

os-2013-76 **Author responses (main points only).** 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 the most important 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 the review comments and our responses in a separate, larger document.

We address the more-critical review first:

Our ref	Your ref.	Page No.	Reviewer comment	Author Response	Importance
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 It is regrettable that you base this work on A1B it is outdated and you use an incomplete ensemble.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 too 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 scenarios.	E
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
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	H

				representative and illustrative of mid-range and high end projections, respectively. We use this wording consistently throughout the m/s now.	
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) 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.	H, E
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	Agreed. Recalculated. The recalculation results in a small change to figs 3, 4 and 5. All now	H, E

			components. Please correct this even if it may be necessary to do some recalculations.	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) [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*/]	
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"> 1) Use acronyms 2) Expand all the acronyms that appear in sub-section headers exactly as they are in table 1 3) Expand all the acronyms that appear in sub-section headers in a less carefully-defined way 4) Expand only some of the acronyms that appear in sub-section headers <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 (& 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>					

My preference is to keep the acronyms in the sub-section headers, but ultimately I guess this is an editorial decision.					
A029			Line 8 no logical sequence of sentences	? Seems OK to us	E
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
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 th 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
B010	p. 2439, l. 19: Pickering (2012) (for example) showed that SLR leads to an 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>The surface wind and atmospheric pressure fields output by the RCMs 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>	H