

os-2013-76 **Author responses (all).** Journal: OS

Title: Sources of 21st century regional sea level rise along the coast of North-West Europe.

Author(s): T. Howard et al.

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MS Type: Research Article

The authors wish to extend their thanks to the two anonymous referees for lots of helpful comments and guidance.

We have tabulated all of the reviewer comments and our responses in the following two tables (one for each reviewer). We have given each comment a reference number (column 1). 'Your ref' (col 2) is the OSD page number for review 10, C849-C853. The final column is our estimate of the importance of the comment/response from an editor or reviewer perspective: comments which involve a significant change are marked 'H'. Comments which require a significant reviewer/editorial judgement are marked with an 'E'. We have also tabulated all of these high-importance/editorial judgement comments in a separate, smaller document.

We address the more-critical review first:

Our ref	Your ref.	Page No.	Reviewer comment	Author Response	Importance
A001	C850	Abs	Line 10. Fully coupled suggests regional sea-level effects drivers of the climate system itself, that seems not a very important feedback. I suspect you merely mean that the system is not treated in a comprehensive way.	Agree. Reworded to "in one self-consistent assessment"	
A002			Line 11. I don't think you consider all processes as you state later yourself in the paper (land water storage, tides etc), so restrict yourself to mentioning what you combine here.	Reworded to "all the processes mentioned above"	
A003			Line 12. I don't think you treat extreme flood height. You specifically focus on 1/50-year events not on the full range of extremes.	Reworded to "the 50-year return flood height"	
A004			Line 12. It is regrettable that you base this work on A1B it is outdated and you use an incomplete ensemble. It is probably a bridge to far to reject the paper on this basis but it implies that it will be hard to compare with more modern results	We accept. It is a structural difficulty that our results were completed only shortly before the publication of AR5, so we were obliged to use AR4 due to the timeframe of the project. Having said that, we did not set out to produce a probabilistic, AR5-based assessment of NW Euro SL change, but rather to compare the contributions, and we anticipate that our main conclusions – that the IDSL component is small and that the surge component may be significant in some locations – will hold under different	E

				scenarios.	
A005			Line 22. You need to place the 22 cm in perspective of the total regional sea level rise.	This is done later in the paper. The point here is to give a benchmark against which to compare the quoted contribution from change in storminess	
A006			Line 24-26. I don't think it is a good idea to use an example in your abstract, which is not extensively discussed in the main text, as is the case here.	? But the main text includes the figures, which include much more information. The idea here was to quote some typical values from the figures so that the reader with access to the abstract can see some of the numerical results without having to consult the figures.	E
A007			Line 26 are changes in vertical land movement significantly contributing to the differences between the locations mentioned? If so quantify.	Yes, regional variations in vertical land movement are significantly contributing to the differences between the locations mentioned. A quantification of this is shown in figure 5.	
A008		Intro	Line 5-6 you may wish to add references to papers attempting to quantify process based contributions of Greenland or Antarctica.	Three references added	
A009		P2436	Line 5. You disqualify the paper by Katsman a bit by arguing is uses rather diverse sources for contributions. Don't you do the same? I suggest you rephrase this sentence.	Rephrased	
A010			Line 8. I suggest rephrasing that the ice loss contribution is based on extrapolation of short time series of observation rather than plausible arguments.	Rephrased	
A011	C851		Line 10. You mix up things here. Slangen et al. 2012 is not an extension of the Katsman work and it does examine regional variations in sea level rather than global variations in sea level. It does so over the entire ocean	We agree and have rephrased the sentence to make the distinction between the two studies clearer.	
A012			Line 19 add after change the period they considered	Done.	
A013			Line 25 you mention that you use improved modeling with respect to Lowe et al. 2009. Specify what you improved. Results in this paper are now impossible to reproduce	We have rephrased this to avoid misleading the reader. Our storm surge modelling is not an improvement on Lowe et al., (2009), but rather we report results from the same simulation over more of the European coastline. (Incidentally, Lowe et al. (2009) represents an improvement over the UKCIP 2002 study, but there is no reason to mention that in the m/s.)	
A014		P2437	Line 5 unclear what upper end means here is that the 100%	The phrase 'upper end' follows the terminology of the UKCP09 Marine	

			percentile?	Report. We have added quotation marks to clarify this. A discussion of the H++ range can be found, for example, here: <a href="http://ukclimateprojections.metoffice.gov.uk/22811">http://ukclimateprojections.metoffice.gov.uk/22811</a>	
A015			Line 16 Here you contradict the abstract by stating that you don't include everything	Please see A002	
A016			Line 19 close to why not the mean or the median?	For some of the process, e.g. thermal expansion, we have a reasonably populated frequency distribution of estimates. For others, such as terrestrial ice melt, we do not. In this case, we believe it is inappropriate to invoke words that have a particular statistical inference or imply a PDF or frequency distribution. This point and A018 are related and we have changed the wording to make it clear that the two scenarios are representative and illustrative of mid-range and high end projections, respectively. We use this wording consistently throughout the m/s now.	H
A017			Line 24 it seems not logical to fit your results to a normal distribution, I think there are arguments that your distribution is positively skewed, so justify this.	Regarding surge, our approach follows Coles 2001. One would expect the distribution of 50-year return period surges to be skewed, since it is an extreme value distribution. There is no reason that I am aware of to suppose that the distribution of surge <i>changes</i> would be skewed. Have added a bracketed phrase to the text, directing the reader to the relevant section. Regarding TE and ADSL, I'm not aware of any reason to assume a skewed distribution?	
A018			Line 28. It is unclear what you mean with you expert judgment of process-based models. I fear it deviates from the consensus in the AR5 report as I get the impression it is based on the approach in Spada et al. 2013 I think? Fine to deviate from AR5 but discuss it explicitly for what reason you did so. I fear that you now only took a pragmatic approach of what you had already available rather than having a solid reasoning to deviate from AR5.	See reply to A016. In the AR5 no PDF for the ice sheet contribution is available and the estimates of dynamic losses are i) almost scenario independent and ii) include a "rapid dynamics" component that was determined from an expert judgement assessment of the literature (see Fig 13.10 of Ch 13). The estimates shown in Table 13.8 for the ice sheets are from a single simulation. AR5 is the consensus assessment of the potential future contribution to sea level rise from various sources, which differ markedly in their estimates. Here, as with many other studies, we make an assessment of the ice sheet contributions that does not disagree with AR5 (i.e. our estimates are bracketed by the AR5 ranges shown in Fig 13.10)	H, E

				but which is not an exact copy. It is not particularly straight forward to combine the ice sheet components shown in Fig 13.10 primarily because the correlation between processes is not defined and, in general, unknown. The ice sheet estimates we use are the same as those used in Spada 2013 (this is explicit in section 3.2), and come from process-based model simulations, some of which were included in the AR5 synthesis. We have modified the text to make these points clearer.	
A019		Nomenclature	Add MME and PPE to Table. I guess you need to explain in a sentence what PPE means	These two abbreviations are already defined in the first paragraph of the nomenclature section. We prefer not to repeat the definitions in the tables, which refer only to components of sea level change.	E
A020			Line 10 Explain that TE+ADSL is what is typically calculated by a GCM	I don't think this would be appropriate in the nomenclature section. Use of GCMs to quantify TE and ADSL is explained in section 3.1, immediately following the nomenclature section	E
A021		P2439	Reference to figure 4 disturbs the flow of the figure numbering	Agree. This ref now moved into section 4.1, thanks.	
A022	C852	P2439	Line 3. You need to compare your results in the light of AR5 in addition to AR4 more extensively than you do.	Reader now referred to section 4.1, where we comment on the AR5 results. Please see also our response to A004	H
A023			Line 8. You suggest yourself that your referencing period is not identical for the different components. Please correct this even if it may be necessary to do some recalculations.	<p>Agreed. Recalculated. The recalculation results in a small change to figs 3, 4 and 5. All now referenced to 1990. Text amended accordingly. However, the TIM and GCFF contribution is calculated as anomalies relative to 1992: a steady state was assumed at 1992, prior to the observed increase in both the runoff and calving fields. This is explained in the companion paper, so we have added a reference to it in the text. In view of this we do not rescale the TIM and GCFF contribution (we do not, for example, multiply by 100./98.) because this would be inconsistent with the steady-state assumption. (in effect we are assuming no significant ice-melt contribution between 1990 and 1992)</p> <p><b>[Note to self: Check that corrected figs are supplied to Ocean Science. Corrected fig 5 also has 'H' instead of 'G'. See ~/ice*/rep*/figs/euro*/ ]</b></p>	H, E

A024			Line 24 Which RCM are used	HadCM3 in RCM format. We have reworded this paragraph to clarify.	
A025		P2440	Indicate at line 3 that you use a mixture of CMIP3 results and HADCM3 PPE results. You undermine your approach, which is probably pragmatic because you don't have for all CMIP3 models the possibility to drive your RCMs but be at least transparent about this.	M/s modified.	
A026		P2441	Line 12. I do find it embarrassing that you use a somewhat outdated data set and then start to make corrections because from this outdated data set you did not use all ensemble members. Sloppy approach	Please see A027	
A027			Line 16. The difference in mean and standard deviation is maybe not large but that does not mean that the pattern is not affected and that is what your interest is, so I guess you do need to redo your calculations with the full set of CMIP3 results. Which 11 members are used?	We used eleven of the CMIP3 models. These were selected from the full set of CMIP3 models on the basis that DSL projections under the A1B scenario and the accompanying parallel sections of simulations with fixed greenhouse gas concentration were both available to us (together with global thermal expansion). Using this subset of models therefore allowed a common approach to be taken in removing model drift in the DSL pattern changes. This is now explained in the text, in section 3.1	E
A028		P2442	Header of section simply use full terms rather than acronyms	I can see the appeal of this, but I think it's problematic. Please see continuation box below...	E
<p>A028 continued. It seems to me that there are four choices here:</p> <ol style="list-style-type: none"> <li>1) Use acronyms</li> <li>2) Expand all the acronyms that appear in sub-section headers exactly as they are in table 1</li> <li>3) Expand all the acronyms that appear in sub-section headers in a less carefully-defined way</li> <li>4) Expand only some of the acronyms that appear in sub-section headers</li> </ol> <p>Problem with (2) is that sub-section headers become very wordy, for example:</p> <p>3.1 Contribution from Global mean thermal expansion of the ocean and regional changes in dynamic sea level for A1B emissions scenario, excluding the effects of ice-loss determined in offline models (&amp; bear in mind that most such sub-section headers appear twice, in section 3 and in section 4 !)</p> <p>Problem with (3) is that section headers become vague/inaccurate/confusing, for example the section on contribution from SRG might become:</p> <p>3.3 Contribution from surge</p> <p>... but, as our other reviewer has emphasised, this section does not consider, for example, changes in surge associated with changes in tidal basin resonance due to mean sea level change, but only those parts from change in atmospheric storminess.</p> <p>Problem with (4) is that section headers become inconsistent, some using acronyms, some not.</p> <p>My preference is to keep the acronyms in the sub-section headers, but ultimately I guess this is an editorial decision.</p>					
A029			Line 8 no logical sequence of sentences	? Seems OK to us	E

A030		P2443	Line 10-14. Seems pointless remark, as Nicholls does not explicitly discuss NW Europe.	Have rephrased the entire paragraph.	
A031	C853	P2443	Line 18. Katsman discussed subsidence, which I guess includes GIA, so please rephrase.	Have rephrased the entire paragraph	
A032		P2444	It seems worth discussing why local sea level rise is larger than the global mean here whereas this is not the case in the two other studies.	The main reason for this is the smaller contribution from the Greenland ice sheet in our projections (see Howard et al., 2014). (The fingerprint of Greenland melt is characterised by less-than-global-mean increases around NW Europe). Sentence to this effect now added to m/s.	
A033		P2446	Indicate position 55°E 55°N in one of the figures.	Now easily identified in fig 3 c/f A037	
A034			Line 16 unclear CI of [-16,37] and mind units seems a double per second in the sentence	Now explicit: confidence interval. Units (centimetres per second per century) are valid (rate of change of wind speed).	
A035		P2447	Why RCP6.0 whereas earlier on you make the comparison with RCP4.5	M/s amended, thanks.	
A036			Discuss why you didn't take a rigorous approach by forcing your RCMs with CMIP5/3 boundary conditions	Please see A004	
A037		Figs	Figure 3 indicate 0/5/10/15 with lines	Agree. Done.	
A038			Figure 4 doesn't use acronyms. GCFF is confusing as it is a quantity with respect to the global mean whereas for instance TE is not expressed with respect to the global mean. Why not make a panel A with all components expressed as global mean values and one panel with all components for NW- Europe. Alternatively add GCFF and TIM, to have the local expression of TIM, which can be compared with the local TE.	Agree. Have split into two panels as suggested.	
A039			Figure 5 high-end to be placed at right side of mid-range.	We cannot see a reason to do this.	E

And for the other review:

Our ref	Reviewer comment	Author response.	Importance
B001	p. 2434, l. 7: storm surge is a better and	Agree; done.	

	more widely used term than storm tide		
B002	p. 2434, l. 26: ...in the rates of...	Done, thanks.	
B003	p. 2435, l. 27: Here, and at several other places in the manuscript 'storm' is used synonymously with 'storm surge'. The focus in this study is on changes in the 50yr storm surge height (not the 'storm height' or the height of a storm). Please check the manuscript and change where necessary.	Agree; done; thanks.	
B004	p. 2436, l. 20: In a recent paper, Muddersbach et al. (Trends in high sea levels of German North Sea gauges compared to regional mean sea level changes, Cont. Shelf Res.) the authors show that in the south-eastern North Sea, a region that is part of the investigation area of the present study, changes in mean and extreme sea levels have been different throughout the second half of the 20 <sup>th</sup> century. These differences are partly a result of changes of the tidal constituents.	Thank you for pointing us to this work. New paragraph and references added to Introduction detailing some of the contributions <i>not</i> considered. (c/f B007)	
B005	p. 2437, l. 2: avoid the term 'absolute sea level' and use 'geocentric' instead	Done.	
B006	p. 2437, l. 6: NW has not been defined	Done.	
B007	p. 2437, l. 15: There are other factors that might influence storm surge statistics but are not included in the study: tides (see my comment above and for example Woodworth (2010): "A survey of recent changes in the main components of the ocean tide" or Müller et al. (2011): "Secular trends in ocean tides: Observations and model results"), and also changes in the seasonal cycle (e.g. Wahl et al. 2014, Rapid changes in the seasonal sea level cycle along the US Gulf coast from the late 20 <sup>th</sup> century). These should at least be mentioned for completeness.	Thank you for pointing us to this work. New paragraph and references added to Introduction detailing some of the contributions <i>not</i> considered. (c/f B004)	H
B008	p. 2437, l. 11: above it was Northwest and NW and North-West in the title, this should be consistent	Done	
B009	p. 2437, l. 23: Even if it is a side note in brackets, I would try to make it a real sentence, e.g. (the masked region is shown in Fig. 2; the values are very similar when using a masked region of half this width)	Done	
B010	p. 2439, l. 19: Pickering (2012) (for example) showed that SLR leads to an	The surface wind and atmospheric pressure fields output by the RCMs	H

	increase in the tidal range, which in turn affects total storm surge water levels. Does the model run conducted for the present study implicitly include this effect or is it run without tidal forcing?	<p>were used to drive a barotropic surge model, CS3 (Flather et al., 1998), in combined surge-and-tide mode. A parallel simulation in tide-only mode allows extraction of the non-tidal surge residual or the skew surge. Changing bathymetry due to increasing mean sea level is not included in the surge model but is added linearly in a separate step (this does not include changing coastline due, for example, to newly inundated areas; the land/sea mask does not change)</p> <p>This is now explained in the text, in section 3.3 Reference to Pickering also added in Introduction. See also B004; B007</p>	
B011	p. 2440, l. 1-5: What is the time period covered by the model experiment?	Now described in text.	
B012	p. 2440, l. 11: The authors use the r-largest approach with $r = 5$ , why exactly 5, is this decision based on any tests or on the available literature or randomly chosen?	See for example Tawn, 1992, Coles, 2001. Comment to this effect added to text.	
B013	p. 2441, l. 2: MME has been defined before (as multi-model ensemble)	Text amended; thanks.	
B014	p. 2441, l. 5: What is exactly meant with 'local spatial mean'? It sounds weird.	Explanation added in text.	
B015	p. 2441, l. 25: Are the values of 20 and 50 cm used for the entire region?	Yes, TIM is a global mean figure, as defined in table 1.	
B016	p. 2443, l. 13: In low agreement with what? With each other, with observations, or with process-based model projections?	Wording changed: "low agreement within semi-empirical model projections..."	
B017	p. 2445, l. 7: add comma before e.g.	Done.	
B018	p. 2445, l. 24: Delete 'for the Dutch coast', it's mentioned in the sentence before.	Done.	
B019	p. 2445, l. 25: I would suggest using 'Den Haag' throughout the paper; people are much more familiar with that	Done; thanks.	
B020	Figure 1: At least in this figure I suggest showing lon/lat values; this makes orientation easier for people not so familiar with the region. Also, you mention a specific grid point ( $5^{\circ}\text{E}$ , $55^{\circ}\text{N}$ ) somewhere in the manuscript, which cannot be found without this information on the axes.	We have added Lat/Lon lines to figure 3 (see A037) in preference to fig 1, because fig 1 is on a non-standard projection. The specific grid point ( $5^{\circ}\text{E}$ , $55^{\circ}\text{N}$ ) is now easily located on figure 3.	
B021	Figure 4: I find the caption confusing. It says that the figure is "showing the 21 <sup>st</sup> century change in 50 yr storm height",	Caption amended.	



	whereas I think it shows the results from quantifying the contributions of different variables to changes in the total water levels associated with a 50 yr storm surge event.		
B022	Figure 5: Again, I would make the end of the caption a real sentence: Further details are explained in the text.	Done.	