

## Author responses to reviewer comments for the manuscript

### *Complementing thermosteric sea level rise estimates*

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

We sincerely thank the two reviewers for many helpful comments, criticisms and suggestions, their overall positive and encouraging remarks. Aiming for depth and clarity in reporting our results, our response to the reviewer remarks have, in our opinion, resulted in an improved manuscript. Please find the point-by-point addressing of all comments below, whereby [changes in the main text are indicated in blue](#).

## 2 Responses to Reviewer 2:

This section details our responses to Reviewer #2:

### 2.1 Special remarks

**2.1.1 Reviewer #2:** *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.*

**2.1.1 Author Response:** In the abstract we start the second sentence now with *“Yet, observational estimates of this volumetric response of the world ocean to temperature changes are sparse, mostly limited to the ocean’s upper 700 m ...”* to point to limits of available data not to the “limits” of warming (as function of depth). We reflect on the latter in more detail throughout the paper.

**2.1.2 Reviewer #2:** *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.*

**2.1.2 Author Response:** We re-wrote the sentence as follows: *“Specifically, based on CMIP5 temperature and salinity data, we provide a compilation of thermal expansion time series that comprise 30% more simulations than currently published within CMIP5.”*

**2.1.3 Reviewer #2:** *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.*

**2.1.3 Author Response:** Generally, the goodness-of-fit captures the root of mean squared (RMS) differences between test and reference data. For our calibration purpose, it is the minimum of root-mean-squared differences between our thermal expansion estimates based on Eq. 3 and Eq. 2 over all individual model-scenarios; we estimate the goodness-of-fit for each model and list it together with the corresponding calibration parameters in the supplementary Table 1. For testing the reliability of our simplified approach estimating thSLR, we calculate the RMS difference between our global mean thSLR time series based on Eq. 2 and the published ones within CMIP5 (1%), the difference between our global mean thSLR time series based on Eq.2 and our thSLR time series based on Eq. 3 taking into account the entire depth as vertical integration limit (5%) as well as the difference between our global mean thSLR time series based on Eq. 2 and our thSLR time series based on Eq. 3 but for vertically integrated values (9%). We tried to extend the text accordingly. See as well our comment to point 1.1.5 by Reviewer #1.

- 2.1.4 Reviewer #2:** Page 1202, line 23: “30% of the net heating” Is this Earth total or global ocean total, it wasn’t clear to me.
- 2.1.4 Author Response:** Thanks for pointing out the missing reference quantity, the world ocean. The sentence reads now: *“The climate system is warming and during the relatively well-sampled recent 40 year period (1971–2010) the world ocean stored 70% of the net oceanic heat gain in depths above and 30% below 700 m (Rhein et al., 2013).”*
- 2.1.5 Reviewer #2:** 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.
- 2.1.5 Author Response:** For the observational record with satellite altimeter data since 1993, we quantify the contribution to global mean SLR from thermal expansion at p.1203, l.26 and now also, like you suggested, the one over the 40 year period: *“Church et al. (2013a) note that 40 % of the observed SLR over 1971-2010 can be attributed to thermal expansion.”*
- 2.1.6 Reviewer #2:** Page 1203: lines 3-10: I believe the recent publication Rose et al. (2014) provides useful background here.
- 2.1.6 Author Response:** Thank you for pointing out this reference. We added the following sentence: *“In turn, processes in the interior ocean cause spatial patterns of ocean heat uptake at the sea surface which define regional and global warming rates (Rose et al. 2014).”*
- 2.1.7 Reviewer #2:** 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).
- 2.1.7 Author Response:** We extended the previous section as follows: *“... ice-sheet and glacier mass and land water storage that combined amount to 60% of the observed global mean SLR over 1971-2010 (Church et al., 2013a).”*
- 2.1.8 Reviewer #2:** 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).
- 2.1.8 Author response:** Thank you for pointing out the regional role of salinity in understanding sea level changes. We adjusted the sentence accordingly and added *“; on regional to basin-scales, however, the role of salinity should not be neglected in sea level studies (e.g. Durack et al., 2014a).”*
- 2.1.9 Reviewer #2:** 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).
- 2.1.9 Author Response:** The sentence reads now *“However, the current climate models of the Coupled Model Intercomparison Project phase 5 (CMIP5) do not include land ice-sheet discharge dynamics and their contributions to the global mean SLR budget (Church et al., 2013b). Furthermore, simulating land ice-sheet discharge dynamics from the Antarctic Ice Sheets, might translate into large uncertainties in climate models ...”*
- 2.1.10 Reviewer #2:** 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.
- 2.1.10 Author 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.”*

**2.1.11 Reviewer #2:** Page 1204, lines 3-5: “. . . assumed to increase..” due to data sparsity.

**2.1.11 Author Response:** We start the sentence now with *“Due to data sparsity observed ....”*

**2.1.12 Reviewer #2:** Page 1204, lines 8-11: “We begin . . . to derive thSLR.” I suggest a rewrite.

**2.1.12 Author Response:** We re-wrote this to: *“We begin by introducing the observed and simulated datasets as well as the method to arrive at thSLR estimates. Subsequently, we calculate the simulated thermal expansion over the entire ocean grid for a number of CMIP5 models that have not published those time series yet.”*

**2.1.13 Reviewer #2:** 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.

**2.1.13 Author Response:** Like we state in the manuscript, the simplified parameterisation of the thermal equation coefficient is based on a simplification of the equation of state of seawater given in appendix 3 in Gill (1982) assuming a constant salinity of 35 PSS-78. It is, for example, included in the reduced-complexity Model for the Assessment of Greenhouse Gas Induced Climate Change (MAGICC) (e.g. Raper et al., 1996; Wigley et al., 2009, Meinshausen et al., 2011) but not published yet. “3500” presents the global mean ocean depth of 3500 m. We polished the paragraph for clarity.

**2.1.14 Reviewer #2:** Page 1205, line 19: psu -> PSS-78.

**2.1.14 Author Response:** We changed the unit of the practical salinity accordingly.

**2.1.15 Reviewer #2:** 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?

**2.1.15 Author Response:** Now we start this paragraph on our simplified approach of thSLR with: *“In order to derive thermal expansion estimates, and zostoga, from hemispherically or globally averaged vertical temperature profiles, rather than from sparsely observed and computationally expensive spatial 3-D fields of temperature, salinity and pressure ... ”* and further down we added: *“As a first step, we use time-dependent vertical global and hemispheric profiles of Theta from the CMIP5 models to test the reliability of thermal expansion estimates based on this simplified approach (Eq. 3).”*

**2.1.16 Reviewer #2:** 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 reader here.

**2.1.16 Author Response:** We incorporated the study by Forster et al. (2013) as follows: *“However, recent literature suggests that the rapid adjustment primarily due to clouds generates forcing variations that cause differences in the projected surface warming among the CMIP5 models even if radiative forcing is equally prescribed for each individual CMIP5 model (Forster et al., 2013).”*

**2.1.17 Reviewer #2:** 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.

**2.1.17 Author Response:** Our model climate drift discussion reads now as follows: *“... a “full linear drift” is removed by subtracting a linear trend based on the entire corresponding piControl scenario for comparison with observational time series. For our globally and hemispherically averaged thSLR time*

*series the sensitivity to the method of drift correction is less than 1% due to a small low-frequency (inter-annual and inter-decadal) variability in the evolution of this integral oceanic property. This contrasts the large low-frequency variability, e.g., in the sea surface temperature evolution (e.g. Palmer et al., 2009). For details about methods of climate drift correction in CMIP5 models see Taylor et al. (2012), Sen Gupta et al. (2013) and the supplementary by Church et al. (2013a)."*

**2.1.18 Reviewer #2:** 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.*

**2.1.18 Author Response:** We note now *"The adjustment of global mean SLR to changes in ocean mass is fast and linear (Lorbacher et al., 2012); thus in the long term, impacts of changing ocean mass on SLR may well become in the primary contribution to the trend in SLR."*

**2.1.19 Reviewer #2:** Page 1207, line 8: *". . .30% more time series of zostoga than previously published." A reference would be useful to a reader here.*

**2.1.19 Author Response:** We extended the text in brackets *"compare Table S1 and Fig. 1, e.g., to Fig. 13.8 in Church et al. (2013a)."*

**2.1.20 Reviewer #2:** 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.*

**2.1.20 Author Response:** We intended the URL for our complementary time series, but after your suggestion we now also provide the possible link for the currently published data.

**2.1.21 Reviewer #2:** 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.*

**2.1.21 Author Response:** We extended the sentence to *"For rcp4.5 in year 2100, the projected model median thSLR and its 90% confidence interval, for example, by the end of the 21st century amounts to  $0.28 \pm 0.06$  m (see Table 1 for more scenario results). The corresponding thSLR published by Church et al. (2013a) is  $0.27 \pm 0.06$  m. For all four RCP-scenarios our results indicate that previous CMIP5 multi-model ensemble estimates by Church et al. (2013a) have been robust, despite being based on 30% less models than used here (Table 1, S1 and Table 13.5 in Church et al. (2013a))."* Please note that our augmentation of observed thSLR estimates is not based on the number of CMIP5 models used but on the multi-model median percentage contribution from different depth intervals to thSLR.

**2.1.22 Reviewer #2:** 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.*

**2.1.22 Author Response:** Please find the following sentence in the introduction of the CMIP5 scenarios *"For projected time series beyond the historical simulations, we use the rcp4.5 simulations consistent with Church et al. (2013a)."*

**2.1.23 Reviewer #2:** 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.*

**2.1.23 Author Response:** We would like to highlight the robustness of simulated thSLR estimates here and have no intention to speculate about reasons that explain differences in model output compared to observations. We checked our results, re-arranged and revised the first part of this paragraph that starts now as follows: *"For the upper 700 m, our extended CMIP5 multi-model median rate of thSLR and its standard deviation globally amounts to  $0.57 \pm 0.03$  mm yr<sup>-1</sup> from 1971 onward to 2010 (Fig. 1b) and is*

*similar to the observed arithmetic mean ...”; near the end it reads: “For the altimetry period (1993-2010), our multi-model median is 1.45 mm yr<sup>-1</sup>, with 1.02 to 1.97 mm yr<sup>-1</sup> as 90% uncertainty, taking into account the contribution of thermal expansion to the global mean SLR from the entire ocean depth. This rate of thSLR equals the corresponding rate of 1.49 mm yr<sup>-1</sup> within the uncertainty 0.97 to 2.02 mm yr<sup>-1</sup> listed in Table 13.1 by Church et al. (2013a) and confirms again the robustness of simulated thSLR estimated presented by Church et al. (2013a) with 30% less models for a multi-model estimate than used here.”*

**2.1.24 Reviewer #2:** *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.*

**2.1.24 Author Response:** Please see our comments on your points 2.1.21 and 2.1.23.

**2.1.25 Reviewer #2:** *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?*

**2.1.25 Author Response:** Yes, your assumption is correct. We re-wrote the sentence for clarification: *“For an ocean warming occurring at depth below 3000 Kouketsu et al. (2011) estimate a similar thSLR over a 40-year period; based on observed and assimilated data it amounts to 0.10 mm yr<sup>-1</sup> and 0.13 mm yr<sup>-1</sup>, respectively.”*

**2.1.26 Reviewer #2:** *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.*

**2.1.26 Author Response:** The sentence reads now: *“For the upper 2000 m, the depth profiles of thermodynamic properties across CMIP5 models are largely aligned with observational depths profiles for Theta and S of the modern day (2005-2013) ocean provided by the Argo program (Roemmich and Gilson, 2009).”*

**2.1.27 Reviewer #2:** *Page 1209, lines 8-28: I really had a hard time following the thread here – I suggest a re-write.*

**2.1.27 Author Response:** Please see our comments on point 1.1.1 by Reviewer #1.

**2.1.28 Reviewer #2:** *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.*

**2.1.28 Author Response:** We provide now some direct quantitative comparison between observations and simulations *“Independent of the concatenated radiative forcing scenario we estimate an increase in the multi-model median contribution to thSLR in the upper 200 m during 1971 to 2010 by 11% (not shown).”*

**2.1.29 Reviewer #2:** *Page 1210, lines 14-16: Model dependent AABW formation rates – a relevant citation would be helpful.*

**2.1.29 Author Response:** We altered the sentence to: *“The thermal expansion related contribution to SLR from depths below 2000 m is larger in the southern hemisphere than in the northern hemisphere. This might be due to model dependent mixing rates forming Antarctic bottom water, that Wang et al. (2014) assigned to CMIP5 model biases in the Southern Ocean’s sea surface temperature.”*

**2.1.30 Reviewer #2:** *Page 1211, line 11: “. . . our results show that observed estimates of thSLR for the upper 700 m. . .”*

**2.1.30 Author Response:** This sentence in the discussion reads now: *“Secondly, we quantify the thSLR contribution from the entire ocean depth in order to complement observational estimates that are*

primarily available for the upper ocean layers down to 700 m (cf. Domingues et al., 2008). Sparse observational evidence points to non-significant contributions to global mean thSLR from depths below 2000 m during 2005 to 2013 (Llovel et al., 2014). Our results suggest that 21<sup>st</sup> century thSLR estimates derived solely based on observational estimates from the upper 700 m would have to be multiplied by a factor of 1.39 (with a 90% uncertainty range of 1.24 to 1.58) in order to be used as approximation for total thSLR originating from the entire water column. Correspondingly, our CMIP5 model analysis suggests that partial thSLR contribution based on hydrographic measurements from the upper 2000 m can be expected to account already for around 85% of the total thSLR and consequently have to be multiplied only by 1.17 (with a 90% uncertainty range of 1.05 to 1.31).

**2.1.31 Reviewer #2:** 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.

**2.1.31 Author Response:** We introduce the percentage augmentation of the total thSLR as contribution/share from different depth intervals when we describe Fig. 3 and pick up the numbers in the discussion (please see our response to your comments 2.1.27 and 2.1.30).

## 2.2 Technical remarks:

### 2.2.1 Text

**2.2.1.a R #2:** Page 1207, line 21: *Additionaly* -> *Additionally*

**2.2.1.a AR:** .corrected.

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

**2.2.1.b AR:** .corrected.

**2.2.1.c R #2:** Page 1212, line 9: *perists* -> *persists*

**2.2.1.c AR:** .corrected.

**2.2.1.d R #2:** Page 1212, line 16: *merdional* -> *meridional*

**2.2.1.c AR:** .corrected.

### 2.2.2 Figures

**2.2.2.a R #2:** *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.*

**2.2.2.a AR:** All figures are revised regarding fonts, size etc. (see point 1.2.10 by Reviewer #1).

**2.2.2.b R #2:** *Figure 1: The standard units of SLR in the literature are mm yr<sup>-1</sup> 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)?*

**2.2.2b AR:** 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 altimeter measurements. 3/ We still explain the numbers in the caption of Fig. 1a: *“the ratio in brackets indicates the number of models of published (solid lines) zostoga and recalculated (dashed lines) zostoga in this study based on simulated temperature and salinity fields.”*

**2.2.2.c R #2:** *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.*

**2.2.2c AR:** 1/ Please see our comment to point 1.2.9 by Reviewer #1. 2/ Thanks, we now mention the model outliers in section 4 of the main text.

**2.2.2.d R #2:** *Figure 3: Using the same vertical scale for each experiment would be much more useful to a reader.*

**2.2.2d AR:** 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.

**2.2.2.e R #2:** *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.*

**2.2.2.e AR:** In Fig. 4 we would like to point out the robustness of the equation of state expressed in the simplified version of the thermal expansion coefficient (Eq. 3) and how the results might differ among the scenarios as well as by using different depth intervals for the vertical integration. Please see our comment to point 1.1.2 by Reviewer #1.

**2.2.2.f R #2:** *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?*

**2.2.2.f AR:** 1/ 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 20<sup>th</sup> century warming in the climatological profiles.”* 2/ Please see our comment on point 1.1.1 by Reviewer #1 covering scenario dependent contributions of thermal expansion from different depth layers to thSLR. 3/ 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 ...”*.

(Boyer, T.P., J. I. Antonov, O. K. Baranova, C. Coleman, H. E. Garcia, A. Grodsky, D. R. Johnson, R. A. Locarnini, A. V. Mishonov, T.D. O'Brien, C.R. Paver, J.R. Reagan, D. Seidov, I. V. Smolyar, and M. M. Zweng, 2013: World Ocean Database 2013, NOAA Atlas NESDIS 72, S. Levitus, Ed., A. Mishonov, Technical Ed.; Silver Spring, MD, 209 pp., doi.org/10.7289/V5NZ85MT)

### **2.2.3 References:**

**AR: We thank you very much for the comprehensive additional literature suggestions!**