

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

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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.

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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 tendencies 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 regional (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 generation 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 1204, lines 8-11: “We begin . . . to derive thSLR.” I suggest a rewrite.

Page 1205, 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 1205, line 19: psu -> PSS-78.

Page 1205, 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 15-19: Some new insights regarding the “adjusted forcing” by Forster et al. (2013) for simulations contributing to CMIP5 would be useful background to the

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reader here.

Page 1206, lines 19-22: model drift – some discussion of this drift correction methodology with reference to Sen Gupta et al. (2013) would be useful.

Page 1206, lines 22-26: This Gregory et al. (2013) correction is certainly justifiable, however as thermal expansion has been estimated as contributing just 40% of the 1971-2010 SLR (Church et al., 2013a) I think some discussion would be useful. The Lorbacher et al. (2012) publication would provide some insights here.

Page 1207, line 8: “. . .30% more time series of zostoga than previously published.” A reference would be useful to a reader here.

Page 1207, line 9: The PCMDI ESGF portal link <http://pcmdi9.llnl.gov/esgf-web-fe> (or an alternative ESGF master node) would be a suitable URL.

Page 1207, lines 12-13: “. . .previous CMIP5 multi-model ensemble estimates by Church et al. (2013a) have been robust. . .” It would be useful to elaborate and quantify this agreement. It would also be useful to elaborate and highlight this result as “observational augmentation of 36%” is noted in the abstract.

Page 1207, line 19: “1993-2010” as the historical simulations nominally end in 2005 and RCP simulations begin in 2006 it would be useful to highlight in methods how temporal splicing was undertaken.

Page 1207, lines 21-25: A rewrite of this sentence would be useful – it was not clear what was being discussed here. If the “hiatus” is a focus, it would be worthwhile noting some relevant citations.

Pages 1207, 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.

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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: Additionaly -> Additionally

Page 1212, lines 1-2: “. . .underlying interannual variability because of the internal variability of ocean dynamics.”

Page 1212, line 9: perists -> persists

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Page 1212, line 16: merdional -> meridional

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?

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