

Second reviews of gmd-2014-245

Referee 1

The manuscript has much improved and I suggest publication once a few minor corrections (detailed below) have been done. The results are explained much better and the ms. is a coherent document now. I'm glad that I understand Fig.3 and 4 now!

I do not need to see the ms. again.

* Detailed remarks: *

l.181 "slight underestimation": I find that the two Figures are ambiguous in this point. In Fig. S3c the observed trends are below the models' trend for a few years, but later they are above.

l.184 "S3c" I guess you mean Fig. S3d.

l.212 "is confined to": missing word/s here.

l.277 "based on 30%" Not quite correct: if you have got 30% more than Church et al., then they have $1-(100/130)=23\%$ less than you.

Fig. S3: There's no explantion of panels c/ and d/ in the caption.

Referee 2

General comments:

Many thanks for providing me with the opportunity to review the resubmission: "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.

I have now reviewed the revised submission and have noted some concerning issues. Firstly the results appear to have changed from the first submission (I note differences in the tabulated results: Table 1 and the results presented in Fig 5 – particularly the mean values), though there is no mention of this change or why it was made in the author response to reviewers? Consequently it made me question the accuracy of the results presented.

I also note that both reviewers noted a difficulty in reading graphics and their content. While it does appear that the authors have revised fonts in many of the figures, I still believe there is a clear opportunity to more clearly present generated results – the suggestion of reconfiguring Figs 3 & 4 which present very similar results into a composite figure appears to have been ignored in the present revision.

Considering the above, I still believe there is additional work required to

improve the current version of the manuscript for publication in Geoscientific Model Development. In particular, the undocumented changes between versions led me to question the underlying analysis and leads me to hesitate publication.

I have noted a series of additional comments and proposed corrections in the following pages.

Specific comments:

The comments below related to the author contributed manuscript page and line numbers, rather than the published numbers as used in the previous review:

Page 1, lines 4-5: "Furthermore, only part of the available climate model data is sufficiently diagnosed." This sentence and a number of others reads awkwardly to me.

Page 2, lines 20-24: The results published in Rhein et al. (2013) also indicated that the ocean dominates changes in the Earth's energy budget with 93% of excess heat during the 1971-2010 period stored in the ocean.

Pages 2-3, lines 55-57: "For the observational record.." I got confused as to what the 34% and 47% relate to – suggest a rewrite.

Page 3, lines 59-62: "...to cover the upper 2000 m at maximum." As noted in the original review, the publications Palmer et al., (2007, 2009) and Smith and Murphy (2007) are relevant here – they were presented alongside the Domingues et al. (2008), Ishii & Kimoto (2009) and Levitus et al. (2012) results presented in Rhein et al (2013; Fig 3.2) – 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, so the statement which persists in this revision is not supported by all available literature. I note in the analysis of Church et al. (2011; GRL) who provided a full energy budget (full depth ocean analysis) which considered the contributions of thermosteric SLR.

Page 4, line 97: As noted in the original review the 6000 denominator is again included and undocumented – I assume this is the assumed ocean full depth of integration?

Page 6, lines 186-189: "For the upper 2000 m.." It would be good to point a reader to Fig 2 at this point the Argo (Roemmich & Gilson) comparison is useful.

Page 9, line 287: "...have to be multiplied only by 1.17.." Why do they have to be multiplied?

Page 13, Table 1: I noted differences with the original submission, why?
Figures:

For completeness I've repeated a large number of comments from the original review:

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.

I also wonder why all model and observational results (panels c and d) appear to have been reset to 0 at 1993 – this information should be included in the caption

The dashed lines noted in the revised caption do not appear to be visible to me.

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. I note Page 6, line 193 “..PSS-78, with only a few model outliers.” It would be useful to highlight this with more descriptive text if the results are worth including.

Figure 3: Using the same vertical scale for each experiment would be much more useful to a reader. I see that all are the same aside from the Historical panel

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?

I was confused by the reviewer response that “We included the estimates from the World Ocean Database..” yet the figure is largely unchanged?