Interactive comment on “Complementing thermosteric sea level rise estimates” by K. Lorbacher et al.

Anonymous Referee #2

Received and published: 21 April 2015

General comments:

Many thanks for providing me with the opportunity to review the submission: “Complementing thermosteric sea-level rise estimates” by Lorbacher and co-authors. Firstly I must apologise to the authors and the Editor for the extreme tardiness in filing my review.

The manuscript revises a number of previously published estimates of simulated ocean heat content change presenting these new results as an integrated thermosteric sea level metric against available observations. As noted by reviewer 1, the new analysis is a useful complement to previously published works and mostly replicates these previous results. I agree with reviewer 1 that the current submission reads as a hurried and unfinished draft, rather than an easy to read and considered contribution. As noted by reviewer 1, key results described in the abstract and discussion is not clearly described or identifiable within text and consequently left this reader with many more questions than answers.

I have noted a series of additional comments and proposed corrections in the following pages. I recommend that the manuscript would benefit from a start to finish re-write to improve the flow and address comments from both reviewers. I do believe this manuscript does provide useful new insights, however believe that the paper requires major revisions before the present contribution is publishable in Geoscientific Model Development.

Specific comments:

Page 1202, line 4: “...mostly limited to the upper ocean layers...” As noted in subsequent text, significant (and potentially accelerating) warming has been documented by Purkey and Johnson (2010) – I believe this should be better reflected.

Page 1202, lines 9-10: “We obtain 30% more thermal expansion time series than currently published” I am a little uncertain about this sentence and what the authors mean. A rewrite would be useful.

Page 1202, lines 18-19: “(goodness of fit..” I was interested to understand this better, however the last sentence of the discussion didn’t provide much information.

Page 1202, line 23: “30% of the net heating” Is this Earth total or global ocean total, it wasn’t clear to me.

Page 1202, lines 24-25: “...the thermal expansion of seawater is a major driver behind SLR.” It would be useful to quantify this statement – Church et al., (2013a) note 40% (0.8 of 2.0 mm yr-1) can be attributed to thermal expansion over 1971-2010.

Page 1203: lines 3-10: I believe the recent publication Rose et al. (2014) provides useful background here.
Page 1203: lines 13-14: "...mass changes together with ocean’s thermal expansion...
Again quantifying this statement would be useful (e.g. Church et al., 2013a).
Page 1203, lines 15-17: "...such as salinity variations associated with freshwater tend-
encies have a negligible effect on seawater density and thus global mean sea level changes..." This statement is true for global integrals, but certainly is not true for reg-
ional (e.g. Durack et al., 2014).
Page 1203, lines 20-23: "...simulating the land ice-sheet... still translates into large
uncertainties in climate models." A more correct statement would be that CMIP5 gen-
eration climate models do not include land-ice contributions to the SLR budget – which
was the motivation for the cited publication Church et al. (2013b).
Page 1204, lines 1-3: "...to cover the upper 2000 m at maximum." The publications
Palmer et al., (2007, 2009) and Smith and Murphy (2007) are relevant here – I also
note that the Smith and Murphy (2007) analysis extends to 5000 m, and the Levitus et
al. (2005) analysis extends to 3000 m. It would also be useful to mention the platform
bias issues – Abraham et al. (2013) provide a nice summary.
Page 1204, lines 3-5: "...assumed to increase.." due to data sparsity.
Page 1205, lines 8-11: "We begin... to derive thSLR." I suggest a rewrite.
Page 1206, lines 10-20: This text was difficult to follow, even after a re-read. From
where did the 6000 and 3500 denominators come from, are these assuming some
depth of integration? A rewrite of this section would be useful.
Page 1206, line 19: psu -> PSS-78.
Page 1206, lines 22-25: "As a first step, we use for each hemisphere... Can the
authors elaborate further on why this simplification is useful?
Page 1206, lines 18-26, 1208, lines 1-11: I really had a hard time determining what
the authors are describing here. In a previous paragraph the new results are noted
as "robust" to Church et al. (2013a) [page 1207, lines 11-14], whereas this paragraph
tends to suggest this is no longer true – and is the discussion on observations, models,
the contrast between these? – A re-write would be useful here.
Page 1208, line 16-19: “Based on observed and, additionally by assimilating…” I assume that the Kouketsu et al. (2011) results described where from two independent analyses?

Page 1208, line 20-23: “…aligned with the Argo observational climatological profiles of potential temperature and salinity for the modern day (2005-2013) ocean (Roemmich and Gilson, 2009).…” It would be useful to provide some additional information here for the reader.

Page 1209, lines 8-28: I really had a hard time following the thread here – I suggest a re-write.

Page 1210, lines 1-5: “…[observations] upper 200 m by 4%.… [models] upper 700 m by 8%.” I wonder why the depth of comparison is not the same; a direct obs vs model contrast would have more utility I believe.

Page 1210, lines 14-16: Model dependent AABW formation rates – a relevant citation would be helpful.

Page 1211, line 11: “…our results show that observed estimates of thSLR for the upper 700 m.”

Page 1211, lines 12-16: “…augmented on average by 36%.” This number and subsequent numbers appears out of the blue to me, I’m a little uncertain how these numbers are supported by the analysis.

Technical corrections:
Page 1207, line 21: Additionally -> Additionally
Page 1212, lines 1-2: “…underlying interannual variability because of the internal variability of ocean dynamics.”

Page 1212, line 9: perists -> persists

Figures:
I found many of the graphics difficult to view as they are small and contain a lot of material – some further work to optimise these results would be useful.

Figure 1: The standard units of SLR in the literature are mm yr-1 to maintain continuity with a large number of publications cited in this manuscript I would suggest altering axes to reflect this. I assume the thin coloured lines indicate a linear fit to the Roemmich and Gilson (2009) and Levitus et al. (2012) plotted timeseries, if yes what is the origin? Additionally I’d check these, they don’t appear to faithfully intersect the timeseries they are calculated from. Figure 1 caption: what are the numbers following each of the experiments: e.g. historical (31/47)?

Figure 2: As noted by reviewer 2, there is little use in plotting a 0 value for Roemmich and Gilson (2009) on panels d and e. There is a note here about model outliers, but I do not recall any discussion of this in the text – if there is some use in highlighting outliers they should be described in text.

Figure 3: Using the same vertical scale for each experiment would be much more useful to a reader.

Figure 4: As noted by reviewer 1 I’m uncertain if this figure shows any new information that was not presented in Figure 3. If the 700-2000 m results are indeed important, I suggest incorporating them into Fig 3 and cleaning a single figure up.

Figure 5: Including observed estimates on this plot would be useful. Ditto to comment (1) from reviewer 1 (deep ocean contribution RCPs vs abrupt4xCO2). Also the spread in the mean and median is quite large, is there a specific reason why median (rather than mean with errors) was selected for use within text?

References:

Interactive comment on Geosci. Model Dev. Discuss., 8, 1201, 2015.