Other references can be added: Mercer (1978); Thomas (1979) [APECS Group Review, Germany]	See comment 8464 above. We have added Mercer (1978) because of that paper's driect connection between greenhouse gas loading and ice loss.
8466	4	32	36	32	36	Maybe remove "both" [APECS Group Review, Germany]	Done
14638	4	32	36	32	36	melt rates and in the [Christophe Deissenberg, Luxembourg]	see comment 8466 above.
18830	4	32	40	32	55	This section should be more in line with Chapter 3 [Frank Pattyn, Belgium]	Agreed. Most of this section of text related to observed processes has been moved to Chapter 3.
8468	4	32	45	32	46	E1b The phrase "where the termini of outlet glaciers are in direct contact with the ocean" is a bit confusing. Ice is ocean-terminating almost everywhere around Antarctica. Perhaps what is meant is "where a general absence of large ice shelves puts tall calving faces in direct contact with the ocean" or something like that. [APECS Group Review, Germany]	Agreed. This text has been rewritten and moved to chapter 3.
17616	4	32	46	32	46	and along the Ant Peninsula... [Jonathan Bamber, UK]	Agreed. This text has been rewritten and moved to chapter 3.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
20758	4	32	48	32	48	Change to 'under the ice shelves', to include direct melting near the grounding line [Tamsin Edwards, UK]	Agreed. This text has been rewritten and moved to chapter 3.
17828	4	32	49	32	55	As above. Check which of these studies make attributions of ice loss to oceanic warming. [Robert Arthern, UK]	Agreed. This text has been rewritten and moved to chapter 3.
17618	4	32	51	32	51	It's not really ocean warming but "changes in ocean heat content on the continental shelf". These are different things [Jonathan Bamber, UK]	Agreed. This text has been rewritten and moved to chapter 3.
20760	4	32	54	32	54	Remove the word 'impactful' if possible... [Tamsin Edwards, UK]	Agreed. This text has been rewritten and moved to chapter 3.
2744	4	32	55	32	55	I would add "as paleo-data evidence suggests that some grounding line retreats in the Amundsen Sea may have occurred as early as in the 1940s (Smith et al. 2016)". Smith, J. A., et al. (2017): Sub-ice-shelf sediments record history of twentieth-century retreat of Pine Island Glacier. Nature 541.763: 77. [Nicolas Jourdain, France]	Agreed. Smith et al., (2016) should be included to provide a 20th century perspoective on retreat. This also relates to recent work (Barlett et al., 2018) hinting that the low viscosity mantle in the ASE region will provide a negative feedback on retreat.
2360	4	32	57	33	15	Consider adding Pollard et al 2015 for a discussion on hydrofracturing enhancing surface melt and weakening the grounding line. (Pollard D., et al. (2015) Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure, EARTH & PLANETARY SCIENCE LETTERS 412:112–121.) [Kristin Campbell, USA]	These processes are now described in a cross-chapter box in Chapter 3.
2486	4	32	57	33	15	Consider adding Pollard et al 2015 for a discussion on hydrofracturing enhancing surface melt and weakening the grounding line. (Pollard D., et al. (2015) Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure, EARTH & PLANETARY SCIENCE LETTERS 412:112–121.) [Durwood Zaelke, USA]	Same as comment 2360
12984	4	32	57	33	15	Consider adding Pollard et al 2015 for a discussion on hydrofracturing enhancing surface melt and weakening the grounding line. (Pollard D., et al. (2015) Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure, EARTH & PLANETARY SCIENCE LETTERS 412:112–121.) [Gabrielle Dreyfus, USA]	Same as comment 2360
8470	4	33	2	33	2	Other references can be added: Mercer (1978); Thomas (1979) [APECS Group Review, Germany]	see comment 8466.
14640	4	33	3	33	3	operates as a positive feedback [Christophe Deissenberg, Luxembourg]	The meaning of this comment is not understood.
8472	4	33	4	33	4	E1a Specify "seaward flow RATE of ice" -- regardless of its thickness, ice will flow. [APECS Group Review, Germany]	Agreed. This has been reworded. These processes are now described in a cross-chapter box.
4328	4	33	5	33	5	grounding line does not have a thickness. This should be "ice thickness at the grounding line". See elsewhere for same correction [Peter Clark, USA]	Agreed. This wording has been changed. The discussion of MISI has been moved to a cross chapter box in Chapter 3.
8474	4	33	5	33	5	Maybe change "grounding line thickness" to "ice thickness at grounding line" [APECS Group Review, Germany]	Agreed. See comment 8474.
8476	4	33	5	33	6	E1a Awkward sentence, recast to show that the increasing grounding line thickness CAUSES the increased flow rate / seaward flux. [APECS Group Review, Germany]	This text has been modified and moved to a cross chapter box in Chapter 3.
4330	4	33	6	33	6	change "as well the seaward" to "causing an increase in the seaward" [Peter Clark, USA]	Agreed. This wording has been changed in the discussion of ice sheet dynamics and modeling, which is now in a cross chapter box in Chapter 3.
17620	4	33	8	33	8	such as vertical land motion due to unloading of the bedrock (Gomez et al...) [Jonathan Bamber, UK]	This wording has been changed in the discussion of ice sheet dynamics and modeling, which is now in a cross chapter box in Chapter 3.
14642	4	33	9	33	9	in addition to the bed slope, [Christophe Deissenberg, Luxembourg]	The intention of this comment is not clear.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8478	4	33	9	33	10	E1a Ice shelf pinning points are a subset within "details of the bed geometry". Suggest rewriting sentence as "… including the width of channelized flow, …, and the details of the bed geometry, such as potential ice-shelf pinning points." [APECS Group Review, Germany]	Agreed. This text has been modified.
3780	4	33	10	33	10	change to "shear" [Ola Kalen, Sweden]	Typo. Agreed.
8480	4	33	10	33	10	Change "sheer" to "shear" [APECS Group Review, Germany]	see comment 8480
8482	4	33	10	33	10	Maybe insert the reference to Figure 4.6 (or another schematic new figure), to illustrate all the process involved in MISI [APECS Group Review, Germany]	MISI was described extensively in AR5, so a figure was not included in the FOD. However, a combined MISI-MICI figure is now included in a new cross chapter box in Chapter 3.
8484	4	33	10	33	10	E1a "shear" not sheer [APECS Group Review, Germany]	Typo. Agreed. See comment 8480.
14644	4	33	11	33	12	Hence, long-term retreat is not necessarily unstoppable on every reverse-sloped bed (Gudmundsson et al., 2012; Parizek et al., 2013; Docquier et al., 2014; Gomez et al., 2015). However, there is growing [Christophe Deissenberg, Luxembourg]	Agreed. These sentences have been reworded.
1472	4	33	12	33	12	Long-term retreat on every reserve-sloped bed and fact that this is not necessarily unstoppable. Pinning points can play an important effect here, which is nicely shown in the numerical modelling study of Favier et al. (2016, TC, doi: □10.5194/tc-10-2623-2016). Consider also mentioning this study. [Harry Zekollari, Switzerland]	Agreed. Favier et al., (2016) is an appropriate reference.
4332	4	33	12	33	12	also Gomez et al. (2010) [Peter Clark, USA]	Agreed, although our assesment is concentrating on post AR5 literature.
11854	4	33	12	33	15	the new reports of Kingslake et al nature 2018 and particularly Barletta et al Science 2018 should be added here - that solid Earth effects can be rapid and act toward stabalising ice sheets (or at least slowing it a little) as noted through model experimetns by Gomez et al [King Matt, Australia]	Agreed. These new studies have been added to the SOD, although we note that Pollard et al (2018) showed a limited potential for ice-Earth interactions to slow near-term retreat, especially in a RCP8.5 scenario. Furthermore, Pine Island Glacier has been retreating since the 1940's- a location where this negative feedback should be operating.
18834	4	33	12	33	15	The use if MISI needs a clear definition, as multiple meanings are currently attached to the related words 'stability' and 'instability', especially with an iconoclastic term as MISI or MICI. Observed changes in glaciers are very commonly associated with use of the word 'instability' (as is the case within the references cited), whereas in its original (Weertman, 1974) and subsequent usage (Schoof, 2007a) it refers to a self-generated growth of a small perturbation (e.g., grounding line retreat or advance). This is also the way it is used amongst modellers when investigating grounding line behaviour. According to the definition of MISI, the observations in Thwaites glacier pertain to accelerated grounding line retreat potentially leading to MISI. Whether we are at the onset of MISI is totally unknown. The observed accelerated grounding line retreat could well be due to increased forcing, which is a linear and not a nonlinear response. In the case of Pine Island Glacier, the increased sub-shelf melt was slowed down recently (Dutrieux et al., 2014). [Frank Pattyn, Belgium]	Agreed. With this in mind, a new cross chapter box describing MISI and MICI has been added to Chapter 3. An attempt has been made here to be more precise and consistent in the use of these terms.
4334	4	33	15	33	15	also Joughin et al. (2014) [Peter Clark, USA]	Agreed.
8486	4	33	15	33	15	E1a add reference Joughin et al. (2014) [APECS Group Review, Germany]	Agreed. See comment 4334.
13090	4	33	15	33	15	Might want to talk about bedrock rebound here (Gomes et al., 2015; Barletta et al., 2018; Kingslake et l., 2018) [Gerhard Krinner, France]	The possible implications of bedrock rebound are now expanded on . Also see comment 11854.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8488	4	33	19	33	23	E1a Suggest rephrasing these two instances of "Totten's" to remove ascribing possession by apostrophe-s to inanimate objects [APECS Group Review, Germany]	To the chapter team's knowledge, there is no grammatical rule in English that inanimate objects can be associated with possesive punctuation. Text associated with modern observations and processes has been moved to Chapter 3.
11856	4	33	21	33	23	Roberts et al have shown that velocity fluctuations of Totten can be explained by natural variability in ocean forcing. That is the signals observed may not be long-term trends. See doi:10.1144/SP461.6 . I think it important to state that many ice shelves are unchanged over decades (Paolo et al; King et al JGR Earth Surface 2009) - these findings place the community well to understand natural variability and detect changes when they emerge [King Matt, Australia]	Agreed. The role of natural variability versus long-term trends should be mentioned explicitely. This text has been rewritten and moved to Chapter 3.
17622	4	33	23	33	23	East Antarctica => part of East Antarctica. It's an important distinction....The former gives the impression that its the whole ice sheet... [Jonathan Bamber, UK]	Agreed. This text has been rewritten and moved to Chapter 3.
8490	4	33	26	33	26	C2 While I agree that there's more work to do on the future of Totten, I dsagree that this is "largely unknown". The Aitken study cited earlier in the sentence does a preliminary exploration of these "implications" for future SLR. I'd suggest "remain unconstrained" instead of unknown. [APECS Group Review, Germany]	Agreed. This language has been changed.
24636	4	33	26			The logic of this sentence seems poorly integrated. If there is evidence for repeated response of Totten glacier to warming including the Pliocene warmth, then this tightening of cause and effect must allow for a more specifc suggestion than: "the implications for its response to future warming are largely unknown"..... This appers like an unfinished line of reasoning. [Hans-Otto Poertner and WGII TSU, Germany]	See repsonse to comment 8490.
8492	4	33	31	33	31	E1a "(quadratic dependency)" -- Either more elaboration is needed, or this should just be removed. Melt rates depend on the square of (T - Tmelt)? Is this parameterized or first principles? What is the citation? [APECS Group Review, Germany]	Yes, this is correct. This quadratic dependence is used in ocean melt rate parameterizations and is largely based on a modeling stydy by Holland et al., 2008). This language has been slightly modified.
14646	4	33	32	33	33	interface, and to the local ice shelf cavity geometry [Christophe Deissenberg, Luxembourg]	Agreed. This wording has been changed.
2746	4	33	32	33	34	Two additional references should be quoted: [1] Mathiot, P., Jenkins, A., Harris, C., & Madec, G. (2017). Explicit representation and parametrised impacts of under ice shelf seas in the z∗ coordinate ocean model NEMO 3.6. Geoscientific Model Development, 10(7), 2849. And [2]: Dinniman, M. S., Asay-Davis, X. S., Galton-Fenzi, B. K., Holland, P. R., Jenkins, A., & Timmermann, R. (2016). Modeling ice shelf/ocean interaction in Antarctica: A review. Oceanography, 29(4), 144-153. [Nicolas Jourdain, France]	These are appropriate references and they have been added.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
2748	4	33	34	33	38	See recent review of Asay-Davis et al. (2017): Since AR5, many ice-sheet models have kept using very idealized melting formulations unrelated to any CMIP projection (e.g. Favier et al. 2014; Joughin et al. 2014). In parallel, several studies have tried to use melt parameterizations depending on CMIP ocean temperatures (e.g. Cornford et al. 2015; De Conto and Pollard 2016; Golledge et al. 2015), but these formulations were very empirical, and based on an even more empirical choice of sector and depth to average the CMIP ocean temperatures. The recent propositions of Reese et al. (2018) and Lazerms et al. (2018) are built upon idealized ocean models (box-model and plume-model respectively) and therefore contain more physics, but they still have to be evaluated. Finally, there have been a few comparisons of parameterizations to ocean/ice-sheet coupled models (De Rydt et al. 2016; Seroussi et al. 2017; Jordan et al. 2018). All of them indicate that even a finely tuned parameterization may overestimate the ice-sheet dynamical response to increased melt rates. [Nicolas Jourdain, France]	An attempt has been made to better represent the state of ice-ocean modeling in SROCC, in part by including the most relevent references listed here, particularly, Asay-Davis (2017)
17830	4	33	35	33	38	See also Snow et al. (2017; doi.org/10.1002/2017GL075745) and Goldberg et al. (2018; doi.org/10.1016/j.ocemod.2018.03.005) for recent development of a synchronous coupling approach that includes exactly this two-way interaction. [Robert Arthern, UK]	The chapter team agrees and these references (and their fundemental findings) have been added.
20762	4	33	40	33	40	Replace "> 3.2 m" with "3.3 m" as quoted in paper abstract. [Tamsin Edwards, UK]	Agreed. This has been changed.
17832	4	33	42	33	43	It would be better if the floating ice in this figure was not so visibly far from hydrostatic equilibrium. Maybe not worth redrawing for a schematic figure, but worth doing if the figure is being changed anyway for some reason. It would be better in this (and similar figures) to make the ratio of floating ice above/below sea level more realistic (approx 1:10). See Box 4.1, Figure 1 for a more realistic depiction. [Robert Arthern, UK]	The chapter team agrees. This figure will be adjusted before final publication of SROCC. It is meant to be schematic in nature, and not necessarily to scale, however the ice thickness could be adjusted to improve the appearance of hydrostatic equilibrium.
8494	4	33	42	34	9	Legend of Figure 4.6 should be in only one page. [APECS Group Review, Germany]	This is a formatting issue that will handled at the production stage.
8496	4	33	43	33	43	Write out "circumpolar deep water" on the diagram -- currently, CDW is the only acronym in the figure. [APECS Group Review, Germany]	Agreed. See response to comment 17832
8498	4	34	7	34	7	E1a cite Bamber et al., (2009) for this sea-level content [APECS Group Review, Germany]	Agreed. Also note that this value should be 3.3m, not 3.2m. See comment 20762 above.
13092	4	34	10	34	10	Might prefer "simple" over "simplistic" (which might carry a value judgment?) [Gerhard Krinner, France]	The chapter team agrees, although we think this comment refers to line 10 on page 36, not page 34.
8500	4	34	11	34	11	Use another word or expression to replace "resolved". Maybe "high spatial resolution ice models" [APECS Group Review, Germany]	The chapter team prefers the current, shorter wording.
1474	4	34	12	34	12	Cf. previous comment: consider referring to study by Favier et al. (2016, TC) here [Harry Zekollari, Switzerland]	Favier et al., 2016 does provide a good example of high-resolution modeling over a limited area domain in East Antarctica, and demonstrates the importance of small-scale bathymetric features in the dynamic respons of specific outlets. This reference is now included.
20764	4	34	12	34	12	Add "e.g." to both citation lists as other studies exist. [Tamsin Edwards, UK]	Agreed. "e.g." has been added. See comment 1474.
8502	4	34	21	34	21	Maybe remove comma after "In an idealized" [APECS Group Review, Germany]	Agreed. The comma has been removed.
8504	4	34	23	34	23	What does "A1B scenario" means? It should be defined in the text [APECS Group Review, Germany]	A1B is now defined explicitly.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
18836	4	34	26	34	26	Reference to Arthern and Whilliams is 2017, not 2015. It is correct in the reference list. [Frank Pattyn, Belgium]	Agreed. This citation has been fixed.
8506	4	34	30	34	30	E1a add "... is already underway, in agreement with Joughin et al. (2014)." [APECS Group Review, Germany]	The recommended wording has been added.
8508	4	34	36	34	36	Mayne use anothet word to replace "resolved". Maybe "spatial resolution"? [APECS Group Review, Germany]	In this instance, the chapter team prefers the original wording.
8510	4	34	39	34	39	Change Figure citation "Fig. 4". Maybe Figure 4.6? Fix citation properly (Scambos et al., 2017) [APECS Group Review, Germany]	The typo has been fixed. The figure is now in Chapter 3.
23988	4	34	39	34	39	Specify which figure [Hans-Otto Poertner and WGII TSU, Germany]	See comment 8510 (above).
20766	4	34	40	34	40	Replace "> 3.2 m" with "3.3 m" as quoted in paper abstract. [Tamsin Edwards, UK]	Agreed. This has been fixed.
4336	4	34	40	34	41	This statement should be made earlier. [Peter Clark, USA]	Agreed. This statement now appears earlier in the document.
4338	4	34	43	34	43	delete "targeted" - jargon [Peter Clark, USA]	Agreed. "targeted" has been deleted.
13008	4	34	43	34	48	In addition to irreversibility, discuss implications of rate of atmospheric and ocean warming on rate of melting and, importantly, amount of committed sea-level rise. (International Cryosphere Climate Initiative (2015), Thresholds and Closing Windows: Risks of Irreversible Cryosphere Climate Change, WWW.ICCINET.ORG/THRESHOLDS, 6-7.) [Gabrielle Dreyfus, USA]	The chapter team agrees. This is an important point, implicit in the discussion section on sea-level projections under various warming scenarios. A few studies have addressed the issue of sea level rise committed from ice sheets. However, we note that few studies have fully explored the effect of various rates and duration of warming. Notable recent exceptions include Snow et al., (2017), and DeConto et al., (2018), which are now included in the assessment.
18840	4	34	44	34	46	See my previous remark on clear definition of MISI. The observations in Thwaites glacier pertain to accelerated grounding line retreat potentially leading to MISI. Whether we are at the onset of MISI is totally unknown. The observed accelerated grounding line retreat could well be due to increased forcing, which is a linear and not a nonlinear response. [Frank Pattyn, Belgium]	The authors acknowledge the importance of defining MISI and agree that observed retreat on reverse sloped bedrock is not necessarily "proof" of MISI. The final sentence of this paragraph was intended to convey this point "However, the irreversibility of retreat and the long-term implications for the wider WAIS remain uncertain". An attempt has been made here and in a cross-chapter box in Chapter 3 to further reinforce this.
4340	4	34	44	34	48	This sentnce does not follow directly from preceding one - just because there is a sensitvity does not suggest that MISI is underway. Need to discuss evidence that ocean warming has been occurring, which then because of the sensitivity has likely led to MISI [Peter Clark, USA]	The authors agree with the comment. This text has been reworded to improve the logic.
8512	4	34	51	34	54	The phrase starting "A sustained ..." to " ... in the near future (Nicolas et al., 2017) " is incompleted. [APECS Group Review, Germany]	The chapter team thinks this sentence is mostly fine as is. The optional comma after "2016" has been deleted.
20768	4	34	52	34	52	Delete first comma [Tamsin Edwards, UK]	Agreed. See comment 8512 above.
18564	4	34	52			no comma needed after 2016 [Christopher Fogwill, UK]	Agreed. See comment 8512 above.
4342	4	34	55	34	55	There are more suitable references for RACMO than this one [Peter Clark, USA]	This is the correct reference to the climate-snowpack model used by Trusel et al., 2016. The snow model is now mentioned explicitly.
322	4	34	58			..balance due to its potential to lower albedo, its ability to saturate... [Kerstin Jochumsen, Germany]	The missing word has been added.
24638	4	35	0			Discussion of individual icesheet models and global sea level models should be comparative and summarized in tabular form. Much text reads like a respective review rather than assessment and can be condensed and/or moved into OSM. [Hans-Otto Poertner and WGII TSU, Germany]	Some of this text has been moved to a cross chapter box in Chapter 3. The model comparison is listed in Table 4.2, and an attempt has been made to shorten this section of text.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8514	4	35	1	35	1	E1a It is "full thickness hydrofracturing" that causes ice-shelf breakup, not merely hydrofracturing, which just refers to the deepening of a water-filled crevasse without necessarily reaching the bottom of the shelf. I think the most accurate approach here is to just delete "hydrofracturing," since these references (especially Banwell) more specificially address water-induced fracture formation, which is more specific than water-enhanced fracture deepening (hydrofracture). [APECS Group Review, Germany]	The chapter team agrees that these definitions must be consistent. "hydrofracturing" has been deleted as recommended by the reviewer.
14648	4	35	2	35	2	When and if in future warming scenarios melt rates will be sufficiently high to trigger [Christophe Deissenberg, Luxembourg]	This wording has been changed. See comment 4344 and 8516 above.
4344	4	35	2	35	3	This is an important question that needs to be assessed, as AR5 did [Peter Clark, USA]	There isn't much litterature to draw from here, but an attempt has been made to address this more directly, in this section of text and the section on projections.
8516	4	35	2	35	3	E1b "When and if melt rates will be…" -- I don't think that "IF" is under debate. I agree that "WHEN" may yet be unconstrained. [APECS Group Review, Germany]	As demonstrated by Trusel et al., (2016), the potential for widespread surface melt is highly scenario dependent. This is now mentioned in the text directly, to improve clarity. Also see comment 4344 above.
2750	4	35	4	35	47	First, Feldmann and Levermann (2015) prescribe melt rates derived from a stand-alone ocean simulation that simulates under-ice-shelf cavities (FESOM model) while Golledge et al. apparently use the surface temperature of a slab ocean model coupled to an atmosphere mode (not representing ice-shelf cavities)l. Then Golledge et al use this slab-ocean temperature to feed the so-called "three equations" to obtain melt rates (e.g. Jenkins et al. 2010), which is not a very classical approach (these equations are meant to be fed by temperature and salinity in the ocean boundary layer underneath ice shelves). Second issue is that they take the CMIP5 sea surface temperature (SST) to calculate the future anomalies, while subsurface warming is more relevant for ice shelf melting. [Nicolas Jourdain, France]	The chapter team is aware of the caveats of Golledge et al's approach to forcing PISM. This text has been modified slightly, but the chapter team feels the text adequately conveys the uncertainty of the approach, without getting into a level of detail better suited for a journal-style critique.
4346	4	35	6	35	6	large spatial [Peter Clark, USA]	Agreed. This text has been modified.
14650	4	35	6	35	6	scale of the region and to complex interactions [Christophe Deissenberg, Luxembourg]	Agreed. The text has been modified. Also see comment 14650 above.
14652	4	35	11	35	11	that can be validated [Christophe Deissenberg, Luxembourg]	Agreed. Typo has been fixed.
20770	4	35	11	35	11	that "can" be [Tamsin Edwards, UK]	Agreed. Typo has been fixed. Also see comment 14625 above.
20772	4	35	11	35	11	Delete comma, if not whole clause after it (irrelevant/confusing), and add 'or ensembles to explore unertainty' or similar. [Tamsin Edwards, UK]	Agreed. Typo has been fixed. Also see comment 14625 above.
8518	4	35	13	35	16	The phrase starting "However …" to "… the essence of MISI (…)." should be changed to make it clearer. [APECS Group Review, Germany]	This text has been changed and now refers to the cross chapter box on ice dynamical processes (MISI and MICI) in chapter 3.
8520	4	35	15	35	15	Change "resolved" to another word. Maybe high "spatial resolution" [APECS Group Review, Germany]	Agreed. This text has been modified.
14654	4	35	15	35	15	shown that simplified [Christophe Deissenberg, Luxembourg]	Agreed. "that" has been added.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
18842	4	35	16	35	18	It is not accurate sub-ice melt (with references to Arthern and Golledge), but how sub-shelf melt is implemented in numerical ice-sheet models (using sub-shelf melting on partially grounded grid cells). [Frank Pattyn, Belgium]	Agreed. This is an important issue and an attempt has been made to clarify and emphasize this point. We also point to the recent work of Yu et al., (2018 TCD) who noted the sensitivity of grounding line retreat to melt schemes that apply oceanic melt on partially floating grid cells.
18838	4	35	17	35	17	Reference to Arthern and Whilliams is 2017, not 2015. It is correct in the reference list. [Frank Pattyn, Belgium]	Agreed. Citation is changed to "2017"
8522	4	35	20	36	28	E1b The extensive treatment of these 4 ice-sheet modeling studies is out of balance with the rest of the chapter. The descriptions are useful, but much more in depth than given to any other study in this chapter. [APECS Group Review, Germany]	The chapter team appreciates this comment, but feels that these papers deserve in-depth discussion because they are the data sources for the updated sea-level projections provided later in the chapter.
20774	4	35	25	35	25	Not just observations of ASE (the whole continent), so delete that part [Tamsin Edwards, UK]	Agreed. The text has been modified.
14656	4	35	25	35	26	THE ROLE OF THE EXPECTED FUTURE CHANGE IN THE FOLLOWING CONSTRUCT IS UNCLEAR: based on observations of Amundsen Sea retreat over the last few decades, and expected future climate change following an A1B emission scenario only [Christophe Deissenberg, Luxembourg]	The chapter team is unsure of the meaning of this comment, but has made attempt to emphasize the caveats of the cited work relative to the projections that follow.
8524	4	35	26	35	26	What does "A1B scenario" means? It should be defined in the text [APECS Group Review, Germany]	Agreed. A1B is now defined.
17834	4	35	26	35	28	Strictly, I think retreat rates used by Ritz et al. are prescribed (then tested against the theory of Schoof 2007a). This is subtly different from parameterising them using the Schoof 2007 theory (as done by Pollard et al., 2009). [Robert Arthern, UK]	Agreed. The wording has been changed slightly for clarity.
8526	4	35	28	35	28	Maybe change "grounding line thickness" to "ice thickness at grounding line" [APECS Group Review, Germany]	Agreed and changed accordingly.
14658	4	35	28	35	28	(slope), of the grounding line thickness following Schoof (2007a), and of a formulation for basal friction. The [Christophe Deissenberg, Luxembourg]	Agreed and changed accordingly.
8528	4	35	29	35	29	Maybe change "iterations" to "simulations", like Monte Carlo analysis? [APECS Group Review, Germany]	The chapter team thinks either "iteration" or "simulation" is appropriate.
4348	4	35	31	35	31	it is directly comparable - A1B lies about midway between RCP6.0 and RCP8.5 [Peter Clark, USA]	While the AIB scenario might be comparible to the RCP scenarios, the ice sheet response under AIB is not. The response of the ice sheets is too non-linear to interpolate between scenarios. The text has been adjusted here for clarity.
3076	4	35	32	35	33	"nearly Gaussian" is not a useful way of specifying a distribution [Robert Kopp, USA]	Agreed. This wording has been changed.
14660	4	35	34	35	34	rather than imported from climate and ocean [Christophe Deissenberg, Luxembourg]	Agreed. Pehaps "coming directly from" is more appropriate.
12586	4	35	35	23	24	In Ritz et al. 2015 the timing of MISI onset is an expert judgement, is not statistically determined. [Dewi Le Bars, Netherlands]	The onset of MISI at each location is assigned a probability, based on expert judgment. The treatment is both statistical and judgment based. The wording has been changed for clarification.
20776	4	35	35	35	37	I would say this is a bit too pessimistic. After all, the CMIP ensemble i.e. GCMs are only calibrated with recent observations. While I agree calibration with the past is no guarantee of success in the future, it does not rely on the future being like the past - only that the model is able to simulate the relationship between the two. [Tamsin Edwards, UK]	The chapter team appreciates this comment and is open to alternative wording, but feels use of the term "adequate" provides sufficient optimism.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14662	4	35	36	35	38	In addition, the model considers only processes associated with MISI but disregards possible contributions from other physical processes which may emerge but for which no recent analogue exists. NOTE: THE “which may emerge” IS RATHER OBSCURE [Christophe Deissenberg, Luxembourg]	Agreed. This wording has been changed and shortened.
18844	4	35	36	35	38	It is rather vague to allude to possible contributions from other physical processes which may emerge. Is it meant to be MICI? If so, then state it. [Frank Pattyn, Belgium]	Agreed. Also see comment 14662 above.
20778	4	35	38	35	39	"Relatively little attention..." This is not fair. The paper only aims to quantify the dynamic contribution, while this statement implies it quantifies the total contribution but is negligent in quantifying SMB changes. And BMB changes are incorporated through the statistical modelling of MISI onset, which is in part based on ocean model projections of melting in different regions under A1B. [Tamsin Edwards, UK]	Agreed. This statement has been deleted.
3078	4	35	39	35	39	What is BMB? [Robert Kopp, USA]	BMB should be defined. This is now fixed. It means (basal mass balance)
3782	4	35	39	35	39	explanation to abbreviation BMB is lacking [Ola Kalen, Sweden]	See comment 3782 above.
4350	4	35	39	35	39	what's BMB? [Peter Clark, USA]	See comment 3782 above.
8530	4	35	39	35	39	"BMB" was not defined before in the text [APECS Group Review, Germany]	See comment 3782 above.
17624	4	35	39	35	39	Don't think BMB has defined, at least for a long time. Just spell out in full. [Jonathan Bamber, UK]	See comment 3782 above.
8532	4	35	41	35	41	No comma before "to simulate ..." [APECS Group Review, Germany]	Agreed. Comma has been deleted.
8534	4	35	41	35	41	Maybe use "Parallel Ice Sheet Model (PISM)" instead "PISM ice sheet model" [APECS Group Review, Germany]	Agreed. PISM is now spelled out.
14664	4	35	41	35	41	(Winkelmann et al., 2011) to simulate [Christophe Deissenberg, Luxembourg]	Agreed. Comma has been deleted.
18846	4	35	41	35	51	Golledge also applies sub-grid melting partially under grounded grid cells, which is comparable to the approach by Arthern and Whilliams (2017). In order to avoid confusion, this should be stated, since the effect on grounding line retreat is similar. [Frank Pattyn, Belgium]	Agreed. We expand on this important point and also cite Yu et al., (2018) who emphasize the sensitivity of GL retreat to this model treatment.
8536	4	35	46	35	47	E1a "simple" is redundant when applied to slab ocean models (you cannot get more simple than a slab ocean) [APECS Group Review, Germany]	The general reader might not know that slab ocean models are simplistic representations of the upper ocean. The chapter tream prefers the current wording.
14666	4	35	47	35	48	mainly through MISI. However, using a more conservative [Christophe Deissenberg, Luxembourg]	Agreed. This wording has been changed.
14668	4	35	51	35	51	parameterizations, the authors do [Christophe Deissenberg, Luxembourg]	Agreed. This wording has been changed.
14670	4	35	53	35	53	order to compute both sub-ice [Christophe Deissenberg, Luxembourg]	Agreed. "Calculate" has been changed to "compute".
8538	4	35	56	35	56	Any "probability range" for RCP8.5 scenario, like for RCP2.6? [APECS Group Review, Germany]	Yes. The probability range for RCP8.5 is now stated.
13598	4	35	56			Please use consistent units - cm / m [Debra Roberts and Durban Team, South Africa]	Agreed and changed accordingly.
8540	4	36	1	36	2	This paragraph should be the last phrase of last paragrph (right?) [APECS Group Review, Germany]	Yes.
14672	4	36	2	36	3	shelves. However, the authors conclude that the single greatest source uncertainty stems from the external forcing. [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
324	4	36	2			source of uncertainty [Kerstin Jochumsen, Germany]	Agreed. Fixed.
2752	4	36	4	36	5	In terms of melting parameterization, DeConto and Pollard is quite different from Golledge et al. (2015). DeConto and Pollard use a more ellaborated melting parameterization giving the expected square deendency on thermal forcing (Holland 2008), fed by the Word Ocean Atlas temperatures at 400m depth for present-day, and future anomalies of 400m temperatures from a single CMIP5 model. As ocean is the main trigger for the instability, it is an important aspect, although I agree that the inclusion of MICI in DeConto and Pollard is an even more important difference. [Nicolas Jourdain, France]	This is correct. This text has been adjusted for additional clarity, including a mention of the ocean forcing in DeConto and Pollard (2016)and updated DeConto et al., (2018).
17628	4	36	4	36	49	This section will need revision, based on new simulations from D&P and a study by Edwards et al (in revision) and will require care in formulation. There are multiple lines of evidence suggesting that Pliocene highstand is a very poor calibration target etc. etc. Present-day trends are also challenging if attribution of trend is undefined (as is the case), while the results appear to be sensitive to these targets. [Jonathan Bamber, UK]	Agreed. These are important points. Updated findings of Edwards and DeConto are now mentioned as are the caveats of using both uncertain paleo and short (modern) calibration targets to calibrate model physics.
17630	4	36	4	39	20	There is a lot of discussion of a single study and MICI here, which feels disproportionate. Given the lack of evidence for MICI in Antarctica and the highly uncertain calibration/parameterization it doesn't seem warranted and is unbalanced. There are many other processes (that have been published on) that may be equally/more important. VLM is one (see e.g. Kingslake 2018 and Barletta 2018 and plenty of earlier studies). These studies sugges the potential for significant -ve feedback on GL migration due to VLM particulary for a weak WAIS mantle and thinner lithosphere. There are, however, several pages devoted to MICI and rather litte on others. Organic/inorganic impacts on GrIS albedo is another that may be important and absetn from models. See e.g. Dumont et al, 2014, Nat Geo., which is not cited at all. Neither is Tedesco et al, 2016, TC on the same topic, etc etc. Albedo is the single most important variable that affects the surface energy balance (and therefore melt) on the GrIS.....(Fitzgerald et al, 2012, JGR). See also comment 31. [Jonathan Bamber, UK]	The chapter team agrees that Greenland albedo-melt feedbacks deserve more emphasis and several new references have been added. However, considering the potential magnitude of Antarctic mass loss to calving of thick marine-terminating ice fronts like that observed in Greenland today, the chapter team feels strongly that this emerging, uncertain topic (Bassis and Walker, 2012; Bassis et al., 2017; Pollard et al., 2015; DeConto and Pollard, 2016; Parizak et al., in review; Wise et al., 2018) deserves thorough discussion. Furthermore, MICI is new since AR5, and is now being considered by a number of international modeling groups.
17626	4	36	5	36	5	delete "fundamental". There are many missing processes not just these two. [Jonathan Bamber, UK]	Agreed. "Fundamental" has been removed.
20780	4	36	5	36	6	This is not correct. Ritz et al. (2015) also includes hydrofracturing. The statistical modelling of MISI onset is in part based on process modelling of firn depletion and collapse of ice shelves by Kuipers Munneke et al. (2014). Only the parameterisation of MICI is new. This statement should also make clear that the MICI is a hypothesis. [Tamsin Edwards, UK]	The chapter team is refering to the inclusion of explicit, physical model treatments, but we do fully acknowledge that hydrofracturing is implicit in the formulation of Ritz et al., (2015). The text here has been adjusted accordingly. We also note that the description of MICI has been moved to a cross chapter bo in Chapter 3. Uncertainty of MICI's role in future sea-level is emphasized throughout, however, we also stress that ice-cliff calving of thick marine terminating ice fronts, the essense of MICI, is an observed process supported by fundamental glaciological principles (stress balance).
8542	4	36	6	36	7	Maybe use "(1)" and "(2)" instead of "1)" and "2)" [APECS Group Review, Germany]	This is editorial and will be standardized during final production.
8544	4	36	7	36	7	no space in "marine-terminating" [APECS Group Review, Germany]	Agreed. Fixed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8546	4	36	8	36	8	E1a specify "and are all enough (~800 m) ABOVE FREEBOARD to generate..." or ABOVE FLOTATION [APECS Group Review, Germany]	This should read "thick enough". Ice >800m thick and at floatation will form calving fronts susceptable to collapse (e.g., Bassis and Walker, 2012).
17836	4	36	8	36	8	Perhaps 'thick enough' less ambiguous than 'tall enough'. The indicative figure includes ice below the waterline. [Robert Arthern, UK]	Agreed. See comment 8546 above.
17838	4	36	10	38	40	I think this wording is about right. It emphasises the small amount of physical theory that lies behind these parameterisations. As a general point, similar care should be taken throughout the rest of the report to emphasise that results that derive from these parameterisations may change upon further theoretical and experimental study of the MICI cliff collapse process. In general, I would favour presenting the most important results, such as sea level curves, with and without MICI contributions, wherever any attempt is made to estimate the latter. We know so little about the potential magnitude of sea level rise from this process. By keeping MICI and non-MICI results available separately the report would still be useful in the event that the single available MICI parameterisation (DeConto and Pollard, 2016) is proved inaccurate. [Robert Arthern, UK]	The chapter team appreciates this comment and we are following the recommended approach in our revised projections. We note that continental-scale simulations including an empirically guided parameterization of ice cliff failure have already changed substantially since DeConto and Pollard, 2016.
14674	4	36	11	36	11	UNCLEAR: ability to match albeit uncertain geological sea level targets [Christophe Deissenberg, Luxembourg]	While the chapter team does not find this wording to be unclear, it has been changed.
4352	4	36	12	36	12	I thought the DP2016 experiments for LIG only invoked oean forcing, as surface temperatures were not high enough to trigger MICI? [Peter Clark, USA]	The chapter team's understanding is that most LIG retreat (especially WAIS) in DP16 is driven by ocean warming. However, some additional East Antarctic retreat is required to satisfy the LIG sea level constraints, particularly early in the interglacial. Inclusion of the cliff-calving parameterization allows some limited retreat of East Antarctica, in addition to WAIS. This point is clarified in the SOD.
4354	4	36	13	36	13	Dutton et al. do not say anything about retreat into deep East Antarctic basins [Peter Clark, USA]	The chapter team uses the word "imply" here, because the upper range of Pliocene sea level estimates requires some contribution from East Antarctica. With that said, this reference isn't the best way to illustrate this point, and Dutton et al (2015) has been replaced.
8548	4	36	14	36	22	E1b The text currently says that mechanisms (plural) other than MICI have been hypothesized, but only one is described (Tsai et al., 2015) and I am not aware of any others. The length of the description of this one alternative process is long and could be improved by shortening it. [APECS Group Review, Germany]	The chapter team agrees. This section has been shortened. A second alternative mechanism is associated with an ice model, sub-grid scale parameterization of ice shelf melt that applies melt at the bottom of partially grounded grid cells. This model treatment has demonstrated retreat into one East Antarctic basin, but its physical validity continues to be debated (e.g., Yu et al., 2018).
8550	4	36	15	36	15	"MICI" was not defined before in the text (only in the Box 4.1, Figure 1) [APECS Group Review, Germany]	Agreed. MICI is now defined in the main text and in a cross-chapter box in chapter 3.
22262	4	36	15	36	15	Be sure that the MICI acronym is defined [Andra Garner, USA]	Agreed. See comment 8550 above.
18848	4	36	17	36	17	Reference to Pattyn (2017) not in the reference list at the end of the chapter. [Frank Pattyn, Belgium]	This reference has been added.
14676	4	36	18	36	18	WAIS and to a major retreat [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8552	4	36	24	36	24	Change citation style: "Scambos et al. (2004); Scambos et al. (2009)" to "(Scambos et al., 2004; Scambos et al., 2009)" [APECS Group Review, Germany]	Agreed. Fixed.
326	4	36	24			add parenthesis for Scambos et al [Kerstin Jochumsen, Germany]	Agreed. Fixed.
8554	4	36	26	36	28	C4 This treatment of the Parizek study is understated in the context of the rest of the section. The leading criticism of MICI within this chapter has been the lack of a process (i.e., MICI is empirically observed and coarsely parameterized). The Parizek study will be the first to ascribe a detailed physical process to MICI (I suppose you might count Bassis and Walker 2012 on that spectrum too), and as such, should be highlighted more. [APECS Group Review, Germany]	While the chapter team agrees in principle, the Parizek study is mainly diagnostic, and simply uses a full stokes ice model to confirm the stress balance analysis of Bassis and Walker (2012). Parizak does offer a mechanistic description of calving once critical stress thresholds are exceeded, so additional emphasis may indeed be warrented. This text has been changed to emphasize these points.
14678	4	36	27	36	27	THIS IS NOT A REFERENCE: Parizek et al. (Submitted) [Christophe Deissenberg, Luxembourg]	Parizek et al., is under review, and will only be included in SROCC if accepted for publication.
14680	4	36	30	36	30	The Inclusion of hydrofracturing and MICI processes substantially increases the projected [Christophe Deissenberg, Luxembourg]	Agreed. Changed.
20782	4	36	30	36	30	Delete comma [Tamsin Edwards, UK]	Agreed. Comma is deleted.
20784	4	36	30	36	31	Need to make clear this statement is based on one parameterisation in one paper. i.e. "Inclusion of hydrofracturing and MICI 'parameterisations by DeConto and Pollard (2016)' substantially..." [Tamsin Edwards, UK]	The chapter team fully agrees and extensive text is devoted to the uncertainties/caveats associated with DeConto and Pollard's parameterization of ice-cliff calving.
18850	4	36	30	36	49	Chapter 3 mentions the study by Edwards et al (in review), which compares DP16 with other studies in a statistically sound way and therefore also reconciles both estimates (given that the DP16) distribution is not Gaussian and is skewed. [Frank Pattyn, Belgium]	The chapter team is aware of the detailed Edwards study, although the statement that hydrofracturing and ice-cliff calving generally increase the Antarctic sea level contribution in future sea level simulatinos remains valid.
20786	4	36	34	36	34	model physics' should read 'model parameters' as this is what is calibrated, not structural changes / explicitly modelled processes. [Tamsin Edwards, UK]	Agreed. This has been changed.
4356	4	36	36	36	36	see previous comment about MICI and LIG [Peter Clark, USA]	See response to comment 3452.
8556	4	36	36	36	36	Check "however" usage. Maybe no ";" after "however", and no comma before. [APECS Group Review, Germany]	Agreed. This has been reworded.
14682	4	36	36	36	37	Targets. However, their simulations do not explore the full parameter space and their simple statistical treatment of ensemble results doesn't provide a [Christophe Deissenberg, Luxembourg]	Agreed. Wording had been changed accordingly.
3080	4	36	36	36	38	Kopp et al. (2017) is a more focused dicussion of this issue than Horton et al. (in press). R. E. Kopp, R. M. DeConto, D. A. Bader, R. M. Horton, C. C. Hay, S. Kulp, M. Oppenheimer, D. Pollard, and B. H. Strauss (2017). Implications of Antarctic ice-cliff collapse and ice-shelf hydrofracturing mechanisms for sea-level projections. Earth's Future 5, 1217-1233. doi: 10.1002/2017EF000663. [Robert Kopp, USA]	Agreed. The reference has been changed.
20788	4	36	37	36	37	Cite Edwards et al. (2018) for 'their simulations do not explore the full parameter space' as the study gives detailed info on this. [Tamsin Edwards, UK]	A place holder for Edwards et al., (2018) has been added as has Kopp et al., 2017.
8558	4	36	38	36	38	Maybe use "(e.g. Horton et al., In Press)" in this citation [APECS Group Review, Germany]	This reference has been added. Also see comment 20788 above.
3082	4	36	39	36	39	Given the asymmetry of the Deconto and Pollard ensembles, mean is not necsarily a great summary statistic. [Robert Kopp, USA]	Agreed, this has been noted and updated with DeConto et al., (2018) which reports the finding using the median.
8560	4	36	39	36	39	E1a Insert "GMSL" after the ranges given here [APECS Group Review, Germany]	Agreed. Fixed.
14684	4	36	39	36	39	0.64–1.14 m by 2100. [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.

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4358	4	36	40	36	40	The high-end emission scenario in Clark et al. (2016) has very similar profile as RCP8.5. As an assessment, you can extract their results for Antarctica at 2500 from their supplementary information. [Peter Clark, USA]	Agreed. This paper is included in the assessment within the context of Antarctica's long-term future.
8562	4	36	42	36	43	The phrase "implying a much reduced … under strong migraytion" refers just to RCP2.6, right? The phrase should be changed to avoid a confusion; maybe remove it, or link it better to RCP2.6. [APECS Group Review, Germany]	Agreed. This is a good suggestion and mitigation in this case is now linked more explicitely to RCP2.6 to avoid confusion.
14686	4	36	43	36	49	probability of extreme sea level rise from Antarctica under strong mitigation. A few individual RCP2.6 ensemble members simulate up to 0.5 m of sea level rise by 2100, mainly through the rapid retreat of the Thwaites Glacier. This result reinforces the uncertainty about the sensitivity of this major outlet glacier to warming reported in other studies (Cornford et al., 2015; Nias et al., 2016; Seroussi et al., 2017). DeConto and Pollard (2016) study lacks a quantitative calibration with present-day retreat rates. There is also a large uncertainty in their SMB model and on the onset of surface meltwater production and sub-ice melt. However, it does point to the potential for physical processes not considered by AR5 to strongly impact future rates of GMSL rise. [Christophe Deissenberg, Luxembourg]	Agreed. This text has been modified accordingly.
8564	4	36	46	36	49	This sentence is too dismissive of DeConto and Pollard (2016) and reads like a criticism of that study for focusing on one novel piece. Instead, the contribution should be highlighted, not the details of the shortcomings. I suggest recasting the sentence something along the lines of "DeConto and Pollard highlighted the potential for physical processes…… but to date, such processes have not been implemented in detailed predictive ice-sheet models that assimilate present-day retreat rates, use sophisticated SMB routines, sub-ice-melt, etc." [APECS Group Review, Germany]	This section of text has been modified in keeping with the spirit of the comment. We also note that DeConto et al., 2018 offers an update that addresses a number of the caveats stressed in the FOD.
11718	4	36	46	36	49	Agreed. [John Church, Australia]	Acknowledged.
4360	4	36	48	36	48	you need to assess this by comparing to Trusel et al. (2015), which with a superior SMB model (RACMO) and ensemble of CMIP5 results arrives at significantly less surface melting than DP2016, which would suggest low confdence in the DP2016 results for 2100 from the MICI mechanism. [Peter Clark, USA]	The chapter team fully agrees. This issue is directly mentioned. We also note an update to DeConto and Pollard (2016) that improves on its SMB forcing, which is much closer to that of Trusel et al. (2015).
8566	4	36	49	36	49	The physical proceess not considered in AR5 are hydrofrature, ice cliff collapse and Marine Ice Cliff Instability (MICI), right? Maybe these could be in brackets in the end of the phrase like "(i.e. hydrofrature, ice cliff collapse and MICI)". It will make it clearer in this phrase. [APECS Group Review, Germany]	Agreed. We now mention these processes directly here, and point the reader to the cross chapter box in chapter 3 that illustrates these processes.
20790	4	36	49	36	49	Edit 'for ice cliff failure, which was not considered by AR5, to be" to clarify the difference between cliff failure (which has strong theoretical foundation) and the hypothesised MICI feedback deriving from cliff failure. [Tamsin Edwards, UK]	Agreed. Also see comment 8566 above.
8568	4	36	52	38	4	Check the place of BOX 4.1 such that its caption will start in the begging of a page. [APECS Group Review, Germany]	This box has been moved to Chapter 3 (cross-chapter box).
6254	4	36	54	37	24	This box is not well structured and written. MICI is mentioned early but not explained. It should be described in detail. While it should be explained it should be tuned down in the entire chapter given that this is only based on two papers by the same author, where it is implemented in a somewhat rough ad-hoc manner. This process receives far too much attention here compared to many other processes that also operate on ice sheet change. [Regine Hock, USA]	The potential for ice-cliff calving to rapidly deliver Antarctic ice to the ocean is a new realization since AR5, now appearing more often in the litterature (Bassis et al., (2017); Wise et al., (2017); Pattyn (2018). The chapter team thinks this warrents significant attention. A new cross-chapter box has been added to Chapter 3 to introduce these concepts earlier in the SROCC.

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1920	4	36	56	37	6	It seems appropriate to mention that this increased melt and rainfall is expected to warm and soften ice sheets/shelves. One study suggests that this process could decrease Greenland ice sheet volume by 5+/-2% (or 33+/-18cm SLE) within five centuries. These particualr mechanism, cryohydrologic warming (AR5 section 4.4.4.2.2) is not included in the conventional models from which this text is differentiating emerging proceses (Colgan al., 2015, Earth Future, Considering therm-viscous collapse of the Greenland Ice Sheet) [William Colgan, Denmark]	Cryohydrologic warming is now mentioned in the text as an emerging processes requiring more study. The chapter team assesess the potential sea-level contribution from this process as relatively small (Ocm per century) but highly uncertain on the timescales being considerd by SROCC. With that said, thermoviscous processes could certainly be important on longer timescales and this is now mentioned. Relating these processes to future climate scenarios in light of uncertain trends in englacial hydrological regimes makes the sea level contribution of thermo-viscous collapse particularly uncertain.
18852	4	36	56	37	6	Detailed bed topography is also very important in controlling MISI. A paper that demonstrates this nicely is Waibel et al (2018). However, even if the paper did not make it to the deadline, it has been stated in many other publications. [Frank Pattyn, Belgium]	The chapter team agrees. Bed topography's control on MISI is mentioned in several places in chapter 4, in the cross-chapter box on MISI and MICI, and is highligheted in the schematic figure of Thwaites Glacier. Waibel et al (2018) has been added to the reference list.
4362	4	37	1	37	1	"retreats down a retrograde bed" [Peter Clark, USA]	Agreed. Changed.
8570	4	37	1	37	2	The text would sound less repetitive if we replace: This increase in seaward ice flow ... by The increase in seaward ice flow [APECS Group Review, Germany]	Agreed. Changed.
8572	4	37	2	37	2	"MICI" could be also defined in text in the BOX 4.1, similar was was done for MISI. [APECS Group Review, Germany]	MISI and MICI are now explained in detail in a dedicated cross chapter box in chapter 3.
4364	4	37	3	37	4	This statement re: Trusel et al. results is inaccurate and needs to be revised. Their results (Fig. 4) only show a significant increase in surface melting on ice shelves for the Antarctic Peninsular, but elsewhere they only report an average of 5-10% increase in area of ice shelves that exceed the inferred threshold for ice-shelf collapse from hydrofracturing. [Peter Clark, USA]	The chapter team agrees. This statement had been revised as recommended.
20792	4	37	3	37	5	The statement about ice shelves needs to be more equivocal, as Kuipers Munneke et al. (2014) predict later ice shelf collapses - mostly next century. [Tamsin Edwards, UK]	Agreed. See comment 4364 above.
14688	4	37	5	37	5	would persist despite the seaward [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.
20794	4	37	5	37	5	"might" persist - ice cliffs might only partially collapse, leaving behind cliffs below the critical height, and might also be buttressed by melange. [Tamsin Edwards, UK]	The word "might" has been added. However, based on observed calving rates at tall ice fronts like Helheim and Jakobshavn despite extensive and thick melange, and theoretical and modeling considerations (Pollard et al., 2018), malange is assessed here to be of secondary importance. Partial collapse of marine-terminating ice fronts are not observed. Furthermore, at high stresses, brittle failure increases with increasing stress much faster than viscous processes. When the Larsen B collapsed in 2002, it exposed a calving ice front at Crane Glacier that persists today.
20796	4	37	5	37	5	would' operate collectively, as this remains a hypothesis. [Tamsin Edwards, UK]	Agreed. This text has beenn changed to better reflect uncertainty.
13094	4	37	8	37	19	This is a bit repetitive. Almost the same text is in P 36 line 30-49 [Gerhard Krinner, France]	Agreed. The discription of MICI has been moved to a cross chapter box in chapter 3, which should help eliminate repetition.
20798	4	37	13	37	13	"Possible geophysical evidence of past 'cliff collapse'.." - iceberg keel marks can only provide evidence on this, not on the nature of a feedback. [Tamsin Edwards, UK]	Agreed. This has been rephrased.
14690	4	37	14	37	14	(Wise et al., 2017). However, this interpretation is based on a single study and speculative. [Christophe Deissenberg, Luxembourg]	This wording had been changed.

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20800	4	37	14	37	14	Grammar - replace comma with colon [Tamsin Edwards, UK]	See comment 14390 above.
4366	4	37	16	37	16	see previous comment on this [Peter Clark, USA]	This text has been changed to reflect an updates to the studies in the FOD.
20802	4	37	16	37	17	Edwards et al. (2018) demonstrates that MICI is not required to facilitate simulation of the Pliocene and LIG. [Tamsin Edwards, UK]	The chapter team is aware of the statistical approach used by Edwards et al (2018). Updated modeling by DeConto et al., (2018) demonstrate the inability of an ice sheet model to capture Pliocene-like retreat without hydrofracturing and ice-cliff calving, although this does not rule out other possible mechanism (Coulomb-friction sliding in the grounding zones for example). This text has been adjusted in light of the rigourous statiustical modeling of Edwards et al. (2018) and updated results of DeConto et al., (2018).
20804	4	37	19	37	19	MICI "may have" the potential' [Tamsin Edwards, UK]	The chapter team thinks "may have" along with "potential" is redundant.
8574	4	37	22	37	25	Box 4.1 Figure 1. suggestion to the design of the top figure: remove calving below melt, it might be confusing if underwater calving is not specify. [APECS Group Review, Germany]	The discription of MICI has been moved to a cross chapter box in chapter 3 using an updated figure.
8576	4	38	7	38	10	The following statement can be remove: “While these simulations point to the potential for a far greater contribution to sea level than other studies, particularly on longer time scales, deep uncertainty remains.” The uncertainty issue has been mentioned in the paragraph before box 4.1 (page 36 lines 46-49: DeConto and Pollard (2016) study lacks quantitative calibration with present-day retreat rates, there is large uncertainty in their SMB model …). Also in box 4.1 the uncertainty on the MICI hypothesis is also mentioned. If the text is remove, there would be a better connection between pages after reading box 4.1. [APECS Group Review, Germany]	The chapter team agrees that repetition should be avoided where possible. DeConto et al., (2018) provide an update to DP16 that includes calibration to modern rates of mass change while using a new SMB model, so the text associated with this comment has been modified substantially.
4368	4	38	10	38	10	make it clear that you are referring to DP2016 [Peter Clark, USA]	Agreed. Fixed. Also see response to comment 8576 above.
4370	4	38	13	38	13	no such thing as saturated meltwater [Peter Clark, USA]	Agreed. Typo has been fixed.
14692	4	38	16	38	17	Kingslake et al., 2017) but surface meltwater was a precursor for the ice shelves which have collapsed (Scambos [Christophe Deissenberg, Luxembourg]	This sentence has been rearranged.
3784	4	38	18	38	18	change to "exclude" [Ola Kalen, Sweden]	Agreed. Also see comment 20806 below.
13096	4	38	18	38	18	Unclear: "methods to exclude" or "methods excluding" ? [Gerhard Krinner, France]	Agreed. Also see comment 20806 below.
20806	4	38	18	38	18	Edwards et al. (2018) only exclude MICI, not hydrofracturing. [Tamsin Edwards, UK]	Agreed. This has been changed.
20808	4	38	18	38	20	Either say 'previous studies' instead of Ritz et al. (2015), or add citations to Little et al. (2013), Levermann et al. (2014) and Ruckert et al. (2016), with which the No-MICI case is found to be consistent by Edwards et al. (2018). [Tamsin Edwards, UK]	Agreed. This text has been changed and references Edwards et al., (submitted).
328	4	38	18			methods to exclude hydrofracturing [Kerstin Jochumsen, Germany]	Agreed. Also see comment 20806 below.

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2754	4	38	22	38	27	The feedback consisting of the injection of freshwater into the Southern Ocean is only one aspect of what is missing. An important aspect that is missing and that could completely change the ice-shelf melt response to anthropogenic forcing is a representation of the under-ice-shelf ocean cavity that connects the Southern Ocean and ice-shelf melt with many feedbacks. See the following papers: [1] Donat-Magnin, M., Jourdain, N. C., Spence, P., Le Sommer, J., Gallée, H., & Durand, G. (2017). Ice-Shelf Melt Response to Changing Winds and Glacier Dynamics in the Amundsen Sea Sector, Antarctica. Journal of Geophysical Research: Oceans. [2] Hellmer, H. H., Kauker, F., Timmermann, R., & Hattermann, T. (2017). The fate of the southern Weddell Sea continental shelf in a warming climate. Journal of Climate, 30(12), 4337-4350. [Nicolas Jourdain, France]	The chapter team agrees. These references highlight the need for more explicit, two-way ice-ocean-wind coupling. This point is also made in a previous section. These references have been added.
8578	4	38	25	38	25	What is "1.5 Sv"? This should be defined in the text [APECS Group Review, Germany]	Sv is the unit Sverdrup, an ocean volume flux of 10^6 m^3 s^-1. This is now defined.
4372	4	38	27	38	27	"trajectory" not the best word here [Peter Clark, USA]	Agreed. Replaced with "evolving".
14694	4	38	30	38	30	atmospheric and ocean forcing and glacial hydrology, there [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.
4374	4	38	34	38	34	The general discussion on MICI is getting disjointed and too long - needs major rewrite [Peter Clark, USA]	The chapter team agrees. MICI is now described in a more concise cross-chapter box in Chapter 3.
20810	4	38	34	38	34	"MICI mechanism" should read "MICI hypothesis", and edit next part to "and the MICI parameterisation by DeConto and Pollard (2016) would add substantially to..." [Tamsin Edwards, UK]	This text has been reworded, and relies on a cross-chapter box for the basic description of MICI.
14696	4	38	37	38	39	processes, including among others the stress regime at the ice front, the water depth, ice thickness, flow speed, conditions at the bed of the ice, pre-existing crevasses, lateral shear, undercutting of the calving face, and tides. [Christophe Deissenberg, Luxembourg]	The chapter team prefers the original wording.
12588	4	38	38	54	55	The ranges quoted here are different from AR5. [Dewi Le Bars, Netherlands]	This text has been reworded, and now relies on a cross-chapter box for the basic definition of MICI. A substantial attempt is made to stress the uncertainty in the potential for this hypothesized mechanism to contribute to sea-level rise.
14698	4	38	42	38	42	is shown to have little [Christophe Deissenberg, Luxembourg]	Agreed. Fixed.
20812	4	38	42	38	42	The 'is shown' should be replaced with 'is found by Pollard et al. (2018)' to make it clear this is one (new) model. [Tamsin Edwards, UK]	Agreed. This text has been reworded.
13600	4	38	42	38	47	Add 'to' before 'have little impact' [Debra Roberts and Durban Team, South Africa]	Agreed. Fixed.
330	4	38	42			sentence is unclear, restructure [Kerstin Jochumsen, Germany]	Agreed. The sentence has been restructured.
14700	4	38	46	38	46	2008). However due to the general lack of observations and to a mechanistic, process [Christophe Deissenberg, Luxembourg]	Agreed. This sentence has been broken up as recommended.
4376	4	38	50	38	51	Based on your summary of these studies other than DP2016, I do not agree with this assessment that they "vary considerably" in their projections. Th outlier is DP2016 because it includes MICI, but as argued above, this has to be assessed with low confidence because the SMB results differ significantly from Trusel et al. [Peter Clark, USA]	The results by DP2016 are not used at all anymore because of the high surface melt rates

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4378	4	38	52	38	52	Saying they represent a "considerable departure" from AR5 is inaccurate. AR5 did not explicitly include a contribution that accounted for MISI (no reliable literature on this at the time), but as you know, they did provide the vry important caveat that IF MISI was triggered this century, it would likely contribute a few tenths of a meter. This is right in line with the MISI studies that have aince been published. [Peter Clark, USA]	we agree with the reviewer the difference is that we now don't have to make the caveat anymore because we have a few studies which quantify MISI.
20814	4	38	52	38	52	This is not correct. Ritz et al. (2015) is only a few centimetres higher than AR5. The current statement compares the R15 95th percentile with the top of the AR5 likely range, which is not a correct comparison. The Golledge et al. results are 10 cm and 39cm, with great uncertainty about which melt parameterisation is more realistic, and the lower number is consistent with AR5. Note that DeConto and Pollard (2016) without MICI is also consistent with AR5 for RCP4.5 (Edwards et al., 2018). [Tamsin Edwards, UK]	the idea was to express that we don't have to make the caveat of "if MISI was triggered" based on the newer studies. The section has been rewritten completey
3084	4	38	52	38	53	This does not seem inconsistent with the statement in the text of AR5 about the potential for Antarctic dynamics to contribute several decimeters above the likely range. [Robert Kopp, USA]	this statement disappeared from the newly written section
14702	4	38	52	38	53	as they demonstrate the possibility for RCP8.5 of 0.3 m or more of sea level rise from the Antarctic by 2100. [Christophe Deissenberg, Luxembourg]	this sentence disappeared
4380	4	38	54	38	55	These are the wrong numbers from Table 13.5 - you want the numbers from the row immediatey above [Peter Clark, USA]	accepted sorry for the confusion
4382	4	38	56	39	3	Rather than just provide this quote with no context, the authors need to discuss this in their own words and emphasize the significance of it with regard to their assessment, with main point being that it is consistent. [Peter Clark, USA]	we have rewritten the section and tried to clarify that the SROCC projections are an expansion of the AR5 as there is new literature to assess.
8580	4	39	3	39	8	Missing a "secondly" explanation in the body of the paragraph (there is just a "firstly"explanation in this paragraph). [APECS Group Review, Germany]	accepted firstly has been removed
4384	4	39	4	39	4	there is no "secondly" [Peter Clark, USA]	accepted firstly has been removed
20816	4	39	4	39	4	Agree there is scenario dependence but not that it is "strong". Little et al. (2013), Levermann et al. (2014), Ritz et al. (2015), Golledge et al. (2015) and Ruckert et al. (2016) and DeConto and Pollard (2016) without MICI all have upper bounds of ~10-20cm for RCP2.6 and ~30-40 cm for RCP8.5 (Edwards et al, 2018). Lower bounds are mostly -10 to +10cm. Only DeConto and Pollard (2016) with MICI shows strong dependence. [Tamsin Edwards, UK]	accepted strong is maybe to strongly expressed we rephrased this to cosniderable
4386	4	39	6	39	6	Saying "in no longer justified" makes it appear that AR5 had a choice to assess re: scenario or not, but this is not the case - there was no literature available to assess this possibility. Please revise accordingly. [Peter Clark, USA]	accepted it said no longer jusitfied directly after no literature available implying a causal relation, so we see nothing wrong in the phrasing nevertheless we changed justified in need to express more clearly that AR5 was limited in the assessment by the lack of literature at this point
17840	4	39	6	39	7	It would be good to provide a reminder of the time horizon of interest here (e.g. 2100). Slow retreat that goes on for a long time would eventually lead to a large sea level rise. [Robert Arthern, UK]	accepted we added that we are t discussing the 21st century
4388	4	39	8	39	8	should have a table summarizing these results [Peter Clark, USA]	accepted the different studies are tabulated in Table 4.2 reference is made to the Table now
4390	4	39	8	39	8	larger than what? [Peter Clark, USA]	accepted than for the lower emission scenarios

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20818	4	39	8	39	8	See comment line 4. I would not say a difference of ~20cm (across probabilistic estimates and Golledge et al.) was "considerable". It is only DeConto and Pollard (2016) with MICI that is the outlier in showing strong scenario dependence. [Tamsin Edwards, UK]	see comment 20816
14704	4	39	10	39	10	is based, first, on averaging [Christophe Deissenberg, Luxembourg]	sentence has been removed
5146	4	39	10	39	12	The meaning is not clear. Do you mean comparing the results of Golledge et al. (2015) and Ritz et al. (2015) for the A1B scenario? What results are in good agreement? [Sai Ming Lee, China]	accepted yes the interpretation is correct the text has been rephrased to clarify
18854	4	39	10	39	15	It is not just an alternative sub-grid interpolation, it is based on applying sub-shelf melt on partially grounded grid points and should be specified), which, given the large sensitivity, is debatable. It is therefore also debatable whether both solutions can be averaged. [Frank Pattyn, Belgium]	accepted we added the caveat. As the paper is not giving a clear preferenc to one or the other simulation we used the average of the two simulations with an important correction as explained in the header of Table 4.2 where we corrected the sea level contribution to the net contribution over the 21st century.
17844	4	39	10	39	20	This methodology seems inadequate and arbitrary at the moment. Whatever further guidance is provided will need to have the rationale behind it explained much more clearly. This is essentially where the probability of a MICI style collapse as described by Deconto and Pollard (2016) is being assessed. It is also where judgements are made as to how much weighting to give each of the Golledge melt rate parameterisations. The evidence that these judgements are based on will need to be made much more transparent. [Robert Arthern, UK]	accepted DP2016 is not used any longer and the assesment is now based on equal weighing of DeConto et al. submitted and Golledge 2015
22264	4	39	10	39	20	I am interested to see how this section changes in the second order draft after additional studies are considered. In the current version, I'm not convinced that the agreement of two alternative parameterization scenarios from a single study (Golledge et al., 2015) warrants support for weighting that study more heavily than others, such as DeConto and Pollard, 2016. [Andra Garner, USA]	accepted with the new study by DeConto we have given equal wait to Deconto et al. submitted and Golledge et al. 2015 and dismissed Deconto and Pollard, 2016
4392	4	39	11	39	11	I think you mean RCP8.5 [Peter Clark, USA]	section has been rewritten
4394	4	39	11	39	11	what are the results? [Peter Clark, USA]	section has been rewritten
17842	4	39	11	39	11	In good agreement with what? [Robert Arthern, UK]	section has been rewritten
20820	4	39	11	39	11	Golledge et al. do not show A1B. [Tamsin Edwards, UK]	section has been rewritten
22266	4	39	11	39	11	Is it A1B scenario the correct scenario here? In the discussion of Golledge et al. (2015) on p. 435, lines 41-51, and in Table 4.2, results for Golledge are discussed for the RCP scenarios, not SRES. [Andra Garner, USA]	section has been rewritten
20822	4	39	11	39	12	Good agreement with what - each other? If so, 10 cm and 39cm cannot be judged as good agreement if ~20cm is 'considerable' difference above. [Tamsin Edwards, UK]	section has been rewritten
3086	4	39	12	39	12	Good agreement with what? [Robert Kopp, USA]	section has been rewritten
14706	4	39	13	39	13	RCP8.5. Second, we [Christophe Deissenberg, Luxembourg]	section has been rewritten
20824	4	39	13	39	15	It should be stated that the DeConto and Pollard (2016) value quoted is a mean, not median (as in the assessment) [Tamsin Edwards, UK]	value is not used any longer
1810	4	39	13	40	1	While the text on page 39 cites deConto&Pollard values without Amundsen Sea temperature corrections (64 cm for RCP8.5 in 2100), the table shows values with the Amundsen Sea corrections (79 cm for the same scenario). [Sybren Drijfhout, Netherlands]	section has been rewritten

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3088	4	39	14	39	15	Why the focus on the ensemble mean? Nothing in the analysis of DP16 indicates that the mean is more likely than any other vaue in their ensemble. What about bias correction in DP16? [Robert Kopp, USA]	DP16 is not used any longer
8582	4	39	16	39	16	Suggestion for the words: highly uncertain, to put in italics. [APECS Group Review, Germany]	DP16 is not used any longer
3090	4	39	18	39	18	Why specify one sigma when already stated that this is a likely range? [Robert Kopp, USA]	section is rewritten
8584	4	39	18	39	18	What is the mean of "one sigma uncertainty". Maybe change it to another expression, to make it clearer. [APECS Group Review, Germany]	section is rewritten
20826	4	39	19	39	20	Why are other studies not included in the expert assessment? [Tamsin Edwards, UK]	the section is rewritten we started with the four process based studies mentioned in Table 4.2 and explained that Ritz could not be used in the quantitative assessment because it has only done A1B, qualitatively it is in agreement with with Deconto et al. (submitted) and Golledge et al. 2015. The outlier is DP2015 where the surface melt is considered to be to high hence the quantitative assessment is based on Golledge et al. 2015 and DeConto et al. (submitted)
17846	4	39	21	39	27	According to Ritz et al.(2015) 'Our process-based, statistical approach gives skewed and complex probability distributions (single mode, 10 cm, at 2100; two modes, 49 cm and 6 cm, at 2200).' [Robert Arthern, UK]	accepted we agree that Ritz et al 2015 and Bamber et al. (submitted) suggest skewed distribution but Golledge et al. 2015 and DeConto et al. (submitted) do not provide evidence for a skewed distribution, so we don t exclude it but we cannot assess it quantitatively
4396	4	39	22	39	22	which studies? [Peter Clark, USA]	Bamber et al. (submitted)
8586	4	39	22	39	22	In the sentence: “Some studies based on expert elicitation point to a non-Gaussian distribution particular after 2100, but process based and observational studies like Ritz et al. (2015) and DeConto and Pollard (2016) do not provide compelling evidence for this.” There are no citations in the paragraph or examples for such studies based on expert elicitation. [APECS Group Review, Germany]	Bamber et al. (submitted)
20828	4	39	22	39	24	This is incorrect. DeConto and Pollard (2016) ensemble data are strongly non-Gaussian, with different modes (low, high, or both) depending on the bias correction and Pliocene calibration, and the main finding of Edwards et al. (2018) confirms the underlying probability distribution is skewed (low mode) and highly non-Gaussian for RCP8.5. [Tamsin Edwards, UK]	DP16 is not used any longer
18856	4	39	23	39	23	remove 'observational', because both studies are process-based studies. Or precise if what is meant by observational (maybe it is meant that models are initialized with observations). Anyway, not clear. [Frank Pattyn, Belgium]	accepted observational is removed
3092	4	39	23	39	24	If you look at DP16 w/ bias correction in 2080, when we have a median of ~0.3 m, we get a 17th/50th/83rd of 16/34/67 cm -- this is significantly asymmetric [Robert Kopp, USA]	DP16 is not used any longer

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4398	4	39	25	39	25	it would be important to separate out the SMB from ice dynamics contributions [Peter Clark, USA]	In principal we agree with this remark, but here we base our quantitative assessment on two dynamical studies for which this separation is not explicitly made and furthermore an experiment with no adjustements in the flow would not fulfill the calibration constraints, so there is not enough literature to make the assesment
14708	4	39	26	39	26	too uncertain to be included in the likely [Christophe Deissenberg, Luxembourg]	accepted rephrased
3786	4	39	30	39	30	explanation to abbreviation BMB in table is lacking [Ola Kalen, Sweden]	accepted and added
3094	4	39	30	39	35	Ocean forcing also contributes to retreat in DP16 [Robert Kopp, USA]	accepted rephrased in the Table, it was meant to express that atmospheric forcing dominates
5148	4	39	30	39	35	Table 4.2 does not include other studies mentioned in Lines 20-22 on Page 35, i.e. Levermann et al. (2014), Winkelmann et al. (2015), Clark et al. (2016). Please include them if possible. [Sai Ming Lee, China]	In order to estimate the sea level contribution for the 21st century we need studies which are forced by the RCP scenarios and contain both an atmospheric and ocean forcing. The work by Levermann et al. is used to support the lower boundary condtion because it only has an ocean forcing. The work by Clark et al. is focussed on the long term. The uncertainty in the physics is too large to allow an assessment on these time scales. These points are hopefully better expressed in the new version of this section.
8588	4	39	30	39	35	Legends for SMB, BMB etc could be inserted in Table 4.2 caption. [APECS Group Review, Germany]	accepted and added
11722	4	39	30	39	35	It is important to consider both changes in surfce mass balance and MISI, and not just MISI. Also I think this table actually compares results for mdels that include SMB changes and those that do not. [John Church, Australia]	accepted we phrase it more accurately now. Ritz et al is ice dynamics only, the other three are total mass balance. However the differenc is vey small (e.g. 2 cm for A1B in 2100).
20830	4	39	35	39	35	This is incorrect, as Ritz et al. does include hydrofracturing- see earlier comment - and does not include SMB changes in the future. [Tamsin Edwards, UK]	see comment 11722
12594	4	39	39	10	20	The choice of Antarctic contribution is the most important for the projections, the discussion should be more substantial. Why using Golledge et al. 2015 and not Levermann et al. 2014 or Ritz et al. 2015? Why using the case without ocean temperature bias correction from DeConto and Pollard 2016 in this discussion while the case with bias correction is used in the table 4.2? Golledge et al. 2015 do not consider the A1B scenario. How is the uncertainty chosen? Standard deviation of 0.1 means that the DeConto and Pollard projection assessed here of 0.64 in 2100 is three standard deviation away from the expected value. Sofor a normal distribution it means it practically impossible.This is not in accordence with the text that says it the new processes are uncertain. Why are other scenarios not discussed here? Choices have been made for them as well. Also how are these subjective choices made? There is a lack of tracability about the method used to take these important decisions. Is it the average best guess of the authors? Which authors? Is it the result of a discussion at the end of which all authors agreed with that number and uncertainty? [Dewi Le Bars, Netherlands]	We understand that the process to arrive at an Antarctic contribution of 0.3+/-0.1 was unclear. We have improved this now considerable by a rewrite of the section. The quantitative assessment is now based on equal weighing of Golledge and DeConto et al. (submitted) because those are the only studies providing all three RCP scenarios. DeConto and Pollard is dismissed because of the too high surface melt. Leverman is incomplete as it is an ocean only experiment and Ritz only provides A1B and only ice dynamics. Any assessment is done with the consensus of all authors
12596	4	39	39	10	20	The discussion focussed on the date of 2100 but then the result of 0.3m applies to the verage period of 2081-2100. This is not consistent. [Dewi Le Bars, Netherlands]	The difference between 2081-2100 is captured in the numbers in Table 4.3.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
12598	4	39	39	22	24	The Gaussian choice is not a good representation of the uncertainty. Levermann et al. 2014 and Ritz et al. 2015 have positively skewed distributions. On the other hand, there are no "compelling evidence" that the uncertainty is Gaussian. In particular this uncertainty should represent the current state of the litterature, given the large numbers from DeConto and Pollard while there are no scenario projecting Antarctica will accumulate 1m of equivalent sea level in the comming century. This is enough evidence to use a skewed distribution. In other words, everybody would agree that the probability of very large mass loss is larger than the probability of very large mass accumulation. Simply because of the physical processes involved that have different time scales. [Dewi Le Bars, Netherlands]	We base our new quantitative assessment on Golledge et al, which has no proper uncertainty estimate and DeConto et al. (submitted) which has no indication for a skewed distribution. Hence it might be that the distribution is skewed but at present there is no way to quantify this. The argumentation along the lines of the physical processes fails because there is also parameter uncertainty and calibration with observations involved effecting the accepted ensemble members. The skewed distribution discussion has been sharpened in the text
12590	4	39	39	30	30	The caption says "the MISI contribution to sea level rise" but DeConto and Pollard is also in which have MICI as well. [Dewi Le Bars, Netherlands]	accepted and rephrased. The Table indicates mainly the SLR of Antarctica, for Ritz it excludes SMB which is however only a few cm. So the studies are more or less comparable.
4156	4	40	1	47	29	I found the structure of section 4.2.3 Projections of Sea Level Change a little hard to follow. For example, the discussion of advances in modelling of ice sheets takes place before a general introduction of what sea level components feed into projections of global mean sea level (GMSL) change. It would seem more logical to briefly appraise the terms that were included in GMSL projections in AR5 before moving on to describe how each term was (or is?) estimated. Similarly, the steps needed to provide regional sea level projections are discussed only after the introduction of probabilistic projections. Many regional projections exist that have not made use of a probabilistic framework. I would suggest that the components of GMSL are discussed upfront, and then include a short section on how these global projections (in general) are regionalised. In my view, this "overview" information shoudl be presented before getting into details of invidual components (such as ice sheet dynamics). In terms of the ESL projections, I think it would be helpful to the reader to differentiate between projected changes in the time-mean water level and changes in the drivers of extreme water levels. It would also be useful to give some examples form the literature about the relative importance of these two aspects, for example Cannaby et al (2016, https://doi.org/10.5194/os-12-613-2016) and Howard et al (2014, https://doi.org/10.5194/os-10-473-2014). [Matthew Palmer, UK]	We have improved the text and hope that the structure is more clear now
17360	4	40	3	40	3	Consider adding a similar table for Paleo estimates of SLR in the past, including temperature and SL ranges. This information is in the text but a table would be useful for overview. [Pamela Pearson, USA]	Rejected this section is on future projections
17632	4	40	3	40	3	Where's the discussion of Glaciers and ice cap projections? There have been updates since AR5....especially Radic et al 2014 and... [Jonathan Bamber, UK]	rejected, Radic et al. 2014 is captured in AR5 so there are no major changes with respect to AR5 with respect to glaciers

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4400	4	40	5	40	7	This is not an appropriate topic sentence for this paragraph, and not sure why this point is even made. [Peter Clark, USA]	What we wish to express is that there are two points explaining the difference between AR5 and the SROCC report. 1. We have for the 21st century a more complete estimation of the Antarctic contribution, hence SROCC values are higher for RCP8.5.2. This extra component has a large uncertainty hence the total uncertainty in GMSL is larger in SROCC than in AR5 for RCP8.5. The text has been improved to clarify this.
14710	4	40	6	40	7	(Church et al., 2013). They are more uncertain as well. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
17634	4	40	8	40	8	see previous comment [Jonathan Bamber, UK]	see 17632
4402	4	40	14	40	14	more information needed: (1) Total-Antarctica AR5 is not very clear, (2) will you have some supplementary explaining how youderived new Antarctic numbers? [Peter Clark, USA]	Total - Antarctica AR 5 is the total in Table 13.5 of AR5 minus the Antarctic contribution. This is explained more clearly in the
3096	4	40	14	40	17	The likely range for Antarctica in RCP 8.5 in 2081-2100 in the table is not concistent with the text. [Robert Kopp, USA]	the section has been rewritten and the calculation as well
3098	4	40	14	40	17	What is meant by "outside likely range" for GMSL in 2200? [Robert Kopp, USA]	see comment 1818 text has been removed
8590	4	40	14	40	17	Legeneds for SMB, DYN, LWS etc could be insert in Table 4.3 caption. [APECS Group Review, Germany]	accepted and corrected
8592	4	40	14	40	17	In Table 4.3, the composition of GMSL could be more clearer. Maybe the caption could explain better it. [APECS Group Review, Germany]	accepted, caption extended
1812	4	40	14	40	18	Numbers for the first 5 terms deviate from AR5, while they are said to duplicate AR5. The total – Antarctica AR5 has much smaller uncertainty ranges then AR5 because, unlike AR5, uncertainties are summed-up quadratically, assuming independence for all terms. This deviation from AR5 is not mentioned, nor motivated. [Sybren Drijfhout, Netherlands]	Numbers are derived from the supplementary material of AR5 and are except of a rounding difference identical to the values in Table 13.5, so we don't understand the first part of the remark. We made one typo in the uncertainty range of the LWS term, this has been corrected but we are not sure whether that is what the reviewer means. The reason that the uncertainty in the total - antarctica AR5 is smaller than the Antarctica AR5 is that the Ant SMB and Ant Dyn are excluded from the sommation and they contribute significantly to the total uncertainty in the AR5 value, hence a value without Antarctic contribution has a much lower uncertainty. If we add those term quadratically to the AR5 - Antarctica value we end up with uncertainties which are nearly identical to the AR5 table again, implying that the Antarctic term is nearly independent of the other terms in AR5. As the total AR5 covariance matrix is not available, there is also no other way to do it. In the end it leads to an underestimation of the uncertainty in the total - antarctica which is at max 2.8 cm to low. For the end result this is negligible as the new uncertainty in the antarctic contribution is very large and thereby overrules the total uncertainty in GMSL. A small caveat below the Table is made to express this.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1814	4	40	14	40	18	The independence assumption is inconsistent with the text and estimates showing dependence on scenario, and hence, temperature, for all components apart from LWS. The dependence/ independence assumption should be explicitly discussed and the assumptions that especially Antarctica DYN (or total) is independent of temperature/scenario can no longer be justified given the choices made. The same seems to hold for glaciers, Greenland SMB, and perhaps Greenland DYN. Accounting for dependence would significantly widen the likely range, without affecting the median values. See also Le Bars et al. (2017) for a discussion in how to deal with uncertainty and dependence of processes and Le Bars (2018): https://eartharxiv.org/uvw3s [Sybren Drijfhout, Netherlands]	we agree that we are now in the position to make an Antarctic contribution which is scenario dependent because we have two model studies which we can use for this purpose. These models include processes that are supposed to include instability mechanisms, hence we consider it to be too much of a shortcut to assume that the Antarctic contribution which is ruled by the ice dynamics is correlated to the other components of sea level rise. Results depends on a zoo of processes with many time scales. In the end the report is an assessment of existing literature and there is no literature to our knowledge which convincingly shows this. The suggested paper introduces a method to take the dependency into account, but this is based on the ad-hoc assumption that there is a correlation between global mean temperature change and the Antarctic contribution in 2100, but this does not follow from physical model simulations.
1816	4	40	14	40	18	It is hinted that the Antarctica estimates are a combination of the Colledge and deConto&Pollard estimates, without making the weighting of these two estimates explicit. Without doing this the process is neither accountable nor transparent. Please explain what you have done. [Sybren Drijfhout, Netherlands]	accepted this has been improved we use an equal wieghting between Golledge 2015 and DeConto (in prep.)
1818	4	40	14	40	18	The 2200 values in Table 4.3 are non-informative and the comment “Outside likely range” is unclear in what is meant. Consider giving lower bounds instead. [Sybren Drijfhout, Netherlands]	accepted we removed them from the Table they are discussed in the long term section as explained earlier in the text
5150	4	40	14	40	18	Suggest explaining how the mid-century (2046-2065) value, as well as decadal means in the 21st century, of the Antarctic Ice Sheet contribution could be obtained. Please publish all data involved in the updated assessment of Antarctica' contribution to sea level rise in an annex/supplementary materials. [Sai Ming Lee, China]	data will be made available in the supplementary information
5152	4	40	14	40	18	Please include results for RCP 6.0 if available. This would help national climate authorities to update their regional sea level rise projection. Besides, also show the original estimates from AR5 for 2081-2100 (i.e. "Total AR5; 2081-2100") for ease of reference. [Sai Ming Lee, China]	Rejected we don't have estimates for the Antarctic contribution to replace the work presented by Church et al. (2013)
5154	4	40	14	40	18	The revised GMSL for 2046-2065 cannot be reproduced by adding up "Total - Antarctica AR5; 2046-2065" and "Antarctica 2046-2065"). Please check. [Sai Ming Lee, China]	accepted and corrected
5156	4	40	14	40	18	The values (either median or likely range) of the following parameters under the three RCPs shown in Table 4.3 are different from the corresponding values shown in AR5 (Chapter 13, Page 1182, Table 13.5): Glaciers, Greenland SMB, LWS, "Total AR5; 2046-2065". [Sai Ming Lee, China]	The data in Table 4.3 follow from the official data provided by IPCC which apperently contain a few rounding differences with the Table 13.5 beside that we introduced a typo for the LWS which we corrected.
20832	4	40	14	40	18	The fact that this is based on only two of several existing studies (with no explanation or justification of the weighting between them. and not making it clear the DeConto and Pollard value is a mean but used as a median), and the fact DeConto and Pollard are non-Gaussian and Golledge et al. (2016) do not estimate a distribution, mean I find both the median and likely range assessments to be not convincing or robust. It does say that future studies will be incorporated, but not past ones, and there are too many unsupported judgements and missing caveats for the current state of assessment. [Tamsin Edwards, UK]	In the mean time a new paper is submitted which has a better surface melt scheme and is calibrated to match the IMBIE results as a consquence the DP16 data are not used any longer in the quantitative assessment. Instead we equally average Golledge et al. 2015 and Deconto (subm.)

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22268	4	40	14	40	18	Although I understand the reasoning that leads to these results, I do have some concern that the projections presented here, based on a number of studies since AR5, are too low. It looks like the results here do represent an upward revision from AR5; for example, GMSL in 2100 under RCP8.5 has moved from 0.74 (0.52 - 0.98) in AR5 to 1.06 (0.82 - 1.33) in SROCC. However, there have been a number of recent studies (many of which are discussed later in the SROCC, under the probabilistic studies section) that have predicted the potential for substantially greater values of GMSL in 2100 (e.g., Grinsted et al. (2015), Jackson et al. (2016), Bakker et al. (2017), Wong et al. (2017), Kopp et al. (2017)). Granted, many (though not all) of the estimates from these studies that exceed the SROCC values are for a range of possibilities that exceeds the "likely" range that is the focus of SROCC. However, it seems that these upper estimates, and the possibility for these more extreme values of GMSL, are worth noting at this point, or else discussing more explicitly in the probabilistic studies section of the SROCC, given the substantial number of publications that suggest possible 2100 GMSL in exceedance of 1.33 m. [Andra Garner, USA]	accepted. We are aware of the fact that there are a number of probabilistic studies yielding higher values for the tails of the distribution often based on DP16. We don't use DP16 because the underlying surface melt is considered to be too high, hence probabilistic estimates using this should also be excluded from the assessment. In addtion, the limited number of two process based studies does not allow at present for an assessment including statements on the very likely range. However, given the fact that we have 2 process-based studies including an estimate of MISI, we don't need to make the caveat "if marine ice sheet instability is inititiated there is a potential additional contribution of.."
17902	4	40	14	40	19	Suggest adding a new version of AR5 figure 13 which could illustrate the revision to the projected sea level rise budget that is summarized in this table. [Haroon Kheshgi, USA]	Figure 4.7 shows both AR5 and SROCC GMSL projections over the 21st century
17904	4	40	14	40	19	Since there are no square brackets yet in the table, this should be corrected, and greater explaination is needed in the caption as in the AR5 version of this table. [Haroon Kheshgi, USA]	accepted, caption has been extended
14712	4	40	16	40	16	Values between square brackets reflect ==> Values in parentheses ???????????????? [Christophe Deissenberg, Luxembourg]	accepted and corrected
14714	4	40	20	40	20	The results presented in Table 4.3 are used to calculate the regional RSL projections in 4.2.3.4 to ?????????????? [Christophe Deissenberg, Luxembourg]	accepted and corrected
8594	4	40	20	40	21	The phrase "Results as presented in ... sea level projections" should be checked; there are two verbs "are", and the phrase seems incomplete. [APECS Group Review, Germany]	accepted and corrected
332	4	40	20			delete "are" after projections [Kerstin Jochumsen, Germany]	accepted and corrected
4404	4	40	22	40	22	divergence in uncertainty not much different from AR5 [Peter Clark, USA]	results are updated
4406	4	40	22	40	22	should emphasize again that the higher numbers with Antarctica MISI are consistent ith AR5 caveat for "several tenths of a meter" [Peter Clark, USA]	accepted and stressed above Figure 4.7
8596	4	40	23	40	23	To make the phrase more explicit, maybe could be added to the end of the phrase: "(RCP4.5 and RCP8.5). [APECS Group Review, Germany]	accepted and corrected, though it only holds for RCP8.5 in the updated results
12592	4	40	40	14	18	What does "outside the likely range" mean? A likely range can always be big enough so that future sea level falls in. I think what is meant is that sea level is very uncertain at these time scale and that the authors do not want to quantify it. This should be more explicit. [Dewi Le Bars, Netherlands]	accepted confusing frasing has been removed

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12600	4	40	40	14	18	The uncertainty of the projections for RCP2.6 and RCP4.5 have reduced compared to AR5 because: (1) Antarctic contributions are more certain and (2) the contributors are assumed independent. None of these hypothesis are justified and the resulting decreasing uncertainty is not consistent with the message that Antarctica is more sensitive than previously believed leading to an increase uncertainty. [Dewi Le Bars, Netherlands]	In the new result only the uncertainty for RCP26 is reduced. This follows directly from a smaller uncertainty in the two studies used. See further comment 1814
4408	4	41	1	41	1	On Figure 4.7 for RCP8.5, for AR5, upper end looks to be lower (0.95) than reported (0.98) [Peter Clark, USA]	accepted and corrected
14716	4	41	1	41	1	report and, for reference, [Christophe Deissenberg, Luxembourg]	accepted and corrected
ß	4	41	1	41	1	As before, where does the time series for Antarctic contribution comefrom? [Peter Clark, USA]	in the SOD the time seris is the average of Golledge et al. 2015 and DeConto (subm.)
3100	4	41	1	41	4	It appears the Antarctic contribution is assumed to be quadratic, but this is not necessarily justified. It's not obvious to me why we would think that the AIS 2050 contribtion would differ from AR5, even in RCP 8.5 (where the figure shows it differing quite a bit). [Robert Kopp, USA]	accepted this assumption of a quadratic curve is not made any longer
5158	4	41	4	41	4	Some confusion on the term "likely range", which is defined as 17-83% confidence interval in Figure 4.7, and the term "5-95 percentile" uncertainty range elsewhere in the report (e.g. page 46, line 30; page 90 line 16). Likely range of projection should be 5-95%. [Sai Ming Lee, China]	rejected in the report 17-83% is used throughout as the likely range
4412	4	41	9	41	9	would be useful to show a differece map from AR5 for RCP8.5 [Peter Clark, USA]	rejected. This has been considered, but not worth showing. It is basically a fingerprint of the extra Antarctic contribution
8598	4	41	9	41	9	What is "w.r.t."? This should be defined in the text [APECS Group Review, Germany]	accepted with respect to
23990	4	41	9	41	9	Please spell out ‘w.r.t.’ [Hans-Otto Poertner and WGII TSU, Germany]	accepted with respect to
17638	4	41	14	41	14	It was unclear how Fig 4.8 bottom right was generated or what it shows. If I understand it correctly, then it is the the gravitational and rotational effects of Antarctic melt that dominate the RSL differences. If that is the case, then it needs to be stated and also stated that it ignores any (potentially substantial) changes in ocean circulation, MDT and heat content uptake that have, to date, dominated the RSL variations and may do so in the future. See e.g. the 25 yr satellite record of SSH or the 50 year reconstruction by Church et al, the paper by Howard T., et al, 2014, Ocean Science on the relative contributions of difference sources to 21st C SLR and Yin, J J et al 2010, J Climate. [Jonathan Bamber, UK]	accepted, we clarified that results are based on Church et al. 2013 and replacing the Antarctic component by the values as listed in Table 4.3 including the gravitational and rotational effects of Antarctic mass change
5160	4	41	14	41	18	According to the caption of Figure 4.8, "…the right column for 2081-2100 the magnitude of the se level rise in meter, the right column the standard error". It is not clear what the right column of Figure 4.8 represents. Magnitude of sea level rise or standard error? [Sai Ming Lee, China]	accepted the caption has been corrected
5162	4	41	14	41	19	Suggest including RCP6.0 in Figure 4.8. [Sai Ming Lee, China]	rejected there is no assesment for the Antarctic contribution for RCP6.0

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8600	4	41	15	41	17	Figure 4.8 in the caption, it is unclear if the data includes the Antarctic contribution. A suggestion could be: Regional relative sea level change for RCP2.6, RCP4.5 and RCP8.5 as used in this report for ESL calculations. Results are based on AR5 and include Antarctic contribution. The latest based on SROCC results shown in Table 4.3. The left column is for the time slice 2046–2065 and the right column for 2081–2100 the magnitude of the sea level rise in meter, the right column the standard error. Results are presented for the difference between 2081–2100 and 1986–2005. The supplementary information shows the results for 2046–2055 and the details of the calculations. [APECS Group Review, Germany]	accepted and extended the caption
3102	4	41	15	41	19	Are these median projections? [Robert Kopp, USA]	accepted and corrected
13602	4	41	16	41	18	Fig 4.8 legend: the sentence "The left column…standard error" is not clear. [Debra Roberts and Durban Team, South Africa]	accepted and corrected
14718	4	41	16	41	18	THIS IS VERY CONFUSING: The left column is for the time slice 2046–2065 and the right column for 2081–2100 the magnitude of the sea level rise in meter, the right column the standard error [Christophe Deissenberg, Luxembourg]	accepted and corrected
8602	4	41	17	41	17	A comma should inserted before "the magnitude" [APECS Group Review, Germany]	caption has been rephrased
11724	4	41	22	43	2	I strongly disagree with the comments here. The AR5 attempted a proabilistic assessment of global averaged AND regional sea level rise, resulting in estimating the likely range. It is true that the AR5 did not extrapolate (as they could have) to give a very likely range. There are a number of reasons for this, including that there was no avialble likely range for other climate paprameters (eg surface temperature) which are essential driver of sea level change. Also, the ice sheet PDF is likely to be asymetric (a discussed in the text), as is climate sensitivity estimates and climate projections, includng surface temperature. Given that the different estimates of the Antarctic contribution vary by an order of magnitude, I have little confidence in any particular PDF description that allows any reasonable scientific extrapolation. A number of papers have papers have ignored these scientifc issues and (in my view blindly) extrapolated (sometimes using fewer models and less information than the AR5) to get very likely ranges and even further out in the tails of the unknown PDF. I know of no evidence that supports this approach. The use of such information would be inaccurate and mislading in planning responses to sea level rise. I would hope that the AR5 would have recognised and assessed the scientific weakness of these (wild) extrapolations and use a sounder scientific approach rather than support this literature. [John Church, Australia]	It is hard to be sure what the reviewer means in detail here as the comment refers to the whole section on probabilistic approaches. Does the reviewer imply the section should not be there at all? We interpret the comment in the sense that the reviewer casts doubt on the added value of probabilistic approaches given a. the limited physical understanding of ice sheets and b. the limitation posed by the fact that temperature is also only known in the likely range. We agree with this and thought we expressed this view with the sentence "An example is the study by Le Bars et al. (2017) who expand the projection by Church et al. (2013) in a probabilistic way with the Antarctic projections by DeConto and Pollard (2016) to obtain a full probability density function for sea level rise. " followed by why these approaches are taken. In order to express our intentions more carefully we now added a sentence before this sentence reading "They achieve this on conditional statements for the Antarctic ice sheet contribution and by ignoring that other climate variables are only presented with a limited likely range as well. As such these probabilistic studies present full probability density function, but they make a priori assumptions violating the idea that a probability density function captures the full range." We hope that we clarified our position more clearly by doing so.
14720	4	42	2	42	2	level), of which many are ‘probabilistic’, [Christophe Deissenberg, Luxembourg]	accepted and rephrased
3104	4	42	4	42	6	Why is Le Bars et al 2017 the primary example of a probailistic projection. There are plenty of earlier examples (e.g. Kopp et al 2014, Jackson and Jevrejeva 2016). [Robert Kopp, USA]	There is no specific reason to do so, it just serves as an example to make the point. We have added the references suggested by the reviewer.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
20834	4	42	4	42	6	It should be pointed out that Le Bars et al. (2017) use a Gaussian assumption, which is not valid - as seen by inspecting the original DeConto and Pollard (2016) data. Edwards et al. (2018) show this distribution shape in more detail. [Tamsin Edwards, UK]	rejected. Though the reviewer is right the point made here is that LeBars serves as an example of a study arriving at a full pdf based on a conditional statement that DP16 describes the full pdf of the dynamic contribution of the antarctic ice sheet.
22270	4	42	4	42	6	While Le Bars et al. (2017) is certainly an acceptable example here, it seems like Kopp et al. (2014) should be mentioned at the introduction of probabilistic studies, as this was the study that first introduced the idea of probabilistic estimates of SLR following IPCC AR5 [Andra Garner, USA]	we have added that reference, there is no principal difference from our perspective as both study take an ad-hoc a priori assumption for the Antarctic contribution
14722	4	42	7	42	7	are necessary from a quantitative risk management perspective [Christophe Deissenberg, Luxembourg]	accepted and rephrased
3106	4	42	7	42	9	The possibilistic approach needs to be better explained. (As applied by LeCozannet, it is not in fact constructed to include all existing probabilsitic estimates.) [Robert Kopp, USA]	We rephrased the sentence
22272	4	42	8	42	9	I suggest altering the description for the Le Cozannet et al. (2017). Although they do attempt to extend the tails of the AR5 projections using information from a select set of studies to determine "upper estimates" of SLR, the paper does not account for "all existing probabilistic estimates." [Andra Garner, USA]	we have removed the word all and maintain the reference as it describes a way to combine different probabilistic estimates
17780	4	42	16	42	29	Reviewer suggests to be consistent in the presentation of uncertainties. The interval 5th to 95th quantile is the interval of mean +/- 1,64 x sigma. Subsequently uncertainties are presented as 1x sigma which makes the section highly unclear [Hessel Voortman, Netherlands]	rejected we can not change the way uncertainties are handled in the literature. There is extensive argumentation in the AR5 report why they use the 5-95 percentile as their likely range. This is a given fact which we can not change for their results.
8604	4	42	17	42	17	Maybe "Thermal expansion" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them
14724	4	42	21	42	21	understood. E.g., Kopp et al. (2014) interpret [Christophe Deissenberg, Luxembourg]	accepted and rephrased
8606	4	42	23	42	23	What is "1sigma"? Maybe a legend/footnote or an explanation in last section [APECS Group Review, Germany]	we have removed this terminology as it was only confusing
3108	4	42	25	42	29	Is this in the right place? [Robert Kopp, USA]	The sentence is indeed oddly positioned and has been removed
8608	4	42	31	42	31	Maybe "Glaciers" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
1476	4	42	31	42	36	Here the glacier projections are very briefly mentioned, as one of the several components in probabilistic GMSL projections. It is clear that the dynamic role of ice sheets will be crucial to determine future SLR, but over the coming decades to century glaciers will nonetheless also have an important contribution. Given their importance, the two sentences dedicated to glaciers seem a bit disproportional (compared to the several pages dedicated to the dynamic effect of ice sheets; but also compared to the past changes in glaciers part (section 4.2.2.3.2)). I do not advocate for a long section, but it would somehow be nice to see (could also be somewhere else in the chapter) the following elements mentioned for future glacier modelling: (i) Since AR5, the first models that explicitly account for the glacier geometry and calving processes have arisen (Huss and Hock, 2015, Frontiers in Earth Science, doi: □10.3389/feart.2015.00054), and new promising efforts, in which ice dynamics are for the first time explicitly considered are now also emerging (Maussion et al., GMDD, doi: □10.5194/gmd-2018-9) (these novelties could potentially also be included in section 4.1.2.: "Advances in this chapter beyond AR5 and SR1.5"), (ii) the projections in Huss and Hock (2015) also account for the fraction of ice that is under sea-level at present, which was not the case in previous studies, (iii) the SLR contribution in Huss and Hock (2015) for the coming century is lower than estimates included in IPCC AR5. A more detailed comparison of the projections based on various models is expected soon from the GlacierMIP project, and could potentially be included in the second draft [Harry Zekollari, Switzerland]	Taken into account: we agree that the advances in published projections for glaciers, and their role as contributor for sea level rise, warrant a more detailed discussion. It was decided to cover this in Ch. 2 for low- and mid-latitude glaciers, in Ch. 3 for high-latitude glaciers, and to only use aggregated numbers (and exclude a more detailed discussion) in Ch. 4. We added references to the corresponding sections.
8610	4	42	38	42	38	Maybe "Land water storage" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them
8612	4	42	45	42	45	Maybe "Ice sheets" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them
14726	4	42	45	42	47	WHAT DO YOU MEAN EXACTLY? : Ice sheets: Existing GMSL projections rely upon some combination of (1) past expert assessments by the IPCC or other forums based on physical models of varying degree of complexity (Meehl et al., 2007; Church et al., 2013), (Katsman et al., 2011); or alternatively (2) structured expert elicitation. ?????? ASIT STANDS, THE SENTENCE IS UNCLEAR [Christophe Deissenberg, Luxembourg]	the sentence has been rephrased to clarify
22274	4	42	45	42	47	Several recent studies (e.g., Kopp et al., 2017, and Bakker et al., 2017) have also used modeling results based on DeConto and Pollard (2016) for ice sheet contributions; it seems that this should be included here, as a source of information for such probabilistic projections. [Andra Garner, USA]	we have added a sentence along this line
3110	4	42	47	42	50	Not an intelligible sentence. [Robert Kopp, USA]	the sentence has been rephrased to clarify
20836	4	42	48	42	48	Not CMIP5, but Little et al. (2013) statistical-physical modelling. [Tamsin Edwards, UK]	this has been captured in the revised version
3112	4	42	52	42	52	"adopted method of postprocessing expert data" is more intelligly described as the "approach used to weight experts based on their performance on calibration questions" [Robert Kopp, USA]	rejected the criticism expressed by de Vries and van de Wal is not on the weighting but on the arbitrary choices for aggregrating the individual ice sheet contributions. This has been clarified better now.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
3114	4	42	53	42	53	See rebuttal by Bamber, J. L., Aspinall, W. P., & Cooke, R. M. (2016). A commentary on “how to interpret expert judgment assessments of twenty-first century sea-level rise” by Hylke de Vries and Roderik SW van de Wal. Climatic Change, 137(3-4), 321-328. [Robert Kopp, USA]	reference has been added
3116	4	42	53	42	55	Horton et al (2014) was not especially in line with earlier IPCC assessments -- they had a likely range for RCP 8.5 of 70-120 cm. [Robert Kopp, USA]	this has been rephrased to express that Horton used an alternative expert judgement, a factual number comparison is not relevant here so we left that out as the comparison is complicated because Horton provides total sea level rather than the ice sheet contribution to total sea level
13604	4	43	0			Table 4.4: spell out all terms in table headings. Define CMIP5 [Debra Roberts and Durban Team, South Africa]	accepted
17260	4	43	4	43	5	space between the title and the paragraph is missing [Iulian Florin Vladu, Germany]	accepted
4158	4	43	4	43	11	You need to differentiate here between contemporary ice/land-water change "fingerprints" and the ongoing effects of GIA. At the moment, the two seem to be combined under (2). What are the long-term processes that lead to vertical land motion that you are eluding to in (3)? In the final sentence and reference to Carson et al (2016) - I don't see any figures in that paper that show the inverse barometer effects - perhaps better to use Church et al (2013) IPCC AR5, since they are shown? Mostly the inverse barometer effect is much smaller than 5 cm? It might be helpful to say that this effect is likely to be most pronounced at the high latitudes? [Matthew Palmer, UK]	Carson et al. 2016 page 277 has the statement of less than 5 cm but we added the reference to church et al. 2013 who has indeed a figure (same data). The difference between point 1,2,3 has been clarified.
14732	4	43	7	43	8	effects (often separated into instantaneous gravitational and rotational effects caused) by the redistribution of mass within the cryosphere and the hydrosphere [Christophe Deissenberg, Luxembourg]	rephrased in line with the suggestion
8614	4	43	8	43	8	Missing bracket: "3)" should be "(3)" [APECS Group Review, Germany]	accepted
8616	4	43	13	43	13	Maybe "Dynamic sea level (DSA)" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them
17640	4	43	13	43	18	see previous comment. DSL will be affected by large volume ice melt, which is presumably not included in any of the CMIP5 simulations [Jonathan Bamber, UK]	we agree a sentence expressing this has been added
11940	4	43	13		14	cite a source apparently [Chukwuma Anoruo, Nigeria]	reference is gien after the mext sentence
14734	4	43	15	43	15	differ with the way the model range is understood and with the modalities of drift correction, if present [Christophe Deissenberg, Luxembourg]	rejected we left as it is to express the two different approaches which are used in the literature
8618	4	43	20	43	20	Maybe "Gravitational-rotational effects" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them
14736	4	43	20	43	20	cryospheric changes, which however may differ in the details. [Christophe Deissenberg, Luxembourg]	accepted
8620	4	43	20	43	26	There is an interesting reference about gradient fingerprint mapping (which could be cited): E. Larour, E. Ivins, S. Adhikari, Should coastal planners have concern over where land ice is melting?, Sci. Adv., 3(11), doi:10.1126/sciadv.1700537. [APECS Group Review, Germany]	We have incorporated this suggestion
11942	4	43	21			list such studies [Chukwuma Anoruo, Nigeria]	reference is given to Slangen et al. 2014b
8622	4	43	28	43	28	Maybe "Long term solid Earth processes" could be in bold [APECS Group Review, Germany]	we have used italics fonts for the four different components to emphasize them

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14738	4	43	30	43	32	This process is incorporated either by physical modeling (e.g., Slangen et al., 2014b) or by estimation of a long-term trend from tide-gauge data (e.g., Kopp [Christophe Deissenberg, Luxembourg]	accepted and rephrased
14740	4	43	30	43	32	This process is incorporated either by physical modeling (e.g., Slangen et al., 2014b) or by estimation of a long-term trend from tide-gauge data (e.g., Kopp et al. (2014), which is then spatially extrapolated. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
3194	4	43	37	43	48	This section doesn't say much about probabilistic projections (what makes a probabilistic projections, where they come from, why they are useful), and not all the studies in Table 4.4 are probabilistic by all definitions of probabilistic. This needs to be sharpened, or retitled something like "Comparison to literature projections of GMSL and RSL" [Robert Kopp, USA]	We have rearranged paragraph 4.2.3.3.3 and 4.2.3.3.4 in order to accommodate the fact that part of the studies in Table 4.5 are more of the semi-empirical type than probabilistic. The title of 4.2.3.3.4 has been changed in line with these changes
3118	4	43	39	43	40	"Supplementary level"? [Robert Kopp, USA]	Rephrased overlap was intended
14742	4	43	40	43	41	simulations. The largest difference can be found in the treatment of the ice dynamics [Christophe Deissenberg, Luxembourg]	accepted and rephrased
17262	4	43	43	43	43	definition of SROCC would add value [Iulian Florin Vladu, Germany]	changed it to this report
12602	4	43	43	43	45	"excluding MICI as this process is highly uncertaint" this is not in accordance with discussion p.39 in which it is said that DeConto and Pollard is "given less weight" in the projections. Which means it has some weight, so the projections do not assume no MICI. [Dewi Le Bars, Netherlands]	We believe we should ignore DP16 because it is obsolete because we use DC18/19 which has a recalibrated SMB scheme and is calibrated with present-day mass loss rates in Antarctica.
12604	4	43	43	51	51	Why is Le Bars et al. 2017 not included in this table? [Dewi Le Bars, Netherlands]	Originally it was not included because it only provides global estimates, but because we have referred to it already we decided to include it in the Table
14744	4	43	44	43	45	because it is based on an assessment of multiple studies, excluding MICI as its results are considered deeply uncertain. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
14728	4	43	48	42	50	WHAT DO YOU MEAN EXACTLY? : Approach (1) is based on CMIP5 calculations in which the uncertainty range was assessed to be larger than the direct uncertainty range from the models yielding that the 5%–95% range was interpreted as the likely range from 17%–83%. I DO NOT UNDERSTAND AND HAVE NO SUGGESTION [Christophe Deissenberg, Luxembourg]	This comment likely refers to page 42 end of the page, we have rephrased the sentence to clarify
3788	4	43	48	43	48	change to "than" [Ola Kalen, Sweden]	accepted
14730	4	43	50	43	53	Approach (2) was based on a formal expert elicitation protocol (Cooke, 1991) instead of physical based models. It led to a significantly higher estimate of the contribution of the ice sheets to the sea level rise, However, the results were criticized as they were obtained by post-processing the expert data (de Vries and van de Wal, 2015; de Vries and van de Wal, 2016). [Christophe Deissenberg, Luxembourg]	also page 42, sentence has been rephrased to clarify the content better
8624	4	43	51	43	53	Certify that Table 4.4 caption will start in the beginning of a page. [APECS Group Review, Germany]	yes we will do
24942	4	43	51	45	1	Tables 4.4 and 4.5 are both much appreciated. [Elizabeth Weatherhead, USA]	Thank you
1478	4	44	1	44	1	Grinsted et al. (2015): 'AR5 projections' for glaciers. But which approach/model is used? [Harry Zekollari, Switzerland]	with AR5 we meant Church et al. 2013 we will change accordingly in the Table
8628	4	44	3	44	3	Some sources in Table 4.5 are not listed in Table 4.4: Nauels et al. (2017b), Nauels et al. (2017a), Bakker et al. (2017b), Wong et al. (2017), Jevrejeva et al. (2012), Schaeffer et al. (2012), Mengel et al. (2016). [APECS Group Review, Germany]	We have clarified the text Table 4.4 describes methods of probabilisitic approaches mainly focussing on the regional sea level changes composed of different contribution to sea level. Table 4.5 shows results of these models and semi-empirical models which directly provide global sea level.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
5164	4	44	3	44	4	Some of the papers quoted in Table 4.5 are not found in Table 4.4 (e.g. Nauels et al. (2017b)). Please also include results for RCP 6.0 from various studies if available. [Sai Ming Lee, China]	We decided not to include RCP6.0 results as too many studies don't have these results. We have synchronised Table 4.4 and Table 4.5
8626	4	44	3	44	4	Certify that Table 4.5 caption will start in the beginning of a page. [APECS Group Review, Germany]	yes we will do
23280	4	45	0	47		These three subsections are the core of substances on coastal impacts and mechanisms (i.e., ice loss, RSL change and ESL occurrence). However, there appear to be a need for a significant revision, not only on substance, but editorial quality. Some obvious issues are listed below. [Y. Jeffrey Yang, USA]	Nothing changed not sure to which three subsection the reviewer refers
13606	4	45	0			Table 4.5 Spell out GMSL. Define RCP. Recommend using cm throughout - whole numbers are easier to assimiilate than decimals. Text uses cm. Sometimes. [Debra Roberts and Durban Team, South Africa]	Maya I think It would be good to have this consistent throughout the chapter Can you manage?
23288	4	45	0			The citation and use of Muis et al (2016) work can be re-examined. The SL modeling replicates 80% gauge station data within 0.2m. Are other 20% with greater modeling differences mostly related to ESLs? Generally the ESLs are extremes in SL measurement datasets. As a consequence, the model poorly simulates the tropical storms (Line 22). It follows with a self-conflicting statement that this model has a high fidelity in improving ESL projections. [Y. Jeffrey Yang, USA]	We agree that we need to stress more at this point that the hydrodynamical models do a good job as long as storm surges are not dominating we improved the text at this point.
3120	4	45	1	45	1	In the table: Horton et al. (in press) is a review based on assessment of the literature similar to the one here and is not an independent estimate. However, results listed in the table as Horton et al (in press) seem to be similar to the Kopp et al 2017 DP16 projections. [Robert Kopp, USA]	Accepted we replace Horton et al. by Kopp et al. as it is numberwise the same and the original
3122	4	45	1	45	1	Probabilistic studies should have ranges, not single values. See Horton et al. (in press), which nicely summarizes all of this and in principle scould just be adopted here. [Robert Kopp, USA]	This is true but not all studies provide a likely range and we stick here to presenting the likely range if provided in the original paper
22276	4	45	1	45	1	Should Kopp et al. (2017) be included in Table 4.5? [Andra Garner, USA]	Accepted we replace Horton et al. by Kopp et al. as it is numberwise the same and the original
3128	4	45	2	45	25	An "approximately" log-linear relationship? [Robert Kopp, USA]	accepted and rephrased
3124	4	45	3	45	18	Should note that the agreement beteen semi-empirical projections and AR5 is a new development since AR5. [Robert Kopp, USA]	we agree a sentence expressing this more explicitly than in the FOD has been added
11944	4	45	4		6	try to summarize this statement. This will make clear the aim. [Chukwuma Anoruo, Nigeria]	the sentence has been rephrased to clarify
14746	4	45	5	45	5	between the observed sea level [Christophe Deissenberg, Luxembourg]	accepted
23992	4	45	5	45	5	Should be ‘sea level rise’ not 'sea-rise' [Hans-Otto Poertner and WGII TSU, Germany]	accepted
14748	4	45	5	45	6	to the sea-rise and, accessorily, trying to quantify the contribution of ice sheet dynamics to sea level rise. [Christophe Deissenberg, Luxembourg]	sentence has been rephrased in line with comment 14746
8630	4	45	6	45	6	Maybe change "secondly" to another word or expression [APECS Group Review, Germany]	sentence has been rephrased in line with comment 14746
14750	4	45	6	45	8	In general, motivated by a mechanistic understanding, semi-empirical models use statistical correlations from time-series analysis of observations to generate projections. [Christophe Deissenberg, Luxembourg]	accepted
14752	4	45	8	45	9	They implicitly assume that the processes driving the observations and the feedback mechanisms remain similar over past and future. [Christophe Deissenberg, Luxembourg]	accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
23284	4	45	8	45	14	The statement refers to the assumed "feedback mechanisms". Please specify or add reference to the feedback mechanisms. The same for the term "recent past" and "hydrofracturing". Is "recent past" the same definition as in Chapter One? I assume the hydrofracturing refers to the ice sheet dynamics, not the other subjects in geological and hydrological terms. [Y. Jeffrey Yang, USA]	A further specification of this is given a few sentences later explaining that the feedback mechanism MISI may be different in future than it was in the past. Hydrofracturing is the process of breaking up ice hselves by access melt water production from the surface as explained earlier in the chapter
14754	4	45	10	45	12	Less and less value is given to semi-empirical models given the ongoing advances in closing the sea level budget and in the process understanding of the dynamics of ice. It is now realized that they poorly capture or miss altogether the recent observed dynamics of changes in Antarctica. [Christophe Deissenberg, Luxembourg]	accepted
20838	4	45	10	45	14	These sentences are poorly written and needs to clarify what is meant by dynamical change not being captured in recent observations. [Tamsin Edwards, UK]	rephrased
4414	4	45	11	45	11	what does "gradually decays" mean? [Peter Clark, USA]	rephrased
14756	4	45	14	45	15	Moreover, the results from these models(e.g., Kopp et al., 2016; Mengel et al., 2016; Mengel et al., 2018) are in general agreement with Church et al. (2013). [Christophe Deissenberg, Luxembourg]	rephrased
14760	4	45	14	45	18	THE LOGIC OF THE STATEMENT IN THESE LINES IS UNCLEAR FOR A NON-SPECIALIST AS ME [Christophe Deissenberg, Luxembourg]	rephrased
14758	4	45	15	45	18	Total sea level projections deviate (e.g., Nauels et al.2017) only if they include specific estimates of the dynamic contribution of Antarctica which strongly deviate from the values adopted by Church et al. (2013), such as the combined hydrofracturing and ice cliff instability mechanism as presented by DeConto and Pollard (2016),. [Christophe Deissenberg, Luxembourg]	rephrased
23286	4	45	15	45	18	Editorial. Total sea level projections deviate from what? [Y. Jeffrey Yang, USA]	from process-based models rephrased
17782	4	45	20	46	43	Considering the dominance of local effects on extreme sea levels (for instance wind setup in shallow seas), the added value of this section in a report with global coverage is limited [Hessel Voortman, Netherlands]	we disagree with this statement what happens at the local scale is what eventually matters for society not just the global mean trend
23994	4	45	20			Suggest to add confidence statements [Hans-Otto Poertner and WGII TSU, Germany]	Rejected. Paragraph 4.2.3.4 forms the introduction to the actual calculations presented in 4.2.3.4.1. For this reason we have a confidence statement below figure 4.10 which carries over to the executive summary in the bullet point on extreme sea level.
3126	4	45	22	45	23	Contrary to this sentence, I don't believe the definition of storm tide incudes waves. [Robert Kopp, USA]	accepted we left out the confusing term storm tides as it is also not used any further.
14762	4	45	22	45	23	storm tides, are very high water levels resulting from a combination of mean sea level, [Christophe Deissenberg, Luxembourg]	Accepted we removed the wording storm tides

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
4160	4	45	22	45	25	Section 4.2.3.4: Extreme Sea Level Projections. Somewhat related to my earlier comment, I would suggest that the authors briefly review the drivers of exteme sea levels: tides, surges and waves, in particular. In my view, it is useful to discuss these transient events separately from the more pervasive time-mean sea level changes associated with thermal expansion and ice melt etc. Note that projections of changes in waves and surges are very uncertain, because they relate to the dynamic response of the atmosphere (and local meteorology) to climate change. In addition, we know that internal variability has a large component at regional scales (so determining to the climate change response itself for these variables is challenging). It is good to see mention of changes in tidal characteristics (e.g. Pickering et al) but note that the details are also uncertain, due to methodological assumptions and little consistency among studies (in terms of the details). [Matthew Palmer, UK]	accepted we added 2 sentences of introduction on paragraph 4.2.3.4 to frame the problem better
11946	4	45	23		24	provide evidence with literature [Chukwuma Anoruo, Nigeria]	rejected here we qualitative explain why GMSL changes affect the ESL characteristics, numerical results are shown in the next paragraph
12606	4	45	45	3	3	The separation between "probabilistic bottom-up" and semi-empirical is strange. Most semi-empirical projections like Mengel et al. 2016, Nauels et al. 2017, Wong et al. 2017 are also probabilistic and bottom-up... [Dewi Le Bars, Netherlands]	accepted line numbering is wrong but the point is taken that the distinction between semi-empirical and probabilistic bottom-up is somewath arbitrary. So we changed the order of the paragraphs and discuss in Table 4.4. and 4.5 both probabilistic bottom-up and semi-empirical studies
12608	4	45	45	3	18	This discussion feels like it was written 3 years ago. Some important litterature is not mentionned: Bakker et al. 2017 (□10.1038/s41598-017-04134-5), Wong et al. 2017 (□10.5194/gmd-10-2741-2017). The idea that process based methods are now at the level that the value of semi-empirical models is decaying is in sharp disagreement with the discussion above about the future of Antarctica that clearly says that some processes are no solved numerically in any model: hydrofracturing and ice cliff failure. Even MISI is parameterized in most ice sheet models or included based on expert judgement and statistical method (Ritz et al. 2015). It is strange to assess the projections in light of Church et al. 2013 because the process-based projections made in this SROCC report show that Church et al. 2013 projections where too low. In fact projections made from the process-based method have risen upward to match semi-empirical projections assessed in Church et al. 2013. Doesn't that fact show the value of semi-empirical projections? [Dewi Le Bars, Netherlands]	accepted line numbering of the comment is wrong. Table 4.4 and 4.5 have been expanded to include more recent studies.MISI is solved in more and more numerical models in an adequate way. For this reason we use two model studies capturing MISI in order to estimate the future contribution of the Antarctic ice sheet. These results are not in contradiction to what is written in the AR5 report. AR5 reads: "Only the collapse of the marine-based sectors of the Antarctic ice sheet, if initiated, could cause GMSL to rise substantially above the likely range during the 21st century. This potential additional contribution cannot be precisely quantified but there is medium confidence that it would not exceed several tenths of a meter of sea level rise. We have now quantified this term to be 0.27±0.18 in 2100.
3130	4	46	1	46	3	I don't think this sentence is either clear or correct. It confuses uncertainty and variability (and possibly trend), and is probably not in the context of the specific projections used here. Also, the paper cited here (Pickering) says changes in MHW exceed ±10% of the SLR in ~10% of coastal cities -- very different from saying that 'variability' in MHW is larger than 'variability' in SLR. [Robert Kopp, USA]	accepted and corrected
13608	4	46	1	46	8	This is an important paragraph that needs to be clearly reflected in SPM [Debra Roberts and Durban Team, South Africa]	accepted a figure for the SPM has been suggested

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13986	4	46	3	46	8	These are the sorts of facts that need to appear in the Executive Summary in order to enhance its policy relevance. [Debra Roberts and Durban Team, South Africa]	accepted there is an executive statement based on this paragraph
14764	4	46	7	46	7	century in the absence of [Christophe Deissenberg, Luxembourg]	accepted and reworded
14766	4	46	10	46	10	The frequencies [Christophe Deissenberg, Luxembourg]	accepted
3132	4	46	10	46	12	Note that these re hydrodynamic models forced by synthetic storms, and must be combined with a tide model to estimate total extreme sea levels. [Robert Kopp, USA]	accepted and added
3134	4	46	10	46	43	What about more detailed studies with synthetic storms, e.g., Lin et al 2016, Garner et al 2017? [Robert Kopp, USA]	accepted these studies have beeen added as examples
14768	4	46	12	46	12	frequency and intensity (that is e.g., the height of a 1/100-year flood) of ESLs. [Christophe Deissenberg, Luxembourg]	accepted
13610	4	46	20	46	20	Acronym 'RMSE' only used once in the chapter. Delete. [Debra Roberts and Durban Team, South Africa]	accepted
3136	4	46	28	46	43	Some of this paragraph reads like a critique of individual studies, not an assessment of the literature. [Robert Kopp, USA]	accepted the wording has been rephrased
3140	4	46	28	46	43	See also D. J. Rasmussen, K. Bittermann, M. K. Buchanan, S. Kulp, B. H. Strauss, R. E. Kopp, and M. Oppenheimer (2018). Extreme sea level implications of 1.5 °C, 2.0 °C, and 2.5 °C temperature stabilization targets in the 21st and 22nd centuries. Environmental Research Letters 13, 034040. doi: 10.1088/1748-9326/aaac87. [Robert Kopp, USA]	accepted and used
3138	4	46	32	46	33	To the contrary, I would expect differences to be smaller for ESL events with a shorter return period, since they are better constrained by data. [Robert Kopp, USA]	accepted and corrected
3792	4	46	46	46	46	The reference to the results in this section is lacking. Data (Woodworth) and method (Arns) are stated, but where were the results published? [Ola Kalen, Sweden]	accepted further details are provided in Frederikse et al. (submitted)
8632	4	46	46	46	46	Check reference to "4.3.2.1". Maybe "4.2.3.2"? [APECS Group Review, Germany]	corrected
3142	4	46	46	47	12	It's unclear from this description how sea level and GPD uncertainty are handled. [Robert Kopp, USA]	rejected the as mentioned the method by Arns et al. 2013
8634	4	46	56	46	56	Check reference to "4.3.2.1". Maybe "4.2.3.2"? [APECS Group Review, Germany]	corrected
3144	4	47	9	47	9	PDRP and AF are not defined. [Robert Kopp, USA]	accepted reference is given to the figure and the caption
8636	4	47	9	47	9	What are "PDRP" and "AF"? These should be defined in the text [APECS Group Review, Germany]	accepted reference is given to the figure and the caption
14770	4	47	14	47	14	1/100 is [Christophe Deissenberg, Luxembourg]	rephrased
3148	4	47	14	47	28	It may be useful to have a metric that reflects timing -- eg the year in which AF_100 = 100 (i.e., the 100-year flood becomes an annual flood) [Robert Kopp, USA]	rejected the number of pages is limited and we restrict ourselves here to 4.9 explaining the different metrics used for different location and the amplification factors. Large amplification factors essential show that the requested year in which AF_100 = 100 is nearby, hence this metric shows the same in a different way.
11948	4	47	14		15	provide evidence with literature [Chukwuma Anoruo, Nigeria]	There is no reference that is what figure 4.10 shows, we clarified this
14772	4	47	15	47	15	once a year under future conditions [Christophe Deissenberg, Luxembourg]	corrected
14774	4	47	16	47	16	scenarios, in the middle of the 21st century [Christophe Deissenberg, Luxembourg]	accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14776	4	47	17	47	18	we can derive several quantities relevant for decision making: the PDRP_1, which is the present-day [Christophe Deissenberg, Luxembourg]	accepted
14778	4	47	19	47	20	Figure 4.9; the SSLR_100, which is the mean [Christophe Deissenberg, Luxembourg]	accepted
14780	4	47	19	47	22	Figure 4.9; the SSLR_100, which is the mean sea level change scaled by the present-day 100-yr return height and is used in Cross Chapter Box 4; and the AF_100, that is the amplification factor during changed conditions for events, which has under present-day conditions a return period of 100 years. [Christophe Deissenberg, Luxembourg]	partly rephrased
3146	4	47	20	47	20	Definition of SSLR_100 is very hard to follow. [Robert Kopp, USA]	that is why it is illustrated in figure 4.9
8638	4	47	21	47	21	Check cross reference to "Cross Chapter Box 4" [APECS Group Review, Germany]	accepted
14782	4	47	23	47	23	as a function of the return period. [Christophe Deissenberg, Luxembourg]	accepted
14784	4	47	25	47	28	The figure shows that, in many locations, events which are currently have an estimated return period of a hundred years or more become annual events by the end of the century, particularly under the higher RCP8.5 scenario. [Christophe Deissenberg, Luxembourg]	partly rephrased
3790	4	47	26	47	26	change to "events which currently" [Ola Kalen, Sweden]	accepted
13612	4	47	26	47	26	Delete 'are' before 'currently' [Debra Roberts and Durban Team, South Africa]	accepted
334	4	47	26			events [Kerstin Jochumsen, Germany]	accepted
336	4	47	26			delete "are" [Kerstin Jochumsen, Germany]	accepted
338	4	47	27			delete "a" before once [Kerstin Jochumsen, Germany]	accepted
13614	4	48	0			Fig 4.9 This looks like a very interesting figure. The legend should explain it more clearly. The yellow and red lines in the top right panel are not related to the yellow and red lines in the graphs, which is confusing. Re observed events (x) how far back do these go? It would be very interesting to see during what years the most extreme events were observed - those x's that sit above the black line on the right frindge in each graph. Label them with the year. Are these most extreme events clustered in the last few decades? Fig 4.10 also looks very interesting. The legends should be very clear, explaining more clearly what these figures show. [Debra Roberts and Durban Team, South Africa]	we improved the explanation
8640	4	49	1	49	1	Check beginning of the caption: "Return periods a set ..". Maybe "Return periods of a set .."? [APECS Group Review, Germany]	accepted
8642	4	49	1	49	1	Fugure 4.9 caption should be below Figure 4.9 [APECS Group Review, Germany]	accepted
13616	4	49	1	49	1	w.r.t.' only used once - spell out in full. [Debra Roberts and Durban Team, South Africa]	abbreviation removed
8644	4	49	1	49	2	The start of the caption of Figure 4.9 is a bit confusing, a suggestion could be: Relation between return height and return period for a set of characteristic tide gauge locations (see upper left for their location) .... [APECS Group Review, Germany]	accepted caption improved
3794	4	49	1	49	6	Reference to author of figure lacking [Ola Kalen, Sweden]	it is specifically made for this report
3452	4	49	1	49	9	Geographical distribution of the gauge locations shows mis representation of Indian Ocean would be good to explain in the text why the tide gauge bias is present [Mahmood Riyaz, Maldives]	there is no specific reason just spave limitations
340	4	49	1			Return periods at a set of... [Kerstin Jochumsen, Germany]	accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
2106	4	49	8	49	10	It would be better to put 0º-meridian in the center of the maps because 0º-meridian is a densely populated area (high exposure). On the contrary, few people live around (low exposure) the 180º-meridian area. Also Figure 4-14 (page 4-90). [Josep Medina, Spain]	rejected we prefer to put the pacific ocean central
17642	4	49	9	49	9	I found fig 4.10 a little difficult to interpret. It looks, to me, like 1/1000 yr events have a return period of a year almost everywhere in bottom right (RCP8.5 2100) but may be I have misunderstood or colour table is misleading? [Jonathan Bamber, UK]	The metric of figure 4.10 will be changed to the amplification factor to ease interpretation
3150	4	49	10	49	13	These maps are hard to read. Color bad needs refinement. [Robert Kopp, USA]	The metric is changed
3796	4	49	10	49	13	Reference to author of figure lacking [Ola Kalen, Sweden]	it is specifically made for this report
8646	4	49	16	49	16	"paragraph" should be replaced by "subsection" (maybe?) [APECS Group Review, Germany]	accepted
8648	4	49	16	49	16	Maybe comma after "In summary" [APECS Group Review, Germany]	accepted
13618	4	49	16	49	23	Another hugely outcome that needs to be reflected in SPM [Debra Roberts and Durban Team, South Africa]	there is a statement in the executive summary of the chapter which will be carried over to the SPM possibly including a figure
14786	4	49	17	49	17	sea level rise events which are [Christophe Deissenberg, Luxembourg]	rejected
3152	4	49	20	49	20	By "storm", I think the authors mean "tropical cyclone" [Robert Kopp, USA]	accepted
14788	4	49	20	49	20	The affected locations [Christophe Deissenberg, Luxembourg]	accepted
3154	4	49	56	50	10	Stratospheric cooling also plays an important role in TC intensification. E.g., Emanuel, K., Solomon, S., Folini, D., Davis, S., & Cagnazzo, C. (2013). Influence of tropical tropopause layer cooling on Atlantic hurricane activity. Journal of Climate, 26(7), 2288-2301. Gilford, D. M., Solomon, S., & Emanuel, K. A. (2017). On the seasonal cycles of tropical cyclone potential intensity. Journal of Climate, 30(16), 6085-6096. [Robert Kopp, USA]	TC are discussed in section 4.2.3.4.3
11728	4	50	1	50	26	This is hardly a globalassessment. I am not up with the latest literature inthis area but there is a concerted effort (COWCLIP) to povide projections of surface waves. [John Church, Australia]	accepted and modified
18286	4	50	2	50	3	Many local and also large-scale hydrodynamic models now in fact do include waves, so this statement needs modifaction; see the paper by Storalazzi et al. 2018 (already cited) and Vousdoukas et al. 2018 (doi:10.1038/s41467-018-04692-w), as well as other and forthcoming studies. [Laurens Bouwer, Netherlands]	accepted. The statement is modified now
14790	4	50	4	50	5	wave height (wave height trough to crest of the highest third of the waves, calculated based on 6 hourly data with 20 CMIP5 models forced with the RCP8.5 scenario) increases29 [Christophe Deissenberg, Luxembourg]	rejected. The information that it is calculated on the basis of CMIP5 projections is essential here so we prefer to keep it out of parenthesis
2108	4	50	7	50	7	The international conference Coastal Structures 2011 was focused on coastal disasters. One of the keynote speakers (Isobe, 2013) analyzed in detail the impact of global warming on a variety of costal structures, much more than simple depth-limited conditions and run-up. [Josep Medina, Spain]	noted
2110	4	50	7	50	7	Isobe, M. (2013). Impact of global warning and adaptation strategy in the coastal zone. Proc. of Coastal Structures 2011, World Scientific, Vol 1, 3-19. [Josep Medina, Spain]	We looked into this reference and found it more relevant to the section 4.3 and 4.4 of the chapter
21662	4	50	8			Arns et al (2017) show this effect, but not for the first time. Needs to described carefully. [Robert Nicholls, UK]	accepted and modified

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14792	4	50	9	50	9	In combination with changes in the tides, design heights [Christophe Deissenberg, Luxembourg]	accepted and changed
17446	4	50	9	50	10	"heights needs" should be "heights need" or "height needs" [Sonya Legg, USA]	accepted and changed
8650	4	50	9	50	11	The paper from Arns et al. (2017) states: [Consequently, sea-level driven changes in wave characteristics, and to a lesser extent, tides, amplify the resulting design heights by an average of 48–56%, relative to design changes caused by SLR alone.…] It is not clear from the sentence if the author is talking about coastal protection design heights. A suggestion could be: "In combination with changes in the tides, coastal protection design heights need to be increased by 48%–56% in the German bight region.…." [APECS Group Review, Germany]	accepted and changed
21664	4	50	9			The main effect that is being seen is reduced depth limitation which affects wave height and energy approaching the coast. More energy in turn leads to more run-up. Even though the paper states greater period this is not the major effect of reduced shoaling and energy dissipation. The text here simply repeats the abstract of the paper which I fear is misleading [Robert Nicholls, UK]	accepted and changed
8652	4	50	11	50	11	Maybe comma after "region" [APECS Group Review, Germany]	accepted and changed
14794	4	50	13	50	13	For the North Sea region under RCP8.5, Vousdoukas et al. (2017) quantify the extreme sea level including a wave model to nearly 1 m. That is the highest in Europe. [Christophe Deissenberg, Luxembourg]	accepted and changed
14796	4	50	13	50	14	For the North Sea region under RCP8.5, Vousdoukas et al. (2017) quantify the extreme sea level including a wave model to nearly 1 m. This is the highest in Europe and corresponds to a 40% increase of the RSLR trends caused by increased storm surge and waves. [Christophe Deissenberg, Luxembourg]	accepted and changed
20866	4	50	14	50	14	RSLR is not defined - perhaps simply 'RSL'? [Paolo Cipollini, UK]	accepted and changed
3318	4	50	15	50	15	Pérez [Castor Muñoz Sobrino, Spain]	accepted and changed
14798	4	50	15	50	15	from once every 100 years to once every year for about [Christophe Deissenberg, Luxembourg]	accepted and changed
14800	4	50	17	50	17	decrease of 10 cm in wave height and period DECREASE OF 10cm IN PERIOD??? [Christophe Deissenberg, Luxembourg]	accepted and corrected
21666	4	50	19	50	26	A paragraph of observations, prognosis or what -- not clear to a reader without reading the sources. [Robert Nicholls, UK]	accepted. The paragraph is now totally changed
21668	4	50	19	50	26	To consider future changes, we need a lot of simulations. Do we have a big enough sample? [Robert Nicholls, UK]	The sentence is now removed
8654	4	50	21	50	21	Maybe "and" before "they" [APECS Group Review, Germany]	rejected. It seems useless
14802	4	50	22	50	23	Results show a strong increase, up to 3m in summer and 0.6 m in winter. However, storm surge and waves do not increase everywhere. Cannaby et al. (2015) argue e.g. [Christophe Deissenberg, Luxembourg]	accepted and changed
16408	4	50	25	50	26	Resources which is not published , although "in press", should be excluded until it is published. The reason for this is because this is a very important report and reference to be referred by the world. [Lee-Sim Lim, Malaysia]	rejected. We are now at the level of the second order draft. Article in press will be published before the edition of the final report. So we can now take them into account from now on. However we agree that in the final report if some references remain in press then they` will be removed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8656	4	50	26	50	26	Maybe it should be written in the end of the paragraph that more studies should be carried out considering others regions along the World. [APECS Group Review, Germany]	accepted and changed
23118	4	50	26			what about rogue waves ? [Jacques Beall, France]	rejected. since the last IPCC report, we are not aware of any published results on rogue waves and climate change that we could report here
21286	4	50	28	51	10	This paragraph discuss the changes in frequnecy and strength of tropical cyclones, nevertheless it does not leave aclear messageas what is the consensust, are the tropicla cyclones staying the same or are they increasing in the 21st century? Can we substantieate the conclusion that it is likely that the intensity of TC will increase using historical trends during periods of warm oceans as in the 50s and 70s in the Atlanctic. [Alejandro Souza, Mexico]	Response: 1. We reword the sentence in order to clarify the assesment. "Through downscaling CMIP5 climate models, one study indicated that an increase in global TC frequency is projected during the 21st century in most locations, especially in the western North Pacific region, North Atlantic and South Indian Oceans (Emanuel, 2013). However, most models still project a decrease or constant global frequency of TCs, but at the same time a robust increase in ratio of intense TCs. This is similar to IPCC AR5 and previous many recent studies (Emanuel et al., 2008; Holland and Bruyère, 2014; Knutson et al., 2015; Kanada et al., 2017; Nakamura et al., 2017; Scoccimarro et al., 2017; Zhang et al., 2017). 2. P50, L9-10. As for the consensus, the assessment results of TCs in the 21st century has been given, i.e., "We conclude that it is likely that the intensity of severe TCs will increase in a warmer climate, but there is still low confidence on the frequency change of TCs in the future".
11730	4	50	29	50	47	I do not understand how the last sentence of this paragraph fits with the rest of the paragraph? [John Church, Australia]	Response: Taken into account- combined with the comment 21286.
14804	4	50	30	50	30	storm surges, high water events, coastal floods, and [Christophe Deissenberg, Luxembourg]	Accepted.
14806	4	50	32	50	32	increase, and [Christophe Deissenberg, Luxembourg]	Accepted.
5166	4	50	37	50	47	The statement “Through downscaling CMIP5 climate models, an increase in global TC frequency is projected during the 21st century in most locations, especially in the western North Pacific region, North Atlantic and South Indian Oceans (Emanuel, 2013)”in lines 45-47 is contradicting with the previous statement in lines 41-42. Suggest making reference to Chapter 6.3.3.1 (p14, lines-14-25) of the current draft for consistence. [Sai Ming Lee, China]	Response: Taken into account- combined with the comment 21286, 11730.
13620	4	50	41	50	41	Tropical cyclones (TCs) [Debra Roberts and Durban Team, South Africa]	Accepted: Add tropical cyclones before (TCs)
14808	4	50	41	50	41	and the simulation capability of TCs has been substantially improved. [Christophe Deissenberg, Luxembourg]	Accepted: change "is" to "has been"
8658	4	50	43	50	43	Maybe "previous" should be replaced by "new" since some references are recent (2014 to 2017) and after AR5 [APECS Group Review, Germany]	Response: Taken into account- combined with the comment 21286.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14810	4	50	51	50	51	continues to increase with improved simulations (Walsh et al., 2016). [Christophe Deissenberg, Luxembourg]	Accepted: remove "the"
3798	4	50	54	50	54	Add reference Kossin, James P. "A global slowdown of tropical-cyclone translation speed." Nature 558.7708 (2018): 104. [Ola Kalen, Sweden]	Accepted: Add the reference Kossin and James (2018).
13622	4	51	7	51	7	Change 'here' to 'there' [Debra Roberts and Durban Team, South Africa]	Accepted
23996	4	51	9	51	10	Add confidence statement for the first finding, i.e. intensity of severe TCs [Hans-Otto Poertner and WGII TSU, Germany]	Response: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high”. Hence, "In this case, the level of confidence need not be explicitly stated." (Please see Paragraphs 8-10, the Points: E) and F) of Paragraphs 11 of "Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties".
8660	4	51	13	51	13	"SH" should be defined in the text [APECS Group Review, Germany]	Accepted: Change that to "Southern Hemisphere (SH)"
14812	4	51	19	51	19	(Michaelis et al., 2017). However, an [Christophe Deissenberg, Luxembourg]	Accepted.
14814	4	51	22	51	22	2013). Note that [Christophe Deissenberg, Luxembourg]	Accepted.
23998	4	51	27	51	29	Add confidence statement [Hans-Otto Poertner and WGII TSU, Germany]	Response: It is the same as the comment 23996."In this case, the level of confidence need not be explicitly stated".
5168	4	51	27	51	35	Suggest including example and references on storm surge risk and impact assessments: 1. Sayaka Hoshino, Miguel Esteban, Takahito Mikami, Hiroshi Takagi, Tomoya Shibayama, 2016 : Estimation of increase in storm surge damage due to climate change and sea level rise in the Greater Tokyo area, Nat Hazards, 80, 539-565 2. Claudia Tebaldi, Benjamin H Strauss and Chris E Zervas, 2012 : Modelling sea level rise impacts on storm surges along US coasts, Environmental Research Letters, 7, 014032. 3. Tomohiro Yasuda, Sota Nakajo, SooYoul Kim, Hajime Mase, Nobuhito Mori, Kevin Horsburgh, 2014 : Coastal Engineering, 83, 65-71. [Sai Ming Lee, China]	Accepted.
14816	4	51	29	51	29	Risk from TC storm surge increases in the [Christophe Deissenberg, Luxembourg]	Accepted.
14818	4	51	38	51	41	very likely that the flood return period in low-lying areas such as coastal megacities has decreased over the past 20th century and high-water events are expected to increase IN FREQUENCY OR IN SEVERITY???? in the future. The compound effects of sea level rise, storm surges and waves on extreme sea levels and the associated flood hazards are assessed in Chapter 6 (Section 6.5.3.3). [Christophe Deissenberg, Luxembourg]	Response: Add "in frequency", i.e., "increase in frequency". The severity is omitted here, because the high-water events imply SEVERITY.
8662	4	51	41	51	41	Check cross reference to 6.5.3.3 [APECS Group Review, Germany]	Reponse: Correct "6.5.3.3" to "6.3.3.3 and 6.3.4"

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3192	4	51	43	51	45	Need uncertainty quantification on these statements. [Robert Kopp, USA]	Accepted: ".....in the future (high confidence)"
14820	4	51	43	51	45	The observed damages from ETCs/TCs in coastal regions have increased over the past 30 years and will continue to do so in the future: under 2.5°C global surface air temperature increase scenario, TCs damages are expected to surge by 63% in the North Atlantic, and by 28% in the Western North Pacific (Ranson et al., 2014). [Christophe Deissenberg, Luxembourg]	Accepted.
18288	4	51	43	51	45	This sentence is imprecise: first of all, the reason that losses are increasing is due to increasing expsoure, see for instance Handmer et al. 2012 in IPCC SREX, and many publications tehrein. This statement needs a qualifier; the increase is not due to anthropogenic climate change. Second, although studies show an increase in damages to doe cyclone intesification (such as Ranson et al. 2014), there are other drivers such as continuing population increases. This section would require a more thorough discussion of hazard and expsoure (and vulnerability) drivers. And needs to include other studie sbesides the cited Ranson et al. paper. [Laurens Bouwer, Netherlands]	Response: Taken into account- combined with the comment 14280. Modified that as the comment 14820 points out.
13624	4	51	45			And in the Indian ocean? East coast of Africa? Africa is also not mentioned in entire section, even though there appear to be global assessments of populations at risk of cyclones. Sea level changes and associated storm surges etc are important for the large coastal populations around the world and deserv a much more detailed discussion, compared with the next section (on long term scenarios) - see next comment. [Debra Roberts and Durban Team, South Africa]	Response: Your comments are very good. However, there are no enough scientific publication provided to support these issues, particularly in East coast of Africa etc. Perhaps, it is suggested to mention that as a gap in the report.
14822	4	51	46	51	46	increased almost [Christophe Deissenberg, Luxembourg]	Accepted.
21670	4	51	49			"poor ETCs/TCs predictability". I really do not understand this point as one of the great developments of ths past few decades (since the 1953 storm surge in the North Sea) has been the development and improvement of storm forecasts including winds, precipitation, surges and extreme sea levels and waves. These predictions are getting better and I see no reason to assume that this will not continue in the short-term mode of weather and event prediction. So even if we have worst storms we will know they are coming and more precisely than we do today. Please review what this is trying to say. [Robert Nicholls, UK]	Response: Change "poor ETCs/TCs predictabiliy and high-intensity landfalls" to " poorly anticipated ETCs/TCs that intensify rapidly just before landfall in a global warmig climate"
14824	4	51	53	51	53	and will increase the risk of related damages or hazards [Christophe Deissenberg, Luxembourg]	Accepted:Change "will result in increasing risk of the related ....." to "will increase the risk of related ....."

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14826	4	51	55	51	56	enhance storm surge, resulting in severe risks and costs, e.g., battering shorelines and damaging structures (Vose et al., 2014). Future heavy precipitations partially induced by extreme ETCs are expected [Christophe Deissenberg, Luxembourg]	Accepted: Remove "For example" and change singular "precipitation" to plural.
14828	4	52	4	52	4	(SLAs) at decadal [Christophe Deissenberg, Luxembourg]	taken into account: text revised to "decadal and shorter"
17448	4	52	4	52	44	An additional uncertainty in sea-level rise prediction is in the ocean climate models' distribution of mixing. Melet et al, 2016 shows that steric sealevel is highly sensitive to the assumptions made about the geographic and vertical distribution of tidally-driven mixing. See Melet, Legg and Hallberg, 2016: Climatic impacts of parameterized local and remote tidal mixing. Journal of Climate, 29(10), DOI:10.1175/JCLI-D-15-0153.1 . [Sonya Legg, USA]	accepted: short statement and reference added
14830	4	52	5	52	5	based on similar efforts [Christophe Deissenberg, Luxembourg]	accepted
14832	4	52	26	52	26	beyond the variability ???? [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
14834	4	52	27	52	27	play a role [Christophe Deissenberg, Luxembourg]	copyedit to be completed prior to publication
8664	4	52	28	52	28	"ENSO" should be defined in text [APECS Group Review, Germany]	accepted
14836	4	52	29	52	29	and mid-latitude regions owing to westward propagating Rossby waves out to 2–5 years (Polkova et al., 2015). Out to ???????????????? [Christophe Deissenberg, Luxembourg]	accepted, text revised to specifiy that the forecast skill extends to 2 to 5 years
22278	4	52	46	52	46	Beyond the literature discussed here, there are a few other studies of note that include projections of SLR beyond 2100, and may be worthy of inclusion in this discussion: Kopp et al. (2014), Kopp et al. (2017), Nauels et al. (2017), Schaeffer et al. (2012), Rasmussen et al. (2018), Bittermann et al. (2017). [Andra Garner, USA]	These references have been invorporated during revision.
21672	4	52	46	54	41	I am pleased to see this section. However, it is not structured as I think would be most useful. There is practical interest in predictions over a century (reflecting the design life of some infrastructure) so there is real stakeholder interest in 2120 (e.g. Nicholls et al., 2013). For example the UK has extended earlier IPCC projections to 2120 to support project appraisal, so what cna this report say about thsi timescale. Beyond this time frames of say 2150, 2200, 2300 and 2500 and then millenia might be considered. A summary table or figure would be useful. Reference: Nicholls, R.J., Townend, I.H., Bradbury, A., Ramsbottom, D. and Day, S. (2013) Planning for long-term coastal change: experiences from England and Wales. Ocean Engineering (doi:10.1016/j.oceaneng.2013.01.025). [Robert Nicholls, UK]	the long-term is discussed in the synthesis section of 4.2
21674	4	52	46	54	41	What about results related to the Paris Agreement. The differences between emission scenarios become clearer post-2100. [Robert Nicholls, UK]	unclear what the reviewer is hinting at

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13626	4	52	46			Section 4.2.3.6 is very long and detailed compared to the previous section. [Debra Roberts and Durban Team, South Africa]	taken into account: the length reflects the significance we see in long-term projections: there is a growing body of literature that points to the importance of near-term mitigation decisions for sea level rise beyond 2100. While we do not address mitigation here, these findings showcase the policy-relevance of long-term projections for short-term decisions. See also comments 19088 and 21672.
21676	4	52	48			"much higher" -- and higher in all cases -- even if we follow the Paris Agreement (Nicholls et al., 2018) and other papers. Reference: Nicholls RJ et al. 2018. Stabilization of global temperature at 1.5°C and 2.0°C: implications for coastal areas. Phil.Trans. R. Soc. A 376: 20160448. http://dx.doi.org/10.1098/rsta.2016.0448 [Robert Nicholls, UK]	accepted and rephrased
4416	4	52	50	52	50	this can be stated more accurately [Peter Clark, USA]	accepted and rephrased
4418	4	52	53	52	54	thisis an inaccurate description [Peter Clark, USA]	accepted and rephrased
4420	4	53	5	53	8	not sure why this sentence (and it's level of detail) is here. [Peter Clark, USA]	accepted, mentioning that thermal expansion continues for centuries is sufficient here.
14838	4	53	6	53	6	traces cases on thermal expansion TRACES CASES??? [Christophe Deissenberg, Luxembourg]	accepted removed
21678	4	53	6			"traces cases" should be "trace gases"? [Robert Nicholls, UK]	accepted removed
14840	4	53	8	53	8	21st century; the induced rise will continue for more than 500 years (Zickfeld et al., 2017). [Christophe Deissenberg, Luxembourg]	accepted sentence has been rephrased
21680	4	53	8			"and for more than 500 years beyond" -- unclear 13 cm/century or 13 cm in total? [Robert Nicholls, UK]	sentence has been rephrase level of detail reduced in line with comment 4420
8666	4	53	15	53	15	This phrase could be incorporated in the paragraph below [APECS Group Review, Germany]	accepted sentence has been removed
14842	4	53	18	53	18	and that the associated surface lowering increases ablation further (positive feedback). [Christophe Deissenberg, Luxembourg]	accepted
4422	4	53	19	53	19	why specify "eastern mountains"? [Peter Clark, USA]	accepted useless addition in this context
11732	4	53	19	53	22	The AR5 used the Robinsn e al. paper to determine the lower threshold of 1C (not 1.5C). The text needs to be checked here and the likely range carefully assessed. Hopefully, the AR6 can make progress from the AR5 on this threshold. [John Church, Australia]	accepted thanks for pointing out
13628	4	53	21	53	22	What is the value of the lower threshold in this study? [Debra Roberts and Durban Team, South Africa]	accepted sentence is removed was out of place
4424	4	53	22	53	22	But this was the study AR5 used to assess the lower range of the threshold! [Peter Clark, USA]	accepted as comment 13628 and 11732
3156	4	53	23	53	24	This seems somewhat contradicted by the previous arguments for intermediate configurations in GrIS during the Last Interglacial. [Robert Kopp, USA]	accepted, there is a seemingly disagreement between a simple model study yielding this lower treshold and the convincing geological evidence that the Greenland ice sheet survived the LIG we addressed this point now explicitly
8668	4	53	23	53	24	In the sentence: "Passing such thresholds produces a long term contribution to sea level rise from the Greenland Ice Sheet of up to 7 m." What is the uncertainty on the 7m ? (low confidence) or (high confidence). [APECS Group Review, Germany]	This number is removed as there is not enough literature to asses this in a meaningful way.
8670	4	53	26	53	26	Check cross reference to "4.2.3.2". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	accepted should be rephrased to cross chapter box on ice dynamics

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11734	4	53	26	53	26	Crossing the threshold does not "produce" the long term contribution but may commit the world to experiencing them. [John Church, Australia]	accepted and rephrased
4426	4	53	31	53	31	if sufficient retreat and warming, at some point SMB will come to dominate [Peter Clark, USA]	rejected this sentence expresses that it might be difficult to regrow a buttressing ice shelves
14844	4	53	34	53	34	to the sea level [Christophe Deissenberg, Luxembourg]	as comment 11950 sentence has been removed
8672	4	53	34	53	42	Another reference is this context (where ice shelves are removed) is Pattyn, 2017 (Sea-level response to melting of Antartic ice shelves on multi-centennial timescales with fasr Elementary Thermomechanical Ice Sheet model (f.ETISH v1.0)) [APECS Group Review, Germany]	accepted reference is included
11858	4	53	34	54	41	the work of Gomez et al should be discussed in these contexts of longer-term as the studies cited here do not all (or not at all) include robust solid earth uplift AND sea level fall associated with ice sheet loss. Barletta et al Science 2018 show these processes could be fast in Pine Island region but the status in East Antarctica and other parts of West Antarctica is more like 100s-1000s of year effects so relevant in this section [King Matt, Australia]	accepted a sentence is added to accommodate the possible effect of the solid Earth interaction on longer time scales.
11950	4	53	34			there is need listing such studies [Chukwuma Anoruo, Nigeria]	rejected the sentence hs been removed completley
14846	4	53	35	53	35	whereby the majority of West-Antarctica decays, ?????????????????? [Christophe Deissenberg, Luxembourg]	accepted awkwardly phrsed and rephrased
11738	4	53	37	53	37	I thik fig 4.11 is meant. [John Church, Australia]	No this refers to the previous sentence being the Golledge et al. 2017 work
8674	4	53	39	53	39	Check cross reference to "4.2.3.2". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	accepted refference should be made to the place where the dynamics of Antarctica are discussed in more detail for the SOD this will be in the cross chapter box XX.
4428	4	53	46	53	46	no - A1B falls between RCP6.0 and RCP8.5 [Peter Clark, USA]	accepted and corrected
14848	4	53	48	53	48	Several parameterizations are used to describe this process. [Christophe Deissenberg, Luxembourg]	accepted
8676	4	53	51	53	51	Check cross reference to "4.4.2". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	accepted as comment 8674
8678	4	53	51	53	51	The phrase "Due to the computational ... West Antarctica" could be improved to make it more clearer (what kind of complexity?) [APECS Group Review, Germany]	rejected the complexity is the adaptive grid explained in the sentence before
20840	4	53	53	53	53	Not "most vulnerable", but the glacier for which the vulnerability was most uncertain. [Tamsin Edwards, UK]	accepted and rephrased
24642	4	54	0			The synthesis section has redundancy to the earlier text but ist text seems much closer to representing an assessment than the review sections. [Hans-Otto Poertner and WGII TSU, Germany]	rejected the apperent redudancy lies in the fact that partly similar papers are discussed in order to get to a projection for 2100 where here
20844	4	54	6	54	10	Edwards et al. (2018) show DeConto and Pollard model also gives very high sea level rise without MICI, indicating that hydrofracturing/MISI are largely behind this. [Tamsin Edwards, UK]	we don't use Deconto and Pollard 2016 anymore
20842	4	54	7	54	8	Ritz et al. also include hydrofracturing. [Tamsin Edwards, UK]	rejected we are not sure what the reviewer wants to argue for in the context that we mention that DP16 yields high rates because of MICI and hydrofracturing.
14850	4	54	9	54	9	(marine ice cliff instability). However, [Christophe Deissenberg, Luxembourg]	accepted

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8680	4	54	10	54	10	Check cross reference to "4.2.3.1". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	accepted refference should be made to the place where the dynamics of Antarctica are discussed in more detail for the SOD this will be in the cross chapter box XX.
20846	4	54	12	54	13	"recent studies…all agree' is inappropriate phrasing for two studies. [Tamsin Edwards, UK]	accepted and rephrased
20848	4	54	16	54	16	Typo: ensemble [Tamsin Edwards, UK]	accepted and corrected
14852	4	54	21	54	23	One should note that here the driving mechanism for the retreat is ocean warming whereas it is a combination of ocean and atmospheric warming in DeConto and Pollard (2016) (see Section 4.2.3.1). [Christophe Deissenberg, Luxembourg]	accepted and rephrased accordingly
20850	4	54	21	54	23	Poorly written sentence. [Tamsin Edwards, UK]	accepted but rephrased in line with comment 14852
18566	4	54	21			comma instead of full stop between timescales and albeit [Christopher Fogwill, UK]	accepted but rephrased in line with comment 14852
8682	4	54	23	54	23	Check cross reference to "4.2.3.1". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	cross reference removed
8684	4	54	25	54	25	Change reference style: "(Clark et al., 2016)" to "Clark et al. (2016)" [APECS Group Review, Germany]	accepted
13630	4	54	25	54	25	Check reference [Debra Roberts and Durban Team, South Africa]	Maya is the link not working??
4430	4	54	35	54	35	and thermal expansion [Peter Clark, USA]	accepted and rephrased
17362	4	54	36	54	36	If this 4 degree figure is considered (only) medium confidence then this is correct of course, however I am not aware of any paleo record showing the GIS maintained at such a high temperature so check or clarify with a reference to time frames. [Pamela Pearson, USA]	There is indeed only medium confidence.The 4 degree is based on modelling studies not on geological evidence. This is now explained more explicitly the first time this is mentioned
14854	4	54	37	54	39	Due to uncertainties regarding the dominant processes that could trigger a major retreat, there is low confidence in more specific estimates of the contribution of the Antarctic ice sheet beyond 2100 (up to 15 m in 500 years). [Christophe Deissenberg, Luxembourg]	accepted and rephrased
12546	4	54	38			"up to 15m" tends to emphasise the highest value. Would it not be more correct to say "from just over 1 m in 1000 years to as much as 15 m in 500 years" (these would be Golledge and de Conto respectively)? [Eric Wolff, UK]	accepted and rephrased
11736	4	54	41	54	41	I think it is important to also comment on the potentially much lower contributions if we substantially mitigated our emissions. This statement should also make its way to the ES. [John Church, Australia]	accepted we have a sentence in the ES reading Sea level rise at the end of the 21st century is strongly dependent on the emission scenario followed, especially in terms of Antarctica's contribution (high confidence).

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13632	4	54	43			Section 4.2.4 should highlight findings that are immediately relevant to governments around the world, namely the higher frequency and seriousness of currently rare extreme sea level events, the number of people and populated places affected, even maps showing where these areas and populations are. These highly relevant details are currently watered down by text that discusses the uncertainties of long-term models. [Debra Roberts and Durban Team, South Africa]	accepted partly. We agree that the section has quite some text on the long-term time scales, but the paleo and the projection and the ice dynamics come together here to explain that a seemingly small difference at the end of the century may end in 10 m difference in the long run. This is an important point to make in a synthesizing section. At the same time we make the point of the exterme events at the end of the section which is important.The synthesis is more easy here as we can simply refer to the figures 4.9 and 4.10 one to one which are the argument that adaptation is inevitable everywhere. The long term is more important for how much adaptation will be needed or whether we prefer to be risk adversive and mitigate. So all in all, we rather do not repeat the ESL section with another figure in this paragraph.
14856	4	54	50	54	50	warm periods the sea level [Christophe Deissenberg, Luxembourg]	accpeted and rephrased
11952	4	54	50		51	provide evidence with literature [Chukwuma Anoruo, Nigeria]	reference are provided in the paleo section
12548	4	54	51			while true, this statement about global mean temperature could be misleading and irrelevant because what is important is polar temperatures.Your current formulation guides the reader to think that a 1 degree global warming due to GHG (as now) is enough to raise sea level by 6-9 m (although unlike the next sentence about the Pliocene, sea level is not actually mentioned), which almost no-one believes. [Eric Wolff, UK]	sea level info is mentioned now for the Last Interglacial, the caveat of a different forcing is added in the same sentence now
4432	4	54	52	54	52	and sea level was? [Peter Clark, USA]	accepted information added
14858	4	54	52	54	54	The sea level estimates for the Mid-Pliocene are highly uncertain but possibly up to 20 m above the current level, with temperatures 1.9 to 3.6 degrees higher and CO2 probably somewhat lower than today. [Christophe Deissenberg, Luxembourg]	accepted and rephrased
14860	4	54	54	54	54	reason for concern [Christophe Deissenberg, Luxembourg]	accepted and rephrased
4434	4	54	55	54	55	need to emphasize that LIG forcing not analogous to today, so not clear that that sensitivity is directly comparable. [Peter Clark, USA]	accepted and more stressed in line with comment 12548
8686	4	55	2	55	3	Add uncertainty language to the sentence: These key finding suggest that with a few degrees of warming, substantial parts of the Antarctic and Greenland ice sheets may disappear on time scales of thousands of years or less (add uncertainty language). However, we lack a firm understanding of the mechanisms which may lead to such an outcome and as a result, the rates of ice loss that may occur. [APECS Group Review, Germany]	rejected the uncertainty language is already provided in the previous sentence
14862	4	55	2	55	3	and to the resulting rates of ice loss. [Christophe Deissenberg, Luxembourg]	accpeted and rephrased
14864	4	55	4	55	4	explain the observed sea level rise. In addition, the temperature changes [Christophe Deissenberg, Luxembourg]	accpeted and rephrased

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14866	4	55	6	55	14	For this reason, much research focuses on mechanisms which could contribute to mass loss for the ice sheets in the possible absence of a strong further warming. Mechanisms put forward are hydrofracturing of ice shelves, Marine Ice Sheet Instability, and Marine Ice Cliff Instability (Section 4.2.3.2). With these mechanisms, a small initial perturbation may induce strong positive feedbacks and thus a more sensitive dynamical response of the ice sheets than observed over the last century. Geological observations provide little constraint on these processes and records of on-going changes since the start of satellite observations are too short allow strong conclusions on possible retreat mechanisms for the ice sheet under present-day climate conditions. However, there is a growing consensus that the ice-ocean interaction may be more important than hitherto assumed (medium confidence). [Christophe Deissenberg, Luxembourg]	accepted and rephrased
4436	4	55	8	55	8	MICI does require strong warming [Peter Clark, USA]	We agree with the reviewer that the formulation required revision. Critical is the presence of ice shelves as long as there are ice shelves marine ice cliff instability will not be very important. Timing of ice shelf decay is uncertain, so we rephrased the sentence completely to be more precise.
8688	4	55	8	55	8	Check cross reference to "4.2.3.2". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	accepted should be cross chapter box in SOD
4438	4	55	16	55	16	worth noting that Church et al. added some ad hoc 20th century contribution from Antarctca, which not be not required with the lower rate for global sea level rise [Peter Clark, USA]	We presume that the reviewer means:"worth noting that Church et al. added some ad hoc 20th century contribution from Antarctca, which IS not required with the lower rate for global sea level rise" Given this assumption we agree with the reviewer but the point we want to make is that there is more evidence now for an acceleration towards the end of the century, hence we maintained the phrasing as is.
8690	4	55	16	55	16	Remove: Where [APECS Group Review, Germany]	accepted and removed
4440	4	55	16	55	25	this paragraph is poorly written [Peter Clark, USA]	accepted we rewrote and shortened the section
14868	4	55	24	55	24	rise and of a decrease in the mass of the ice sheets, there is further evidence of the need [Christophe Deissenberg, Luxembourg]	accepted and rewritten
14870	4	55	27	55	27	Against this background of an improved understanding of the present-day rates of change together with a poor [Christophe Deissenberg, Luxembourg]	accpetd and rewritten along the line of comment 8692

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8692	4	55	27	55	33	The text is hard to understand, a suggestion could be: In the context of improved understanding of the present-day rates of change, but a poor understanding of drivers of ice sheet mass changes, we evaluated projections of models of future sea level rise. Where an increase in the understanding of the processes of future retreat of the Antarctic ice sheet allows us to include processes like MISI in the likely range of the projections (Section 4.2.3). An improved comprehension on century time scales also implies that, especially for projection later in the 21st century, process-based models are more informative than empirical models that are based on statistical correlations over the recent past. [APECS Group Review, Germany]	accepted and rephrased accordingly
4442	4	55	29	55	30	this is a bit deceptive. It's not that AR5 did not understand MISI (they devoted an entire Box to it!), it's that there was no evidence that it had started. That's why it was not included in the likely range. But again, AR5 concluded that IF MISI began this century (with papers published the following year suggesting it had), then its contrinution to the likely range would be several tenths of a meter. Please represent these arguments accurately. [Peter Clark, USA]	accepted a more accurate description is needed which we have provided now.
8694	4	55	30	55	30	Check cross reference to "4.2.3". Maybe "4.2.3.2"? [APECS Group Review, Germany]	rejected 4.2.3 is the projection section and that is intentionally referred to
21682	4	55	30			MISI does not appear to be defined [Robert Nicholls, UK]	This is defined in the cross chapter box to which we refer often
8696	4	55	37	55	37	Check reference to "Figure 4.10". Maybe "Figure 4.7"? [APECS Group Review, Germany]	accepted reference should be given to figure 4.11
24644	4	55	37		39	The uncertainty in timescale is not fully captured. With the difference in long-term sea level it would be useful to qualify the timing and its uncertainty range in a prominent way and with high level of clarity. [Hans-Otto Poertner and WGII TSU, Germany]	accepted we cuased confusion by refering to the wrong figure. 4.11 is provided as an illustration of the the divergence on long time scale with appropriate referencing we hope this is clairified
13634	4	55	39	55	39	Suggest spelling out the range if possible. [Debra Roberts and Durban Team, South Africa]	Figure 4.11 is meant to illustrate the strong divergence on the long time scale not the microlevel of dm and cm in the 21st century.
3158	4	55	43	55	44	"Only probabilistic scenarios can be defined" -> "probabilisic scenarios can only be defineD" [Robert Kopp, USA]	accpeted and rephrased
3800	4	55	43	55	44	Change to "Hence, probabilistic scenarios can only be defined..." [Ola Kalen, Sweden]	accpeted as previous comment rephrased
14872	4	55	44	55	44	can be defined, which strongly depend [Christophe Deissenberg, Luxembourg]	accepted komma added
23240	4	55	47	55	47	whether or not this strong divergence between scenarios "beyond 2100" will develop' [Tamsin Edwards, UK]	accepted paragraph rewritten
14874	4	55	47	55	48	WHAT DO YOU MEAN HERE? IN MY UNDERSTANDING WE KNOW VERY WELL IF AND WHEN SCENARIOS DIVERGE, AT LEAST IN A PROBABILISTIC SENSE. DO YOU MEAN WHETHER OR WHEN THE ONE OR THE OTHER OF TWO POSSIBLE DIVERGENT FUTURES WILL BE REALIZED? IN ANY EVENT, A REFORMULATION IS NEEDED Whether and when this strong divergence between the scenarios will develop is impossible to judge based on existing literature and may well be convincingly shown after a tipping point is passed. [Christophe Deissenberg, Luxembourg]	accepted paragraph rewritten

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14876	4	55	47	55	57	HERE MY REFORMULATION OF THE WHOLE PARAGRAPH -- LOTS OF GUESSING HERE! The strong divergence between the scenarios makes it impossible to predict the future evolution on the basis of the existing literature. The specific trajectories that will be followed may depend crucially on if and when certain tipping points are reached. Most critical in that respect are presumably the tipping points corresponding (1) to the threshold where the ablation in Greenland becomes larger than the accumulation irrespective of the magnitude of the calving flux, causing an irreversible and nearly full retreat of the ice sheet; and (2) to the thresholds for loss of ice shelf stability in West-Antarctica, which depend on surface melt and sub-ice melt in combination with geometrical condi-tions favoring retreat. We may be able to better predict which trajectory of GMSL in Figure 4.11 will be followed after the tipping point is passed. Improved physical modelling may, together with dedicated monitoring systems, help refine our understanding of the relevant mechanisms. We conclude that the sea level rise at the end of the century is strongly dependent on the emission scenario indicating the importance of mitigation in minimizing the risk to low-lying coastlines and islands (high confidence). [Christophe Deissenberg, Luxembourg]	accepted paragraph rewritten
20852	4	55	47	55	57	Poorly written paragraph - needs shorter, clearer sentences. [Tamsin Edwards, UK]	accepted paragraph rewritten
21684	4	55	47	55	57	the use of tipping point is confusing -- "a tipping point" and "the tipping point" are both used -- needs review. [Robert Nicholls, UK]	accepted paragraph rewritten
8698	4	55	48	55	48	There is a letter "i" with lower font size than the rest of the text. [APECS Group Review, Germany]	accepted paragraph rewritten
8700	4	55	51	55	51	Maybe use another word to replace "secondly" [APECS Group Review, Germany]	accepted paragraph rewritten
3804	4	55	53	55	53	Reference to what study is behind figure 4.11 is lacking [Ola Kalen, Sweden]	Accepted we have added references and described more carefully what Figure 4.11 wants to convey.
3802	4	55	55	55	55	change to "systems" [Ola Kalen, Sweden]	accepted section has been rephrased
17364	4	55	55	55	57	Important sentence perhaps to raise up to ES. Consider adding, "In periods beyond 2100, emissions scenarios become even more important to protection of not only low-lying, but increasingly higher coastal regions above 2 meters." [Pamela Pearson, USA]	We agree with the reviewer this message is brought forward to the Executive summary
20854	4	55	55	55	57	Strongly" is just not substantiated - see earlier comments. [Tamsin Edwards, UK]	accepted not at the end of the century but we intended to say on millennial time scales as shown in the figure. We corrected this omission
21686	4	55	55	55	57	I do not agree that sea-level rise at the end of the century is strongly dependent on emission scenarios (see Nicholls et al., 2018). The effects are really apparent in the 22nd and 23rd Century and sea-level rise is a long timescale phenomenoa. Reference: Nicholls RJ et al. 2018. Stabilization of global temperature at 1.5°C and 2.0°C: implications for coastal areas. Phil.Trans. R. Soc. A 376: 20160448. http://dx.doi.org/10.1098/rsta.2016.0448 [Robert Nicholls, UK]	accepted not at the end of the century but we intended to say on millennial time scales as shown in the figure. We corrected this omission

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24640	4	56	0			Climate physics and associated sea level rise are covered in the first 54 pages. etter integration of WGI and WGII material with an assessment of vulnerabilities would be useful to bring policy relevant meterial up front as needed. [Hans-Otto Poertner and WGII TSU, Germany]	The model adopted in this chapter is that we have the integration in the ES, followed by the physical justification of the claims and a bridging section between physics and adaptation 4.2.4 followed by the WG2 material. Section 4.2.4 aims to bring accross what eventually matters for adaptation (and mitigation)
24646	4	56	0			Figure 4.11 would benefit from uncertainty ranges and an explanation why RCPs 2.6 or 4.5 would lead to overshoot? [Hans-Otto Poertner and WGII TSU, Germany]	partly accepted increasing error bars over time are shown. We don't intend to argue that there is overshoot and we will adjust the figure accordingly
2112	4	56	1	56	1	This schematic conceptual illustration is wrong. The lines are not showing an accelerated trend but the opposite. The problem seems to be caused by using natural scale in ordinate axis and logarithmic scale in abscissa axis. Please, use natural scale or logarithmic scale in both axis. [Josep Medina, Spain]	rejected The scales are chosen to illustrate that seemingly small differences at 100 year time scale may turn out to be very large differences on long time scales. After 100 years it is not about the details any longer but the big picture therefore we use dotted lines.
1820	4	56	1	56	5	Figure 4.11 seems drawn by the authors based on their own expert judgment? Again a reference and/or explanation how values after year 100 were estimated is required, even if they are indicative, and also an explanation what these scenarios mean after year 2100. Constant CO2 until year 1000? Especially the low upper bound on RCP2.6 is worrying. Are the authors sure Thwaites will not have a destabilizing impact on the broader WAIS as suggested in the caption of Fig. 4.6? What about the 4 examples of MISI already going on as detailed on page 33? This also applies to the small uncertainty bands given to Antarctic Ice Dynamics under RCP2.6 and to a lesser extent under RCP4.5. I am not sure the report in Table 4.3 and Fig. 4.11 is consistent with the findings described under 4.2.3.1.2 [Sybren Drijfhout, Netherlands]	no it is not drawn on expert judgement. Up to 100 years it is based on Table 4.3 and internally consistent in the chapter. The dotted line indicates a future where the Antarctic contribution is added on top of the 100 year projection in Table 4.3. The indication of the uncertainty arises from averaging two studies providing estimates for the long term contribution of Antarctica. This is all much better explained in the text now.
11740	4	56	1	56	5	The origins of this figure need to be clear. [John Church, Australia]	It is based on projection up to 100 years as presented in 4.2.3 and inidcative based on two studies . We considerably improved the writing of this section and the way we calculated this.
17784	4	56	1	56	5	Time horizon of 1000 years is irrelevant in a policy-making context. 200 years is sufficient for that applications. Reviewer suggests to use linear scale of the time axis, not logarithmic scale [Hessel Voortman, Netherlands]	rejected. The policy relevecance lies in the fact that choices made now which yield only decimeter differences at the end of the century may imply a diference of 10 meter after 1000 years.
21688	4	56	1	56	6	Figure 4.11. Sea level peaks under RCP2.6 and RCP4.5 about 600 to 700 years into the simulations -- to me this is a new result and hence is worthy of discussion -- so why? Maybe there is a journal paper that is coming? [Robert Nicholls, UK]	Acceped this was wrong and a plotting artefact
21690	4	56	1	56	6	Figure 4.11. what about uncertainty throughout the simulations? [Robert Nicholls, UK]	Accepted the figure is much better explained
21692	4	56	1	56	6	Figure 4.11. The forcing is not clear in terms of arbituary time and the RCPs -- needs more explanation. [Robert Nicholls, UK]	Accepted the figure is much better explained
12550	4	56	1			You show a rise of 10 m in 1 kyr for RCP8.5. I know this is only menat to be schematic but it will certainly be used and misused. The rest of the chapter gives no basis for the number that I can see, so are you happy to justify that number? It will be cited! [Eric Wolff, UK]	accepted, the figure needed a better embedding we have attempted to do so.
8702	4	56	2	56	2	Check Figure 4.11 caption. The end of the first phrase is missing something ("projections for the " ?) [APECS Group Review, Germany]	Accepted the figure is much better explained
3160	4	56	2	56	5	This figure seems silly, and should be made more robust or deleted. [Robert Kopp, USA]	Accepted the figure is much better explained
3806	4	56	2	56	5	Reference to what study is behind figure 4.11 is lacking. Explanation to vertical error bars are lacking in the figure text [Ola Kalen, Sweden]	Accepted the figure is much better explained

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20856	4	56	2	56	5	Caption is unclear and needs to say which papers/table are behind the numbers [Tamsin Edwards, UK]	Accepted the figure is much better explained
8704	4	56	10	56	10	Check cross reference to "4.2.3". Maybe "4.2.3.4"? [APECS Group Review, Germany]	rejected 4.2.3 is on the projections we added in addition a reference to a Table.
21694	4	56	10	56	13	Good and clear. [Robert Nicholls, UK]	Accepted thanks
14878	4	56	14	56	14	return periods will decrease dramatically. ??????? [Christophe Deissenberg, Luxembourg]	accepted in order to circumvent what a decrease in return period is we expressed it in terms of frequency
21696	4	56	14	56	16	This is a better case for the importance of mitigation and 21st Century benefits, but it requires the wave run-up component. [Robert Nicholls, UK]	rejected unclear what the reviewer means the strongest case for mitgation is the divergence of the Antarctic scenarios in the long run. There is no compelling evidence that ESL will change due to run-up changes.
8706	4	56	16	56	16	The need for more studies in another areas like South America, India, Africa etc could be highlighted in the end of subsection 4.2.4. This is clear in Figure 4.10. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	accepted we have made a new choice of examples to reflect those regions better in Figure 4.10
24000	4	56	19			Why is it 'sea level change' here, when the main title refers to 'sea level rise'? [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - Title has been modified
22280	4	56	23	56	26	I suggest re-wording the opening sentences of this paragraph. The way it is written now, it sounds as though the authors are implying that we're currently in a low-emissions scenario (RCP2.6), and could see even more severe SLR if we move towards high emissions (RCP8.5). [Andra Garner, USA]	Taken into account. The whole introduction of 4.3 had been modified. The new version addresses this comment.
10738	4	56	23	56	32	Very relevant paragraph [Jacques Andre Ndione, Senegal]	Noted
21698	4	56	31			Still unclear what responses means here. [Robert Nicholls, UK]	Taken into account. Refernce to section 4.4 had been removed from this paragraph.
13636	4	56	35	59	21	Box 4.2 This box contains a great deal of information that is highly relevant to policy makers, but that receives superficial treatment, especially when compared with the seemingly indepth treatment of ocean science topics in previous section. For policy makers information that brings the message home in terms of human vulnerabilities will increase the impact of this SROCC. [Debra Roberts and Durban Team, South Africa]	Taken into account - text revised to express more clearly that the purpose of the box is the highlight the methodological advences in assessment and less so the new insights for exposure and vulnerability
8708	4	56	37	59	19	Few studies about South America and Africa (and another parts of Asia) are presented and exemplified. If there are no studies in these regions (and elsewhere in the World), it should be highlighted along all the report. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	Taken into account in other parts of the Chapter. This section highlight methodological developments and thus geographic representation is less in the focus. We however made and effort to increase geographic diversity in general and the representation of South America and Africa in particular throughout the chapter.
14880	4	56	40	56	42	level rise-related coastal hazard risk and enable the identification and localization of appropriate adaptation and risk reduction strategies. However, the relevant strategies are very context-specific and they are not yet widely adopted (high evidence, high agreement). [Christophe Deissenberg, Luxembourg]	Accepted – text revised
18290	4	56	41	56	41	Unclear if the adoption of assessments, or risk reduction strategies is not widespread. Probably, it is the latter. I would suggest to split this long sentence, and explain that assessments are done in many places, but implementation of strategies not. [Laurens Bouwer, Netherlands]	Accepted – text revised
13638	4	56	42	56	42	Italicise confidence language [Debra Roberts and Durban Team, South Africa]	Accepted – text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13640	4	57	12	57	29	This is very important: populations at risk of sea level rise for instance. Is this available at the global scale? Even for currently observed changes, especially coastal storm surge etc. Apparently so? (line 50-53). This information deserves a figure, eg a map of where highest populations at current and immediate future risk are. [Debra Roberts and Durban Team, South Africa]	Rejected - this box is concerned with methodological advances rather than with exposure which is dealt with in Section 4.3.2 .
14882	4	57	18	57	18	SOMETHING MISSING HERE? modelling such as for diurnal differences [Christophe Deissenberg, Luxembourg]	Accepted – text revised
3162	4	57	31	57	56	Many of these studies do not look at the dynamics of migration, instead assuming that people are sitting ducks and continue to live in at-risk areas until the year upon which the study focuses. [Robert Kopp, USA]	Taken into account - the intention of this section was to note that few studies emerged, which start to taka into account the futur dynamics including migration
3164	4	57	31	57	56	These descriptions are hard to follow for someone not deeply familiar with the literature (e.g., want is a 'state-and-transition' model?) [Robert Kopp, USA]	Taken into account - the section has been revised
21700	4	57	31	57	56	Do these consider any feedback of growing hazards on migration? [Robert Nicholls, UK]	Yes, some of the cited studies concider migration in exposure assessment such as (Merkens et al., 2016; Hauer, 2017)
10740	4	57	32	57	33	The issue dealing with the use of cuurent data of population, land use and ecosystem against projected hazards needs a lot clarification… In fact, if projected data of of population, land use and ecosystem are available, why not usiing those ones? If not, the alternative solution will be to use existing data; isn't it? If i take of Africa, where census population are sometimes not available, existing data can be used. [Jacques Andre Ndione, Senegal]	In many cases projected data for exposure and vulerability is not available. The message of the section is that without these projections, future risk is calculated by combining present-day exposure and vulnerability with future extant of se-level rise and coastal flooding. There are few studies only which have taken the next steps and use projections of exposure and vulnerability. However, these projecttions also come with uncertanity. The section is rewritten to make this more clear.
18292	4	57	33	57	37	Please add other studies, including Jongman et al. 2012 (Global Environmental Change, doi: 10.1016/j.gloenvcha.2012.07.004. [Laurens Bouwer, Netherlands]	Taken into account - added
14884	4	57	46	57	47	(Shepherd, 2015); and anticipate future risk by projecting the evolution of socially vulnerable sub-populations (Hardy and Hauer, 2018). [Christophe Deissenberg, Luxembourg]	Taken into account - the sentence has been revised
11954	4	57	53		55	show evidence with gloss [Chukwuma Anoruo, Nigeria]	The comment is unclear
18294	4	57	54	57	54	Please rephrase to "is projected to increase". [Laurens Bouwer, Netherlands]	Accepted
8710	4	57	55	57	57	About the grouth of population in exposure in the LECZ in Asia and Africa, what are the assumpitions and type of projection (geometric?) of the demographic models? (since it is observed a decrease in the fertility in the all the World, even in developing countries). The rates of grouth presented in the report seems very high (and maybe, unrealistic). [APECS Group Review, Germany]	Taken into account - Merkens et al. take the population projections with all underlying assumptions, e.g., fertility rates, from the SPP database of IASS. These national projections of population growth are then sub-nationally refined based on observed differences between urban/rural and coastal/non-coastal growth and urbanization for the coastal zone without, however, changing the national numbers.
10742	4	58	3	58	5	It would be good to restructure this sentence in that way: "It is recognised that climate risk is not just hazard driven, but also a socio-economic phenomenon that evolves with changing societal and institutional conditions (high evidence, high agreement)." The weakness of institutions is very relevant to be discussed, in the case it's a very harmful and imperilling issue that can exacerbate contextual vulnerabilit and outcome vulnebility… [Jacques Andre Ndione, Senegal]	Accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
23196	4	58	5	59	2	Regarding multiple hazard approaches, the example of the NATHAT in Haiti is exemplary, which includes tectonic (earthquake) and coastal (hurricanes and storms) coastal hazards in an integrated and systemic perspective adressing vullnerability and risk management. Prepared by prepared by the Government of Haiti, with support from the World Bank, the Inter-American Development Bank, and the United Nations System, it also shows that international institutions are on the forefront of the reflection on vulnerability and risk assessment and management thinking. NATHAT, 2010. Analysis of Multiple Natural Hazards in Haïti. République de Haïti, Port au Prince, 63 p. [Sebastian Weissenberger, Canada]	Noted, the section mainly lists advances since AR5 and thus not all references are concidered
8712	4	58	11	58	11	To follow previous style, maybe change numeration i), ii), …, v) to (i), (ii),…,(v) [APECS Group Review, Germany]	Accepted
18296	4	58	17	58	20	Projections are also made on the basis of extrapolating empirial information of past trends in flood vulnerability; please add for instance Jongman et al. 2015 (doi:10.1073/pnas.1414439112); Mechler and Bouwer 2015 (doi:10.1007/s10584-014-1141-0); and Kreibich et al. 2017 (doi:10.1002/2017EF000606). [Laurens Bouwer, Netherlands]	Accepted
14886	4	58	22	58	22	is exceedingly hard to predict [Christophe Deissenberg, Luxembourg]	Accepted
18298	4	58	23	58	26	Another new method is the proposed inclusion of human bahaviour, please include the discussion by Aerts et al. (doi:10.1038/s41558-018-0085-1). [Laurens Bouwer, Netherlands]	Accepted
11610	4	58	28	58	43	I think it would be great if the author gives such example of Social-ecological vulnerability. To direct the reader to the real world. [Sulistyawati Sulistyawati, Indonesia]	Taken into account
23194	4	58	28	58	43	Social-ecological vulnerability assessments have also be performed in developed nations less dependent on coastal natural resources. A good example is the method developed by Meur Féréc et al. (2006) in France which includes exposure, management, remembrance and perception of risks as elements. Meur-Ferec, C., P. Deboudt, V. Morel, 2008, Coastal Risks in France: An Integrated Method for Evaluating Vulnerability, Journal of Coastal Resarch, 24, 178–189. [Sebastian Weissenberger, Canada]	Taken into account, an example of a study in developed nations has been included. However, we selected a more recent, post-AR5 reference for this purpose.
21702	4	58	28			New open access book by Nicholls et al (2018b) on integrated assessment of coastal Bangladesh maybe relevant here. Reference RJ Nicholls, CW Hutton, WN Adger, SE Hanson, MM Rahman, M Salehin (editors) 2018. Ecosystem Services for Well-Being in Deltas: Intergated Assessment for Policy Analysis. Springer, Freely downloadable at https://link.springer.com/book/10.1007%2F978-3-319-71093-8 [Robert Nicholls, UK]	Taken into account in other parts of the Chapter. The book is a very valuable source of information and referenced now in other parts of the chapter. Here it fits less well as it is not providing new methodologies for SES-type vulnerability assessments.
10744	4	59	14	59	14	Instead of writing "(e.g. proximity to the sea cannot always be fully compensated by being wealthy) (Tonmoy and El- Zein, 2018).", please write "(e.g. proximity to the sea cannot always be fully compensated by being wealthy; Tonmoy and El-Zein, 2018)." [Jacques Andre Ndione, Senegal]	Accepted – text revised
24002	4	59	31	59	32	'Development' and especially 'underdevelopment' are very outdated terms. Suggest to revise wording, e.g., simply delete 'under-development'. The message of the sentence stays the same by referring to different 'patterns of development' (which is still quite vague…) [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - Word "under-development" removed

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
23188	4	59	37	59	39	For historically rooted drivers of coastal vulnerabilities, see also the study of coastal zones in Canada, France and Senegal, including changes in resource use, in urbanisation and transport patterns and the rise of tourism in Weissenberger, S., M. Noblet, S. Plante, O. Chouinard, J. Guillemot, M. Aubé, C. Meur-Ferec, É. Michel-Guillou, N. Gaye, A. Kane, C. Kane, A. Niang, A. Seck, 2016. Changements climatiques, changements du littoral et évolution de la vulnérabilité côtière au fil du temps : comparaison de territoires français, canadien et sénégalais. VertigO, Volume 16, Issue 3, DOI: 10.4000/vertigo.18050 [Sebastian Weissenberger, Canada]	Taken into account - This paper had been included
21704	4	59	44			"sediment transport " -- again sediment supply or sediment budget would be a better word [Robert Nicholls, UK]	Taken into account - "Transport" replaced by "supply" (as "budget" does not refer to a process per se).
18300	4	59	51	59	55	I disagree with this paragraph. Many studies are available that specifically deal with local SLR projections and impacts. Lack of local projections of SLR has not been a limitation; rather global projections have been downscaled or adapted. Also, effects of SLR are typically felt through extremes. So there is a valid focus on tropical cylones, surges etc. as is done in Chapter 6 (and Chapter 4). All coasts have some resilience against average sea-level and annual extremes. Please revise, and bring in line with common vulnerability and adaptation assessments. [Laurens Bouwer, Netherlands]	Regarding SLR projections, while local projections exist for absolute SLR (and are indeed based on downscaling exercise), local-scale models still suffer from capturing the diversity of impacts
3166	4	59	54	59	56	This is contradicted earlier in the chapter. Local SLR projections have been developed by several authors (e.g., Slangen et al 2014, Kopp et al 2014) for sites around the world. [Robert Kopp, USA]	Accepted – Text revised (i.e., removal of the sentence "...is the relative lack of local SLR projections in many plorldwide, given that exposure and vulnerability are context-specific per essence.") .
21534	4	60	0			Consider also additional locations such as the Louisana (United States) as an example of regions on sea level rise threat. The loss doesn't have to be of indigenous and local knowledge, but of knowledge of modern inhabitants as well. https://www.ecowatch.com/louisiana-sea-level-rise-2178631264.html [Tseng Rose, USA]	Taken into account - This point is already considered at the end of the 1st paragraph on section 4.3.2.6.2.
3168	4	60	10	60	10	This sentence is barely intelligible. [Robert Kopp, USA]	Taken into account - Rewording.
13642	4	60	14	60	16	It is a pity that the authors feel there is not space to report further on important emerging issues. Perhaps this will receive due attention later in the report? [Debra Roberts and Durban Team, South Africa]	Taken into account - The initial formulation was clumsy. The new version no longer refers to length limitations, and rather emphasises the relevance of the four examples of anthropogenic drivers described in sections 4.3.2.6.1 to 4.3.2.6.4.
8714	4	60	16	60	17	Check cross reference to "Cross Chapter Box 3" [APECS Group Review, Germany]	Taken into account - Checked
10746	4	60	22	60	30	This section can be improved based on findings coming from the "Global Gender Environment Outlook", published by UN Environment in 2016... [Jacques Andre Ndione, Senegal]	Rejected - This GGEO report provides a broad overview of the nexus between gender and the environment, but is too general on climate change with no specific extended discussion on sea-level rise. It does not bring new material on gender and sea-level rise, and thus cannot be included into SROCC Chapter 4.
11956	4	60	26			after for example, it is prefarable that the citations in line 29 and 30 be lifted. [Chukwuma Anoruo, Nigeria]	Rejected - This part reports on the conclusions of recent scientific publications. This is the mandate of the IPCC.
11482	4	60	42	60	42	there is a typo, should be 'determining' [Kirsten Davies, Australia]	Accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
11484	4	60	42	60	42	This comment refers to the use of ILK. I think it is inappropriate to group together indigenous knowledge with local knowledge the two groupings are different. I note that IPBES now uses IPLC (indigenous people and local communities) and notes how the two are separate, but they still tend to group together in their reports. So I dont believe that this approach solves the problem. I also note that in the content of this FOD, the emphasis is placed on indigenous and not local knowledge. I suggest that you rethink. [Kirsten Davies, Australia]	Taken into account – Text revised according to decisions made within the SROCC Cross-Chapter Box 3 on "Indigenous knowledge and local knowledge", which now distinguish between the two, as you suggest. Terms are also included in SROCC Glossary.
24944	4	60	43	60	46	Consider adding Weatherhead et al., 2010. [Elizabeth Weatherhead, USA]	Rejected - The SROCC mandate is to stock take on the post-AR5 literature as much as possible. Also, the current draft of Chapter 4 does not discuss the added-value of merging indigenous knowledge and modern scientific methods.
3456	4	60	46	60	56	It would be good to explain one of these examples in bit more detail in box [Mahmood Riyaz, Maldives]	Rejected - Unfortunately, SROCC boxes have been discussed already and there is no space for adding some. The references cited in the text provide more details.
14888	4	61	11	61	11	mechanisms will [Christophe Deissenberg, Luxembourg]	Accepted
8716	4	61	16	61	16	Maybe "LK" should be also defined in text [APECS Group Review, Germany]	Taken into account – Now SROCC distinguishes between indigenous knowledge and local knowledge, with a dedicated cross-chapter box (No. 3 within Chapter 1). Terms are also included in SROCC's Glossary.
24004	4	61	16	61	16	Unclear acronym LK – probably for local knowledge, but not very common, suggest to spell out, or refer to ILK [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account – Now SROCC distinguishes between indigenous knowledge and local knowledge, with a dedicated cross-chapter box (No. 3 within Chapter 1). Terms are also included in SROCC's Glossary.
8718	4	61	19	61	19	Maybe change citation style: "(Lazrus (2015), p. 56)" to "(Lazrus, 2015, p. 56)" [APECS Group Review, Germany]	Accepted
16082	4	61	25	61	26	Presume it should be "developed", not "developing", since it goes on to refer to the U.S. [Nathan Ross, New Zealand]	Accepted
13990	4	61	38	61	56	Given the significance of urbanisation as a global megatrend and the amount of infrastructure in the coastal zone some of these figures (and related figures from 4.3.3.3.1) should appear in the Executive Summary to indicate the extent of the risk in coastal areas. [Debra Roberts and Durban Team, South Africa]	Accepted
21708	4	61	41	61	56	There is redundancy in this paragraph [Robert Nicholls, UK]	Taken into account
21706	4	61	41			How important is " (re) settlement of some indigenous communities" -- given the huge demographic trends discussed earlier I would think this is orders of magnitude smaller and of little consequence except in few locations. [Robert Nicholls, UK]	Taken into account - Sentence added to highlight there little importance globally, but there critirole in shaping vulnerability locally.
8720	4	61	43	61	44	Check brackets in the phrase and citation after "Zone" [APECS Group Review, Germany]	Taken into account
14890	4	61	44	61	44	and in significant [Christophe Deissenberg, Luxembourg]	Accepted
24006	4	61	45	61	55	Need to revise these sentences and references – includes duplications. [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - Rephrasing
8722	4	61	45	61	56	Check the phrase between lines 45 and 56. There are repetead phrases (line 45, line 49-50) [APECS Group Review, Germany]	Taken into account - Rephrasing

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14892	4	61	46	61	46	countries, as documented in extensive [Christophe Deissenberg, Luxembourg]	Accepted
14894	4	61	48	61	49	2016) and the Pacific, where ~57% of Pacific Island countries' built infrastructure is located in risk-prone coastal areas (Kumar and Taylor, 2015) (high evidence, high agreement). [Christophe Deissenberg, Luxembourg]	Accepted
8724	4	61	56	61	56	Few studies about South America and Africa (and another parts of Asia) are presented and exemplified. If there are no studies in these regions (and elsewhere in the World), it should be highlighted along all the report. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	Taken into account - New references have been included
24008	4	62	1	62	1	Depopulation trends can be observed in many small islands that are dependent areas of continental states, with similar implications, e.g., loss of local knowledge, lack of human capital, expertise, social cohesion, etc. Suggest to cover this here as well. [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account (through the issue of rural exodus especially)
21710	4	62	1	62	6	Similar processes described in the Maldives -- Speelman (2015) PhD thesis University of Southampton. Reference: Speelman, Laurens Hessel (2015) Empirical analyses of migration in small islands: the role of environmental and social factors. University of Southampton, Faculty of Engineering and the Environment, Doctoral Thesis, 201pp. Available at https://eprints.soton.ac.uk/397375/ [Robert Nicholls, UK]	Taken into account
8726	4	62	8	62	13	Check the citation style, when an entire phrase (quoted phrase) from a reference is reproduced in report text. Maybe use 'phrase' instead of "phrase" [APECS Group Review, Germany]	Accepted
8728	4	62	11	62	13	What is reference of the quoted phrase "while traditional … of tourism"? It seems there are multiple reference (Ball et al., Nurse et al. and Duvat et al.), is it correct? [APECS Group Review, Germany]	Accepted
13644	4	62	16	62	32	The section should also explore the contributions of SLR to the fracturing of social capital (e.g. the loss of social capital due to migration as a response to SLR) [Debra Roberts and Durban Team, South Africa]	Taken into account - No specific references found, but reference made to sub-section 4.3.3.6.4 on Social values at risk from SLR.
23192	4	62	16	62	32	It is to note that social capital is sometimes also referred to by other qualifiers, such as social cohesion, cf this study in New Brunswick, Canada: Rabeniaina, T. R., O. Chouinard, S. Weissenberger, 2014. L'adaptation en zone côtière: de la parole aux actes: études de cas de deux territoires du Sud-Est du Nouveau-Brunswick (Canada) concernant les dynamiques sociales et les apprentissages mutuels de communautés côtières face aux enjeux climatiques et à la gouvernance locale. Études caribéennes Volume 27-28, DOI: 10.4000/etudescaribeennes.6970. [Sebastian Weissenberger, Canada]	Taken into account
13256	4	62	16	62	35	This chapter mentions Social capital and Risk perception. These generic terms should be included in Chapter 1. [Zelina Zaiton Ibrahim, Malaysia]	Taken into account
2156	4	62	21			replace the word 'etc' with 'among others' [Chandani Appadoo, Mauritius]	Accepted
260	4	62	35	62	35	This section would benefit form a sentence stating that risk is perceived subjectively and so differentiatedly [Robert Oakes, UK]	Accepted
13646	4	62	37	62	37	Delete 'and' before 'Australia' [Debra Roberts and Durban Team, South Africa]	Accepted
262	4	62	38	62	38	Xynthia storm, should be "Storm Xynthia" [Robert Oakes, UK]	Accepted

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21712	4	62	40			in addition many of the buildings impacted by Xynthia were single storey -- so there was no possibility of vertical evacuation -- similar to New Orleans [Robert Nicholls, UK]	Taken into account
264	4	62	44	62	44	"Aftermath" is ambiguous - does it refer to risk memory was affected after Katrina, or did the disaster occur in the aftermath of Katrina? If the latter, then might be better to not mention aftermath - the conditions and impacts mean it was a disaster during, not just after the impact of the storm. [Robert Oakes, UK]	Taken into account
8730	4	62	47	62	49	Check the citation style, when an entire phrase (quoted phrase) from a reference is reproduced in report text. Maybe use 'phrase' instead of "phrase" [APECS Group Review, Germany]	Accepted
8732	4	62	47	62	49	What is reference of the quoted phrase "gender, political part … value orientations"? It seems there are multiple reference (Kellens, Carlton and Jacobson, Lujala etc), is it correct? [APECS Group Review, Germany]	Accepted – problem fixed
16084	4	62	47	62	49	Also, hyperbolic disaster narratives, such as "climate refugees" and "sinking states", create a sense of hopelessness and risks diverting international finance assistance away from relevant vulnerable communities. See: Alberto Costi and Nathan Jon Ross "The Ongoing Legal Status of Low-Lying States in the Climate-Changed Future", Chapter 6 in Petra Butler and Caroline Morris (eds) "Small States in a Legal World" (Springer, 2017) at 102-103. Nikita Perumal "'The places where I live is where I belong': community perspectives on climate change and climate-related migration in the Pacific island of Vanuatu" (2018) 13(1) Island Studies Journal 45 at 50. [Nathan Ross, New Zealand]	Taken into account - This point (and reference Perumal 2018) has been integrated to the Cross-Chapter Box 5 on "Low-lying islands and coasts". Not accessed the Costi and Ross chapter.
24010	4	62	52	63	6	This section is to conceptual and unspecific to SLR. [Hans-Otto Poertner and WGII TSU, Germany]	Rejected - These sentences are used to show that the level of knowledge on the processes driving climate change-related hazards (and physical impacts) is usually considered as important to drive people's risk perception, and then exposure and vulnerability. They introduce the point that besides growing literature focussed on climate change at large, emerging one addresses SLR in particular. As there is not a huge amount of literature in SLR risk perception, some general statements are neede to guide the reader.
8734	4	62	57	63	1	Check the citation style, when an entire phrase (quoted phrase) from a reference is reproduced in report text. Maybe use 'phrase' instead of "phrase" [APECS Group Review, Germany]	Accepted
8736	4	63	9	63	10	Check the citation style, when an entire phrase (quoted phrase) from a reference is reproduced in report text. Maybe use 'phrase' instead of "phrase" [APECS Group Review, Germany]	Accepted
8738	4	63	13	63	14	Check the citation style, when an entire phrase (quoted phrase) from a reference is reproduced in report text. Maybe use 'phrase' instead of "phrase" [APECS Group Review, Germany]	Accepted
8740	4	63	13	63	14	What is reference of the quoted phrase "markedly … local environment"? It seems there are multiple reference (Milfont, Lujala, van der Linden etc), is it correct? [APECS Group Review, Germany]	Accepted – problem fixed

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13648	4	63	13	63	15	The source and page number of direct quotation should be provided. All other references could be inserted before the direct quote. [Debra Roberts and Durban Team, South Africa]	Accepted – problem fixed
21714	4	63	17	66	42	Section 4.3.2.3. This section does not seem well structured or thought through [Robert Nicholls, UK]	Taken into account - section revised
24012	4	63	17			Although this section is titled 'Environmental Drivers…' it discusses a lot of actually anthropogenic drivers, e.g., the drivers causing subsidence (oil, water extraction), coastal squeeze (coastal infrastructure), etc. Maybe the term 'drivers' is not used consequently here. [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - text revised (it is called Recent knowledge on the environmental dimension of exposure and vulnerability)
14896	4	63	30	63	30	local adaptation to sea [Christophe Deissenberg, Luxembourg]	Taken into account - text revised
21716	4	63	35	64	48	Section 4.3.2.3.1 This section has an unclear title -- called Recent Knowledge, but the core content seems to be ecosystem dynamics and loss [Robert Nicholls, UK]	Taken into account - text revised (Recent knowledge on the environmental dimension of exposure and vulnerability)
3170	4	63	38	63	40	Isn't land use change/fragmentation at least as an important driver of ecosystem stability? [Robert Kopp, USA]	Accepted - Sentence revised
14898	4	63	48	63	48	(Thompson et al., 2015). Likewise, responses are [Christophe Deissenberg, Luxembourg]	Taken into account - text revised
24014	4	63	57	63	57	The ecotone concepts is hardly applied anywhere in IPCC assessment, suggest to use a different term here, or be more specific. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - text revised - other terms used to better convey the idea
3320	4	64	3	64	4	In my experience, the combination of climate change and SLR can result in the regional dissparition of a number of species and ecosystems, even at mid latitudes (e.g. Muñoz Sobrino et al. (2018). Vegetation History and Archaeobotany 27: 551-576). [Castor Muñoz Sobrino, Spain]	Taken into account - text revised and citation added
8742	4	64	7	64	7	Check the year of the reference "Noto and Shurin" to fix the citation [APECS Group Review, Germany]	Taken into account - text revised and reference year added
8744	4	64	10	64	10	Check the year of the reference "O'Meara et al." to fix the citation [APECS Group Review, Germany]	Taken into account - text revised and reference year added
8746	4	64	12	64	12	Maybe change citation style: "(e.g. Crotty et al. (2017))" to "(e.g. Crotty et al., 2017)" [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
3322	4	64	16	64	27	Not always. Vulnerability also depends on the morphology of the basin and its suitability to be able to reproduce comparable ecosystems inland, as well as the RSL rise. [Castor Muñoz Sobrino, Spain]	Taken into account - text updated to include additional variables
8748	4	64	21	64	22	Maybe change citation style: "Koch et al. (2015) and Smoak et al. (2013)" to "Koch et al., 2015 and Smoak et al., 2013" [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
24016	4	64	25	64	27	Use a confidence statement here. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - text updated to include uncertainty statement
24018	4	64	29	64	29	'population growth' instead of 'human growth' [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - text uupdated to make it clear we are referring to human population growth

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
3324	4	64	29	64	48	In my opinion there could be a repeated lack in the document (perhaps due to the scale adopted?) that might be a serious fault. Coastal lagoons are also valuable coastal ecosystems/habitats threatened by the global change. They are amongst the most vulnerable and ephemeral coastal habitats, typically lasting tens to hundreds of years (Gonenc & Wolflin 2005; Aliaume et al. 2007). Because of their vulnerability, saline lagoons are listed as a priority habitat under Annex I of the European Union Habitats Directive (EEC 1992). Recommendations for their conservation include the carrying out of ecocomplex approaches to identify their inherent dynamism, in order to promote their conservation, management and connectivity with other coastal habitats (Beer & Joyce 2013). Please, consided them, if useful. Find the references and an example of a site threatened at mid latitude in Muñoz Sobrino et al. (2016). Boreas 45: 729-753. [Castor Muñoz Sobrino, Spain]	Accepted - text updated and coastal lagoons mentioned.
17388	4	64	29	64	48	"Coastal squeeze" can prevent inland migration of intertidal areas too (at the open coast and in estuaries). It would be worth expanding this wording slightly to be more inclusive of the unvegetated habitat values (e.g. intertidal value as habitat for shorebirds see climate change paper here - https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4182597/). People often associate habitat with just the vegetated parts - good to expand on that understanding. [Helen Kettles, New Zealand]	Accepted- text updated to include the broader land matrix present along coastal areas.
14900	4	64	30	64	30	structures that restrict tides and thus interrupt mass flow processes (water, nutrients, sediments), impact tidal [Christophe Deissenberg, Luxembourg]	Editorial – copyedit to be completed prior to publication
8750	4	64	31	64	32	Maybe change citation style: "(Burdick & Roman 2012)" to "(Burdick and Roman, 2012)" [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
8752	4	64	32	64	34	Check citation style of the rerefences in lines 32 to 34 [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
14902	4	64	34	64	34	Breininger et al 2017). They depend on the type of ecosystem and its conservation status, and on the interactions with [Christophe Deissenberg, Luxembourg]	Editorial – copyedit to be completed prior to publication
24020	4	64	35	64	42	Revise sentence structure [Hans-Otto Poertner and WGII TSU, Germany]	Editorial – copyedit to be completed prior to publication
8754	4	64	36	64	37	Check citation style of the rerefences in lines 36 to 37. There is an extra bracket after "2016" [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
2158	4	64	37			use of brackets to be checked. Et al. should have a full stop after al. [Chandani Appadoo, Mauritius]	Editorial – copyedit to be completed prior to publication
8756	4	64	38	64	38	There is an extra bracket after "activities". [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
2160	4	64	38			DOC to be written in full [Chandani Appadoo, Mauritius]	Taken into account - text revised; written in full and abbreviation noted
3326	4	64	46	64	46	Insert a point (.) before However [Castor Muñoz Sobrino, Spain]	Taken into account - text revised
2162	4	64	46			Full stop to be added after the bracket [Chandani Appadoo, Mauritius]	Editorial – copyedit to be completed prior to publication
12234	4	64	50	65	12	Coastal squeeze may also be excacerbated by illegal commercial-scale extraction of oceanfront beach sands as documented by Antia (2013a) on the Nigerian coast. Reference: Antia (2013a) Coastal erosion of Akwa Ibom State, S. E. Nigeria. Nigerian Geological Survey Agency Bulletin no. 45, 209 p. Federal Republic of Nigeria. ISBN:978-978-52505-1-0 [Effiom Edem Antia (Prof), Nigeria]	Taken into account - the suggested reference was not accessible. However the topic has been taken up based on additional literature search

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21718	4	64	50	65	35	Section 4.3.2.3.2 Coastal Squeeze -- isn't this an element of the previous section -- not a sectio wgan in its own right. What is the point of the section? I note that it has been defined earlier in the Chapter so cross-referencing is needed. [Robert Nicholls, UK]	Taken into account - section structure has been revised, text part is now more integrated
2164	4	64	55			An indication in terms of rate of acceleration of coastal squeeze will be useful [Chandani Appadoo, Mauritius]	Taken into account - literature serach was performed but no sufficient evidence was encountered, which could have been presented
2166	4	65	6			defence instead of defense [Chandani Appadoo, Mauritius]	Accepted - text revised
24022	4	65	28	65	29	From a system's perspective I would rather say the loss of a buffer zone increases vulnerability. If you only refer to settlements or infrastructure that is located behind the buffer zones, then, indeed, exposure increases. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - text revised
14904	4	65	29	65	29	coastal ecosystems [Christophe Deissenberg, Luxembourg]	Accepted - text revised
2114	4	65	37	65	51	Subsidence is also described in 4-27-29 to 37. Please, re-write the paragraphs to be consistent. Additionally, it is relevant to point out the observations given by Esteban (2018) on extreme cases of land subsidence in Tokyo, Yakarta and Danajon Bank (small island in the Philippines). Some are highly populated coastal communities adapting to several meter of land subsidence (ratios up to 215 mm/year in Yakarta) and Danakon Bank is small island with a poor community adapting to a sudden earthquake-induce subsidence of 70 cm. [Josep Medina, Spain]	Subsindence is mainly dealt with now in Chapter 4.2. The suggested reference Estaban (2018) was included.
24946	4	65	37	66	2	Very happy to see this section. Summary is very nice. Expansion would be appropriate. [Elizabeth Weatherhead, USA]	Subsindence is mainly dealt with now in Chapter 4.2.
3218	4	65	37	66	3	This section should be tied to discssion earlier in this chapter. Here, the focus of the chapter is on exposure and vulnerability; subsidence as a physical process belongs earlier. [Robert Kopp, USA]	Accepted - text revised, we refer now to Section 4.2.2.5 when we refer to subsidence in context of exposure
12408	4	65	38	65	38	is "coastal" necessary in "coastal deltas"? [Sylvain Ouillon, France]	yes, as there are also inland deltas
12410	4	65	38	66	2	"the rates of natural land subsidence are accelerated because of human activities, such as extraction of natural resources…" Nothing is told about the weight of buildings in megacities, which has induced subsidence of around 5m in Tokyo, 4m in Jakarta, 3m in Osaka and Shanghai, etc. (Nicholls, 2011). I would suggest to add this information. Ref.: Nicholls, R.J., 2011. Planning for the impacts of sea level rise, Oceanography, 24(2), 144-152 (http://www.jstor.org/stable/24861275) [Sylvain Ouillon, France]	Subsindence is mainly dealt with now in Chapter 4.2. The reference Nicholls, 2011 have been included into section 4.2.2.5.
15626	4	65	41	65	48	It has been mentioned that the rate of subsidence in Bangladesh is ranging from 0 to >18 mm yr-1 (Higgins et al. (2014). However, Nichalos (2016) mentioned that the broad regional subsidence varies from 2 to 3 mm yr-1, and it can be higher in localized (Brown and Nicholls, 2015; Higgins et al., 2014). Reference: "Nicholls, R.J., Hutton, C.W., Lázár, A.N., Allan, A., Adger, W.N., Adams, H., Wolf, J., Rahman, M., Salehin, M.1, 2016. Integrated assessment of social and environmental sustainability dynamics in the Ganges-Brahmaputra-Meghna delta, Bangladesh. Estuarine Coast. Shelf Sci. 183 (Part B), 370–381." [Akm Saiful Islam, Bangladesh]	Subsindence is mainly dealt with now in Chapter 4.2. The subsidence in the GBM delta is included into section 4.2.2.5.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14906	4	65	42	65	42	much higher than the eustatic sea level rise [Christophe Deissenberg, Luxembourg]	Text is removed from the SOD, Subsindence is mainly dealt with now in Chapter 4.2.
21720	4	65	42	65	45	The numbers for subsidence in the GBM delta are different to those presented earlier in the chapter -- and different sources -- earlier source was Brown and Nicholls (2015) [Robert Nicholls, UK]	Text is removed from the SOD, Subsindence is mainly dealt with now in Chapter 4.2.
14908	4	65	54	65	54	proceeds at a faster rate than the [Christophe Deissenberg, Luxembourg]	Text is removed from the SOD, Subsindence is mainly dealt with now in Chapter 4.2.
2168	4	66	2			its instead of is [Chandani Appadoo, Mauritius]	Text is removed from the SOD, Subsindence is mainly dealt with now in Chapter 4.2.
3172	4	66	4	66	41	Much of this is about physical processes, not exposure and vulnerability -- should it be earlier? [Robert Kopp, USA]	The section is rewritten. Physical processes are not in the focus. The section rather focus on the interactions of sea-born and hinterland processes at the coast shaping coastal exposure and vulnerability
21724	4	66	6	66	34	Useful references are Dunn et al (2018) and Nicholls et al (2016) References: Dunn, F. E., Nicholls, R. J., Darby, S. E., Cohen, S., Zarfl, C., & Fekete, B. (2018). Projections of historical and 21st century fluvial sediment delivery to the Ganges-Brahmaputra-Meghna, Mahanadi, and Volta deltas. Science of the Total Environment, 642, 105-116. DOI: 10.1016/j.scitotenv.2018.06.006; Nicholls, R. J., Hutton, C. W., Lazar, A. N., Allan, A., Adger, W. N., Adams, H., ... Salehin, M. (2016). Integrated assessment of social and environmental sustainability dynamics in the Ganges-Brahmaputra-Meghna delta, Bangladesh. Estuarine, Coastal and Shelf Science, 183, 370-381. DOI: 10.1016/j.ecss.2016.08.017 [Robert Nicholls, UK]	Taken into account - suggested references have been taken up.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
12416	4	66	6	66	41	Copy of another comment on page 68: I don't know which section is the most appropriate to this addition, but the reports lacks of development on one key point related to coastal erosion, mainly along deltas: The riverine sediment supply to the coast is a major forcing that has suffered, during the last few decades, drastic declines resulting from damming, Land Use Changes and sand mining (Ouillon, 2018). The risk of erosion at the coast is intended to drastically increase as the continental sediment supplies are reduced. Vörösmarty et al (2003) explained that, in developed basins, the reduction in sediment supply was of the order of 50%, and that the global decline could be estimated at 25%. Since then, some articles quantified the decline in watershed sediment inputs more precisely, on many basins. Just two examples: on the Red River, the Hoa Binh Dam (built in the 1980s) caused a drop of 65% in sediment supply to the sea (Vinh et al., 2014); in the Mekong Delta, the decline between the 1970s and the period 2009-2016 is estimated to be around 75% (Dang et al., 2018). A list could be compiled in the text or in a Table, from published papers and major watersheds. As we know that the impact of damming takes time to develop downstream (e.g. Brandt 2000), it is now very difficult to estimate the decline of sediment supply which will result in 20 or 50 years from only existing dams. However, we have to underline as well that in some basins, dam removal is now increasing (Bellmore et al. 2017). The need to develop basin-scale management and collaborations around the watershed-estuary-coastal waters continuum is mentioned in Ouillon (2018). If this suggestion catches your attention and if you wish, I am ready to participate in writing such additional paragraph with one or a few colleagues working on watersheds. Quoted references : Bellmore J.R., Duda, J.J., Craig, L.S., Greene S.L., Torgersen C.E., Collins M.J., Vittum K., 2017. Status and trends of dam removal research in the United States. WIREs Water, 4:e1164, doi:10.1002/wat2.1164 Brandt, S.A, 2000. Classification of geomorphological effects downstream of dams. Catena, 40, 375–401, doi:10.1016/S0341-8162(00)00093-X Dang T.H., Ouillon S., Giap V.V., 2018. Water and suspended sediment budgets in the lower Mekong from high-frequency measurements (2009-2016), Water, 10, 846; doi:10.3390/w10070846 Ouillon S., 2018. Why and how do we study sediment transport? Focus on coastal zones and ongoing methods. Water, 10, 390; doi:10.3390/w10040390 Vinh V.D., Ouillon S., Tanh T.D., Chu L.V., 2014. Impact of the Hoa Binh dam (Vietnam) on	Taken into account - suggested references have been taken up. We did not do an overall review (and the suggested table) of the literature on catchmant level changes since the overall focus of the chapter remains Sea Level Rise.
21722	4	66	8			"erosion" should be "sediment supply" [Robert Nicholls, UK]	Accepted – text revised
12412	4	66	9	66	13	A newly published paper quantifies the decrease in sediment supply by the Mekong river from 1970s to 2009-2016 by 75%, see Dang et al. (2018). This reference may be added at the end of the sentence on Anthony's conclusions. Ref: Dang T.H., Ouillon S., Giap V.V., 2018. Water and suspended sediment budgets in the lower Mekong from high-frequency measurements (2009-2016), Water, 10, 846; doi:10.3390/w10070846 [Sylvain Ouillon, France]	Accepted – reference added
10796	4	66	19	66	19	Levees not levies [Thomas Spencer, UK]	Accepted – text revised
13650	4	66	19	66	19	Change 'levies' to 'levees' [Debra Roberts and Durban Team, South Africa]	Accepted – text revised

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12414	4	66	24	66	27	"rates of sedimentation during flooding seasons can vary depending on multiple factors…." Don't forget that in meso-tidal and macro-tidal estuaries, most of the sediment supply during floods is depositing in the coastal zones (around the prodelta) and that a large part of these sediments are brought back to the estuary during the low flow season by tidal pumping and settle there, contributing MAINLY IN THE DRY SEASON to subsidence. See an example in Lefebvre et al. (2012) on the Red River who measured a deposition rate in the lower estuary in dry season three times higher than in the wet season (based on measurements in the water column). And the accepted paper by Gugliotta et al. (2018) on the Mekong delta (based on the analysis of sediment characteristics in the river bed and its dynamics). A medium- to long-term consequence of sea level rise will be an enhanced sedimentation further upstream in estuaries, and a silting-up of estuarine navigation channels, with high economic consequences for cities with a large estuarine harbour. This was observed by Haiphong city in North Vietnam, where the authorities decided to build a new harbour further downstream, for a cost estimated at 2 billions US$ (Vinh et al., 2018). Quoted references: Gugliotta M., Saito Y., Nguyen V.L., Ta T.K.O., Tamura T., 2018. Sediment distribution and depositional processes along the fluvial to marine transition zone of the Mekong River delta, Vietnam. Sedimentology, doi: 10.1111/sed.12489; Lefebvre J.P., Ouillon S., Vu Duy Vinh, Arfi R., Panche J.Y., Mari X., Chu Van Thuoc, Torreton J.P., 2012. Seasonal variability of cohesive sediment aggregation in the Bach Dang-Cam Estuary, Haiphong (Vietnam), Geo-Marine Letters, 32 (2), 103-121, doi:10.1007/s00367-011-0273-8; Vinh V.D., Ouillon S., Uu D.V., 2018. Estuarine Turbidity Maxima and variations of aggregate parameters in the Cam-Nam Trieu estuary, North Vietnam, in early wet season, Water, 10, 68; doi:10.3390/w10010068 [Sylvain Ouillon, France]	Accepted – text revised, references have been included
3808	4	66	40	66	40	change to "sight of" [Ola Kalen, Sweden]	Accepted – text revised
10798	4	66	40	66	40	Without losing sight of this fact, not Without losing sight this fact, [Thomas Spencer, UK]	Accepted – text revised
13652	4	66	40	66	40	Insert 'of' before 'this fact' [Debra Roberts and Durban Team, South Africa]	Accepted – text revised
24650	4	66	45		54	For a clearer message it may be even more useful to address whether climate change constrains sustainable development and to what extent. [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - This point is rather considered in the impacts sub-section (4.3.3)
21726	4	66	45			"played a major role" -- based on earlier evidence in the chapter "the major role" [Robert Nicholls, UK]	Accepted
8758	4	66	49	66	49	Maybe change citation style: "(Ford et al. (2015), p. 1046)" to "(Ford et al., 2015, p. 1046)" [APECS Group Review, Germany]	Accepted
21728	4	66	54			suggest add a comment about the hazard mitigation benefits [Robert Nicholls, UK]	Taken into account - Paragraph modified + these aspects are also discussed in section 4.4 on Responses.
2178	4	67	0			Some font size for Figure 4.12 too small [Chandani Appadoo, Mauritius]	Taken into account - Figure 4.12 has been completely reworked.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
2728	4	67	0			Figure 4.12. Suggest boxes of drivers to be of same size while boxes of consequences can be of different sizes. [Poh Poh Wong, Singapore]	Taken into account - Figure 4.12 has been completely reworked.
13656	4	67	0			Figure 4.12 (1) Include the box labels above each box, not in a colour-coded legend. They are too far removed. Arrows are too small. The overall layout can be much improved. Fonts must be bigger, they are too tiny and inconsistent. (2) Yellow box: needs reworking. Also the 'human dimensions'. The way humans fit into this schematic needs careful consideration. Perhaps there can be some distinction between human physical / built systems (settlements, infrastructure etc), social systems (economics, lifestyles, culture, etc) and human production and consumption (fisheries, agriculture, water, livelihoods, etc). (3) Please note spelling. [Debra Roberts and Durban Team, South Africa]	Taken into account - Figure 4.12 has been completely reworked.
2170	4	67	4			remove the word 'last' [Chandani Appadoo, Mauritius]	Accepted
21730	4	67	11			Key point that is not said "direct destruction of habitats is still widespread due to conversion to other land use and indirect non-climate stresses" [Robert Nicholls, UK]	Accpeted - Text improved accordingly
3328	4	67	20	67	20	Also lagoons [Castor Muñoz Sobrino, Spain]	Accepted- suggested text added
2172	4	67	21			more clarity needed on what the authors mean by 'this..' [Chandani Appadoo, Mauritius]	Taken into account- text revised from "this" to "these impacts on ecosystem and natural resources directly and indirectly impact human systems"
2174	4	67	22		23	more clarity on what the authors mean by 'human systems' [Chandani Appadoo, Mauritius]	Taken into account- changed "human systems" to "society"
2176	4	67	22			/' to be replaced by comma [Chandani Appadoo, Mauritius]	Accepted
8760	4	67	26	67	26	Text inside Figure 4.12. Several typos and suggestions: 1. In the gray box for Atmospheric greenhouse gas concentration, it should be gas with an s not a z. 2. In the green box, below benthic communities, it should be Mangroves not Mangrives. 3. In costal erosion box, it could be shorter if written: tidal and extreme flooding. 4. at the ice and glaciers grey box; should be ice and glaciers melting and warming. 5. bottom yellow box, below societal changes: remove capitals in traditional knowledge. [APECS Group Review, Germany]	Taken into account - Figure 4.12 has been completely reworked.
15962	4	67	26	67	26	Spellig mistakes across several areas of Fig 4.12: Grey box: "Atmospheric greenhouse gas"; Green box: "Primary production"; "(Seagrasses, coral reefs, mangroves, salt marshes, oysters, macroalgae, etc); Key top right: "Cryosphere"; yellow box: "Loss of traditional knowledge, low risk perception, socioeconomic inequalities, gender..." Also suggest "Thermal expansion and modifications to oceans stratificiation and circulation" is reworded to something like "Thermal expansion and modification of ocean stratificiation and circulation" [Tim Riding, New Zealand]	Taken into account - Figure 4.12 has been completely reworked.
15964	4	67	26	67	26	Suggest Figure 4.12 is reworded for flow and readability. [Tim Riding, New Zealand]	Taken into account - Figure 4.12 has been completely reworked.
22282	4	67	26	67	26	This figure needs to be adjusted in order to allow text to be larger. It's not readable at 100% (or even larger) in its current state. I suggest trying to reduce the amount of text, and making boxes larger where possible and necessary (no need for half the figure to be taken up by very long, thin arrows). [Andra Garner, USA]	Taken into account - Figure 4.12 has been completely reworked.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13654	4	67	26	67	27	It is quite difficult to read the text in the figure. There is room to increase the size. [Debra Roberts and Durban Team, South Africa]	Taken into account - Figure 4.12 has been completely reworked.
17264	4	67	26	67	28	the legend in the figure is too small [Iulian Florin Vladu, Germany]	Taken into account - Figure 4.12 has been completely reworked.
21732	4	67	26	67	32	Figure 4.12 -- I like this figure and wonder if responses might be added to it. [Robert Nicholls, UK]	Rejected - Section 4.3 focuses on impacts and reponses are discussed in section 4.4. N.B 1: note that this figure had been completely modified. N.B 2: A new figure at the very begining of the chapter now provides the big picture, from physical processes to hazards, impacts and responses.
16428	4	67	27	67	29	I very much agree with this language, which is also consistent with lkanguage later on on zooplankton impacts of OA, but the OA summary presented in p. 3. l. 40-46 is much more simplistic in its negativity. Should the main message not be consistent with the diversity shown in the text? [Coswig Kalikoski Daniela, Italy]	Rejected- Problems with the identification of the corresponding text in the FOD. A mistake with page/line numbering?
3174	4	67	29	67	32	This figure is in desparate need of proofreading and careful thinking. A few examples: "gas", not "gaz"; storms are also affected by ocean temperature. [Robert Kopp, USA]	Taken into account - Figure 4.12 has been completely reworked.
2180	4	68	3			Sentence structure to be review, use of 'for example' not clear [Chandani Appadoo, Mauritius]	Taken into account - Figure 4.12 and its caption have been completely reworked.
21738	4	68	7			Section 4.3.3.1 lacks a global perspective. This comes later when we have human impacts -- should these sections be merged? [Robert Nicholls, UK]	Taken into account - It would be very difficult to start with impacts on human activities given the outline of the section (now better captured in new Fig. 4.12). However, former subsection 4.3.3.1.4 has been adapted and placed at the begining of section 4.3.3.1 to provide a broader perspective.
24652	4	68	7			title reads as if it wants to repeat or complement the first half of chapter? A stronger message would result from inclusion of vulnerable systems [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account - We hope the new structure of tsection 4.3.3. as a whole provides more clarity. However, important is to note that while section 4.2 deals with the consequences of sea-level change in terms of hazards (flooding events, e.g.), section 4.3.3 addresses the consequences for territories, resources and people. New figure 4.1 makes this rational clear.
21734	4	68	11			"sediment transoort" to sediment supply or sediment budget [Robert Nicholls, UK]	Taken into account - Sentence removed.
21736	4	68	14	68	15	assuming no adaptation is a strange assumption as while it is standard we are pretty confidence that adaptation is occurring and hence we know the results here refer to no real world. [Robert Nicholls, UK]	Accepted - Sentence modified, reads now: "The sections below describe some of the above-mentioned impacts, both observed and projected, but assuming business-as-usual adaptation efforts (i.e. no major additional efforts compared to what is –or not– currently done). This reflects a relative gap in the scientific literature on the consideration of the benefits of enhanced adaptation to reduce future risks…".
3176	4	68	18	68	20	A storm is not a "parameter" [Robert Kopp, USA]	Accepted - Sentence revised
10800	4	68	20	68	20	landfall location (size, topography, etc.). It would be better to talk about landscape or geomorphic setting here - open coast, embayment, estuary, back-barrier… [Thomas Spencer, UK]	Taken into account - sentence modified + whole sub-section restructured (merge with previously 4.3.3.3.1).
14910	4	68	21	68	21	In addition to being temporary [Christophe Deissenberg, Luxembourg]	Taken into account - sentence modified + whole sub-section restructured (merge with previously 4.3.3.3.1).
21740	4	68	22			Chronic floods are not properly defined -- they are when "flooding due to high tides occur regularly under calm weather" [Robert Nicholls, UK]	Taken into account - sentence modified + whole sub-section restructured (merge with previously 4.3.3.3.1).

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10802	4	68	25	68	26	this appears written from the assumption that this flooding is recent and is a result of sea level rise. But there might be many reasons for marine flooding. Are we sure that flooding of this level has not occurred previously? [Thomas Spencer, UK]	Taken into account - sentence modified + whole sub-section restructured (merge with previously 4.3.3.3.1).
10804	4	68	25	68	38	these exercises all suggest that the coast can be treated as a simple bathtub. But coastal flooding is in reality much more complex, with flooding of variously vegetated and urbanised surfaces and where coastal morphology can change with sediment erosion, transport and deposition. [Thomas Spencer, UK]	Taken into account - The whole sub-section has been restructured and we hope now reflects a more comprehensive understanding.
3330	4	68	26	68	28	Paleodata also exist in a number of coastal areas worldwide. Should be this point also mentioned here? [Castor Muñoz Sobrino, Spain]	Rejected - Out of the scope of this chapter
8762	4	68	29	68	29	Maybe change citation style: "(e.g. Pearson et al. (2017))" to "(e.g. Pearson et al., 2017)" [APECS Group Review, Germany]	Accepted
8764	4	68	30	68	30	Check citation style of Vitousek et al. [APECS Group Review, Germany]	Accepted
13658	4	68	30	68	38	This is a important statement (line 30-33) but then text instantly reverts back to the insecurity of science (line 36-38), undoing the power of the statement. Is this a real risk, yes or no? If yes, what are the implications? Huge! This needs to come across. [Debra Roberts and Durban Team, South Africa]	Taken into account - The whole sub-section has been restructured and we hope now reflects a more comprehensive understanding. In addition, implications and responses are discussed in sections 4.3.4.2 and 4.4, respectively.
10806	4	68	36	68	37	contrasting changes in extreme wave energy fluxes' Why? Not explained. [Thomas Spencer, UK]	Accepted - Sentence modified
238	4	68	40	68	53	Investigation of SLR effects on thalweg line and river boundaries: Sea level rise, changes ocean circulation, coastal currents and sedimentation regimes. This phenomena can change river mouth and river boundaries and result in political and legal problem. [Abbas Einali, Iran]	Rejected- referenced section is on more general shoreline erosion and not on the effects of thalweg line migration and resulting change for river boundaries.
21742	4	68	40	68	56	New reference on shoreline trends [Robert Nicholls, UK]	Taken into account- Liijendijk et al. (2018) reference added
23120	4	68	40			Not sure if it's the correct place, but a big issue is the extraction of sand and sediments by humans, from beaches and from the sea floor. In some countries there is a huge nedd for sand (sout west Asia) and it's disappearing from deltas without any régulation and controls [Jacques Beall, France]	Taken into account- extraction of sand taken into account by stating "a continued increase in anthropogenic pressure". Also considered in section 4.3.2.3.
10718	4	68	41	68	56	Please add also costal erosion of permafrost thawing and iced shores - up to 100-200 m per year in Russian Arctic: http://www.zikj.ru/images/25/7.pdf [Oxana Lipka, Russian Federation]	Taken into account- Barnhart et al. (2014) added to reference on erosion in arctic to provide more recent reference decrease in seasonal sea ice and impacts on icy/permafrost coastlines.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
12404	4	68	41	68	56	About coastal erosion, this subsection only mentioned "an expected increase in anthropogenic pressure" (Line 56). In my opinion, this point is not sufficiently developed, especially along deltas. The riverine sediment supply to the coast is a major forcing that has suffered, during the last few decades, drastic declines resulting from damming, Land Use Changes and sand mining (Ouillon, 2018). The risk of erosion at the coast is intended to drastically increase as the continental sediment supplies are reduced. Vörösmarty et al (2003) explained that, in developed basins, the reduction in sediment supply was of the order of 50%, and that the global decline could be estimated at 25%. Since then, some articles quantified the decline in watershed sediment inputs more precisely, on many basins. Just two examples: on the Red River, the Hoa Binh Dam (built in the 1980s) caused a drop of 65% in sediment supply to the sea (Vinh et al., 2014); in the Mekong Delta, the decline between the 1970s and the period 2009-2016 is estimated to be around 75% (Dang et al., 2018). A list could be compiled in the text or in a Table, from published papers and major watersheds. As we know that the impact of damming takes time to develop downstream in the basin, until the fluvial-to-marine transition zone (e.g. Brandt 2000), it is now very difficult to estimate the decline of sediment supply which will result in 20 or 50 years from only existing dams. However, we have to underline as well that in some basins, dam removal is now increasing (Bellmore et al. 2017). A last sentence may also mention that such a connection between watersheds and coastline retreat encourages scientists and managers to develop collaborations on the watershed-estuary-coastal waters continuum (or on the catchment connectivity) (Ouillon, 2018). If this suggestion catches your attention and if you wish, I am ready to participate in writing such additional paragraph in collaboration with one or a few colleagues working on watersheds. Quoted references : Bellmore J.R., Duda, J.J., Craig, L.S., Greene S.L., Torgersen C.E., Collins M.J., Vittum K., 2017. Status and trends of dam removal research in the United States. WIREs Water, 4:e1164, doi:10.1002/wat2.1164 Brandt, S.A, 2000. Classification of geomorphological effects downstream of dams. Catena, 40, 375–401, doi:10.1016/S0341-8162(00)00093-X Dang T.H., Ouillon S., Giap V.V., 2018. Water and suspended sediment budgets in the lower Mekong from high-frequency measurements (2009-2016), Water, 10, 846;	Taken into accout- explicitely mention delta in discussion of erosion and reference section in report on deltaic subsidence. More on anhropogenic drivers is also provided in section 4.3.2.
19172	4	68	41	68	56	Consider including a brief review of erosion on permafrost coasts, which is very significant in the Arctic and there are several recent publications about this issue. [Goncalo Vieira, Portugal]	Taken into account- added more recent reference on erosion in arctic (Barnhart et al., 2014) that discusses sea ice loss and impact on erosion of icy and permafrost coastlines.
8766	4	68	45	68	45	Check the phrase "Since the AR5....". It seems there is an extra "for" after "appreciation" [APECS Group Review, Germany]	Accepted
10808	4	68	47	68	49	important feedbacks between biological and physical processes'. This is quite vague. What are these feedbacks and how do they give these results? [Thomas Spencer, UK]	Taken into account - added reference to Wright et al., (2019) book chapter which provides a very recent review of these feedbacks.
8768	4	68	49	68	49	Check reference to "Cross Chapter Box 2" [APECS Group Review, Germany]	Taken into account - Rather refrence to CCB 5
8770	4	68	49	68	49	Insert a space before the phrase "In a global ..." [APECS Group Review, Germany]	Accepted
12402	4	68	49	68	49	A blank is missing before "In a global..." [Sylvain Ouillon, France]	Accepted
15966	4	68	49	68	49	Add space between sentences "..0.3 cm yr-1) over the past few millennia (Woodruff et al., 2013; Cross Chapter Box 2).In a global review on.." [Tim Riding, New Zealand]	Accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13660	4	68	49	68	53	Islands getting bigger with SLR?? Please explain? Does this belong under 'Coastal erosion"? [Debra Roberts and Durban Team, South Africa]	Taken into account - The cited litterature (review article) provides evidence that with enough sediment supply, shorelines can maintain despite current SLR rates. However, the sentence now also mentions the role of anthropogenic drivers in urban islands (coastal defences helps stabilizing the islands, reclamation works make them bigger).
8772	4	68	50	68	50	Check the submitted reference (Duvat) [APECS Group Review, Germany]	Taken into account - Status updated
10810	4	68	53	68	53	is expected'. No, certain authors hold these views. The text should reflect that there are differing views on this point. Perhaps 'although it has been argued' [Thomas Spencer, UK]	Taken into account - "expected" has been removed and "it has been argued" added based on the reviewers suggestion.
24024	4	68	53	68	56	Suggest to add confidence statement here. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - We added "robust evidence, high agreement" as Duvat's conclusions rely on a literature review and consider >700 islands in 30 atolls.
21744	4	68	56			anthrogenic pressure is often already high so a problem without any further increase. [Robert Nicholls, UK]	Accepted - "expected" is replaced by "continued" to acknowledge that the process has been ongoing for decades already.
2730	4	69	1	69	8	To conform with the rest of subtopics, references are required for this subsection and not a statement on evidence and agreement. Examples: (a) 73% of SIDS are at risk from groundwater pollution often worsened by seawater intrusion and salinization (UNESCO-IHP & UNEP, 2016. Transboundary aquifers and groundwater systems of small island developing states: status and trends, summary for policy makers.) (b) Salinization especially in coastal areas of Asia and much of SE Asia (World Water Development Report 2015. Water for a sustainable world.) (c) Bangladesh situation (S. Dasgupta et al. 2014. River salinity and climate change: evidence from Coastal Bangladesh). [Poh Poh Wong, Singapore]	Taken into account - Text shortened and confidence staement removed. Refrences are detailed in the following sub-sections.
3810	4	69	1	69	8	This section could be improved with added references, but have no suggestion as to which [Ola Kalen, Sweden]	Taken into account - Text shortened, as it is only an introductory sentence. Refrences are detailed in the following sub-sections.
236	4	69	1	69	53	Salinization of river and It's effects on agriculture due to climate change: In salt wedge estuaries fresh water on top layer is used for agricultural activities. Global warming resulted in an increase in sea water salinity and decrease in river discharge. Therefore, fresh water layer is Thinning in top layer of salt wedge, So fresh water and it's consumption is restricted. [Abbas Einali, Iran]	Rejected - Lack of evidence and published work that this is directly related to sea-level rise. This may also be the result of reduced river runoff, in relation to water consumption.
21746	4	69	1	70	16	Salinisation is good, but much longer than erosion and especially flooding -- is this balance warranted? [Robert Nicholls, UK]	Taken into account - The new structure provides more balance
12236	4	69	10	69	26	Tidal range variability, used as a surrogate for different SLR scenarios, has been documented as a major factor determining both the degree of salinization and heights of static levels of coastal groundwater along microtidal and mesotidal segments of the Nigerian coast (Antia, 2013b). However, SLR will only amplify and accentuate tidal range impacts, including farther upland excursion. Reference: Antia (2013b) Tidal and potential sea level rise effects on coastal groundwater and surface water bodies of Lagos and Cross River States. Nigerian Geological Survey Agency Bulletin no. 47, 207 p. Federal Republic of Nigeria. ISSBN:978-978-52505-5-8. [Effiom Edem Antia (Prof), Nigeria]	Good suggestion, but reference not found.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15628	4	69	34	69	36	It has been mentioned that higher salinity levels have also been reported in the Gorai river basin, Southwestern Bangladesh (Bhuiyan and Dutta, 2012). However, the cause of increasing salinity in the Gorai River is not primarily due to the sea level rise as mentioned in the earlier sentence. Mirza (1998) mentioned that India commissioned a barrage on the Ganges River at Farakka in April 1975 to divert water and make the Bhagirathi-Hooghly navigable. This diversion has reduced the fresh water supply and increased the salinity of the Gorai river from the Ganges during the dry season (Mirza, 1998). Reference: "Mirza, M. M. Q. (1998). Diversion of the Ganges water at Farakka and its effects on salinity in Bangladesh. Environmental management, 22(5), 711-722." [Akm Saiful Islam, Bangladesh]	Taken into account - text revised by mentioning other anthropogenic factors influencing salinity intrusion
2182	4	69	36			use of word 'ca.' not clear [Chandani Appadoo, Mauritius]	Taken into account - text revised
2184	4	69	37			molluscs instead of mollusks [Chandani Appadoo, Mauritius]	Taken into account - text revised
12418	4	69	40	69	40	"low-flow events such as in the dry season" may be replaced by "low-flow periods" [Sylvain Ouillon, France]	Taken into account - text revised
12238	4	69	55	70	16	The morphodynamic characteristics of the coast exert significant influence on the pore water salinity of coastal soils as exemplified along the highly dissipative transgressive mud segment of the Nigerian coast where the groundwater conductivity, averaged over several tidal cycles, is of the order of 100 times higher than that of the adjacent surface water body that is linked to the ocean (Antia, 2014). Reference: Sea level vulnerability survey of the coastal zone of Ondo State, Southwestern Nigeria: Case study of coastal waters of Igbokoda. Nigerian Geological Survey Agency Occasional Paper no. 26, 108 p. Federal Republic of Nigeria. ISSBN: 978-978-53211-4-2. [Effiom Edem Antia (Prof), Nigeria]	Good suggestion but no access to the report. A PDF version had been asked to the reviewer for a pdf version (no answer at the date of the SOD submission).
2186	4	70	3			to specify which 'biomass' are being referred to [Chandani Appadoo, Mauritius]	Taken into account - text revised (rice and cotton)
3332	4	70	5	70	5	Ebro Delta may be in Spain (or in Catalonia according to others) but never in Italy. A confusion is possible with the Po Delta, Italy? [Castor Muñoz Sobrino, Spain]	Taken into account - text revised
12638	4	70	5	70	5	The sentence "In a study in the Ebro Delta, Italy, soil salinity was ..." should be "In a study in the Ebro Delta, Spain, soil salinity was ..." [Alejandro Cearreta, Spain]	Taken into account - text revised
21748	4	70	5			Ebro delta is in Spain? [Robert Nicholls, UK]	Taken into account - text revised
3334	4	70	7	70	9	Change in environmental conditions can also result in the genesis of biogas. E.g.: Martínez-Carreño, N. & García-Gil, S. 2013. Marine Geology 344, 82–100. [Castor Muñoz Sobrino, Spain]	Rejected - the study relates holocene relative sea-level variations with the type (fine- vs. coarse-grained) and distribution of facies (lateral and vertical) that generate and/or allow gas accumulation or escape. It doesnt address recent changes and impacts and would not fit to the scope of this section.
8774	4	70	8	70	8	What is "Corg"? Maybe a footnote or legend in parenthesis could be insert in the text [APECS Group Review, Germany]	Taken into account - text revised (Organic carbon)
234	4	70	18	70	33	Changes of wave pattern and wave energy flux in shoreline due to SLR: sea level rise, increases wave height and energy. Higher wave can damage to coastal instruction. [Abbas Einali, Iran]	Taken into account - The process is in particular suggested in Albert et al. (2016). Other papers have presented modelling results (Grady et al., 2013, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50577) that are now included.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21750	4	70	18	70	33	There is an up-to-date review in Nicholls et al (2016b) on these issues. [Robert Nicholls, UK]	Taken into account- Alternative reference not found. However, the statement is better supported by Nicholls and Cazenave (2010), now included -- Nicholls, R.J. and Cazenave, A., 2010. Sea-level rise and its impact on coastal zones. science, 328(5985), pp.1517-1520.
13662	4	70	19	70	19	Check the reference [Debra Roberts and Durban Team, South Africa]	Taken into account
8776	4	70	20	70	20	Check reference style of Wong (2014) [APECS Group Review, Germany]	Taken into account
8778	4	70	24	70	24	Check reference style: "(e.g. Romine et al. (2013); Le Cozannet et al. (2014))" to "(e.g. Romine et al., 2013; Le Cozannet et al., 2014)" [APECS Group Review, Germany]	Taken into account
24026	4	70	29	70	33	Suggest to add confidence statement here [Hans-Otto Poertner and WGII TSU, Germany]	Taken into account
14912	4	70	31	70	31	that as SLR keeps rising the frequency, severity, and duration of hazards and related impacts will [Christophe Deissenberg, Luxembourg]	Taken into account - SLR (sea level rise) will continue to rise is redundant and was edited to "as sea level contiues to rise"
13664	4	70	35	70	35	Shouln't it be qualified as marine ecosystem? Check usage across the chapter. [Debra Roberts and Durban Team, South Africa]	Rejected- Tidal wetlands can technically be in fresh water and as such are not marine. The more general term therefore is determined to be more appropriate.
3336	4	70	39	70	40	Again, coastal lagoons could be iconsidered here. [Castor Muñoz Sobrino, Spain]	Accepted- suggested text added
21754	4	70	41			Seperating sand dune vegetation seems rather artifical -- the Chapter can define coastal ecosystems as it likes and I would advocate an examination of coastal ecosystems, including dunes. [Robert Nicholls, UK]	Taken into account - Changed sand dunes to forests abutting tidal wetlands which is likely a better example of a terrestrial ecosystem impacted by SLR.
2188	4	70	43	71	21	Discussion on mangroves to be separated from salt marshes in the paragraph [Chandani Appadoo, Mauritius]	Taken into account - We have changed chapter title to "Tidal Wetlands" so as to provide better clarification why these two ecosystems are grouped together.
2190	4	70	43	71	21	More elaborate discussion on mangroves, and indication of studies done on particular forests/mangrove species to be included [Chandani Appadoo, Mauritius]	Taken into account - We have added a sentence to acknowledge changes in growth rate and productivity associated with different mangrove species.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
10812	4	70	44	71	21	global environmental change is also likely to lead to changes in growth rates and productivity of different mangrove species, including the replacement of environmentally sensitive species by those possessing greater climatic tolerance. A reconstruction of the historic response of mangroves to increases in CO2 concentrations over the past two centuries suggests significant differences among species in their response to atmospheric change (Reef and Lovelock, 2014). Some interactions are likely to be highly complex: thus increased air and water temperatures may encourage the polar expansion of mangroves while at the same time changing tropical storm magnitudes and frequencies, with implications for changing forest canopy structure and the establishment of mangrove propagules. Furthermore, it is becoming increasingly clear that the interaction among global change and other biological and environmental factors (such as habitat loss) and the interaction with the processes that influence the adaptive capacity of a species (e.g. changes to a species' fundamental niche) are difficult to predict or model. Unlike the way in which it is possible to infer near-future mangrove distributions with sea-level rise from established relationships between water level limits and mangrove extent (e.g., Queensland, Australia: Wolanski et al., 1992; Bermuda: Ellison, 1993), it is a much stiffer challenge to model the range of non-sea-level controls discussed above in the context of large-scale climate change models (Spencer et al., 2009). [Thomas Spencer, UK]	Taken into account- The initial sentence of reviewer's comment has been added to the text with provided reference as well as the more general review by Krauss et al., (2014)
14914	4	70	45	70	45	under SLR is the ability [Christophe Deissenberg, Luxembourg]	Accepted
14916	4	70	46	70	46	wetlands to grow and increase [Christophe Deissenberg, Luxembourg]	Accepted
14918	4	71	5	71	5	are just as important, if not more, than [Christophe Deissenberg, Luxembourg]	Accepted
8780	4	71	5	71	6	Check reference style: "(e.g. Mariotti and Carr (2014))" to "(e.g. Mariotti and Carr, 2014)" [APECS Group Review, Germany]	Accepted
14920	4	71	12	71	12	landward migration, making it difficult [Christophe Deissenberg, Luxembourg]	Accepted
21756	4	71	19			Coastal squeeze -- no cross-referencing to other sections [Robert Nicholls, UK]	Taken into account - cross-reference added
12096	4	71	23	71	50	I don't see a mention of coral reefs' wave-breaking and wave-energy-damping function here; the paragraph mostly focuses on whether reef accretion will keep pace with SLR. But alterations of coral reef 3D structure from changes in growth, breakage, disease, or acidification can profoundly affect their ability to provide this function. That idea doesn't appear explicitly here or in the subsequent paragraph that focuses on coastal protection. [Sarah Cooley, USA]	Accepted - This important point had been included.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
10596	4	71	23			According to a recent study : "Although many reefs retain accretion rates close to recent SLR trends, few will have the capacity to track SLR projections under RCP4.5 scenarios without sustained ecological recovery, and under RCP8.5 scenarios most reefs are predicted to experience mean water depth increases of more than 0.5 m by 2100. " (Perry et al 2018). Reference : Perry et al. (2018). Loss of coral reef growth capacity to track future increases in sea level. Nature [Adrien Comte, France]	Taken into account - Reference added
14922	4	71	24	71	25	Coral reefs have recently become iconic symbols of the threat to ecosystems and communities of climate-related ocean change, especially ocean warming and acidification, even under a RCP2.6 scenario. [Christophe Deissenberg, Luxembourg]	Accepted
10814	4	71	24	71	50	This is a good summary but I think it would be helpful to point out the different settings for coral reefs and thus the potentially different stressors and their impacts i.e. Shelf reefs v. oceanic reefs, Atlatic v. Indo-Pacific province reef morphologies, sea level histories and species diversity (3 acroporids in the Caribbean, over 300 in the 'coral triangle' [Thomas Spencer, UK]	Taken into account - Sentence added
8782	4	71	29	71	29	Check reference style. Maybe use "(Gattuso et al., 2014, p. 97)" [APECS Group Review, Germany]	Accepted
8784	4	71	37	71	37	Check reference style. Maybe use "(Wong, 2014, p. 379)" [APECS Group Review, Germany]	Accepted
12094	4	71	39	71	40	A key idea of the Yates et al. 2017 BGS paper is (quoting their abstract) "Erosion of both coral-dominated substrate and non-coral substrate suggests that the current rate of carbonate production is no longer sufficient to support net accretion of coral reefs or adjacent habitats. We show that regional-scale loss of seafloor elevation and volume has accelerated the rate of relative sea level rise in these regions. Current water depths have increased to levels not predicted until near the year 2100, placing these ecosystems and nearby communities at elevated and accelerating risk to coastal hazards." In other words, the combination of erosion, human amendment, and acidification is wearing away at the seafloor in carbonate-sediment-dominated regions, heightening risk from SLR by altering seafloor topography. This could be a key, unaccounted for, unexpected surprise in some regions trying to prepare for SLR risk. I find this idea does not come through in the current mention of the Yates paper. [Sarah Cooley, USA]	Accepted - This important point had been included.
8786	4	71	41	71	41	Check reference style: maybe use "(e.g. Eyre et al., 2018; Albright et al., 2018)" [APECS Group Review, Germany]	Accepted
13666	4	71	41	71	41	Inster 's' into 'society'' [Debra Roberts and Durban Team, South Africa]	Rejected - Word not found (mistake in the page/line numbering on the comment?)
21288	4	71	52	72	8	The problem with seagrasses is that they compete for habitat with coral reefs and they can easily invade areas and take over the corals [Alejandro Souza, Mexico]	Rejected - Out of the scope of this chapter
3812	4	71	55	71	55	Remove '. Change to "through" [Ola Kalen, Sweden]	Taken into account
14924	4	71	55	71	56	(i.e., erosion) and changes in light levels, and sometimes through effects on adjacent ecosystems; Saunders [Christophe Deissenberg, Luxembourg]	Accepted - Sentence modified
8788	4	71	56	71	56	Check reference style. Maybe use "Saunders et al., 2013" [APECS Group Review, Germany]	Accepted
8790	4	72	4	72	6	Check the references (Chapter 5, Submitted etc) [APECS Group Review, Germany]	Accepted

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21758	4	72	6	72	8	I note a positive benefit of climate change here for sea grasses -- I asked a general question earlier in this regard so good to see the limited positive benefits being highlighted [Robert Nicholls, UK]	Taken into account- Horstman et al. (2014) reference now provided (next sub-section) and references therein.
2192	4	72	10	72	26	More information and references on the role of mangroves in coastal protection needed [Chandani Appadoo, Mauritius]	Taken into account- Additional sentence added with citation of review article that discusses wave attenuation by mangroves.
10816	4	72	10	72	26	What is the point in these global level, single percentage statistics? What is interesting is the range of attentuation recorded and reasons for this range - because that gives insight into the important controlling processes. Thus for example, wave energy dissipation is related to wave height / water depth relationships. This can be given an environmental change context. [Thomas Spencer, UK]	Taken into account- added text to acknowledge high degree of variablity around averages due to coastal setting and individual storm characteristics.
14926	4	72	13	72	13	impacts may [Christophe Deissenberg, Luxembourg]	Accepted
21760	4	72	14			"reduce wave heights" -- where? [Robert Nicholls, UK]	Taken into account - identifiy that this is for 69 locations provided in Narayan et al. (2016) reference
21762	4	72	16			The numbers give an impression of high quantification which is misleading: "70%, 72%, 31% and 36%" -- these numbers are based on a limited sample and are very context specific. As we develop more understanding so our view will change --- the key point according to this data is the rank order. Also reading this I am surprised that mangroves are substantially below saltmarsh. That does not really make sense and needs more thought. [Robert Nicholls, UK]	Taken into account - Limited number of sites is no acknowledged with range provided. Additional citations and references therein on mangroves provided (Hostman et al, 2014) that observes greater attenuation than Narayan et al. (2016).
10818	4	72	19	72	21	Not entirely clear what point is being made here. Moller et al. (2014; Nature Geoscience 7: 727-731) show that attentuation does still take place under surge condtions. [Thomas Spencer, UK]	Taken into account - Added sentence that discusses range for storm surge attenuation to show that in some cases surge is amplified in marsh systems.
8792	4	72	21	72	21	Missing a end bracket. See "surge (i.e. wave...." [APECS Group Review, Germany]	Taken into acount - Sentence removed.
8794	4	72	21	72	21	Check reference "Shepard CC, 2011" (why this CC?) [APECS Group Review, Germany]	Taken into acount - Sentence removed.
8796	4	72	22	72	22	Check reference "In review" [APECS Group Review, Germany]	Checking with editor on status of paper
18302	4	72	28	72	36	What is missing from this section 4.3.3.3 is the trends and observed changes in coastal impacts. Almost all subsections deal with projections only. I would expect a discussion also on literature that documents past trends, and current coastal risks. Importantly, there is evidence that coastal risks are to some part decreasing. See for instance past reduction in storm surge morbidity and mortality from Bouwer and Jonkman 2018 (doi:10.1088/1748-9326/aa98a3), and also other studies including Lumbroso et al. 2017 (doi:10.5194/nhess-17-1357-2017). These are important pointers to adaptation potentials/vulnerability reduction. [Laurens Bouwer, Netherlands]	Rejected - outsdie the scope of the chapter. We only report on trends that have been attributed to climate chnage or SLR.
21766	4	72	28	74	52	Merge Section 4.3.3.3 with the earlier section on Physical Effects. [Robert Nicholls, UK]	Accepted - text revised
21768	4	72	33	72	34	Does this section consider adaptation or not? [Robert Nicholls, UK]	Accepted - yes, it does. We now state this clearly up front
18402	4	72	38	74	52	Somewhere in this section, please consider inclusion of risk to cultural heritage. Archaeological sites, historical structures, burial grounds, etc are (often) non-adaptive and nonrenewable. The destruction or loss of such sites is strongly linked to losses of cultural identity, as well as coastal tourism and values, and should be at least mentioned. [Jeneva Wright, USA]	Accepted comment; reference: Marzeion, B. and Levermann, A., 2014. Loss of cultural world heritage and currently inhabited places to sea-level rise. Environmental Research Letters, 9(3), p.034001.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
19074	4	72	39	73	6	Rasmussen, D. J., Bittermann, K., Buchanan, M. K., Kulp, S., Strauss, B. H., Kopp, R. E., & Oppenheimer, M. (2018). Extreme sea level implications of 1.5° C, 2.0° C, and 2.5° C temperature stabilization targets in the 21st and 22nd centuries. Environmental Research Letters, 13(3), 034040. can be referenced here. It contains global projections and highlights differences in regions. [Carl-Friedrich Schleussner, Germany]	Accepted - text revised
19076	4	72	39	73	6	Small islands will be particularly affected by marine flooding and should be highlighted here. See for e.g.: Cashman, A., & Nagdee, M. R. (2017). Impacts of Climate Change on Settlements and Infrastructure in the Coastal and Marine Environments of Caribbean Small Island Developing States (SIDS). Science Review, 155-73. [Carl-Friedrich Schleussner, Germany]	Taken into account - we now state this up front. Unfortuntly there is hardly any literature that projects future SLR impacts on small islands specifically (the paper mentioned does not do this either)
3458	4	72	43	72	43	What is SSPS [Mahmood Riyaz, Maldives]	Taken into account - acronym described
8798	4	72	43	72	43	Maybe a legend of "SSP" could be also inserted in the text [APECS Group Review, Germany]	Accepted - text revised
8800	4	72	53	72	53	Check cross reference to "4.2.3.". Maybe "4.2.3.2"? [APECS Group Review, Germany]	Accepted - text revised
23292	4	72	55	73	23	Again the passages infer that SLR is the main cause of coastal flooding, with citation of Hinkel et al (2014) that 0.2-4.6% world population would be flooded annually in 2100. I would be more cautious in making this statement. Coastal flood has complex hydrological drivers, and SLR as projected is only one factor. Increased precipitation intensity in coastal areas, compounded by land use changes, perhaps is a much more important factor. Attributing SLR to the annual flooding of 0.2-4.6% world population is a significant scientific overreach that needs careful examination. [Y. Jeffrey Yang, USA]	Accepted - We now refer to the complexity of coastal flooding in the introductory paragraph of this section.
16430	4	73	0	73		Fig. 5.15 is too complex and has too much information to be read, let alone understand. [Coswig Kalikoski Daniela, Italy]	Accepted - Figure revised
8802	4	73	2	73	2	Maybe an extra comma after "Abadie et al. (2016)" [APECS Group Review, Germany]	Accepted - text revised
21770	4	73	3			Turkey may be a country in Europe, but Izmir is a city in Asia. [Robert Nicholls, UK]	Accepted - text revised
13668	4	73	4	73	6	Four cities in Europe are mentioned but only two cities from the global study. Please include more information from this study for the rest of the world. [Debra Roberts and Durban Team, South Africa]	Accepted - text revised
21772	4	73	4			For Rotterdam this might be the numbers if there is no adaptation, but the Delta Commision is investing 1 billion Euros per year in adapting the Netherlands and defence upgrades are happening around Rotterdam -- so how credible is this number [Robert Nicholls, UK]	Accepted - text revised
18304	4	73	8	73	8	Besides structiural protection, also forecasting and early warning of extreme sea-levels and cyclones have improved, thereby reducing risks of loss of life. [Laurens Bouwer, Netherlands]	Accepted - text revised
24028	4	73	8	73	8	Does this refer to all kinds of coastal protection, in general? [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - refers to hard and sediment-based protection - text revised
19078	4	73	8	73	23	There are also limits to coastl protection from SLR. See for e.g. Kwadijk, J. C., Haasnoot, M., Mulder, J. P., Hoogvliet, M., Jeuken, A., van der Krogt, R. A., ... & de Wit, M. J. (2010). Using adaptation tipping points to prepare for climate change and sea level rise: a case study in the Netherlands. Wiley Interdisciplinary Reviews: Climate Change, 1(5), 729-740. [Carl-Friedrich Schleussner, Germany]	Taken into account. - we now forward reference to Section 4.4.3.1.4 which adresses limits to coastal protection
8804	4	73	11	73	11	Check cross reference to "4.4.4.2" [APECS Group Review, Germany]	Accepted - text revised

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8806	4	73	13	73	13	Check cross reference to "4.4.2" [APECS Group Review, Germany]	Accepted - text revised
13670	4	73	13			Coastal protection is widespread What does 'widespread' mean in absolute, global terms? What proportion of global coastal cities are protected? Same word used in p79-line7. It sounds like they are used in specific places, especially in the global North, but not all around the globe. Perhaps 'well established' is more accurate. [Debra Roberts and Durban Team, South Africa]	Accepted - we now forward reference to the section that describes the current geographic extend of coastal protection
2116	4	73	14	73	21	Relative SLR of several meters caused by land subsidence is related to 4-27-29 to 37 and section 4.3.2.3.3 Subsidence. Please, be consistent in the description. Esteban (2018) describe three case studies of human adaptation to fast subsidence which may be a good indicator of human adaptation to SLR. [Josep Medina, Spain]	Accepted - text revised
21774	4	73	17			Kaneko and Toyota (2011) is a good reference on subsidence and its mitigation in Asian cities -- but it does not really talk about how the cities have adapted. [Robert Nicholls, UK]	Accepted - text revised
8808	4	73	20	73	20	Check cross reference to "4.4.6" [APECS Group Review, Germany]	Accepted - text revised
8810	4	73	21	73	21	Check cross reference to "4.4.4" [APECS Group Review, Germany]	Accepted - text revised
24030	4	73	21	73	21	Does this refer to small island states in general, or small island developing states (SIDS), low-lying islands, or small islands, in general? Please be specific and consistent regarding terminology. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - text revised
8812	4	73	23	73	23	Change citation style: use "(Hinkel, 2018)" [APECS Group Review, Germany]	Accepted - text revised
14928	4	73	23	73	23	coast (Hinkel, 2018). [Christophe Deissenberg, Luxembourg]	Accepted - text revised
21776	4	73	27			Change "digital elevation methods" to "elevation data" [Robert Nicholls, UK]	Accepted - text revised
14930	4	73	29	73	30	that between 20% and 70% living in the 100-year coastal floodplain could be affected depending on the [Christophe Deissenberg, Luxembourg]	Rejected - this is not the intended meaning
3460	4	73	32	73	32	"global elevation data can result in differences of about 50%" if this is the case in big coastal areas what will be status in small island states where the scale of elevation change is relatively small [Mahmood Riyaz, Maldives]	Noted: Local assessments on, e.g. small islands can use more ccurate local lidar data
8814	4	73	34	73	34	Insert a space betwee number and unit: use "3 m" [APECS Group Review, Germany]	Accepted - text revised
8816	4	73	36	73	36	Check cross reference to "4.2.2.5" [APECS Group Review, Germany]	Accepted - text revised
19080	4	73	39	73	46	This section would benefit from including assessments of coastal erosion at additional spatial scales and not limiting to global. The other sections all include regional, national and local impacts. Coastal erosion is highly dependent on small-scale conditions and this should be reflected in the text. [Carl-Friedrich Schleussner, Germany]	Taken into account - We kept the global perspective in this paragraph (also to address other comments from other reviewers on more global pictures in terms of impacts), but mentioned that local situations can differ from the global trend. Depending on space availability, we plan to add some local scale examples, e.g. to show the range of situations or some extreme ones.
19082	4	73	39	73	46	This section should also include observed impacts of coastal erosion and not only focus on the physical impacts. See for e.g. Karlsson, M., van Oort, B., & Romstad, B. (2015). What we have lost and cannot become: societal outcomes of coastal erosion in southern Belize. Ecology and Society, 20(1). [Carl-Friedrich Schleussner, Germany]	Accepted - New sentence added (including the reference).
3462	4	73	39	73	57	This is extremely general and global and does not refract the situation is small islands, would it be possible to be more specific [Mahmood Riyaz, Maldives]	Taken into account - We kept the global perspective in this paragraph (also to address other comments from other reviewers on more global pictures in terms of impacts), but mentioned that local situations can differ from the global trend.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
13672	4	73	39	73	57	It would help if newly introduced concepts ''not discounted" and "normalized rice production index" received a brief explanation. [Debra Roberts and Durban Team, South Africa]	Taken into account - Sentences have been simplified.
14932	4	73	40	73	40	Only one global study [Christophe Deissenberg, Luxembourg]	Taken into account - Sentence modified.
266	4	73	40	73	46	Elsewhere in the chapter the problems with projecting human mobility are discussed. If such a study is cited it would be better to use much weaker language - "would" should be replaced with "it has been estimated that" [Robert Oakes, UK]	Accepted
14934	4	73	41	73	41	to be lost through enhanced coastal erosion [Christophe Deissenberg, Luxembourg]	Rejected
14936	4	73	42	73	42	associated with (not [Christophe Deissenberg, Luxembourg]	Accepted
14938	4	73	45	73	45	would reduce the 21st century impacts [Christophe Deissenberg, Luxembourg]	Taken into account
17390	4	73	48	73	48	Add to this section something about how this impact could also present an opportunity for carbon farming (restoring coastal wetlands in a production landscape). [Helen Kettles, New Zealand]	Rejected - The SROCC report focuses on Adaptation (see framing in Chapter 1) and only Chapter 5 includes some mitigation aspects (blue carbon).
21778	4	73	48	74	9	Book by Nicholls et al (2018) and especially the chapter on agriculture in coastal Bangladesh by Clarke et al (2018) is relevant here. Reference RJ Nicholls, CW Hutton, WN Adger, SE Hanson, MM Rahman, M Salehin (editors) 2018. Ecosystem Services for Well-Being in Deltas: Intergated Assessment for Policy Analysis. Springer, Freely downloadable at https://link.springer.com/book/10.1007%2F978-3-319-71093-8. See Derek Clarke, Attila N. Lázár, Abul Fazal M. Saleh, Mohammad Jahiruddin Prospects for Agriculture Under Climate Change and Soil Salinisation. Pages 447-467 Open Access [Robert Nicholls, UK]	Taken into account
14940	4	73	51	73	51	affects production and food security [Christophe Deissenberg, Luxembourg]	Accepted
8818	4	73	56	73	56	Where is Ebro delta? Maybe the country name could be inserted in the text [APECS Group Review, Germany]	Taken into account
14942	4	73	56	73	56	estimating for the latter a decrease [Christophe Deissenberg, Luxembourg]	Accepted
14944	4	74	4	74	5	Kabir et al., 2018), while oilseed, sugarcane and jute cultivation was reported to be already discontinued due to the current [Christophe Deissenberg, Luxembourg]	Taken into account
19084	4	74	11	74	23	SLR will also affect tourism through effects on critical transportation modes such as air and sea ports. This is particularly relevant for islands. See: Monioudi, I. N., Asariotis, R., Becker, A., Bhat, C., Dowding-Gooden, D., Esteban, M., ... & Phillips, W. (2018). Climate change impacts on critical international transportation assets of Caribbean Small Island Developing States (SIDS): the case of Jamaica and Saint Lucia. Regional Environmental Change, 1-15. [Carl-Friedrich Schleussner, Germany]	Accepted - Interesting reference added
21780	4	74	11	74	23	What about the interaction between tourism and climate policy which might curb flying and hence much high value coastal tourism? [Robert Nicholls, UK]	Accepted - We included this important point, but briefly as the SROCC does not aim at providing detailed analyses of the mitigation-adaptation nexus.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14946	4	74	18	74	20	Since AR5, forecasting the consequences of climate effects and especially the impacts of SLR per se on global-to-local tourism flows remains challenging (Rosselló-Nadal, 2014; Wong, 2014). [Christophe Deissenberg, Luxembourg]	Taken into account
13674	4	74	20	74	22	The point being made here is not clear. [Debra Roberts and Durban Team, South Africa]	Taken into account - Rewording.
14948	4	74	21	74	21	Addo, 2013), all the more as tourism [Christophe Deissenberg, Luxembourg]	Taken into account - Rewording.
8820	4	74	22	74	22	Check cross reference to "4.3.2" [APECS Group Review, Germany]	Taken into account
21782	4	74	25	74	36	Book by Nicholls et al (2018) and especially the chapter on fisheries in coastal Bangladesh andf the Bay of Bengal by Barange et al (2018) is relevant here. Reference RJ Nicholls, CW Hutton, WN Adger, SE Hanson, MM Rahman, M Salehin (editors) 2018. Ecosystem Services for Well-Being in Deltas: Intergated Assessment for Policy Analysis. Springer, Freely downloadable at https://link.springer.com/book/10.1007%2F978-3-319-71093-8. See Manuel Barange, Jose A. Fernandes, Susan Kay, Mostafa A. R. Hossain, Munir Ahmed, Valentina Lauria. Marine Ecosystems and Fisheries: Trends and Prospects. Pages 469-488 Open Access [Robert Nicholls, UK]	Rejected - This chap^ter does not deal specifically with SLR
8822	4	74	28	74	30	Multiple phrases and bracktes [APECS Group Review, Germany]	Taken into account
14950	4	74	32	74	33	manifestations and, especially, to the local fishery-dependent communities' ability [Christophe Deissenberg, Luxembourg]	Taken into account
21784	4	74	38	74	52	What is meant by Social Values -- I think this could be clearer at the beginning. [Robert Nicholls, UK]	Taken into account - Rewording.
11014	4	74	38	74	53	I would suggesst that you include the loss of habitability of regions of established residence, whether due to flooding, erosion, salinization, etc. Here is a reference to cite on that point A universal model for predicting human migration under climate change: examining future sea level rise in BangladeshBy: Davis, Kyle Frankel; Bhattachan, Abinash; D'Odorico, Paolo; et al.ENVIRONMENTAL RESEARCH LETTERS Volume: 13 Issue: 6 ArticleNumber: 064030 Published: JUN 2018 [Ben Orlove, USA]	Accepted - Interesting reference added
11016	4	74	38	74	53	Here is another reference of a coastal/island case of loss of habitability. Climate-induced migration: Exploring local perspectives in Kiribati By: Allgood, Lacey; McNamara, Karen E. SINGAPORE JOURNAL OF TROPICAL GEOGRAPHY Volume: 38 Issue: 3 Pages: 370-385 Published: SEP 2017 [Ben Orlove, USA]	Accepted - Other interesting reference added
11018	4	74	38	74	53	Here is a reference which discusses impediments to inland resettlement following threats to habitability from SLR, with examples from Florida and China Impediments to inland resettlement under conditions of accelerated sea level rise By: Geisler, Charles; Currens, Ben LAND USE POLICY Volume: 66 Pages: 322-330 Published: JUL 2017 [Ben Orlove, USA]	Taken into account - This reference is under consideration for sub-section 4.3.3.5 (on retreat including migration,)
14952	4	74	43	74	44	both the physical/ecological/human impacts' importance for and their distribution within a given society. [Christophe Deissenberg, Luxembourg]	Taken into account - Rewording.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
16086	4	74	45	74	52	The social values are also encapsulated in Cernea's IRR model (arguably more comprehensively than Graham's). See: Michael M Cernea "Impoverishment Risks, Risk Management, and Reconstruction: A Model of Population Displacement and Resettlement" (Keynote Paper presented to the UN Symposium on Hydropower and Sustainable Development, Beijing, October 2000). [Nathan Ross, New Zealand]	Rejected - Although interetsing publication, we try in this report to rely mostly on very recent (i.e. post-AR5) and SLR-focussed papers.
21786	4	74	50			Is "territorial sovereiginty" a social value? -- seems like a national or international issue? [Robert Nicholls, UK]	Taken into account - Rewording for clarification
268	4	74	52	74	52	A literature on loss and damage and in particular non-economic loss and damage is emerging [Robert Oakes, UK]	Taken into account - Issues related to loss and damage have been introduced in SROCC
8824	4	74	57	74	57	Check cross reference to "4.2" [APECS Group Review, Germany]	Taken into account - Checked
8826	4	75	2	75	2	Check cross reference to "Cross Chapter Box 5" [APECS Group Review, Germany]	Taken into account - Checked
8828	4	75	4	75	5	Check cross reference to "4.3.2.1" and "4.3.2.2" [APECS Group Review, Germany]	Taken into account - Checked
18306	4	75	9	75	46	Please note that Chapter 6 also provides a definition of compound events/risks, see page 7. [Laurens Bouwer, Netherlands]	We refer to Chapter 6 at the end of the first paragraph as the source for the most general information about compound events; this is meant to include defintions. Here we wanted to give a flavor for the range of definitions offerred in the literature.
18306	4	75	9	75	46	Please note that Chapter 6 also provides a definition of compound events/risks, see page 7. [Laurens Bouwer, Netherlands]	We refer to Chapter 6 at the end of the first paragraph as the source for the most general information about compound events; this is meant to include defintions. Here we wanted to give a flavor for the range of definitions offerred in the literature.
21788	4	75	9	75	46	Compound events is a rather vague section with lots of non-coastal examples.Suggest shortening dramatically or even remove and discuss compound events under flooding. [Robert Nicholls, UK]	We have removed material not related to coastal events
21788	4	75	9	75	46	Compound events is a rather vague section with lots of non-coastal examples.Suggest shortening dramatically or even remove and discuss compound events under flooding. [Robert Nicholls, UK]	We have removed material not related to coastal events
24032	4	75	9			This section can be reduced in length by cutting some of the conceptual parts (which overlap with Ch1 and partly Ch6). Concentrate here on SLR related compound events as reflected in the literature, if possible. [Hans-Otto Poertner and WGII TSU, Germany]	see response to comment 1873
24032	4	75	9			This section can be reduced in length by cutting some of the conceptual parts (which overlap with Ch1 and partly Ch6). Concentrate here on SLR related compound events as reflected in the literature, if possible. [Hans-Otto Poertner and WGII TSU, Germany]	see response to comment 1873
10820	4	75	14	75	15	What is the reasoning behind why 'tropical cyclones follwing similar paths' give effects that are greater than the simply additive? Evidence? Reference to impact of NW Europe winter storms of 2013/2014? [Thomas Spencer, UK]	material removed during revision
10820	4	75	14	75	15	What is the reasoning behind why 'tropical cyclones follwing similar paths' give effects that are greater than the simply additive? Evidence? Reference to impact of NW Europe winter storms of 2013/2014? [Thomas Spencer, UK]	material removed during revision
14954	4	75	19	75	19	effects by reducing the capacity to respond to subsequent events, for example, by exhausting the financial and [Christophe Deissenberg, Luxembourg]	passage rewritten so as to make meaning clearer
14954	4	75	19	75	19	effects by reducing the capacity to respond to subsequent events, for example, by exhausting the financial and [Christophe Deissenberg, Luxembourg]	passage rewritten so as to make meaning clearer
21790	4	75	25			Micronesia, Mariana Island and Papua New Guinea -- should this be widened to most reefs? [Robert Nicholls, UK]	here we are quoting AR5. a new statement toward the end of the final paragraph makes such a generalization for the future.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21790	4	75	25			Micronesia, Mariana Island and Papua New Guinea -- should this be widened to most reefs? [Robert Nicholls, UK]	here we are quoting AR5. a new statement toward the end of the final paragraph makes such a generalization for the future.
10822	4	75	27	75	28	Need to explain why / how [Thomas Spencer, UK]	see response to comment 1879
10822	4	75	27	75	28	Need to explain why / how [Thomas Spencer, UK]	see response to comment 1879
21792	4	75	27			While cities on river deltas are clearly candidates for compound events -- do we know that they are subject to high levels of risk or is this just an assertion? Given the long timescales of large catchments a coastal storm will not normally produce high flows in the timescale of the coastal storm. Smaller catchments as in Houston and Harvey did produce major compound floods -- so think about this. [Robert Nicholls, UK]	We rewrote the comment about cities to reflect a more specific and defensible claim
21792	4	75	27			While cities on river deltas are clearly candidates for compound events -- do we know that they are subject to high levels of risk or is this just an assertion? Given the long timescales of large catchments a coastal storm will not normally produce high flows in the timescale of the coastal storm. Smaller catchments as in Houston and Harvey did produce major compound floods -- so think about this. [Robert Nicholls, UK]	We rewrote the comment about cities to reflect a more specific and defensible claim
21794	4	75	30	75	37	Remove non-coastal examples [Robert Nicholls, UK]	see response to comment 1873
21794	4	75	30	75	37	Remove non-coastal examples [Robert Nicholls, UK]	see response to comment 1873
10824	4	75	35	75	37	if there is 'high confidence' the reference should be given. Otherwise this is an unsupported assertion. [Thomas Spencer, UK]	Confidence statements draw on all preceding references in the same section or paragraph and do not require separate citations.
10824	4	75	35	75	37	if there is 'high confidence' the reference should be given. Otherwise this is an unsupported assertion. [Thomas Spencer, UK]	Confidence statements draw on all preceding references in the same section or paragraph and do not require separate citations.
14956	4	75	42	75	42	Consequences include undermining [Christophe Deissenberg, Luxembourg]	material removed during revision
14956	4	75	42	75	42	Consequences include undermining [Christophe Deissenberg, Luxembourg]	material removed during revision
14958	4	75	43	75	46	Similarly, as demonstrated by a hypothetical scenario (Lunt et al., 2016), teleconnected flooding and drought linked to El Nino that result in crop yield declines and severe food security disruptions worldwide provide an example of climate events at widely separated locations with compound impacts. [Christophe Deissenberg, Luxembourg]	material removed during revision
14958	4	75	43	75	46	Similarly, as demonstrated by a hypothetical scenario (Lunt et al., 2016), teleconnected flooding and drought linked to El Nino that result in crop yield declines and severe food security disruptions worldwide provide an example of climate events at widely separated locations with compound impacts. [Christophe Deissenberg, Luxembourg]	material removed during revision
21798	4	75	48	76	18	Under reasons for concern what about adaptation? Given we can adapt at least in the 21st Century this must influence our analysis of reasons for concern. As this section comes before Section 4.4, should it be moved afterwards at the end of the Chapter. [Robert Nicholls, UK]	Taken into account - (i) We developed a very new analysis that now fully considers adaptation. (ii) At this stage, we decided to keep this risks of impact analysis (complemented with a bruning ember diagram) at the end of section 4.3, as a syntehtic perspective on possible impacts in the future; while we take into account adaptation scenarios, we do not consider the full complexity of what makes adaptataion efficient or not, what is actually done in section 4.4 where various parameters are better considered (including econcomical, social and political aspects). However, the location of this section will be discussed again at LAM4.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
14960	4	75	50	75	54	Low-lying islands, coasts and communities provide relevant illustrations of some of the Reasons for Concern (RFCs) developed in reference to one of the core objectives of the UNFCCC by the IPCC since AR3 (McCarthy, 2001) and describing potentially dangerous anthropogenic interference with the climate system. In particular, the RFCs illustrate the risks to unique and threatened systems (RFC1), and the risks associated with extreme weather events (RFC2) and with the uneven distribution of impacts (RFC3). [Christophe Deissenberg, Luxembourg]	Taken in to account - Rewording.
17266	4	75	52	75	53	Consider replacing "core objectives the UNFCC" with "key component of the ultimate objective of the UNFCCC". The Convention has one objective described in Article 2. This is widely known as the ultimate objective of the Convention. [Iulian Florin Vladu, Germany]	Taken in to account - Rewording.
21128	4	76	0	76		This section is called, "Responding" but it's really just about coastal protection. There have been some suggestions about cooling ice shelves around Antarctica by pumping cold seawater, blanketing European glaciers etc that aren't at all mentioned. I'm not saying it will work, but can we completely disregard? [Thomas Wagner, USA]	The section defines 'responses' broadly and includes 4 approaches - protection being only 1 of them - that together broadly constitute adaptation - as per the literature.
21128	4	76	0	76		This section is called, "Responding" but it's really just about coastal protection. There have been some suggestions about cooling ice shelves around Antarctica by pumping cold seawater, blanketing European glaciers etc that aren't at all mentioned. I'm not saying it will work, but can we completely disregard? [Thomas Wagner, USA]	Taken into account - We now state that mitigating greenhouse gases and geo-engineering climate and sea-levels fall beyond the scope of this chapter
836	4	76	0			There is a fifth possible response to sea level rise. It might be called Sea Level Restoration by Refreezing or Ice Thickening. If successful, no other responses might be needed. It involves using wind power to pump water onto sea ice so that the floating and grounded ice arrays so generated hold back warming water from melting and disintegrating ice shelves and glaciers at the same time as convecting excess heat to the tropopause and sending frigid brine, CO2 and oxygen to the abyssal depths, thereby cooling the ocean and enhancing the carbon pump and polar habitats. The concept is described at https://unfccc.int/documents/65014 and in more detail in the Ice Shield documents at https://www.climate-restoration-foundation.com/winwick-business-solutions. [William Clarke, Australia]	Taken into account - We now state that mitigating greenhouse gases and geo-engineering climate and sea-levels fall beyond the scope of this chapter
838	4	76	0			Peer-reviewd documents include https://link.springer.com/article/10.1007/s10584-005-5933-0 https://asu.pure.elsevier.com/en/publications/arctic-ice-management and http://pubs.aina.ucalgary.ca/arctic/Arctic33-1-168.pdf. There are also many industry publications on ice road construction and ice thickening to make drilling platforms and other structures. [William Clarke, Australia]	Taken into account - We now state that mitigating greenhouse gases and geo-engineering climate and sea-levels fall beyond the scope of this chapter
840	4	76	0			A combination of Protection and Advance measures would be the use of ice sheld arrays to protect Inuit settlements from coastal erosion and to reclaim ice field habitat punctuated by polynyas for sea-ice access and wildlife habitat. No Retreat measures would then be needed. [William Clarke, Australia]	Taken into account - but no literature found.

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21864	4	76	1	108	1	Where are institutional arrangements discussed that seem important to strategic planning for dynmaic coastal areas including climate change. I am thinking of Shoreline Management Planning (Nicholls et al., 2013) and also Delta Planning (Seijger et al., 2017). Adaptive delta planning is touch on but I do not get a solid point. Reference: Nicholls, R.J., Townend, I.H., Bradbury, A., Ramsbottom, D. and Day, S. (2013) Planning for long-term coastal change: experiences from England and Wales. Ocean Engineering (doi:10.1016/j.oceaneng.2013.01.025). Seijger, C., Douven, W., Halsema, G. Van, Hermans, L., Evers, J., Phi, H. L., … Phi, H. L. (2017). An analytical framework for strategic delta planning : negotiating consent for long-term sustainable delta development, 568. http://doi.org/10.1080/09640568.2016.1231667 [Robert Nicholls, UK]	This point is well made. A section addressing this matter is under development and will be completed post-SOD (see 4.4.4.1 and 4.4.4.2).
21864	4	76	1	108	1	Where are institutional arrangements discussed that seem important to strategic planning for dynmaic coastal areas including climate change. I am thinking of Shoreline Management Planning (Nicholls et al., 2013) and also Delta Planning (Seijger et al., 2017). Adaptive delta planning is touch on but I do not get a solid point. Reference: Nicholls, R.J., Townend, I.H., Bradbury, A., Ramsbottom, D. and Day, S. (2013) Planning for long-term coastal change: experiences from England and Wales. Ocean Engineering (doi:10.1016/j.oceaneng.2013.01.025). Seijger, C., Douven, W., Halsema, G. Van, Hermans, L., Evers, J., Phi, H. L., … Phi, H. L. (2017). An analytical framework for strategic delta planning : negotiating consent for long-term sustainable delta development, 568. http://doi.org/10.1080/09640568.2016.1231667 [Robert Nicholls, UK]	Taken into account - We have now added a subsection on governance to each type of response
21866	4	76	1	108	1	Where is the need for coastal information and monitoring to support responses to sea-level rise discussed? [Robert Nicholls, UK]	Accepted and taken into account in section under development for post-SOD (4.4.4.2)
21866	4	76	1	108	1	Where is the need for coastal information and monitoring to support responses to sea-level rise discussed? [Robert Nicholls, UK]	Accepted - Text revised
21868	4	76	1	108	1	Where is detecting sea-level rise acceleration from data as opposed in models discussed -- we talk about this like it is easy -- in practise it is hard (e.g., Haigh et al., 2014). Reference Haigh, I. D., Wahl, T., Rohling, E. J., Price, R. M., Pattiaratchi, C. B., Calafat, F. M., & Dangendorf, S. (2014). Timescales for detecting a significant acceleration in sea level rise. Nature Communications, 5, 3635. DOI: 10.1038/ncomms4635 [Robert Nicholls, UK]	paragraph 4.2.2 on observed changes in sea level extensively discusses the acceleration issue
21868	4	76	1	108	1	Where is detecting sea-level rise acceleration from data as opposed in models discussed -- we talk about this like it is easy -- in practise it is hard (e.g., Haigh et al., 2014). Reference Haigh, I. D., Wahl, T., Rohling, E. J., Price, R. M., Pattiaratchi, C. B., Calafat, F. M., & Dangendorf, S. (2014). Timescales for detecting a significant acceleration in sea level rise. Nature Communications, 5, 3635. DOI: 10.1038/ncomms4635 [Robert Nicholls, UK]	paragraph 4.2.2 on observed changes in sea level extensively discusses the acceleration issue
8830	4	76	2	76	2	Check cross reference to "4.3.3.1.4" [APECS Group Review, Germany]	Accepted - Checked and modified
8830	4	76	2	76	2	Check cross reference to "4.3.3.1.4" [APECS Group Review, Germany]	Accepted - Text revised

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21796	4	76	2	76	4	Rather than just accept O'Neill et al (2017) I think the evidence in the chapter should be the basis for arguments in this section. [Robert Nicholls, UK]	Accepted - New analysis provided based on the development of our own assessment in chapter 4 (+ development of a burning ember diagram for various geographies). Our results however confirm O'neill et al. (2017) conclusions, together with bringing new insigths.
21796	4	76	2	76	4	Rather than just accept O'Neill et al (2017) I think the evidence in the chapter should be the basis for arguments in this section. [Robert Nicholls, UK]	Accepted - Text revised
8832	4	76	6	76	6	check cross reference to "4.2.3". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	Taken into account - Text revised
8832	4	76	6	76	6	check cross reference to "4.2.3". Maybe "4.2.3.1.2"? [APECS Group Review, Germany]	Accepted - Text revised
8834	4	76	12	76	12	Check reference to "6.2" (Section?) [APECS Group Review, Germany]	Accepted - Checked
8834	4	76	12	76	12	Check reference to "6.2" (Section?) [APECS Group Review, Germany]	Accepted - Text revised
8836	4	76	13	76	13	Check reference to "4.2.2.5" and "4.2.3.3" [APECS Group Review, Germany]	Accepted - Checked
8836	4	76	13	76	13	Check reference to "4.2.2.5" and "4.2.3.3" [APECS Group Review, Germany]	Accepted - Text revised
8838	4	76	15	76	16	Check reference style [APECS Group Review, Germany]	Taken into account
8838	4	76	15	76	16	Check reference style [APECS Group Review, Germany]	Accepted - Text revised
8840	4	76	18	76	18	Few studies about South America and Africa (and another parts of Asia) are presented and exemplified in Section 4.3. If there are no studies in these regions (and elsewhere in the World), it should be highlighted in the end of the Section. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	Taken into account - Examples from Latin America, Africa and Asia included when possible.
8840	4	76	18	76	18	Few studies about South America and Africa (and another parts of Asia) are presented and exemplified in Section 4.3. If there are no studies in these regions (and elsewhere in the World), it should be highlighted in the end of the Section. This could draw scientists' attention to these regions. [APECS Group Review, Germany]	Accepted - Text revised - but few studies are available for these regions
21816	4	76	21	77	19	Where do things like hazard mapping, warning systems, etc sit in this framework. To me they are very important. I use to map these to Accommodate and Retreat, but this knowledge supports all adaptation and in the case of Protection helps understand and manage residual risk. Hence, I have started to consider cross-cutting "Information Measures" as in Nicholls (2018) which is already cited. However, it is handled, these measures need to be presented as they have major benefits if used appropriately. [Robert Nicholls, UK]	These informational measures are treated under accommodation - institutional, because the main aspect is the creation of institutions (laws, policies, networks) that generate, communicate and use information
21800	4	76	21			At last Responses are well defined. [Robert Nicholls, UK]	Accepted - Text revised
10748	4	76	29	77	19	It would be helpful to highlight measures dealing with laws and institutional issues in this section... [Jacques Andre Ndione, Senegal]	Accepted - There is now a subsection for each type of response highlighting governance aspects.
8842	4	76	29	77	22	The Chapter is too long and the primary aim of the SROCC is to update knowledge since AR5. Section 4.4 is very comprehensive, but could be shortened by referncing the knowledge at AR5, e.g. Section 4.4.2 can be limited to one sentence per response measure. The specific examples can be moved to Section 4.3. [APECS Group Review, Germany]	Taken into account - We now have combined former section 4.3 and 4.4 and reference more AR5 knowledge. AR5, however, had nothing on the different types of responses and a lot of new literati has emerged since then.
3178	4	76	34	76	34	It is implausible that a protective structure would entirely eliminate a risk of flooding. [Robert Kopp, USA]	Accepted - Text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21290	4	76	34	76	47	As in the case of the hard stuctures and the ecosystemb based measures, we should include a refernce for the soft apporach I suggest: Marcel J.F. Stive, Matthieu A. de Schipper, Arjen P. Luijendijk, Stefan G.J. Aarninkhof, Carola van Gelder-Maas, Jaap S.M. van Thiel de Vries, Sierd de Vries, Martijn Henriquez, Sarah Marx, and Roshanka Ranasinghe (2013) A New Alternative to Saving Our Beaches from Sea-Level Rise: The Sand Engine. Journal of Coastal Research: Volume 29, Issue 5: pp. 1001 – 1008. [Alejandro Souza, Mexico]	Accepted - Text revised
21802	4	76	34			Protection cannot "completely prevent" coastal impacts -- under protection residual risk always remains. [Robert Nicholls, UK]	Accepted - Text revised
8844	4	76	39	80	6	On page 76 line 39 the abbreviation for ecosystem-based measures is EBM, on page 80 line 6 the abbreviation is EbA. These two abbreviations are both used on multiple places in the chapter. For consistency it would be better to choose one. [APECS Group Review, Germany]	Accepted - Text revised
10828	4	76	40	76	40	natural ecosystems can also provide storage of flood waters which can reduce water levels up-estuary [Thomas Spencer, UK]	Accepted - Text revised
21804	4	76	41	74	42	point (b) also raises elevation [Robert Nicholls, UK]	Accepted - Text revised
21806	4	76	47			could add at the end "-- a flood event in a highly populated and/or economically important area often triggers a protection response." or something similar. [Robert Nicholls, UK]	Accepted - Text revised
10826	4	76	49	77	2	Surprising not to see mention of the Dutch 'zandmotor' here [Thomas Spencer, UK]	Rejected - we include this example further below.
3464	4	76	49	78	2	Could include land reclamation practices in small islands to add/create more land [Mahmood Riyaz, Maldives]	Accepted - Text revised
21808	4	76	51			polderisation also must involve "improved drainage/pumping" as well as a dike system. [Robert Nicholls, UK]	Accepted - Text revised
23294	4	77	0	78		Heading format needs attention. [Y. Jeffrey Yang, USA]	Accepted - Text revised
842	4	77	0			A new category, Reverse, might be included at the start of Table 4.6 to cover such measures. Reversing ice loss would eventually tend to slow, halt or even reverse inundation in warmer regions. [William Clarke, Australia]	Taken into account - We now state that mitigating greenhouse gases and geo-engineering climate and sea-levels fall beyond the scope of this chapter
21810	4	77	2			Incomplete reference: "Royal Insititute of British Architects and the Institution of Civil Engineers, 2010" -- maybe shorten to RIBA and ICE 2010. [Robert Nicholls, UK]	Accepted - Text revised
14962	4	77	4	77	4	but reduce vulnerability. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
8846	4	77	4	77	12	In the Accommodation measures paragraph, the category is first split in 'physical issues' , 'diversification of livelihoods' and 'institutional approaches'. Then it is split in ' physical accomodation and ' accomodation for salinity intrusions'. It would be more clear if this paragraph is related to table 4.6, in which the category is split in 'physical' and 'institutional' and then describe the difference between these two. [APECS Group Review, Germany]	Accepted - Text revised
21812	4	77	4	77	12	Again accomodation is not just to SLR -- the US National Flood Insurance Program uses accommodation with no consideration of rising sea levels [Robert Nicholls, UK]	Taken into account - we now mention that all responses are carried out for multiple reasons
24948	4	77	4	77	12	Several studies have come out showing that real estate values in the US are downgraded based on coastal flooding risk. This is in line with the number of private companies offering to assess risk of coastal flooding and the responsive insurance market. –Although this might go better in 4.4.3.1. [Elizabeth Weatherhead, USA]	Taken into account in the section on coastal flood impacts in Section 4.3

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8848	4	77	6	77	11	Also: cultivation of saline-tolerant crops is mentioned twice [APECS Group Review, Germany]	Accepted - Text revised
14964	4	77	10	77	10	which impose standards [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
2732	4	77	12			paddy' ('padi' in Malay) means 'rice'. Therefore use 'paddy field' or 'rice field' but never 'rice paddy' or 'rice paddies'. (Americans got it wrong when they went into the Vietnam war; the British use it correctly; see L.D. Stamp, 1929. Asia: regional and economic geography, Methuen, and all British geography textbooks). [Poh Poh Wong, Singapore]	Accepted - Text revised
21814	4	77	14	77	19	What about habitat creation measures and managed realignment which is practised widely in UK and elsewhere in Europe and further afield as well. [Robert Nicholls, UK]	Accepted - This is now considered in the section on retreat that follows below.
270	4	77	15	77	19	Confusing overlong sentence. What is State-to-local? [Robert Oakes, UK]	Accepted - Text revised
14966	4	77	17	77	18	and develops form small communities and individual assets to, more rarely, large populations ?????????????????? [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
14968	4	77	22	77	22	TABLE 4.6. THE FORMATTING OF THE TABLE SHOULD BE IMPROVED ("BROKEN" TEXT). THE DESIGNATION "IMPACTS ADRESSED" MAY BE CONFUSING. "PROBLEMS DRESSED" MAY BE MORE APPROPRIATE. HOWEVER, "IMPEDED DRAINAGE" IS A PROBLEM CREATED .... [Christophe Deissenberg, Luxembourg]	Rejected - this is the terminology generally used in the literati
17392	4	77	22	77	22	Add impact of "coastal squeeze" - which encompasses wetland changed and loss ...plus intertidal change and loss. [Helen Kettles, New Zealand]	Rejected - this is subsumed under wetlands
2194	4	77	22			The text in tables should be adjusted so that it does not appear as broken words [Chandani Appadoo, Mauritius]	Editorial – copyedit to be completed prior to publication
16432	4	77	27	77	33	as mentioned before, there is confusion in equating "potential catch" to "realised catch". A decrease of ~10% in catch potential can result in an increase in realised potential in the future compared to the present, if management is currently inefficient. To say that the changes "result in a loss of 3.4 Mt" is misleading. Furthermore, fisheries removes a much larger percentage of the total biomass of target species every year. This needs to be mentioned too, as it puts climate change impacts (in terms of total catches) in context, and suggest the changes are minimal. However, the text misses the important conclusion of existing evidence that the impacts are large at regional level, but up and down (Barange et al. 2014; Blanchard et al. 2012). [Coswig Kalikoski Daniela, Italy]	This comment likely relates to Ch 5, not Ch 4
16434	4	77	37	77	38	if this is implying that declines in fish stocks reduces productivity (not production) then this sentence is incorrect. The first rule of fisheries management is to reduce the biomass to a point where productivity is maximal. [Coswig Kalikoski Daniela, Italy]	This comment likely relates to Ch 5, not Ch 4
16436	4	77	54	77	55	It is simplistic, if not meaningless, to say that climate change and OA will impact the sustainability of aquaculture. We culture about 500 different species, marine, brackish, freshwater, in ponds, cages,... everything affects their sustainability. If we are going to talk about CC impacts we should avoid simplistic and meaningless statements that could be misinterpreted. [Coswig Kalikoski Daniela, Italy]	This comment likely relates to Ch 5, not Ch 4
272	4	78	1	78	1	Seems strange to consider "Displacement" a "deasure" which "addresses" something [Robert Oakes, UK]	Accepted - Text revised

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274	4	78	1	78	1	Is migration unplanned because not a policy? Usually migration is differentiated from displacement because it is a decision, therefore planned is ambiguous [Robert Oakes, UK]	Accepted - We now distinguish between individual and social level decsions
3180	4	78	3	78	3	Advance is discussed earlier as a strategy; why is it not included in this section? [Robert Kopp, USA]	Accepted - Now it is
24034	4	78	3			Suggest to structure this section according to the categories introduced above, i.e., protect, advance, accommodate, retreat, and possibly mixed/combined forms. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - Text revised
21818	4	79	1	83	12	The structure for this section differs from what was presented earlier. More thought on common typologies is need and this needs to be applied consistently. More generally, this is a weakness elsewhere in Chapter 4. [Robert Nicholls, UK]	Accepted - Structure has been revised and made consistent
13676	4	79	7	79	7	Similar attempts are also being made in Africa. The example of Eko Atlantic City in Nigeria should be cited here. [Debra Roberts and Durban Team, South Africa]	Accepted -we now include this example in the subsection on advance
8852	4	79	8	79	8	Please be consistent with the list of examples you provide. Here you cite random data for a maritime region (Pacific region), a region (Northern Ireland) and a city (new-York City). It would make more sense to cite research that focuses on similar geographic levels. [APECS Group Review, Germany]	Accepted and taken into account with revisions in SOD.
2196	4	79	8			If there are studies or reports done in African region, african cities, then these need to be added, so that a global pircure is obtained. [Chandani Appadoo, Mauritius]	Accepted - we have added examples from Africa
8854	4	79	10	79	11	“in countries such as …” why did you choose to mention these counties among all? If the estimates are only available for these counties, please mention it. [APECS Group Review, Germany]	Taken into account - These are just examples; text completed
2118	4	79	10	79	13	Indonesia should be included in the list of countries (see Esteban, 2018). [Josep Medina, Spain]	Accepted
3466	4	79	11	79	11	not many examples from small islands states, why? [Mahmood Riyaz, Maldives]	Taken into account - Examples of small islands included
8856	4	79	12	79	12	“but many more people living above hight tides are also proctected through hard structures” as reported here, it is an irrelevant information that sounds unimportant. If this information is important to the report, please make a new sentence and elaborate on this topic. [APECS Group Review, Germany]	Noted and taken into account in revisions to SOD.
8858	4	79	14	79	14	“In coastal lowlands” is that a continuation of the previous sentence? It is unclear if the coastal lowlands are located along the US coastline or in general around the world. This sentence lack of a reference. [APECS Group Review, Germany]	Accepted - Text modified
8860	4	79	17	79	18	“(…) harbor area.” Lack of reference. [APECS Group Review, Germany]	Taken into account - Texte modified
13678	4	79	17			'All major coastal cities'? On pg 80-line8 it says that "hard adaptation measures whose global distribution is not known in detail". [Debra Roberts and Durban Team, South Africa]	Taken into account - Texte modified
8862	4	79	20	79	20	remove the “alone” after Shanghai. [APECS Group Review, Germany]	Taken into account - Texte modified
8850	4	79	24	77	33	Unquoted, this is very close to the original published text: "although SLR driven changes in wave and tides characteristics amplify the expected design heights of the infrastructure by an average of 48–56%, when compared with design changes caused by SLR alone (Arns et al., 2017)." Consider paraphrasing. Also I think the statement could be mis-intepreted that the total design height might be increased by 48-56%, rather than the proportion of additional design height due to SLR. [APECS Group Review, Germany]	Accepted - Text revised

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14970	4	79	25	79	25	anticipation of future [Christophe Deissenberg, Luxembourg]	Editorial comment noted and taken into account in revisions to SOD
3814	4	79	26	79	26	change to "then" [Ola Kalen, Sweden]	Accepted
8864	4	79	26	79	26	“If sea level rise (…) than it” misspelling: it should be THEN. [APECS Group Review, Germany]	Taken into account - Texte modified
8866	4	79	26	79	30	This whole sentence is too long and unclear. Please break the sentence in 2 or 3 to give a clear information to the reader. [APECS Group Review, Germany]	Taken into account - Texte modified
14972	4	79	26	79	30	Whenever the sea level rise is considered in the planning process, it is standard practice to increase the height of the planed coastal defences by the regionally projected SLR height. However, this does not that into account that the SLR driven change in wave and tides characteristics increases the appropriate height of the defences by an additional 48–56% on the average (Arns et al., 2017). [Christophe Deissenberg, Luxembourg]	Editorial comment noted and taken into account in revisions to SOD
8868	4	79	30	79	33	Too long. Break the sentence. [APECS Group Review, Germany]	Taken into account - Texte modified
24950	4	79	35	79	37	I am surprised by these sentences given the ongoing flooding in Southern Florida, parts of India, China and other coastal regions. [Elizabeth Weatherhead, USA]	Rejected - Original sentence not found (mistake in line/page numbering?)
14974	4	79	36	79	36	that the conclusions are [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
3338	4	79	37	70	38	Limited evidence, medium evidence and medium agrrement. Inconsistent use of italics? [Castor Muñoz Sobrino, Spain]	Taken into account - Texte modified
14976	4	79	38	79	38	small islands (Nurse et al., 2014). [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
8870	4	79	38			Uncerainty language not in italics: medium evidence and medium agreement [APECS Group Review, Germany]	Taken into account - Texte modified
14978	4	79	39	79	39	about the limitations [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
14980	4	79	42	79	42	planning), and technical skills [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
14982	4	79	43	79	44	option, since the protection they provide may lead to additional developments which become increasingly risky as the SLR continues. [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
8872	4	79	44	79	44	You give here an example but it is unclear what it illustrates: is it an example of maladaptive option? Please conclude the example. [APECS Group Review, Germany]	Taken into account - Texte modified
14984	4	79	45	79	45	2006) of considerable investments into the engineered protection of coastal areas (Kates et al., 2006); [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
8874	4	79	48	79	48	”best responses”: to what? please find a better terminology. [APECS Group Review, Germany]	Taken into account - Texte modified
14986	4	79	52	79	52	as among different populations groups [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified
8876	4	79	53	79	53	“about the willingness to pay”: find a better terminology. [APECS Group Review, Germany]	Taken into account - Texte modified
8878	4	79	56	79	56	”(…) confirms a major conclusion of he AR5-cycle”: which conclusion? Please refer to the conclusion here. [APECS Group Review, Germany]	Taken into account - Texte modified
14988	4	79	56	79	56	These last points confirm a major conclusion of the AR5-cycle: the relevance of technical and [Christophe Deissenberg, Luxembourg]	Taken into account - Texte modified

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8880	4	79	57	79	57	L57: “(as well as others)”: which others? Be more specific. [APECS Group Review, Germany]	Taken into account - Texte modified
8882	4	79	57	79	57	L57: remove “critically” [APECS Group Review, Germany]	Taken into account - Texte modified
24654	4	80	0	82		It seems section on EbA is reviewing availability of material, directing to those sources, more than assessing the scope, use and effectiveness of EbA? [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - Text revised
15190	4	80	0	102		ALMOST NO MENTION IS MADE OF DISCOUNTING AND THE ASSOCIATED PROBLEMS. IT MIGHT BE APPROPRIATE TO ELABORATE A BIT MORE ON THE PROPERTIES AND USE OF DISCOUNTING, AND ON ALTERNATIVE DISCOUNTING SCHMES SUCH AS HYPERBOLIC DISCOUNTING. ALSO, IT WOULD MIGHT BE INTERESTING TO SPEAK ABOUT THE PROBABILISTIC INTERPRETATION OF DISCOUNTING (DISCOUNTING FACTOR = INSTANTANEOUS PROBABILITY OF THE END OF THE WORLD). THIS BRINGD WITH IT ADDITIONAL MOTIVATION FOR THE USE OF ROBUST APPROACHES, WHICH MAKE AN END OF THE WORLD IMPOSSIBLE “WITH PROBABILITY 1” [Christophe Deissenberg, Luxembourg]	Noted. The application of discount rates is highlighted in a variety of places in 4.4.3 and 4.4.5 but further attention may need to be given to the overarching import and use of discounting post-SOD.
15194	4	80	0	102		A POINT MAY BE WORTH MENTIONING. THRESHOLDS ARE NOT ONLY PHYSICAL, BUT ALSO DECISIONAL. SOME POLICY OPTIONS CAN ARE CURRENTLY OPTIMAL OR AT LEAST SATISFACTORY MAY BECOME “BAD” (ALTHOUGH TECHNICALLY STILL FEASIBLE) AT SOME LATER POINT OF TIME. E.G., IT MAY BE OPTIMAL TO BUILD DYKES UNTIL THE SEA REACHES A CERTAIN LEVEL, BUT NOT AFTER THIS LEVEL IS REACHED. A FORMAL THEORY FOR SUCH THRESHOLD PHENOMENA AND RELATED COMPUTATIONAL APPROACHES IN OPTIMAL CONTROL PROBLEMS CAN BE FOUND IN THE “SKIBA POINTS” LITERATURE, WHICH HAS BEEN APPLIED TO NUMEROUS CONCRETE PROBLEMS, ALSO IN ENVIRONMENT. SEE E.G. DEISSENBERG, CH. G. FEICHTINGER, W. SEMMLER, ET F. WIRL (2004), “HISTORY DEPENDENT MULTIPLE EQUILIBRIA AND GLOBAL DYNAMICS IN EFFICIENT INTERTEMPORAL OPTIMIZATION MODELS”, IN BARNETT, W., CH. DEISSENBERG, ET G. FEICHTINGER (EDS.), ECONOMIC COMPLEXITY: NON-LINEAR DYNAMICS, MULTI-AGENTS ECONOMIES, AND LEARNING, ISETE VOL 14, ELSEVIER, AMSTERDAM, 91-122. [Christophe Deissenberg, Luxembourg]	Noted. Adaptation limits and barriers are addressed in 4.4.5 but further attention will be focused on decision thresholds in 4.4.4.2 to be developed post-SOD. Note that the SROCC is an assessment of post-AR5 literature.
14990	4	80	1	80	4	A growing literature also advocates using combinations of options and their sequencing through time (see Section 4.3.4.2). An underlying issue about the choice of technical and engineering options remains, and of any other option, the difficulty to integrate a long-term challenge such as SLR. [Christophe Deissenberg, Luxembourg]	Taken into account - we included new sections addessing different options in comparison
13992	4	80	6			Need to standardise the term (and acronym) used - either Ecosystem Based Adaptation (preferred) or ecosystem based measures - same comment applies to the acronym for Community based adaptation. [Debra Roberts and Durban Team, South Africa]	Taken into account - We use now Ecosystem-based Adaptation
8884	4	80	8	80	9	Modify sentence to “ The current distribution of coastal ecosystems is well-studied in comparison to hard adaptation measures, whose global distribution is not known in detail (reference). Meanwhile, potential restoration extents are not well understood.” [APECS Group Review, Germany]	Taken into account - We added references and revised the sentence.

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8886	4	80	10	80	11	This sentence is not clear. “where these ecosystems occur naturally”: what do you mean by naturally? Most of the ecosystems have been transformed by humans throughout time. Very few remained pristine. Is this sentence very relevant for the paragraph? [APECS Group Review, Germany]	Taken into account - text revised
14992	4	80	10	80	11	Ecosystem-based adaptation, by definition, can only exist and function where the ecosystem occurs naturally. [Christophe Deissenberg, Luxembourg]	Taken into account - text revised to also address comment no. 8886
8888	4	80	11	80	11	Remove “probably” [APECS Group Review, Germany]	Taken into account - text revised
8890	4	80	12	80	12	“Wessel and Smith, 1996” this reference is older than the previous IPCC report. Is it necessary to mention it here? [APECS Group Review, Germany]	Yes, it is still relevant for baseline information
8892	4	80	14	80	14	“covered by habitat” change to inhabited. [APECS Group Review, Germany]	The sentence changed, suggestion doesnt fit anymore
8894	4	80	14	80	14	“the spatial resolution (…)” please rephrase this sentence. As such it is unclear. [APECS Group Review, Germany]	Accepted - text revised
8896	4	80	16	80	16	“totaling” I would suggest to use “and cover” [APECS Group Review, Germany]	Accepted - text revised
14994	4	80	17	80	17	(Burke, 2011). The extent of other coastal habitats is less [Christophe Deissenberg, Luxembourg]	Accepted - text revised
8898	4	80	18	80	18	remove “well” and “these” [APECS Group Review, Germany]	Accepted - text revised
8900	4	80	19	80	19	replace “These estimates” by “Such estimates” [APECS Group Review, Germany]	Accepted - text revised
14996	4	80	19	80	20	These estimates can be used to assess the economic or societal value of each ecosystem at multiple spatial scales. [Christophe Deissenberg, Luxembourg]	Accepted - text revised
8902	4	80	20	80	20	move “globally” to the end of th sentence. [APECS Group Review, Germany]	Accepted - text revised
8904	4	80	23	80	23	“and is a political, scientific and technological challenge and endeavor” in which aspect? This requires a bit more explanation. [APECS Group Review, Germany]	Accepted - text revised
14998	4	80	23	80	23	sustainability and are a political, scientific and technological challenge and endeavor (Scarano, 2017). EbA [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editoril changes doesnt fit anymore
8906	4	80	24	80	24	“are” instead of “is” [APECS Group Review, Germany]	This section changed and the suggested editoril changes doesnt fit anymore
8908	4	80	24	80	24	move the references to the end of the sentence [APECS Group Review, Germany]	This section changed and the suggested editoril changes doesnt fit anymore
8910	4	80	26	80	28	You already abbreviated EbA and NbS in L22 so there is no need of re-doing it here. “While EbA is used in this section (…)”L26; change to “EbA is used in this section to define related solutions and implementation examples that are found in the literature under the terms Ecosystem-based disaster Risk (….)” [APECS Group Review, Germany]	This section changed and the suggested editoril changes doesnt fit anymore
8912	4	80	29	80	29	Remove end of the sentence from “depending on their specific (…)” [APECS Group Review, Germany]	This section changed and the suggested editoril changes doesnt fit anymore
15000	4	80	29	80	30	on their specific aim, design, and implementation. [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editoril changes doesnt fit anymore
8914	4	80	30	80	30	”The main challenges” [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8916	4	80	32	80	32	“Today”, this word is timeless. Please add a date or a reference to time. [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8918	4	80	32	80	32	Start the sentence by “There is growing number of (…) although engineered and technological adaptations options (…)” [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
24036	4	80	32	80	53	Need to add references [Hans-Otto Poertner and WGII TSU, Germany]	This section changed and the suggested changes doesnt fit anymore

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15002	4	80	33	80	33	coast, there is a growing number [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editorial changes doesnt fit anymore
8922	4	80	34	80	34	The first part of the sentence lacks of an ending. As such it does not mean anything. “a number of countries” How much? If this is not known, please remove “a number”. [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
15004	4	80	34	80	35	communities implement ecosystem-based coastal adaptation measures which are are increasingly [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editorial changes doesnt fit anymore
8920	4	80	34	80	39	This sentence is too long. Please break it. [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8924	4	80	39	8	39	“mainstreaming” is not the right terminology here [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
11742	4	80	42	80	42	Seychelles [John Church, Australia]	This section changed and the suggested editorial changes doesnt fit anymore
15006	4	80	42	80	45	Seychelles [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editorial changes doesnt fit anymore
2198	4	80	42			Seychelles instead of Seychalles [Chandani Appadoo, Mauritius]	This section changed and the suggested editorial changes doesnt fit anymore
8926	4	80	47	80	47	“mainstreaming” is not the right terminology here [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
15008	4	80	47	80	47	mitigation, or biodiversity protection, is increasingly recognized as a mainstream option. Examples [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editorial changes doesnt fit anymore
8928	4	80	48	80	48	remove “the inclusion of” [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
15010	4	80	49	80	49	Sri Lanka), strategies for forests that [Christophe Deissenberg, Luxembourg]	This section changed and the suggested editorial changes doesnt fit anymore
8930	4	80	51	80	52	why Nationally Determined Contributions is capitalized if not abbreviated? [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8932	4	80	56	80	56	Replace “incl.” by “including”. Add a dot at the end of the sentence. [APECS Group Review, Germany]	Accepted - text revised
8934	4	81	0	81		Column 1 should be renamed as “Scope”. You should add another column called “Spatial Extent” to make this information clear to the readers. [APECS Group Review, Germany]	Accepted: renemed into "spatial extent" but no new column added
8936	4	81	0			Table 4.7: In the second and third row, first column, the geography of these databases is missing [APECS Group Review, Germany]	Accepted - text revised
8938	4	81	0			Table 4.7: in the seventh row, second column, the link is there twice [APECS Group Review, Germany]	Accepted - text revised
276	4	82	1	82	1	Suggest including "livelihoods" - pressure on livelihoods and resources manifests itself through impacts on livelihoods [Robert Oakes, UK]	I can not locate where this comment fits.
8940	4	82	3	82	3	“ecosystem-based practices”: is that similar to EbA? If yes then use EbA otherwise explain what is the difference between both. [APECS Group Review, Germany]	Accepted - text revised (ecosystem-based adaptation measures)
8942	4	82	4	82	4	remove comma after “communities” [APECS Group Review, Germany]	Accepted - text revised
8944	4	82	4	89	40	On page 82 line 4, CBA is the abbreviation for Community-based adaptation. On page 89 line 40, CBA is the abbreviation for cost-benefit analysis. Now it can be confusing when this abbreviation is used what you are referring to. It would be better to have two different abbreviations [APECS Group Review, Germany]	Taken into account - abbriviation for community-based adaptation was removed

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8946	4	82	4	93	48	On page 82 line 4, community-based adaptation is abbreviated as CBA, on page 93 line 48, it is abbreviated to CB. It would be more consistant to use one abbreviation throughout the entire chapter/report. [APECS Group Review, Germany]	Taken into account - abbriviation for community-based adaptation was removed
8948	4	82	8	82	8	what mean natural? “in the face of SLD”, maybe change to “to answer SLR-related hazards”. [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8950	4	82	8	82	10	This part of the sentence starting from “in projects in global (…)” does not fit with the first part of the sentence and does not mean anything. Please edit the sentence to make it clear. [APECS Group Review, Germany]	This section changed and the suggested editorial changes doesnt fit anymore
8952	4	82	12	82	12	“Retreat” Wouldn’t “Adaptation” fit best? [APECS Group Review, Germany]	Noted. The section on Retreat has been revised to include new material on human mobility, including migration, displacement and planned relocation (see e.g., 4.4.3.5)
24038	4	82	12			This section could mention that migration, e.g., from small islands, is common generally as a livelihoods strategy, and therefore, it is difficult to assess whether migration is the response to climate change, or if climate change is simply an amplifier (if at all). Hence, there is quite limited evidence for climate-induced migration. [Hans-Otto Poertner and WGII TSU, Germany]	Noted. Taken into account in revisions to SOD (see e.g., 4.4.3.5)
15012	4	82	14	82	14	is exacerbating the vulnerability of millions [Christophe Deissenberg, Luxembourg]	Accepted and taken into account in revisions to SOD.
278	4	82	21	82	21	Eqaully many practioners and academics view mobility as loss and damage - Pegram, J., and Oakes, R. (2017). No Place to Call Home: Protecting Children’s Rights when the Changing Climate Forces them to flee. Unicef UK. https://downloads.unicef.org.uk/wp-content/uploads/2017/04/No-Place-To-Call-Home.pdf?_ga=2.73316244.623674493.1494945170-1964912592.1483624860 [Robert Oakes, UK]	Noted. Taken into account in revisions to SOD (see e.g., 4.4.3.5.5)
16088	4	82	23	82	27	Significantly higher risks also in low-lying atoll States - Kiribati, Tuvalu, Marshall Islands, Maldives - because it could affect (and fragment) entire peoples. So, while the numbers may be comparatively small, the impacts on cultures and nations are potentially vast. [Nathan Ross, New Zealand]	Taken into account in the assessment of literature on retreat (see 4.4.3.5)
13680	4	82	25			Africa has many low-income countries with low-lying coastal areas. Do the publications cited here not mention Africa? Egypt - fine, but what about Mozambique? Most of West and equatorial Africa? THis list includes Iraq but not Kenya, Somalia, Nigeria, Mexico, Brazil. Where are all the global South countries? Throughout the report small islands get mentioned frequently, the entire continents of Africa or South America, with their enormous coastline, rarely. This is hard to understand. [Debra Roberts and Durban Team, South Africa]	Noted. Revisions to the SOD include assessment of available post-AR5 literature from Africa and South America, including observed retreat (section 4.4.3.5.1).
8954	4	82	27	82	27	” This is also true”, replace by “Risks are also high”. [APECS Group Review, Germany]	Editorial suggestion noted and taken into account.
15014	4	82	31	82	31	Economic and human losses may also arise from [Christophe Deissenberg, Luxembourg]	Editorial suggestion noted and taken into account.
280	4	82	31	82	33	Sentence needs reordering - the losses are not at risk, rather there is a risk of losses [Robert Oakes, UK]	Editorial suggestion noted and taken into account.
3182	4	82	33	82	35	With that SLR projections? Check to make sure SLR projection-dependent statements are consistent through text. [Robert Kopp, USA]	Noted and taken into account in revisions to SOD.
282	4	82	33	82	37	IDMC's new report will be out on May 16th with updated stats [Robert Oakes, UK]	Noted.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15016	4	82	34	82	35	estimated that SLR in a 2°C warmer world could globally submerge by the end of this century land that is currently home to 280 million people (Strauss et al., 2015). This raises major concerns about the [Christophe Deissenberg, Luxembourg]	Editorial suggestion noted and taken into account.
284	4	82	37	82	39	Two issues are being conflated here. On the one hand, most academics agree that environmentally related mobility occurs in a political, economic and cultural context as shown by the Foresight Report 2011. This means it is often difficutlt to ascribe environmental chnage as the reason for mobility. On the other hand, it is normally considered problematic to project numbers who will move in the future as such models rely on assumptions concerning not only physcial, but also economic, social and psychological processes while generally minimising the agency of the affected peoples to adapt to changes and make their own decisions. [Robert Oakes, UK]	Taken into account in revised section on Retreat.
15018	4	82	37	82	39	IDMC, 2016). However, it remains scientifically challenging to estimate future displacement associated with SLR. Recent literature emphasizes that economic and political factors are powerful drivers of human mobility associated with disasters (Stapleton et al., 2017). Despite [Christophe Deissenberg, Luxembourg]	Accepted and taken into account.
286	4	82	39	82	40	"despite this" suggests a serious dearth of science or will. Yes there are data gaps, but we know that people have been and will be displaced/relocated, so better to delete. [Robert Oakes, UK]	Accepted and taken into account.
15020	4	82	40	82	40	over the last decade in both the scientific [Christophe Deissenberg, Luxembourg]	Editorial suggestion noted and taken into account.
288	4	82	42	82	42	The taskforce was mandated to "develop recommendations for integrated approaches to avert, minimize and address displacement related to the adverse impacts of climate change". Without this context it could be seen to be more powerful than it is [Robert Oakes, UK]	Noted and taken into account in revisions to SOD (section 4.4.3.5.7)
16090	4	82	44	82	47	Climate change is already identified as a reason for migration by 23 per cent of I-Kiribati migrants and eight per cent of Tuvaluan migrants. See: Climate Change and Migration in the Pacific: Links, attitudes and future scenarios in Nauru, Tuvalu, and Kiribati (United Nations University Institute for Environment and Human Security). [Nathan Ross, New Zealand]	Noted.
290	4	82	46	82	46	NZ to introduce CC refugee visa [Robert Oakes, UK]	Noted - this was investigated in 2017 but not implemented.
15022	4	82	47	82	47	p. 1625). The Environmental [Christophe Deissenberg, Luxembourg]	Editorial suggestion noted.
292	4	82	47	82	49	Strange to include another projection here [Robert Oakes, UK]	Taken into account in revised SOD.
3184	4	82	47	82	49	Environmental Justice Foundation does not sound like a primary, peer-reviewed analysis. Replace with appropriate peer-reviewed analysis. [Robert Kopp, USA]	Accepted - Text revised
294	4	82	50	82	50	Recent reseach in Kiribati has shown that the primary reason given for moving was related to the environment in 14% of cases 2005-2015. Oakes, R., Milan, A., and Campbell, J. (2016). Kiribati: Climate change and migration - Relationships between household vulnerability, human mobility and climate change. Report No.20. Bonn: United Nations University Institute for Environment and Human Security (UNU-EHS). http://collections.unu.edu/view/UNU:5903 [Robert Oakes, UK]	Noted.
15024	4	83	4	83	4	and develops from ????? [Christophe Deissenberg, Luxembourg]	Editorial comment noted and taken into account in revisions to SOD

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296	4	83	10	83	10	"raised controversial views" does not make sense [Robert Oakes, UK]	Editorial comment noted and taken into account in revisions to SOD
15026	4	83	10	83	10	(Kura et al., 2017), and is widely controversial (Genovese and [Christophe Deissenberg, Luxembourg]	Editorial comment noted and taken into account in revisions to SOD
298	4	83	12	83	12	It should be added that relocations can be successful when carried out in a participatory nature and cultures and livelihoods are protected (UNHCR, 2016a: 2016b) [Robert Oakes, UK]	Taken into account
13994	4	83	14			Key cost figures should be drawn up into the Executive Summary. [Debra Roberts and Durban Team, South Africa]	Noted
8956	4	83	19	83	19	Remove “Finally” [APECS Group Review, Germany]	Accepted - Text revised
2200	4	83	19			synthesize instead of synthesis [Chandani Appadoo, Mauritius]	Accepted - Text revised
21822	4	83	23	83	39	In a forthcoming World Bank report by Nicholls et al (2018c) we show that if protection is followed widely, the bulk of the costs are maintenance rather than capital investment. This is an important message as most focus in protection is on the captial cost and maintenace is forgotten. This might provide a good figure. [Jochen Hinkel is a coauthor so knows the report] Reference: Nicholls, Lincke, Hinkel et al. Global Investment Costs for Coastal Defence Through the 21st Century. Report to the World Bank. [Robert Nicholls, UK]	Accepted - Reference and message added
15028	4	83	24	83	25	As shown in Table 4.8, the protection costs can be expressed as costs per unit of length protected and of height of the structure. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
8958	4	83	24	83	28	You use sometimes the singular for cost (L24: “protection cost”) and the plural form (L25 “a number of unit costs”). This distinction is not always clear. [APECS Group Review, Germany]	Editorial – copyedit to be completed prior to publication
15030	4	83	25	83	26	calculated diverse unit costs for defences which were used in [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
15032	4	83	27	83	28	have been further used in a series of global assessments reviewed by Jonkman (2013) and in global adaptation cost estimates [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
15034	4	83	31	83	32	Defences depend on good maintenance to remain effective. The annual maintenance budget is in the order of 1% to 2% of the capital costs (Jonkman, 2013). [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
15036	4	83	33	83	33	For some types of infrastructure such as surge barriers, the costs could be higher, but they are poorly [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
21820	4	83	34			Insert "Protection-based" before adaptation to saltmarsh intrusion -- there are many non-protection adaptations to salinisation such as changing to more salt-tolerant crops, genetic engineering of this, etc. [Robert Nicholls, UK]	Accepted - Text revised
21824	4	83	38			Good summary table [Robert Nicholls, UK]	Noted -Thanks.
15038	4	83	42	83	42	delivery times the [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
8980	4	84	0	84		Table: name the first column [APECS Group Review, Germany]	Accepted - Text revised
15040	4	84	1	84	1	and an innovative [Christophe Deissenberg, Luxembourg]	Editorial – copyedit to be completed prior to publication
8960	4	84	4	84	4	“need to be describe” is maybe not the best term. “need to be accounted” is maybe better [APECS Group Review, Germany]	Accepted - Text revised
8962	4	84	6	84	10	This whole section lacks references. [APECS Group Review, Germany]	Accepted - References added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8964	4	84	7	84	7	“also often” often does not bring any information? How many cases? Based on what? What is the accuracy of this statement? [APECS Group Review, Germany]	Accepted - Text revised
8966	4	84	8	84	8	Without a reference the first part of the sentence is not accurate; dunes can be large enough to represent a substantial part of the beach (e.g. Dune du Pilat, France). [APECS Group Review, Germany]	Accepted - Text revised
15042	4	84	8	84	10	While the dune is a small part of the overall beach volume, it is critical to the defence provided by nourishment, and attention needs to be given on its monitoring and maintenance. The capital costs for dunes are like those for beach nourishment, although placement and planting vegetation may raise costs. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
24040	4	84	15	84	26	A statement of high confidence should be followed by strong evidence, which is not in this case. [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - Text revised
8968	4	84	17	84	17	“There is also A risk of locking into”, you should maybe specify, locking who? [APECS Group Review, Germany]	Accepted - Text revised
8970	4	84	20	84	20	review the sentence “Many hard defenses (…) protection” [APECS Group Review, Germany]	Accepted - Text revised
8972	4	84	22	84	22	remove “or otherwise” [APECS Group Review, Germany]	Accepted - Text revised
8974	4	84	23	84	23	“The loss of habitats violates many statutes such as the EU habitats Directive” be more specific: in which direction does it violate these statutes? [APECS Group Review, Germany]	Accepted - Sentence dropped
3468	4	84	23	84	24	(e.g. in Vietnam, Bangladesh, Egypt, Malaysia, Thailand, Myanmar, the Philippines, Indonesia, China and Iraq),are all the countries in example lower income counties small islands and low coastal areas? [Mahmood Riyaz, Maldives]	Accepted - Text revised
21826	4	84	24	84	26	Link possible sand shortage for nourishment to the wider literature on a global sand shortage? -- this is part of a bigger problem. [Robert Nicholls, UK]	Accepted - Link established
21828	4	84	28	85	1	In general, there is a wide literature that coastal ecosystems are declining and hence the protection they provide must be similarly falling. Hence simply stabilising these areas would be a major change, require a lot of effort and provide great benefits. I think that this point should this point be made? [Robert Nicholls, UK]	Accepted - Text revised
15044	4	84	33	84	34	Not enough is known about the cost sources to make generally valid estimations of the unit costs across large spatial scales. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
8976	4	84	36	84	36	Remove “In general” and add a citation to the first part of the sentence [APECS Group Review, Germany]	Accepted - Text revised
10830	4	84	44	84	44	This is too simplistic. Biogenic structures are characterised by strong threshold effects. Thus for example, vegetated platforms, often as a result of root or rhizome networks are highly resistant to raised energy levels until the resistenace of the sediment and root complex is overcome when they collapse catastrophically. 'Blow outs' in seagrass beds for example. [Thomas Spencer, UK]	Accepted - Text revised
8978	4	84	45	84	45	remove “a body of” [APECS Group Review, Germany]	Accepted - Text revised
21830	4	84	51			Table 4.9 -- dunes appear in two sections. I am not worried about this but does it suggest that nourishment should be considered am ecosystem-based protection approach? Might help to tighten the chapter. [Robert Nicholls, UK]	Accepted - We have removed dunes from this table.
8982	4	85	9	85	9	Remove “Generally” [APECS Group Review, Germany]	Accepted - Sentence dropped

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
8984	4	85	9	85	11	This statement lacks of explanation. Is there a reason why these study do not take into account beaches and dunes? [APECS Group Review, Germany]	Accepted - We now provide an explanation up front when the term is introduced.
10832	4	85	13	85	14	repetition. Substance: Field studies of Iris Moeller under-referenced in this chapter [Thomas Spencer, UK]	Accepted - Text revised
8986	4	85	23	85	23	What does “typical” mean in this context? Do you mean common? If yes, common where? [APECS Group Review, Germany]	Accepted - Text revised
8988	4	85	26	85	27	"substantial" economic benefits contradicts "actual size" given the confidence statements. Conider removing "substantial" or re-word to incorporate the essence of the final sentence - that the cost-benefit of ecosystem based measures is geography-dependant. [APECS Group Review, Germany]	Accepted - Text revised
15046	4	85	30	85	30	b) save millions of dollars [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
8990	4	85	30	85	33	Not sure this enumeration with a), b), c), and d) is needed here. [APECS Group Review, Germany]	Accepted - Text revised
8992	4	85	36	85	36	Move the last part of the sentence “based on different SLR scenarios in the US” to the beginning of the sentence: “For example, based on different SLR scenarios in the US, the number of people (…)” [APECS Group Review, Germany]	Accepted - Text revised
15048	4	85	39	85	40	It is therefore difficult to extrapolate the physical or economic benefits across geographies. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
13682	4	86	5	86	5	incl.' should be written in full 'including' [Debra Roberts and Durban Team, South Africa]	Accepted - Text revised
2202	4	86	6			defence instead of defense [Chandani Appadoo, Mauritius]	Accepted - Text revised
8994	4	86	7	86	8	like other 'hard' engineering structures'. Ecosystem-based measures did not fall under hard structures in table 4.6, they were two different categories. [APECS Group Review, Germany]	Accepted - Text revised
8996	4	86	8	86	8	Why using bracket for “hard”. You used the word without brackets earlier in the text [APECS Group Review, Germany]	Accepted - Text revised
8998	4	86	8	86	8	“engeneering structure”. You use different terms throughout the section 4.4: structure, protection, defense … For consistency I would suggest to choose one. [APECS Group Review, Germany]	Accepted - Text revised
21832	4	86	12			The biggest drawback is the space that is required (Royal Society, 2014, Resilience to Extreme Weather) which is often why the ecosystems have decline in the first place -- and today that space is often not available. [Robert Nicholls, UK]	Accepted - Text revised
9000	4	86	13	86	13	Add “natural wetlands and reefs can increase the risk of flooding in some instances.” [APECS Group Review, Germany]	Accepted - Text revised
15050	4	86	13	86	14	Like any other feature that interacts with coastal processes, natural wetlands and reefs can increase flooding in some instances. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
11958	4	86	13		14	provide evidence. [Chukwuma Anoruo, Nigeria]	Accepted - Evidence is provides in the two sentences that follow
9002	4	86	14	86	14	remove “This can happen”, capitalize the F and add in “For example flooding can happen” [APECS Group Review, Germany]	Accepted - Text revised
9004	4	86	18	86	18	If there is a lack of studies on the drawbacks, it might be good to add a sentence and emphasize the need for further research. [APECS Group Review, Germany]	Accepted - Text revised
9006	4	86	23	86	23	remove “will” [APECS Group Review, Germany]	Accepted - Text revised
15052	4	86	24	86	24	The major drawbacks include [Christophe Deissenberg, Luxembourg]	Accepted - Text revised

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21834	4	86	30			avoid non-metric units or at least concert. [Robert Nicholls, UK]	Accepted - Text revised
9008	4	86	33	86	33	move 21st century to the end of the sentence. [APECS Group Review, Germany]	Accepted - Text revised
21836	4	86	35	86	37	What about the Richard Tol unit costs for migration which are empirically derived and used in Hinkel et al (2013). [Robert Nicholls, UK]	Accepted - Text revised
15054	4	86	36	86	36	Assessments of the few data available indicate that the monetary cost of managed retreat [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
9010	4	86	39	86	39	wrong enumeration, it should be 4.4.4.4 instead of 4.4.4.5 [APECS Group Review, Germany]	Accepted - Text revised
16092	4	86	39	86	39	Numbering sequence is out: goes from 4.4.4.3 to 4.4.4.5. [Nathan Ross, New Zealand]	Accepted - Text revised
9012	4	86	46	86	51	This sentence is too long, please shorten or make 2 sentences out of it. In this case I suggest to break the sentence after L48 “compared to no adaptation”. [APECS Group Review, Germany]	Accepted - Text revised
9014	4	86	46	86	51	This sentence is quit confusing for me. Needs revision. Maybe it is in fact two sentences, and something went wrong behind the '4%'? Now it seems as if both the discount rate of 4% and a discount rate of up to 6% is used. [APECS Group Review, Germany]	Accepted - Text revised
9016	4	86	49	86	49	What does “ind” means? [APECS Group Review, Germany]	Accepted - Text revised
15056	4	86	49	86	49	4%. And that ?????? [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
15058	4	86	50	86	51	to 90% of the global floodplain population, under SLR scenarios from 0.3 to 2.0 m, five Shared Socio-economic Pathways (SSPs), and discount rates up to 6% (Figure 4.13). [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
9018	4	86	53	86	53	replace “to protect” by “protecting” [APECS Group Review, Germany]	Accepted - Text revised
9020	4	86	54	86	54	remove the “and” before “beyond” [APECS Group Review, Germany]	Accepted - Text revised
9026	4	87	0	87		Figure 4.13: Countries in white should be explained: Are they countries with NO DATA or countries without coasts, or both. Add this information in the legend. [APECS Group Review, Germany]	Accepted - Figure revised. Country information has been taken out.
9028	4	87	0			In figure 4.13, the abbreviation BCR is not introduced in the text. It would help to make clear in the caption that it is the benefit-cost ratio that is mentioned. [APECS Group Review, Germany]	Accepted - Figure revised
24656	4	87	5			Text structure does not flag those 4 response measures mentioned? [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - Structure has been revised
19116	4	87	15	87	15	considerations [Anna Zivian, USA]	Accepted - Text revised
9022	4	87	16	87	16	remove “generally” [APECS Group Review, Germany]	Accepted - Text revised
21838	4	87	28			"cills" -- do not understand [Robert Nicholls, UK]	Accepted - Text revised
9024	4	87	35	87	35	remove “planting” at the end of the sentence. [APECS Group Review, Germany]	Accepted - Text revised
15188	4	88	0	102		THESE PAGES ARE IN PART VERY REPETITIVE AND ARE MORE A LITERATURE CEMETARY THAN A STRUCTURED; PURPOSEFUL ESSAY. MANY STATEMENTSARE RATHER TRIVIAL. I BELIEVE THAT THERE ADDED VALUE IS SMALL AND THAT THEY SHOULD BE SHORTENED AND RESTRUCTURED INTO A MORE CONVINCING WHOLE. [Christophe Deissenberg, Luxembourg]	Accepted - These sections have been extensively revised
15060	4	88	2	88	2	benefits. This may [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
9030	4	88	3	88	3	remove “will” [APECS Group Review, Germany]	Accepted - Text revised
9032	4	88	5	88	5	remove “will” [APECS Group Review, Germany]	Accepted - Text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
9034	4	88	13	88	13	End the sentence at the end of the line and add s to “protection measures.” [APECS Group Review, Germany]	Accepted - Text revised
21840	4	88	13	88	15	No room in urban areas -- building well on the lack of space issue above. [Robert Nicholls, UK]	Accepted - Text revised
9036	4	88	14	88	14	start new sentence with “Morever” [APECS Group Review, Germany]	Accepted - Text revised
15062	4	88	14	88	15	Geographies. In some cases, there may be biases towards hard structures. [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
9038	4	88	14			What is EBS? This would need an introduction. Or is it a typo from EBM? [APECS Group Review, Germany]	Accepted - Text revised
15064	4	88	19	88	19	of these hybrid solution are very recent. There are few data on their effectiveness or on the costs-benefits [Christophe Deissenberg, Luxembourg]	Accepted - Text revised
21842	4	88	26	88	27	Able to keep pace in a moodel -- do we trust the model -- at least add "in a model". More generally as many people are advocating relatively untried EBA approaches one needs to be cautious about endorsing particular measures and thinking how we establish experiments to learn about these methods. [Robert Nicholls, UK]	Accepted - Text revised
3816	4	88	30	88	30	change to "Only the retreat.." [Ola Kalen, Sweden]	Accepted - Text revised
24658	4	89	0			A simple question: How high can dikes be built to protect vulnerable human populations sustainably, e.g. in the Netherlands, and why? When are these and other adaptation limits reached? [Hans-Otto Poertner and WGII TSU, Germany]	Accepted - we now answer this question in Section “ 4.4.3.1.3 Effectiveness of hard protection
24042	4	89	2			Suggest to reduce the length of this section, e.g., regarding introduction and explanations of approaches. Maybe they can be compared/synthesized in tables? [Hans-Otto Poertner and WGII TSU, Germany]	Accepted – text revised, table included
9040	4	89	5	89	5	replace “gave” by “presented” [APECS Group Review, Germany]	Accepted – text revised
15066	4	89	20	89	20	Decision analysis does not posit that there are purely objective ways of making decisions. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15068	4	89	24	89	24	decision making approaches, with formal decision analysis being but one approach to inform [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15070	4	89	25	89	28	complex and involve long term investments, as it is frequently the case in coastal context. But it can or should be embedded in a social process that ensures social needs and objectives are properly accounted for. See also Sections 4.4.5.1 and 4.4.5.2. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
21844	4	89	26			define long term [Robert Nicholls, UK]	Taken into account - “long term” has been removed.
9042	4	89	30	8	30	remove “Generally” [APECS Group Review, Germany]	Accepted – text revised
15072	4	89	30	89	33	The analysis of adaptation options should consider all available knowledge, including all major uncertainties, ambiguities in expert opinions, and differences in approaches. A partial consideration of uncertainty and ambiguity may misguide the choice of adaptation options (Renn, 2008; Jones, 2014; Hinkel, 2016). [Christophe Deissenberg, Luxembourg]	Accepted – text revised
21846	4	89	33	89	37	I think this is saying consider all uncertainites and what they comprise? I think the non-climate factors could be emphasised more. [Robert Nicholls, UK]	Accepted – text revised

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15074	4	89	40	89	42	THE STATEMENT MAY BE CCEPTABLE FOR THE PURPOSE OF THE REPORT. HOWEVER, IT IS INEXACT. THE GOALS OF A CBA ARE TO FIND OUT BY HOW MUCH THEBENEFITS OF AN INVESTMENT EXCEED ITS COSTS, AND THUS TO ALLOW A RANKING OF INVESTMENT ALTERNATIVES. [Christophe Deissenberg, Luxembourg]	Taken into account - sentence was dropped
9044	4	89	40			Is CBA the abbrevation for community-based adaptation like on page 82 or cost-benefit analysis. Make it consistent [APECS Group Review, Germany]	Accepted – abbreviation is not used any more
9046	4	89	41	89	41	What is NPV? [APECS Group Review, Germany]	Accepted – abbreviation is explained
15078	4	89	44	89	45	I AN NOT SURE I UNDERSTAND. YOU STATE THAT "Without SLR, the application is straight forward." AND THEN EXPLAIN HOW IT CAN BE APPLIED IN THE CASE OF A SLR! I THINK THAT YOU WANT TO SAY THAT: "IF ONE DISCARDS THE FACT THAT THE POLICY-MAKERS MAY CHOOSE DIFFERENT EMISSION POLICIES AND THUS DIFFERENT SLR FUTURES, A CBA ANALYSIS WOULD IMPLY THE FOLLOWING STEPS". [Christophe Deissenberg, Luxembourg]	Taken into account - sentence was dropped
15076	4	89	49	89	49	highest expected NPV [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15080	4	89	57	89	57	RCP8.5, a more rapid [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9048	4	90	0	90		Figure 4.14: Lower panel: add a title to the y and x-axis (latitude, longitude). Add a title to the timeline (year). [APECS Group Review, Germany]	Accepted - Figure revised
9050	4	90	0	90		Figure 4.14: Upper panels: add a title to the x-axis (years) of the left and middle panels. [APECS Group Review, Germany]	Accepted - Figure revised
9052	4	90	0	90		Figure 4.14: It would be interesting to know why these cities were chosen among others to highlight the scenario? This can be clarified in the caption. [APECS Group Review, Germany]	Accepted - Figure revised
15082	4	90	1	90	5	Over a sufficiently short time horizon, the SLR does not differ much under both scenarios and CBA can be applied as previously described. What “sufficiently short” means practically depends on the location, because both the relative and the extreme sea level rise varies from place to place as discussed in Section 4.2.3.4.1, see Figure 4.9. In locations where the variability in the tide gauge record is large with respect to the relative sea level rise it takes longer before the differences in the annual maxima sea level under different scenarios becomes apparent [Christophe Deissenberg, Luxembourg]	Accepted – text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15084	4	90	1	90	5	I AM NOT CONVINCED THAT I UNDERSTAND WELL. WHAT YOU APPEAR TO SAY IS THAT AT A LOCATION WITH A LOT OF SHORT-TERM VARIABILITY IT WILL BE DIFFI-CULT TO DIFFERENTIATE BETWEEN SLR AND SHORT-TERM EVENTS ON THE BASIS OF OBSERVATIONS. IN OTHER WORS IT IS DIFFICULT TO ESTIMATE A NOISY TREND WHEN IT IS OVERLAPPED BY A LOT OF NOISE. BUT FUTURE REAL TIME OBSERVA-TIONS PLAY NO ROLE IN THE CBA, WHICH IS DONE EX ANTE ON THE BASIS OF A PRE-DICTION OF FUTURE LOCAL SLR. THIS LIKELY CONFUSION IS ALSO APPARENT IN THE SENTENCE “it takes longer before the difference between scenarios becomes apparent in the annual maxima sea level.”. THE DIFFERENCE WILL NEVER BECOME APPARENT BECAUSE HIS-TORICALLY ONLY ONE SCENARIO WILL BE REALIZED. EX ANTE, NONETHELESS, ONE CAN FROM THE ONSET COMPUTE THE EXPECTED DIFFERENCES BETWEEN LOCAL SLR UNDER DIFFERENT SCENARIOS SCENARIOS. THUS I SUGGEST DELETING THE WHOLE ARGUMENTATION – OR AT LEAST REFORMULATING IN A PROPER WAY. [Christophe Deissenberg, Luxembourg]	Taken into account - This is a misunderstanding of our text and we improved the text in order to make clear that we are not speaking about “future real-time observations” but only about the projections provided in this report.
3186	4	90	4	90	7	This seems like a very trigger-sensitive measure; it implies the 'year of divergence' is one in which there is a 90% chance the RCP 2.6 and RCP 8.5 scenarios are indistinguishable. [Robert Kopp, USA]	Taken into account - we clarify that the 90% is just a subjective example and provide some motivation for this number.
15086	4	90	5	90	11	WE HAVE THE SAME PROBLEM HERE. YOU USE AN ARBITRARY CRITERIUM, 90% DIVERGENCE TO SAY “AS LONG AS WE STAY BELOW 90% WE DO NOT NEED TO DIFFERENTIATE”. THE ARGUMENT IS PURELY HEURISTIC. IT MAY CORRESPOND TO WHAT IS DONE IN PRACTICE FOR SIMPLICITY’S SAKE BUT HAS NO SCIENTIFIC FOUNDATION. I WOULD REFORMULATE SAYING FOR EXAMPLE: “FOR SIMPLICITY SAKE WE MAY IN PRACTICE NEGLECT THE DIVERGENCE AS LONG AS IT DOES NOT EXCEED SOME TRESHOLD, SAY, 90% DIVERGENCE. LOOKING AT FIGURE 4.14, ONE RECOGNIZES THAT IT WILL BE THE CASE FOR ….ETC.” (It WOULD BE GOOD BUT NOT IMPERATIVE TO DEFINE WHAT YOU MEAN EXACTLY UNDER 90% DIVERGENCE.) “USING THIS CRITERION IMPLIES THAT WE NEED TO DEFINE PROBABILITIES FOR THE OCCURRENCE OF EACH SCENARIO AFTER THE YEARS OF SCENARIO DIVERGENCE.” BUT WE HAVE AN ADDITIONAL PROBLEM: SHOULD WE BE USING SCENARIOS PROBABILITIES FROM THE YEARS OF LOCAL DIVERGENCE (DIFFERENT PROBABILITIES FOR EACH SITE), FROM THE YEAR WHERE DIVERGENCE FIRST OCCUR AT ANY SITE, OR FROM YEAR 0? THE LATTER SEEMS THE CORRECT ONE, SINCE ANYWAY THE RESULTS IN EARLIER YEARS ARE BY ASSUMPTION ALMOST INDEPENDENT OF THE CHOSEN SCENARIO PROABILITIES. IN OTHER WORDS, THE WHOLE ARGUMENTATION IS LARGELY VOID IN THE CONTEXT OF CBA. WHAT REMAINS IS A CUTE REMARK THAT SPEED WITH WHICH THE IMPACT OF DIFFERENT SCENARIOS DOMINATES THE BACKGROUND NOISE DIFFERS FROM LOCATION TO LOCATION. [Christophe Deissenberg, Luxembourg]	Taken into account - This is a misunderstanding of our text and we improved the text in order to avoid this misunderstanding.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
21848	4	90	13	90	20	Figure 4.14 is good for scenario divergence and links back to my earlier comments about when scenarios separate. This is new and needs to be intergated into the chapter, including cross-referencing. [Robert Nicholls, UK]	Accepted – text revised
21852	4	90	13	90	20	How would we see divergence in real data rather than modelled scenarios -- it is harder to see divergence when the typical mean sea-level rise variance is included (Haigh et al., 2014). Presumably even harder in extremes data. So how does this compare with the real world? Reference: Haigh, I. D., Wahl, T., Rohling, E. J., Price, R. M., Pattiaratchi, C. B., Calafat, F. M., & Dangendorf, S. (2014). Timescales for detecting a significant acceleration in sea level rise. Nature Communications, 5, 3635. DOI: 10.1038/ncomms4635 [Robert Nicholls, UK]	Accepted - Indeed these tow issues are related.
10720	4	90	15	90	19	Information about Arctic is missed. Please add - this map is very important [Oxana Lipka, Russian Federation]	Rejected – we can only show tide gauge stations for which sufficient data is available
21850	4	90	17			Why 90%? Presumably this is being drafted for a journal article which can be cited? [Robert Nicholls, UK]	Taken into account - we clarify that the 90% is just a subjective example and provide some motivation for this number.
2680	4	90	42	90	48	The probabilistic ranges (5 & 95%) in this chart, how were used to calculate the sea level? [Mohammad Javad Zareian, Iran]	Accepted – text revised
15088	4	91	1	91	4	The main argument for assigning probabilities is that otherwise the decision-makers are likely to assume that all possible scenarios are equally likely (Schneider, 2001). Arguments against include that the space of possible future emissions is insufficiently sampled by any number of scenarios, and that a significant disagreement on the probabilities of the different scenarios is likely (Lempert, 2001; Stirling, 2010). In the SLR literature only very studies did assign probabilities to emission scenarios (Woodward, 2014; Abadie, 2018). There is high agreement not to do so and hence not to apply utility optimisation methods such as CBA. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
13684	4	91	1	93	1	Replace 'with' with 'which' [Debra Roberts and Durban Team, South Africa]	Accepted – text revised
21854	4	91	1	108	1	Sources for adaptation pathways are overly dominated by the work of Haasnott and her coworkers. At the Adaptation Futures conference in South Africa there were a number of groups talking about adaptation pathways in coastal zones and I would suggest the literature base be expanded to include those sources from Australia and New Zealand in particular. [Robert Nicholls, UK]	Accepted - references from Australia were added.
9054	4	91	3	91	3	“Generally there is high agreement not to do so” it would be good to give the reason why this is not recommended. [APECS Group Review, Germany]	Accepted – text revised
15090	4	91	4	91	4	There is, however, an extensive literature [Christophe Deissenberg, Luxembourg]	Accepted – text revised
24044	4	91	6	91	7	Provide references for ‘extensive literature’ [Hans-Otto Poertner and WGII TSU, Germany]	Accepted – text revised
9056	4	91	12	91	12	Would be interesting to add a table with locations where the scenario-based CBA has been used. [APECS Group Review, Germany]	Rejected - this would indeed be nice, but this would be a major research undertaking
15092	4	91	12	91	12	(Scussolini, 2017), and many other locations. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9058	4	91	13	91	13	“the problem faced by coastal decision makers” remove “is facing” [APECS Group Review, Germany]	Accepted – text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15094	4	91	15	91	15	be used as inputs in the robust and [Christophe Deissenberg, Luxembourg]	Accepted – text revised
24952	4	91	18	91	56	There is considerable movement of communities to remove critical infrastructure in advance of flooding events. As an example, NYC removed electrical boxes from subway tunnels prior to flooding from Sandy. These responses could possibly belong here. [Elizabeth Weatherhead, USA]	Rejected - outside the scope of the section. Disaster preparedness options are treated in the accomodation section
13686	4	91	19	91	27	Concepts introduced could do with brief explanation eg regret, reversibility, security margins, minimax, info gap theory. [Debra Roberts and Durban Team, South Africa]	Accepted - Text revised
9060	4	91	20	91	20	what do you mean by “states-of-the-world”? Does that refer to SLR scenarios? [APECS Group Review, Germany]	Accepted – concept is explained in the introductory sectiond
15096	4	91	20	91	20	are ‘robust’) under a wide [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15098	4	91	21	91	21	to create a large ensemble [Christophe Deissenberg, Luxembourg]	Accepted – text revised
10750	4	91	25	91	25	Please add Hallegate (2009) in the References section! [Jacques Andre Ndione, Senegal]	Accepted – text revised
15100	4	91	25	91	25	(Hallegatte, 2009). RDM also I WOULD ARGUE THAT MINMAX APPROACHES EG ARE FUNDAMENTAL IN RDM [Christophe Deissenberg, Luxembourg]	Rejected - there are different usages of the term RDM in the literature. Our text illustrates two alternative ones (wide and narrow sense)
19118	4	91	29	91	29	particularly [Anna Zivian, USA]	Accepted – text revised
15102	4	91	29	91	37	RDM approaches are suitable for coastal adaptation decision making for two reasons. First, they address the problem of deep uncertainty, that is the situation where no probability distribution can be attached to states-of-the-world (Lempert, 2001; Weaver, 2013), which is the case for longer term SLR decision making as discussed above. Second, even if a probability distribution is available, they may be preferred over expected utility maximization if the decision makers are risk or uncertainty averse, which is frequently the case in a coastal context (Hinkel, 2015). Nonetheless, few applications are to be found in the literature. Brekelmans (2012) minimize the average and maximum regret across a range of SLR scenarios for dike rings; Lempert (2013) apply RDM to Hoh-Chi-Minh City. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15104	4	91	29	91	37	RDM approaches are suitable for coastal adaptation decision making for two reasons. First, they address the problem of deep uncertainty, that is the situation where no probability distribution can meaningfully be attached to states-of-the-world (Lempert, 2001; Weaver, 2013), which is the case for longer term SLR decision making as discussed above. Second, even if a probability distribution is available, they may be preferred over expected utility maximization if the decision makers are risk or uncertainty averse, which is frequently the case in a coastal context (Hinkel, 2015). Nonetheless, few applications are to be found in the literature. Brekelmans (2012) minimize the average and maximum regret across a range of SLR scenarios for dike rings; Lempert (2013) apply RDM to Hoh-Chi-Minh City. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9062	4	91	30	91	30	remove “that is”, replace the following “that” by “for which” [APECS Group Review, Germany]	Accepted – text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
9064	4	91	31	91	31	what do you mean by “states-of-the-world”? Does that refer to SLR scenarios? [APECS Group Review, Germany]	Accepted – definition included in the introduction of this subsection
9066	4	91	48	91	48	“The adaptation pathways” [APECS Group Review, Germany]	Accepted – text revised
9068	4	91	49	91	53	This information should be kept but placed in the legend of Figure 4.15. Instead you should sum up the concept in a short text here. [APECS Group Review, Germany]	Taken into account - we dropped Figure 4.15
21856	4	91	49			Suggest referencing Ranger et al (2013) as it talks well about London experience with adaptation pathways. [Robert Nicholls, UK]	Rejected - the London experience was already featured in a box in AR5
15106	4	91	50	91	51	information on the time until when options are effective (these times are called adaption tipping point) [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15108	4	91	52	91	53	A complete adaptation pathways plan thus suggest policy actions for the short to medium term within a longer-term pathway. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15110	4	91	55	91	55	may trigger a decision [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9080	4	92	0	92		Figure 4.15: Improve the resolution of the figure. Elaborate the caption with the information from the text (see comment above) [APECS Group Review, Germany]	Taken into account - we dropped Figure 4.15
15112	4	92	6	92	7	because it shows decision makers that several possible pathways lead to a same desired future (Haasnoot, 2013; [Christophe Deissenberg, Luxembourg]	Accepted – text revised
19120	4	92	10	92	14	this figure is very complicated and too abstract. The text itself is much clearer. Adjust or remove the figure [Anna Zivian, USA]	Accepted – figure has been removed
15114	4	92	15	92	20	While adaptation pathways analysis centres on the pathways’ flexibility, it cannot answer the questions of an economically efficient flexibility and of the timing of adaptation. Delaying decisions and opting for flexible measures may introduce extra costs, because flexible measures are often more expensive than inflexible ones and flood damages may occur whilst delaying the decision. An important consideration therefore is to balance the cost of delaying decisions with the benefits of deciding later when having more information at hand. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9070	4	92	18	92	18	remove “flood”, this statement here seem to be general, not only for floods. [APECS Group Review, Germany]	Accepted – text revised
15116	4	92	20	92	23	YOU MIGHT WANT TO ADD TO THE LIST OPTIMAL DUAL CONTROL, e.g. Wittenmark, B. "Adaptive Dual Control Methods: An Overview". Lund University. CiteSeerX 10.1.1.25.7446 [Christophe Deissenberg, Luxembourg]	Rejected - dual control methods are certainly interesting but we can not enumerate all methods and we are not aware of sea-level applications of this method.
9072	4	92	25	92	26	this sentence does not bring much information [APECS Group Review, Germany]	Rejected - this sentence is essential
9074	4	92	26	92	26	replace “apply” with “applied” [APECS Group Review, Germany]	Accepted – text revised
9076	4	92	35	92	35	Which categories are you referring to? [APECS Group Review, Germany]	Accepted – text revised
9078	4	92	35	92	35	correct “in adaptation” [APECS Group Review, Germany]	Accepted – text revised
15118	4	92	35	92	35	Furthermore, approaches can be combined. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15120	4	92	36	92	36	options can be [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9082	4	93	7	93	7	remove “Furthermore” [APECS Group Review, Germany]	Accepted – text revised

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15122	4	93	7	93	12	There is wide agreement that next to making robust and flexible decision today, decision making should be robust and flexible, and include a monitoring and evaluation of options and sea level variables in order to learn from past decisions and collect information to inform future decisions (Haasnoot, 2013; Barnett, et al., 2014; Burch, et al., 2014; Wise, et al., 2014; Kelly, 2015; Lawrence and Haasnoot, 2017). The existence of a monitoring strategy that helps to identify needed shifts in policy sufficiently early is of utmost importance (Hermans, et al., 2017). [Christophe Deissenberg, Luxembourg]	Accepted – text revised
15124	4	93	7	93	12	SINCE THESE LINES APPLY TO ALL METHODS, INCLUDING CBA, I SUGGEST PUTTING THEM AFTER LINE 38 p. 89. NOTE YOU COULD ALSO MENTION THAT THE MONITORING AND UPDATING PROCESS CAN BE AD HOC OR CAN BE BUILT IN, BASED ON SCIENTIFIC ANALYSIS, IN THE METOD ITSELF. [Christophe Deissenberg, Luxembourg]	Accepted – text revised
9084	4	93	12	93	12	this sentence is unclear [APECS Group Review, Germany]	Accepted – text revised
9086	4	93	14	93	14	production is not the right term here. Change to “collect” [APECS Group Review, Germany]	Accepted – text revised
10752	4	93	27	93	27	Please add Hallegate (2012) in the References section! [Jacques Andre Ndione, Senegal]	Accepted – text revised
21860	4	93	35	99	28	Too long a section on CBA and in parts no references at all -- where is this material coming from? If there is no literature then this should be stated to stimulate research, and avoid the authors driving the content. [Robert Nicholls, UK]	Text shortened and sources of material are now clearly referenced; there is ample literature on the topic
9088	4	93	42	93	42	Review the whole section to clarify it and be more straightforward. There are NO references in this paragraph. I suggest to write the section with bullet points or to bold the first [APECS Group Review, Germany]	The section has been revised, in line with the helpful comments provided
9090	4	93	43	93	43	Remove “reflection on” in the title [APECS Group Review, Germany]	Done
24046	4	93	43			Suggest to reduce length of this section and concentrate more on new knowledge post-AR5. Also, add references. [Hans-Otto Poertner and WGII TSU, Germany]	This advice has been followed for the SOD
24048	4	93	48	93	48	Use CbA as abbrevation [Hans-Otto Poertner and WGII TSU, Germany]	This advice has been followed for the SOD
9092	4	93	49	93	49	“a few decades or so” and “now” does not provide any time constrain. Both terms are relative in time. Please provide dates. [APECS Group Review, Germany]	More specific timing is indicated
15126	4	93	56	93	56	of existing or emerging developments [Christophe Deissenberg, Luxembourg]	The threats are more than those related to "development"; revised to read "a range of other existing or emerging pressures".
9114	4	94	0	94		REFERENCES ARE MISSING [APECS Group Review, Germany]	As noted, the summary is derived from AR5 and SR1.5. This will be made clearer
9094	4	94	3	94	3	“Where”: in which country? City? You should provide the geographic level at which these activities where implemented. [APECS Group Review, Germany]	The geographic specificity has been improved
9096	4	94	24	94	24	Remove “But” replace by “however” [APECS Group Review, Germany]	The suggested chenge has been made
9098	4	94	28	94	28	remove line break [APECS Group Review, Germany]	The suggested chenge has been made
9100	4	94	31	94	31	Remove “But” replace by “however” [APECS Group Review, Germany]	The suggested chenge has been made
9102	4	94	36	94	39	Review the whole sentence. As such it is not clear what you are referring to. [APECS Group Review, Germany]	The sentence has been revised to improve clarity
9104	4	94	42	94	42	change the ned of the first sentence to “increasingly be used/implemented” [APECS Group Review, Germany]	The suggested chenge has been made
9106	4	94	45	94	45	remove “an” and add a S to “optionS” [APECS Group Review, Germany]	The suggested chenge is not consistent with the meaning of the statement

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
9108	4	94	47	94	48	This does not seem right. Are equity and gender a type of community participation? If not then the first part of the sentence does not match the second one. [APECS Group Review, Germany]	Addressing gender and equity considerations are an important aspect of community engagement. No change made.
9110	4	94	47	94	57	There are many repetition between both paragraphs. You can try to shorten these two. [APECS Group Review, Germany]	The repitition has been removed.
15128	4	94	50	94	50	and may have different preferences [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made
9112	4	94	52	94	52	Remove “Fundamentally”. What do you mean by “voluntary actions”? Are these free? [APECS Group Review, Germany]	The suggested chenge has been made; voluntary actions are those undertaken without be oblidged to do so
15130	4	94	52	94	52	Voluntary actions [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made
15132	4	94	57	94	57	are needed. To achieve [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made
9126	4	95	0	95		REFERENCES ARE MISSING [APECS Group Review, Germany]	Text shortened and sources of material are now clearly referenced; there is ample literature on the topic
9116	4	95	4	95	4	What do you mean by “mainstream”? this doesn’t seem to be the right term. Replace “grounded” by “based” [APECS Group Review, Germany]	"mainstream" is a commonly used and accepted term; it means, in part, to integrate. "Grounded" has been changed to reflected
9118	4	95	5	95	5	“isolation”, do you mean alone? [APECS Group Review, Germany]	The suggested chenge has been made
9120	4	95	9	95	9	“often” does not bring any information here. [APECS Group Review, Germany]	The suggested chenge has been made
15134	4	95	9	95	9	principles of equity and justice, and to ensure access [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made
9122	4	95	11	95	11	break the sentence after “governance regimes” Start a new sentence at “The more priviledge” [APECS Group Review, Germany]	The suggested chenge has been made
9124	4	95	11	95	14	review the whole sentence, it is unclear. [APECS Group Review, Germany]	The sentence has been revised to improve clarity
17268	4	95	46	95	46	Replace "Capacities" with "Capabilities" (see Article 3.1 of the Convention) [Iulian Florin Vladu, Germany]	The suggested chenge has been made
10754	4	95	46	95	51	Defending transformative adaptation today is a complusory! [Jacques Andre Ndione, Senegal]	The intent of this comment is unclear; please review and comment on the SOD
21406	4	95	46	95	51	“Common by Differentiated Responsibilities and Respective Capacities is not a scientific term, and is not defined by the UNFCCC. These sentences should be removed. [Alice Alpert, USA]	Common but Differentiated Responsibilities and Respective Capabilities (CBDR–RC) is a principle within the United Nations Framework Convention on Climate Change (UNFCCC) that acknowledges the different capabilities and differing responsibilities of individual countries in addressing climate change.
3188	4	95	53	95	54	CBA is used as an acronym with multiple meanings in the text. [Robert Kopp, USA]	Now using Cba
15138	4	96	4	96	4	Garschagen (submitted) THIS IS NOT A PROPER REFERENCE!!! [Christophe Deissenberg, Luxembourg]	The reference will be updated for the SOD and the final draft report
19122	4	96	5	96	17	cite Arnstein, Sherry R. "A ladder of citizen participation." Journal of the American Institute of planners 35.4 (1969): 216-224. [Anna Zivian, USA]	This reference is very old and has been reflected in earlier assessments
19124	4	96	5	96	17	a diagram here would be useful [Anna Zivian, USA]	Agree, but unfortunately space (including space for diagrams) is very limited
9128	4	96	20	96	20	The reference HAQUE should not be capitalized. [APECS Group Review, Germany]	The reference will be updated for the SOD and the final draft report

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15136	4	96	28	96	33	The warning (based on a review of community-based adaptation research in the Canadian Arctic) of Ford et al. (2016b) against assuming that research has a positive role to play in community adaptation just because it utilizes participatory approaches has wide implications: Participation in CB research can perpetuate the privilege of Western knowledge over local values and indigenous knowledge and can further marginalize communities if power relations are not addressed. Moreover, as CB does not necessarily prevent maladaptation. [Christophe Deissenberg, Luxembourg]	The sentence has been revised to improve clarity
24050	4	96	35	96	46	Add references. [Hans-Otto Poertner and WGII TSU, Germany]	Text shortened and sources of material are now clearly referenced; there is ample literature on the topic
19126	4	97	1	97	2	Cite Tribbia, John, and Susanne C. Moser. "More than information: what coastal managers need to plan for climate change." Environmental science & policy 11.4 (2008): 315-328. [Anna Zivian, USA]	This is a dated (i.e. pre AR5) reference
15140	4	97	33	97	33	provided different outcomes than [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made
23298	4	97	48	97	54	I recommend authors to mention urbanization as process to strongly connect with the migration processes, especially to slums and favelas without appropriate logistics, hyginene, water and electricity access, etc. and with high criminality. [Robert Stojanov, Czech Republic]	Unfortunately, space limitations do not allow us to go into this level of detail. It is covered within by" social and economic conditions"
9130	4	98	2	98	2	Check the reference: is it Gorddart or Goddart (L8)? [APECS Group Review, Germany]	Thank you for noting the error.
9132	4	98	44	98	44	You mean PAR projects? [APECS Group Review, Germany]	Thank you for noting the error.
19128	4	98	44	98	44	PAR not PRA [Anna Zivian, USA]	Thank you for noting the error.
24052	4	98	44	98	46	Add reference [Hans-Otto Poertner and WGII TSU, Germany]	Text shortened and sources of material are now clearly referenced
23190	4	98	44	98	54	Regarding PRA projects in New Brunswick and Québec, good references are also: Guillemot, J., M. Aubé, 2015. L'adaptation aux changements climatiques dans la Péninsule acadienne: rôles d'acteurs clés dans l'émergence d'un dialogue articulé à l'échelle régionale. VertigO, Hors-série 23, DOI: 10.4000/vertigo.16664. Plante, S., O. Chouinard, G. Martin, 2011, Gouvernance participative par l'engagement citoyen à l'heure des changements climatiques: Études de cas à Le Goulet, Pointe-du-Chêne et Bayshore Drive (Nouveau-Brunswick), Territoire en mouvement, Volume 11, DOI: 10.4000/tem.1234. Weissenberger, S., O. Chouinard,2015. Adaptation to Climate Change and Sea Level Rise - The Case Study of Coastal Communities in New Brunswick, Canada. Springer Netherlands, VI, 100 p. [Sebastian Weissenberger, Canada]	Thank you for including these suggestions. The post AR5 references will be included, as space allows
15142	4	98	53	98	53	remains highly [Christophe Deissenberg, Luxembourg]	The suggested chenge has been made

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
16438	4	99	14	99	36	This section, which is crucial, is based on Golden et al. 2016. However, there are others papers that do not take the same approach: Merino et al. 2012 Glob. Env. Change; Barange et al. 2014, etc., for a number of reasons. Even ststaments in this chapter in page 77 (a potential catch decline ~10% by 2050) is not consistent with statements (and the numbers!) cited in this section. There are also significant publications from the FAO to both show the dependency of fish for nutrition and food security, and the relative risks of climate change (compared to other stressors). This section overlooks a large volume of evidence. [Coswig Kalikoski Daniela, Italy]	This comment likely relates to Ch 5, not Ch 4
15144	4	99	16	99	16	formulating adaptation [Christophe Deissenberg, Luxembourg]	Accepted
15146	4	99	24	99	26	assessed by incorporating adaptation pathways in the Dutch Delta Programme. [Christophe Deissenberg, Luxembourg]	Editorial comment noted and to be taken into account in finalising SROC
15148	4	99	38	99	39	Adaptating to a new climate state, instead of adaptating to ongoing rates [Christophe Deissenberg, Luxembourg]	Editorial comment noted and to be taken into account in finalising SROC
10756	4	99	56	99	57	is it possible to insertin this section the framework develmoped by Stephens et al. (2017) [Jacques Andre Ndione, Senegal]	Noted. To be addressed in revisions to section post-SOD.
9134	4	100	3	100	3	remove line break [APECS Group Review, Germany]	Accepted.
9136	4	100	6	100	6	remove line break [APECS Group Review, Germany]	Accepted.
9138	4	100	11	100	11	remove line break [APECS Group Review, Germany]	Accepted.
9140	4	100	12	100	12	this sentence does not make sense to me, please rewrite. [APECS Group Review, Germany]	This section to be revised post-SOD.
9142	4	100	22	100	22	I think you mean long-term instead of “short-term (approximately 20 years)” [APECS Group Review, Germany]	This section to be revised post-SOD.
9144	4	100	28	100	28	for consistency with the second part of the sentence, rewrite “Bergen on the west coast of Norway (…)” [APECS Group Review, Germany]	This section to be revised post-SOD.
9146	4	100	31	100	31	replace “statistical formulations” by “statistical analyses”. [APECS Group Review, Germany]	This section to be revised post-SOD.
15150	4	100	32	100	32	into account can reduce the projected median damage costs by an order of magnitude. [Christophe Deissenberg, Luxembourg]	This section to be revised post-SOD.
15152	4	100	42	100	44	As a result, a robust strategy, i.e., one that performs well over a wide range of plausible futures/views, may be the most appropriate alternative. [Christophe Deissenberg, Luxembourg]	This section to be revised post-SOD.
9148	4	100	49	100	49	Make sure that “adaptation pathways” need a S. It is not consistent throughout the chapter, especially in section 4.4.5.3.4 [APECS Group Review, Germany]	Noted and editorial changes to be made before publication.
15154	4	100	49	100	49	The call for monitoring and evaluating adaptation pathways [Christophe Deissenberg, Luxembourg]	Noted and editorial changes to be made before publication.
15156	4	100	49	100	50	from the assumption that the adaptation pathways are being implemented and help identify [Christophe Deissenberg, Luxembourg]	Noted and editorial changes to be made before publication.
15158	4	100	51	100	52	However, Wise et al. (2014) suggest that adaptation plans are often either not implemented at all or that only the smaller incremental measures are. [Christophe Deissenberg, Luxembourg]	Editorial comments noted and to be taken into account for publication.
23122	4	100	52			adaptation plans are not the priority for politics. They need to be much more sensibilised and trained to those issues. Ther is also a gap between national regualtion and local "strategies" and plans [Jacques Beall, France]	Noted. To be addressed in revisions to section post-SOD.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15160	4	100	53	100	55	van der Brugge and Roosjen (2015) explain that the implementation and effectiveness of adaptation pathways might require changes in institutional and socio-cultural structures. ???? THE ORIGINAL SENTENCE IS VERY DIFFICULT TO MEANINGFULLY INTERPRET [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
13688	4	101	1	101	1	Insert 'of' after 'think' [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
9150	4	101	2	101	7	rewrite. This part is confusing and hard to read. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9152	4	101	2	101	7	This sentence is hard to read and understant. Rewriting could help [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15162	4	101	3	101	3	in a dynamic framework (Brown et al., 2014), [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
9154	4	101	3	101	6	Be consistent with the numbering. Now it is 1, 2, 3, iv. [APECS Group Review, Germany]	Accepted. Sections renumbered.
9156	4	101	7	101	7	remove quotes for “adaptation pathway” as it isn’t used with quotes earlier in the chapter. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9158	4	101	8	101	8	remove quotes for “cautious and staged implementation” [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9160	4	101	12	101	12	why do you provide a page number to this reference? Other references in the chapter don’t have page numbers. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15164	4	101	13	101	14	a real potential for a better integration of SLR and gradual changes more broadly. ???? [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
23124	4	101	17			You should give an example with the "loi littoral" in France, régulation about the costline, which was set up to avoid constructions close to the sea. It was not made for adaptation, but it's useful today. This law is threatened very often by different lobbies to make exceptions, but now with SLR it's a unique tool to go forward. [Jacques Beall, France]	Noted. To be addressed in revisions to section post-SOD.
24054	4	101	19			Suggest to concentrate only on SLR-related findings and reduce conceptual sections. The table and figure are already a good approach, and the explanatory text can be shortened accordingly. [Hans-Otto Poertner and WGII TSU, Germany]	Noted. To be addressed in revisions to section post-SOD.
9162	4	101	22	101	22	“changes” plural, add S. “Those”, which are those? Be more precise. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9164	4	101	25	101	25	remove “and the need to do so”. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15166	4	101	25	101	25	dealing with uncertainty; [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
9166	4	101	26	101	28	This part should be moved to the beginning of the paragraph. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15168	4	101	28	101	28	(Lawrence and Haasnoot, 2017). [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
21862	4	101	30	101	52	Why is transformation and transformative adaptation being discussed? I thnk because the challenge demands it? But there is little argument and this is coming across a little bit like a text book rather than an IPCC assessment. Also need to cross-reference, including to other relevant chapters in the Special Report. [Robert Nicholls, UK]	Noted. To be addressed in revisions to section post-SOD.
9168	4	101	31	101	31	“discuss the emerging literature” this is a blurry term. Is that a review? Then use the word review. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24954	4	101	31	102	6	This entire section seems to be based on one paper (Burch et al., 2017. Either shorten or add additional information. This section seems like advocacy, so perhaps shortening would be better. [Elizabeth Weatherhead, USA]	Noted. To be addressed in revisions to section post-SOD.
15170	4	101	33	101	33	which suggest [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
10762	4	101	52	102	6	It would be benefit to merge this section (4.4.6.3.3) and the former one (4.4.6.3.2) in a single section… [Jacques Andre Ndione, Senegal]	Noted. To be addressed in revisions to section post-SOD.
9170	4	101	53	101	53	remove “were”. Add “point” after “tipping”. Add a S to “towards” [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
13690	4	101	53	101	53	Delete 'were' after 'Burch et al. (2017)'. Alternatively, rephrase as 'Burch et al. (2017) also discussed…' [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
13692	4	101	53	101	53	Provide page number of quotation [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
15172	4	101	53	101	53	Burch et al. (2017) discuss the tipping [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
13694	4	101	54	101	54	End the sentence with a full stop. [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
9172	4	101	55	101	56	“This is to capturing (…)” irrelevant sentence in this paragraph [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15174	4	101	55	101	56	change research to capture the broader trends and patterns in the South. [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
15176	4	101	56	101	56	They recognize that transformational approaches are [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
15178	4	102	1	102	1	adaptation, that is, the uncertainty about the severity of the climate impacts and the purported benefits of adaptation. [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
15180	4	102	1	102	4	This includes the uncertainty about the costs of transformation, which are often unknown but presumed to be high. The costs of incremental adaptation, on the other hand, can be calculated. Burch et al. (2017) also state the existence of numerous institutional barriers, ranging from cultural norms to existing complex legal [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
9174	4	102	3	102	6	Is that a quote? Then add quotation marks. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
15182	4	102	8	102	16	THIS JUST REPEATS LINES 48-56 p. 100 THAT I EDITED. [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
9176	4	102	8	102	38	This text does not address the subject mentioned in the title, it is rather a continuation of the previous paragraph. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9178	4	102	9	102	9	The term “stems” is not appropriate, I think you mean “stands” [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
13996	4	102	12			This is a repeat of the statement on pg 100 line 52 [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
3818	4	102	18	102	18	change to "Some analyses indicate" [Ola Kalen, Sweden]	Noted. To be addressed in revisions to section post-SOD.
15184	4	102	18	102	18	Some analyes indicate [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
13696	4	102	22	102	22	What does 'ICZM' mean? An acronym is unnecessary since it was used only once in the chapter. [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
15186	4	102	25	102	38	HERE ALSO YOU ARE REPEATING YOURSELF [Christophe Deissenberg, Luxembourg]	Noted. To be addressed in revisions to section post-SOD.
9180	4	102	27	102	27	Assign the Figure number, 4.16. Remove the quote to “Current situation” and give a precise date. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9182	4	102	29	102	29	remove quotes to “transfer station” [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9184	4	102	34	102	34	remove S to “present” [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
9186	4	102	41	102	41	close parenthesis after 2017. Remove (Table 1). [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
10760	4	102	41	102	42	Please delete "Table 1" after the reference Hermans et al, 2017 [Jacques Andre Ndione, Senegal]	Noted. To be addressed in revisions to section post-SOD.
9190	4	103	0	103		Table: Bad resolution [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
24956	4	103	1	108	19	I see case studies for a number of locations, but case studies for Charleston and Miami could help explain the variety of the issues associated with coastal flooding. [Elizabeth Weatherhead, USA]	Noted. To be addressed in revisions to section post-SOD.
13698	4	103	7	103	8	The sentence is not clear. Consider rephrasing. [Debra Roberts and Durban Team, South Africa]	Noted. To be addressed in revisions to section post-SOD.
9188	4	103	8	103	8	This can not be stated as such! Either explain here or remove the sentence. [APECS Group Review, Germany]	Noted. To be addressed in revisions to section post-SOD.
2734	4	103	17			Why a statement? Would be better if this is a conclusion on climate resilient development pathways. There should be a conclusion for the entire chapter linking up the statements in the summary : e.g. CC and SLR are inevitable….but require more information….various impacts….wide range of adaptation….developmement of adaptation pathways…..but not forgetting context-specifics. [Poh Poh Wong, Singapore]	Noted. To be addressed in revisions to section post-SOD.
9208	4	104	0	104		Figure 4.3: Improve resolution [APECS Group Review, Germany]	Resolution will be improved for the published report
9208	4	104	0	104		Figure 4.3: Improve resolution [APECS Group Review, Germany]	Resolution will be improved for publication of the SROCC
9192	4	104	1	105	26	Box 4.3 introduction suggests the aim of the Box is to describe responses to coastal hazard risks; but the Fiji example mostly describes the risks and observed impacts. Since the Chapter is very long, the paragraphs about increased flood risk (lns 27-32) and the cost of flooding (ln 39-4) can be shortened or omitted. [APECS Group Review, Germany]	Responses are given considerable space - see table and associated text. But length of box has been reduced, in line with comment
9194	4	104	1	107	32	There is no use of the IPCC uncertainty language on statements in this Box. If this is not deliberate, consider adding confidence statements about projections (e.g. lines 10-14 page 105 and lines 5-6 page 107). [APECS Group Review, Germany]	Revised projections will be included in the SOD, and these will include uncertainty
9196	4	104	1	107	32	The use of "return periods" rather than probability per annum. I feel that return periods can be mis-intepreted by members of the public and other non-specialised readers. It can be inferred as once an event happens you don't expect another to happen for xx-years, which is incorrect. I prefer an annual probability or expectation. [APECS Group Review, Germany]	Return period is used elsewhere in this Chapter and elsewhere in the SROCC; while there is a possibility of misinterpretation, and you suggest, such technical information is aimed at those who understand the term
6340	4	104	9	105	39	The choice of Nadi river Basin as a case study is an interesting one. As a resident of the town and having researched Nadi and Fiji for over 10 years, the phenomena of floods and environmental change is a combination of both 'climate'-related impacts and human induced impcats (agriculture, land clearing, poor land use planning). Line 15 should reflect this. See for example study of climate vulnerbility in Nadi - Chandra and Gaganis 2015 Climate and Development [Alvin Chandra, Australia]	This point is made later in the case study - "But the increased frequency of flooding is not all attributable to increases in sea level and extreme rainfall events. River channels have become filled with sediment, largely owing to deforestation of the hinterland. Much of the mangrove fringe has been sacrificed for development of various kinds. Like all river deltas, the one on which Nadi is located is subsiding." The reference will be added

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
24056	4	104	9			Be clear if the whole text and evidence of this case study is based on a single reference. Suggest to support the findings with further evidence. [Hans-Otto Poertner and WGII TSU, Germany]	This will be clarified in the SOD, including additional references
2204	4	104	10			Full reference should not be in text, just authors and year [Chandani Appadoo, Mauritius]	This will be corrected in the SOD
5170	4	104	16	104	16	Please note that the torrential rain induced by tropical cyclones can also contribute to coastal flooding [Sai Ming Lee, China]	This point is covered in the following text: "Tropical cyclones are particularly hazardous because of their potential to also elevate coastal sea levels due to storm surges and high waves. In addition to causing flooding of low-lying coastal terrain, higher coastal sea levels during a storm surge can slow the drainage of floodwater from coastal river systems to the ocean."
2206	4	104	24			colour coding in legends and in Figure, not very clear for Figure 1 in Box 4.3 [Chandani Appadoo, Mauritius]	Resolution and clarity will be improved for the published report
9198	4	104	27	104	27	"People and built assets in the Nadi River flood plain are already being affected by climate change." Provide a reference / Chapter section for an attribution study. [APECS Group Review, Germany]	A reference will be added
6342	4	104	27	104	32	This paragraph will benefit from references - see Chandra and Gaganis. 2015. Deconstructing vulnerability and adaptation in a coastal river basin ecosystem: a participatory analysis of flood risk in Nadi, Fiji Islands Climate and Development 8(3): 256-269 [Alvin Chandra, Australia]	The reference will be added
9200	4	104	28	104	28	Add “long-term increase”. Remove “but” and replace by “This increase”. [APECS Group Review, Germany]	"Increase" added twice; however, "But" is retained to emphasise the comment that follows.
9202	4	104	29	104	31	It would be preferable to replace "the return periods of extreme rainfall events have decreased" with "extreme rainfall events have become more likely" [APECS Group Review, Germany]	This change has been made
9204	4	104	30	104	30	It should be increase instead of decrease. [APECS Group Review, Germany]	Not in reference to return period, but note comment and response immediately above
6344	4	104	34	104	36	Nadi is a key entry and port for the Pacific Islands, and the River Basin and Coastal area has suffered from immense tourism development, particulalry the mangrove ecosystems. Line 34-35 could make reference to the multi-dimmesnional nature of climate and non-climatic impacts in SIDS, particulalry with Tourism (see Chandra and Gaganis 2015) [Alvin Chandra, Australia]	These points are covered in the body of Chapter 4
9206	4	104	36	104	37	"Like all river deltas … subsiding." A naturally-functioning river delta can accrete as well as subside. Consider emphasising the human-induced cause here, something like "Human-induced changes to the function of the river delta Nadi sits on mean parts of Nadi are subsiding and sinking relative to local sea level." [APECS Group Review, Germany]	Subsidence is not only due to "human-induced changes to the function of the river delta". "All" has been changed to "many".
9212	4	105	0	105		Table 1: colum 3: the title should be “Consistency with SROCC Assessment” [APECS Group Review, Germany]	Column 3 is not about "consistency". Column 3 shows how information and understanding changes when information consistent with the SROCC findings are considered.
9210	4	105	1	105	1	“resulting in 4 deaths”. [APECS Group Review, Germany]	Text changed to "including four fatalities".
20498	4	105	3			Perhaps show an approximate amount in a more widespread currency (such as USD, which is used elsewhere in the document) to give readers a sense of financial magnitude (FJ$ less than half USD). [Frank Oliva, Canada]	The changes have been made

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Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
6346	4	105	10	105	14	Reference the sea levels data - see Report by CSIRO on Fiji: Climate Variability, Extremes and Change in the Western Tropical Pacific: New Science and Updated Country Reports 2014 [Alvin Chandra, Australia]	This is covered by the references cited at the end of the para.
3190	4	105	21	105	23	"call for significant changes in the basis for the design and planning of the structural and related interventions" is a policy prescriptive statement. [Robert Kopp, USA]	Sentence rewritten accordingly.
9214	4	106	6	106	6	for consistency, move the end of the sentence to the beginning. “Shanghai is the economic center of China, located (…)” [APECS Group Review, Germany]	Accepted - text revised.
9216	4	106	11	106	11	Replace "largest historical flood event" with "largest recorded flood events" (Historical has different time scales for different people, and 'largest' can be defined in several ways including physical height or extent, damage or fatalities) [APECS Group Review, Germany]	Accepted
9218	4	106	26	106	26	remove “of”. Remove “again”. Replace “over” by “through” [APECS Group Review, Germany]	Accepted
9220	4	106	27	106	27	“in NYC”. Replace “The rate in Shanghai” [APECS Group Review, Germany]	Accepted - text revised.
9222	4	106	30	106	30	“mean rate of sea level rise” [APECS Group Review, Germany]	Accepted - text revised.
9224	4	106	31	106	31	replace “over” by “for the” [APECS Group Review, Germany]	Accepted
9226	4	106	33	106	35	what do you mean by exposure? Is it the impact? [APECS Group Review, Germany]	The sentence has been rephrased to clarify
9228	4	106	44	106	44	who is “we”? rewrite the whole sentence. [APECS Group Review, Germany]	Accepted - text revised.
11960	4	109	0			reference 1 should follow the same format of order references [Chukwuma Anoruo, Nigeria]	Accepted
10834	4	109	1	109	1	Surprising to see no reference in this chapter to the work of Stijn Temmerman (Antwerp) [Thomas Spencer, UK]	Accepted - text revised.
12422	4	109	3	109	4	Please move the reference to Cazenave A., etc. [Sylvain Ouillon, France]	Accepted
2120	4	112	48	112	49	Incomplete reference. [Josep Medina, Spain]	Thank you. We have fixed this reference.
11964	4	115	41			Reference : Emanuel K.A 2017b. Provide publication version and page numbers. [Chukwuma Anoruo, Nigeria]	Accepted
12406	4	118	58	118	60	ref. is not standard [Sylvain Ouillon, France]	Accepted
11962	4	118	58			the reference is in block format. Please correct that. [Chukwuma Anoruo, Nigeria]	Accepted
1404	4	125	3			Mahmood, A., B., 2016: The Interrelation Between the North Atlantic Oscillation (NAO) and Regulated River Discharge at the Baltic Sea Drainage Basin as well as Mean Sea Level at the Baltic Sea-North Sea Region. PHD thesis. University of Szczecin, Poland. [Ali Mahmood, Iraq]	Noted.
12420	4	135	59	135	61	please move the reference to "Masson-Delmotte, V. et al." [Sylvain Ouillon, France]	Accepted
24058	4	138	1	138	7	Same reference as above (P137L58-63) [Hans-Otto Poertner and WGII TSU, Germany]	Accepted

SROCC First Order Draft Expert Review Comments - Chapter 4							
Comment id	Chapter	From page	From line	To page	To line	Comment	Chapter Team Response
18742	4	139	39	140	27	The section addresses the drivers for the loss of local and indigenous knowledge, the loss of which makes local and/or indigenous communities less capable of preparing for shocks and significant changes even if such change can be slow and cumulative signs visible but which failed to be understood by the very communities who used to be able to decipher them. In trying to understand the reasons for indigenous knowledge not being practised or in instances of their being forgotten, the reasons given are many including the introduction of imported food and the decline in human-nature ties through unsustainable livelihood practices and poor consideration of natural hazards. It can be argued that the introduction of imported food is a manifestation /reflection of something else not the cause of loss of fisheries or coastal management long practised, practices of which are no longer of interest to the local or indigenous communities in question. The cause of decline could be decisions made elsewhere as in centralised control over natural use decisions through government development policies so that long held community management systems are ignored at best or considered a hindrance to development at worse. Similarly, the loss of cultural ties with insitu environmental features (e.g. guardians of the sea and coves), is viewed as cause of increased social vulnerability because such environmental features are now ignored or are no longer respected leading to unsustainable livelihoods. The major issue is the loss of cultural ties, but how did it come about? Had this question been asked, namely 'how did cultural ties get lost in the first place', such loss is transformed into an effect of something else that was happening, most likely larger than the local, and at other times in combination with it. Consequently, missing in this analysis (section 4.3.2.2) are two key drivers. First, a key driver is loss of control and access over natural resources. In instances where state control of coastal and marine resources are manifest through policies on fisheries, no take practices. The second key driver of loss of local or traditional knowledge is the ideology of development and modernisation in many developing countries that in brief, links being modern with one type of development only, namely industrialisation, high technology and monetisation of the economy. By implication traditional knowledge is considered not modern. The ideology which by definition starts from the top is translated in the race for the biggest dams, the highest buildings (soon surpassed by yet higher buildings in other developing countries), in sum, a type of development that equates bigger with being better; hence, more modern. The ideological spread is seen from below when young people no longer trusts traditional medicine, nor interested in learning or acquiring traditional medicinal knowledge, parents who want children not to remain farmers or fishers and, by extension, the status accorded to high energy use in fisheries production for export as in aquaculture vis a vis production for local use, which is counted as 'consumption' and other similar 'desires' to be modern. Hence, in this sense, the driver of loss is buried in layers of local ideological formation. The intermesh of these two key drivers one extra-local, the other - local, could provide an anchor for most of the effects of loss of traditional knowledge listed in section 4.3.2.2.2 of the report. Reference used: Fadzilah Majid Cooke, Ejria Salleh and Lee Hock Ann (eds) (2018) Aquaculture Production in Sabah, Malaysia: Implications for Society, Culture and Ecology. Research Unit for Ethnography and Development, Universiti Malaysia Sabah, Kota Kinabalu, Malaysia. [Fadzilah Majid Cooke, Malaysia]	Noted, taken into account in revised SOD, and to be further developed for final submission (e.g., 4.4.4.2).
21858	4		1	93	56	There is quite a bit of repetition. [Robert Nicholls, UK]	Accepted - text revised.