Author reply to reviewers comments on
Comparing tide gauge observations to regional patterns of sea-level change (1961-2003)
Under review for Earth System Dynamics

We would like to thank both anonymous referees and S. Dangendorf for their useful comments on the manuscript, which we think really improved the quality of the paper. In this document we provide a point-by-point response to all the comments, and indicate which parts of the manuscript have been changed accordingly.

Referee #1

I felt, however, that we need to see the uncertainties in each of the contributions (as well as a total – as shown in Fig.8a) and in the TG data. This would give a sound base on which to view the global findings, especially since we sometimes look at relatively small numbers.

We agree with this comment, and we have estimated uncertainties for the various contributions (new figure 3) and the total (new figure 4, lower panel) accordingly. The patterns are discussed in Section 3.1 in view of the uncertainties.

For the TG data, error estimates are not provided by PSMSL, and will depend on measurement instrument, location, maintenance, etc. The TG uncertainty in Figure 8a is therefore only based on the spread in the group of observations, and not on the uncertainty in each TG individually.

Referee #2

(1) Results/uncertainties/robustness conclusions:

(1a) – I strongly agree with the comment from Reviewer#1 copied below.
"I felt, however, that we need to see the uncertainties in each the contributions (as well as a total – as shown in Fig.8a) and in the TG data. This would give a sound base on which to view the global findings, especially since we sometimes look at relatively small numbers."

Yes, we agree this is a good idea too. Please see our reply to this comment of reviewer #1.

(1b) – Could the findings be dependent on the specific choice of data sets used for the contributions, particularly the dominant GIA (e.g., Peltier and Paulson?) and steric (e.g., Levitus and Ishii?) components, given that there are known regional differences between published products?

We agree that this needs to be discussed, and we have therefore expanded on this issue in Section 4.1.

Fig. 6 in Feng et al. (2013) shows differences between the Peltier and Paulson GIA models along parts of the European coast, over their study period.

Using Lambeck GIA instead of Peltier GIA improves the match between models and observations in 5 regions, but it decreases the match in the other regions. The resulting R^2 is 0.80 and the regression coefficient 0.95, which is a lower R^2 and similar regression coefficient as before. We therefore decided to use the Peltier GIA as the standard dataset in this study. This is now discussed in Section 4.1. We have not compared to the Paulson GIA, since Peltier and Paulson are essentially based on the same ice model (ICE5G) and viscosity profile (VM2), so the differences between Lambeck and Peltier are likely to be larger.

Fig. 11 in Cazenave and Nerem (2004) shows differences in regional long-term trend patterns between Levitus and Ishii thermosteric sea level products.

We realize that there are differences between the datasets, but had to use Levitus because they provide gridded temperature and salinity at various depth levels until 2000m, while Ishii and Domingues data are only until 700m, and Domingues data is not available in gridded format, but only as a global mean. This is clarified in the text (Section 2.2)

I suggest the authors to briefly articulate why they might have preferred/selected specific data products/models (as done, for instance, for the set of tide gauges and glacier observations used) and discuss in section 4 how these choices might influence their regional/local results.

Reasons are now provided in Section 2.2, and the influences of these choices are discussed in section 4.1.

(1c) – Church et al. (2011) showed that there are larger uncertainties affecting estimates of the contributions in the 1960s for the global sea level budget (their Figure 2b). In the latest IPCC AR5, the global sea level budget was assessed from 1970s rather than 1960s (Church et al., 2013, Box 13.1, Fig. 1).
So, would it be relevant to consider the 1971(2)-2003 trend period in the submitted manuscript? (at least to test whether it might have any influence on results)

We agree that this would be an interesting and relevant thing to do. However, changing the period is not as trivial as it sounds. The period discussed in IPCC AR5 is 1971-2010 and in Church et al (2011) 1972-2008. While for most of the data sources used in this paper it would be feasible to change to this period, the bottleneck is the glacier dataset, which ends in 2003, which is why we chose to end our analysis in 2003, as we state in the paper. The reason for choosing 1961 as a start year is to match the period studied in IPCC AR4, which was the latest IPCC report available at the time of initiating this project. This is now mentioned in Section 1 of the main text.

Moreover, examining the period 1972-2003: a) would require changes in the methodology due to the shorter period (e.g., the tide gauges are now selected based on the requirement that at least 20 years of data is present in the 43-year

period. Changing the period would therefore lead to a different TG dataset and require a full reanalysis of all tide gauge records); b) would still be a different period from those described in Church et al 2011 and IPCC AR5, and thus never fully comparable – which would be the goal of the exercise in the first place, and c) would increase the relative uncertainty and importance of accuracy of the nodal corrections which introduce variability at 20-year time scales. We therefore are bound to the trend analysis over the 1961-2003 period.

On the bright side, the analysis done in this paper does show that there is a certain degree of agreement between the changes observed in the tide gauges and the different contributions observed with other methods, despite the larger uncertainties for the earlier period. We therefore hope to have justified the period chosen, although it may seem a bit arbitrary.

(2) Results/discussion: new results/previously published results/conclusions

The level of the discussion (section 4) is not as strong as it could be for three reasons:
(i) It is entirely devoted to the closure of the global sea level budget while the results of the submitted manuscript mainly concern regional/local budgets. These new regional/local results should be discussed in view of previous published literature to clarify improvements (e.g., more contributions and locations considered, reduced uncertainties, etc).

We agree. We have now added a Section 4.1, which discusses the influence of changes in the contributions used to compare to the tide gauges. This part consists partly of the previous section 3.4, but is expanded by including the influence of uncertainties and the influence of other datasets. Also, results of recent work by Wöppelmann are discussed here. In addition, the part on the global mean budget closure (Section 4.2) has been shortened to leave more room for the regional results.

I also noted that there are no similar published studies (background, e.g.: Plag, 2006) cited in the introduction (section 1)

Apologies for this omission. The paper by Plag (2006) is now discussed in the introduction, as well as some other studies on regional patterns.

and that, in several occasions, results in terms of expected spatial patterns (Section 3) which have been described before in the literature are not associated with a citation (e.g., Tamisiea and Mitrovica, 2011).

This is because these references were already included in Section 2.2, where the data was described. We have now included references where appropriate.

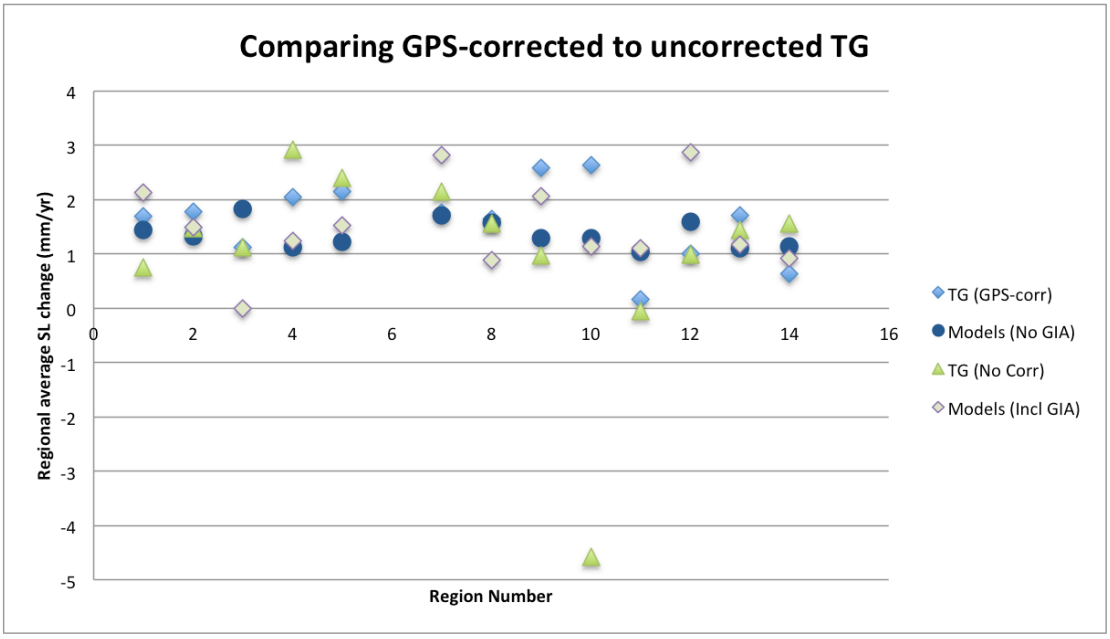
(ii) Please see comment (1b).

The influence of data source choice is now included in the discussion (sect 4.1).

(iii) The authors might not be aware of a recent publication by Wöppelmann et al. (2013, accepted online February 13, published March 6, 2014) in which a set of tide gauges have been corrected using GPS (under 2 main assumptions). Some of the tide gauge locations used in Wöppelmann et al. (2013) are likely to overlap with the tide gauges used in the submitted manuscript. So, the quality of the manuscript will certainly benefit if it further considers in its results/discussion the overlapping set of tide gauges with GPS correction and any implications for the conclusions of the paper.

We thank the reviewer for pointing us to this paper, which we weren't aware of at the time of submitting our manuscript. Indeed, there are 51 TG stations that overlap between the two datasets. Although the data presented by Wöppelmann et al. (2013) cover a different period, we could extract the GPS correction from their supplementary data, under the assumption that the GPS signal is stationary over the period studied (as is assumed in Wöppelmann et al., 2013). We use the extracted GPS signal to correct our TG data. We then compare, for the 51 TG only, the GPS-corrected TG signal to the models, where the GIA signal is excluded from the models to avoid double counting, because GIA is incorporated in the GPS-signal (among other processes). For the same 51 TG locations, we also compare the UNcorrected TG signal to the full model dataset. The region wise result is shown in the figure below.

The figure shows how the GPS correction gives similar or better results in 11 of the 13 regions tested, there were no GPS corrected stations in region 6. In the figure, the blue symbols (GPS corrected) are often closer together than the green symbols (no GPS). This test shows that, if GPS would be available for a larger set of TG models, it would be beneficial for the amount of explained variability. We have added this to the Discussion (Sect.4.1).



(3) Description of data sets/traceability calculations:
The description of the data sets/"models" used is rather incomplete. For example:

It is not until page 180, line 25, that the reader finds that the net contributions are on a 1 x 1 degree grid.

Thanks, this is now mentioned first thing in Section 2.2.

Data sets tend to have continuously updated versions. What is the downloaded version for the steric contribution from Levitus? Download date? Grid resolution? Although it is mentioned "monthly gridded" (page 174, line12), could the temporal resolution actually be yearly or pentadal?

That is a good point. Indeed, Levitus is pentadal, not monthly, this has been corrected in the text. The requested information has been added to Section 2.2.

The approach used by Landerer et al. (2007) is cited to include the effect of changes in ocean bottom pressure but there is no self-contained/brief explanation for the reader to quickly understand how the calculations were performed. I suggest the authors to provide a brief description in the text for data sets/calculations and relevant details as part of a supplementary material section.

Added to Section 2.2:
Using their Eq.3, we first compute the bottom pressure anomaly for each depth layer, using the area of each layer, the total ocean area, and the global mean steric anomaly of each layer. For each ocean grid point, the bottom pressure anomaly is summed over the available depth layers. Finally, the SSH change at each grid point is computed by adding the bottom pressure anomaly to the steric change of the water column.

As part of the suppl. material, I would also suggest the inclusion of a series of spatial maps for individual contributions which were combined in Fig.2. For example:
- Greenland ice sheet
- Antarctica ice sheet
- steric 0-2000 m: temperature plus salinity (SSH)
- steric >2000 m: temperature plus salinity (SSH)
- OBP (SSH)
- dam impoundment (TWS)
- groundwater extraction (TWS)
And, if ESD permits, to make results/maps available online after publication.

Good idea, done.

Minor details:
- page 171, line 8: new reference available: Holgate, S.J.; Matthews, A.; Woodworth, P.L.; Rickards, L.J.; Tamisiea, M.E.; Bradshaw, E.; Foden, P.R.; Gordon, K.M.; Jevrejeva, S., and Pugh, J., 2013. New data systems and products at the Permanent Service for Mean Sea Level.

Thanks, changed.

- page 171, lines 11-17: " process that is not included in this study is vertical land movement from subsidence or tectonics. These changes can be measured by GPS, which can then be compared to the TG time series, but only for short time series and in limited locations (e.g., Han et al., 2014). In this study we therefore focus on how much of the regional sea-level measurements can be explained without or before the use of GPS. Tide gauge measurements that are clearly affected by vertical land motions are therefore discarded (Sect. 2.1)." Please see Wöppelmann et al. (2013) and relevant references therein.

References have been added to introduction.

- page 173, line 16: "per region" What were the criteria used to divide into regions? Based on previous literature (e.g., Jevrejeva et al., 2006)? How the tide gauges from one region correlates with each other?

The regions are based on a common ocean basin, country and/or coastline. Where this lead to a very large spread (e.g. US West coast and Baltic Sea), the regions were determined by looking for the optimal correlation in subregions. This is now mentioned in the last sentence of Section 2.1.

page 175, lines 25-26: for example, could refer the reader to suppl. material for OBP spatial pattern if it is made available.

Done, see supplementary Figure 1.

Two examples of possible missing citations:
- page 176, lines 3-4: "The AL pattern (Fig. 2e) shows a strong meridional signal, indicating a decrease of pressure near the poles and an increase in equatorial regions." Has this pattern been observed/discussed before? Citation?

Added Wunsch & Stammer, 1997.

- page 186, lines 1-4: "From Sect. 4, it appeared that the closure of the global mean sea-level budget really depends on the period that is chosen. Uncertainties in the measurements of the contributions, such as ocean temperature or glacier mass change, rapidly increase when going back further in time." Citation?

This paragraph has been removed from the manuscript altogether, because it focused on the global mean, which is not the main point of the paper. The citation required would have been IPCC AR5, chapter 3.

Reply to comment by Sönke Dangendorf

For the tide gauge in the German Bight (probably Cuxhaven) it is important to notice that the PSMSL record contains a mix of mean sea level (MSL) and mean tide level (MTL) (see also PSMSL tide gauge documentation).

Thank you for pointing us to this issue. We have checked the PSMSL documentation for all stations used in our study, and found that for 5 out of 278 stations MTL has been included in (part of) the PSMSL time series. We have removed 4 stations from our analysis, and replaced the Cuxhaven record by the data you provided. We note that the trends of the 4 discarded time series were no outliers like the Cuxhaven record, and the removal of these records has no influence on the main conclusions of the paper.

We also thank you for providing us with data of 12 tide gauge stations in the German Bight area, which we have added to our analysis (included in region 8).

In the process of checking the tide gauge documentation, we came across recently updated time series in Denmark, following the Technical Report 13-15 'Sea level data 1889-2012 from 14 stations in Denmark Mean, maximum and minimum values calculated on monthly and yearly basis including plots of mean values' from the DMI of September 2013. This affected 9 records in region 9 of our study (order +/-0.1 mm/yr), and these have been updated accordingly. The mean trend of region 9 does not change as a result of these updates.