Author responses to technical corrections for the manuscript

Complementing thermosteric sea level rise estimates

Geosci. Model Dev. Discuss., 8, 1201–1223, 2015

Dear Dr. Bob Marsh.

We thank you for considering our manuscript for publication provided after addressing the technical corrections from the two referees to your satisfaction. Please find our point-by-point response below, with changes in the main text indicated in blue and previous responses indicated in grey.

Sincerely,

Katja Lorbacher et al.

1/ Responses to Reviewer #1

1.1 *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.*

For clarification we added: "... for the period 2005-2013 ..."

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

Yes, thanks.

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

We added the missing words from the previous response as follows: "... the upper 700 m in the first 20 years".

1.4 *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.*

Thanks for this comment. You are correct if the reference is the same, though our references for the percentages are different. For clarification we changed the second sentence in the discussion to: "Firstly, based on CMIP5 temperature and salinity data for a range of scenarios, we calculate a compilation of thermal expansion time series that comprise 30% more simulations than currently published within CMIP5. This accounts for 50% more models in the multi-model ensemble estimates than used by Church et al. (2013a). However, our results confirm the robustness of these previous CMIP5 multi-model thSLR estimates." And we added in the first paragraph of section 3: "These complementing *zostoga* time-series contribute 50% more CMIP5 models to multi-model ensemble thSLR estimates than previously used by Church et al. (2013a); they are available at http://climate-energy-college.net/complementing-thermosteric-sea-level-rise-estimates and as supplementary material."

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

We added the explanation.

2/ Responses to Reviewer #2

Specific comments

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

Since we received no comment about this sentence so far, we are inclined to leave it unchanged.

We went through the manuscript and can only speculate about other sentences that read awkwardly to you. Since we haven't received more specific comments about the readability of specific sections, no further changes have been introduced to the text.

2.2 *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.*

Thanks, the sentence reads now: "The climate system is warming and during the relatively well-sampled recent 40-year period (1970-2010) the world ocean absorbed 93% of the Earth's radiative energy excess, whereby 70% of the net oceanic heat gain is found in depths above and 30% below 700 m (Rhein et al., 2013)."

2.3 *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.*

The percentages quantify the contribution of thermal expansion to global mean SLR for the satellite altimeter period, whereby 34% are based on observational and 47% on the corresponding model data. For clarity we changed the sentence to: "Since the beginning of the satellite altimetry era in 1993, the contribution of thermal expansion to global mean SLR is estimated to be 34% (observations) and 47% (simulations), respectively (see Table 13.1 in Church et al., 2013a)."

2.4 *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.*

Firtly, we would like to cite our original response: Thank you very much for pointing to these references. The studies by Palmer et al. (2007, 2009) and the one by Smith and Murphy (2007) are mainly concerned with the masking of the warming trend of the subsurface ocean due to natural variability in the temperature time-series, whereby subsurface covers a maximum depth of 500 m. We are mainly concerned with the spatial and temporal oceanic temperature data coverage to arrive at observed long-term time-series of thSLR with contributions to this integral value from the entire water column. And the current time-series by **Levitus et al. (2012) do not exceed depths of 2000 m. The authors quote "A lack of high-quality CTD and reversing thermometer data at depths exceeding 2000 m in recent years precludes us from producing recent analyses for deeper depths."** However, we added as last sentence of this paragraph "For details on the spatial and temporal coverage and quality of oceanic temperature measurements that underlie thSLR estimates we refer to Abraham et al. (2013) and references therein."

Additionally, we would like to note that Rhein et al. (2013, Fig. 13.2) presents time-series of global mean upper (0-700 m) ocean heat content changes by Palmer et al. (2007) and Smith and Palmer (2007). They do not show the volumetric response to these temperature changes, the thermosteric sea level. To our knowledge these time-series do not seem to be publicly available to test e.g. our 0-D approach (and they also are not cited by Church et al. 2013a). The (HadCM3) model analysis by Smith and Murphy (2007) exceeds 5000 m but not their assessment with regard to temperature observations (1st paragraph on page 14 in Smith and Murphy, 2007).

The full depth thermosteric sea level estimates by Church et al.'s (2011) are the sum of estimates of the upper 700 m by Domingues et al. (2008), from depths below 3000 m by Purkey and Johnson (2010) and from depths between 700-3000 m by Levitus et al. (2005) and Antonov et al. (2005). **The author's note about the latter estimates, "but these estimates may be biased low because of inadequate deep-ocean sampling"** (2nd paragraph of section 2.2 in Church et al., 2011).

In our opinion, we cite the most relevant and reliable sources of observed global mean long-term time-series of thermosteric sea level rise. Citing Abraham et al. (2013) and referring to the references therein we think to have found a compromise.

2.5 *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?*

Yes, the 6000 denominator represents a mean maximum ocean depths in the models. We added around line 102: "... and a mean maximum ocean depth of 6000 m in Eq. (3)".

2.6 *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.*

We point to Fig. 2 in the subsequent sentence. However, we separated the sentences now with a semicolon.

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

In response to reviewer's comment 1.1.3 we decided in the revised manuscript version to use multipliers and not percentages to quantify the increase on thSLR estimates derived solely based on observational estimates from the upper 700 m (2000 m) in order to account for thSLR contributions from deeper levels.

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

Apologies. While mending Fig. 1 we noticed one typo in the uncertainty interval and that the multi-model medians for year 2100 are relative to 1986-2005 and in contrast to Fig. 1 that shows the thSLR relative to year 1900. Please note that the underlying analysis that estimates our thSLR time-series is correct. We thank you for mentioning this inconsistency and we checked carefully the consistency of figure and numbers in the text.

Figures

Figure 1: *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. 2/ 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. 3/ 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. 4/ The dashed lines noted in the revised caption do not appear to be visible to me.*

1/ Thanks for pointing to the importance of maintaining consistency with literature regarding the units of SLR. Our focus in Fig. 1 is on the temporal evolution of thSLR depending on the vertical integration limit. In our opinion these show more clearly the model spread and the decline of low-frequency (inter-annual to inter-decadal) variability of the time series as function of depth, thus make possible a direct comparison with IPCC-AR5 results (Table 13.5 in Church et al., 2013a). However, we are showing the evolution of the rate of thSLR in the supplementary material (Fig. S3).

2/ We checked the linear fit to the data by Roemmich and Gilson (2009) and Levitus et al. (2012), with the linear fit originating in year 1993, the beginning of the satellite sea level altimetry.

3/ see 2/ though the explanation of the reference year is included in the caption now.

4/ For the idealized experiment, the lines are clearly visible, though for the historical and RCP scenarios they might be masked by the amount of data.

Figure 2: *1/ 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.*

1/ We prefer to include Roemmich and Gilson's values for consistency and to visualise that the observed warming over the last ten years is not noticeable against the amount of warming projected until the end of 21st century. However, we don't show the data in the final version but added two figures of simulated vertical thermal expansion profiles.

2/ Thanks, we now mention the model outliers in section 4 of the main text: "Strong outliers (values far outside the whiskers and the 90% confidence interval) are found in the depth range below the main thermocline between 700-2000 m independent of the scenario and spatial averaging."

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.*

The vertical scale should now be consistent for the radiative forcing experiments though still different to the historical one for better readability of the numbers.

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.*

In Fig. 4 we would like to highlight the robustness of the equation of state expressed in the simplified version of the thermal expansion coefficient (Eq. 3) by showing how the results might differ among the scenarios as well as by using different depth intervals for the vertical integration. Fig. 4 shows clearly that our results do not depend crucially on our calibration parameters but on individual model characteristics like the 3D-pattern where the heat was taken up and redistributed in the interior ocean. ...

Figure 5: 1/ Including observed estimates on this plot would be useful. Ditto to comment (1) from reviewer 1 (deep ocean contribution RCPs vs abrupt4xCO2). 2/ 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? 3/ I was confused by the reviewer response that "We included the estimates from the World Ocean Database ..." yet the figure is largely unchanged?

1 and 3/ **We included the estimates from the World Ocean Database** (Boyer et al., 2013) due to their global horizontal resolution and coverage of the entire ocean depth **in the discussion:** "For climatological temperature and salinity profiles (Boyer et al., 2013), the difference between the mean (1200 m) and median (700 m) depth in is even greater compared to our model diagnostic results of the historical scenario. This can be explained by a reduced vertical temperature gradient within the main thermocline and a weaker stratification above the main thermocline induced by the absent end of 20th century warming in the climatological profiles." Additionally, now **we show** the climatological values as horizontal lines **in Fig. 5**.

2/ We would like to quantify here, that the vertical distribution of thermal expansion is skewed towards greater depths (Fig. 2e) and the mean depth is always deeper than the median depth. In general, for skewed distributions it is not at all obvious whether the mean or median is the more meaningful measure. In our case, in particular with the difference between mean and median depth of thermal expansion you can make an assumption how deep a warming signal might have penetrated and how large the vertical temperature gradient that defines the main thermocline might have become; so we show both measures as multi-model mean. We make the following statement in the discussion: "The mean depths are 100 (300) m lower than the medians for the idealized (RCP) scenarios and 400 m for the historical scenario (Table S5). This indicates a positive skewness of the vertical distribution of thermal expansion because of its long tail towards depths below 700 m." And adjusted the figure caption slightly for increased clarity "The multi-model mean depth and standard deviation (in m) from where the individual model mean and median depth of thSLR originates ...".