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Interactive comment on "A comparison of data weighting methods to derive vertical land motion trends from GNSS and altimetry at tide gauge stations" *by* Marcel Kleinherenbrink et al.

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General comments:

This paper addresses the methods of estimating the linear trend of the vertical land motion (VLM) at tide gauge (TG) stations using GPS and satellite altimetry minus TG observations (ALT-TG). Since the satellite altimetry and most of the GPS data are not provided at the TG location itself, the paper focuses on the different choices to extrapolate these datasets to the TG location. Some of the investigated choices have been used in past sea-level studies and are relevant for comparison purposes, whereas others (especially the treatment of satellite altimetry observations) are new in this pa-

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Discussion paper

per. The comparison of the different choices provides valuable information to other scientists working on this subject and the preferred choices of the authors lead to a reduction of the VLM differences between GPS and ALT-TG techniques compared to previous studies.

The writing is clear globally, but some sentences (details given below) need clarification or correction. Parts of the methodology need also clarification (for instance, concerning the pole tide or the use of errorbars). The authors focus on describing the results of their analysis without going in depth with their discussion and implications, which undermines the conclusions to some extent.

Specific comments:

The title: I would suggest changing “data weighting methods” by “methods” or “approaches”.

The abstract needs to be improved to make it more clear and self-contained. As it is now, it looks like a compressed listing of the results so it may be hard for the readers to understand without a minimal background and way out (recommendations or take-home messages).

P1L18: several VLM processes are modelled. This is very ambiguous. It may be true (we could model anything), but only GIA models are actually being used. Later on, it’s said that local VLM processes cannot be captured by models.

P2L11: Actually, Santamaría-Gómez et al. 2017 did not conclude on the accuracy, but they show bigger differences between ULR and NGL than ULR and the other solutions being compared. Compared to the other solutions, NGL velocities also had larger errorbars to accommodate these differences.

P3L16-20: In addition to the ocean signal, the ALT-TG correlation can be used to infer the correlation between the TG record and the VLM *of* the TG itself, especially with low-pass filtered series as you did, for instance if the VLM at the TG is not linear during the altimetry period. This is inseparable from the ocean signal you mention (see discussion in Santamaría-Gómez et al. 2014, JoGE).

P4L5-7: Note that all the ULR solutions have been computed using the CATREF software (Altamimi et al. 2016). CATS has been used to re-estimate the trend uncertainty, but the estimated trend does not change statistically. The “slight change in trend” comes together with the increased uncertainty and it is just the consequence of inverting a more complex covariance matrix in time with probably a small contribution also from the different use of spatial covariance between CATREF and CATS.

P4L29: I assume the approaches using the longest time series (5) and the smallest error (6) are also using the closest GPS station, but it would be better to clarify.

P5L3: It would be better to add here the equation of the approach (8) to see how the approaches (4 distances) and (7 weighted mean) are combined. To me, this is in theory the best approach since it uses more information available that the other approaches. However, the way the distance and uncertainty are combined may still be very important. Also, the propagation of the VLM uncertainty from the GPS to the TG should be commented on as it varies for each approach.

Section 2.3: I am confused about which altimetry series did you use and when. You say that an “additional” filtered set was used to test interannual correlation (P6L10-12) and that before estimating the correlation, you removed residual seasonal cycles (P6L15). So, where do these residual seasonal cycles come from if the series were filtered? Is the yearly moving-average not enough to remove unmodeled Sa tides from altimetry or the low-pass filter allows for annual variations at the TG? Finally, the filtered series were kept for the analysis (P7L1), but Figures 1 and 5 show both filtered and unfiltered series.

P6L13-15: Is it necessary to remove the ocean pole tide from the ALT and TG records? Are they significantly different? Concerning the solid Earth pole tide, I would suggest adding that the RADS solid Earth pole tide model is consistent with the Desai’s model concerning a linear mean pole trajectory, so that the interannual vertical deformation is preserved in the TGs when subtracting one and adding the other (I assume this was the purpose, but it could be said explicitly). However, what is the rationale for adding the IERS solid Earth pole tide to the TG records after removing the RADS model (P8L1-3)? Contrary to the RADS or Desai's models, the IERS solid Earth pole tide model does not correct the interannual deformation (see King and Watson, 2014). The interannual deformation was removed by the RADS model and is not restored by the IERS model. In doing so, the ALT-TG VLM will not be consistent with the GPS VLM that is still affected by this interannual deformation from the IERS model. If I understood your treatment, I think you should add the Desai's model in both cases.

P8L5-8: note that the IERS conventions were updated about this issue in June 2015, and even though the issue still persists, most of the GPS VLM estimates are based on the old IERS implementation, at least the ULR and NGL solutions you used. The 0.1 mm/yr error arises in a regional sea-level reconstruction using GPS-corrected TG records with old IERS model. The VLM effect at individual GPS sites may be 3 times larger (King and Watson, 2014). Explain how this error is corrected using the mass redistribution fingerprints.

Section 2.4 could be integrated into the 2.3

P9L10: change ULR by ULR5, which is the solution used by Wöppelmann and Marcos, 2016

Section 3.1 and elsewhere: direct/indirect are ambiguous terms. I would suggest using GNSS and ALT-TG for consistency.

Figure 3 and elsewhere: change spread by range

P10L7: change solutions by weighting methods for consistency or even to approaches, which may be more appropriate.

P11L1-4: The range values are driven by the extremes, which are obtained from the “mean”, “median” and “inverse distance” approaches. None of these approaches is using the information provided by the VLM errorbars, which can be as large as 1 mm/yr, and only the “median” approach is less affected by outlier VLM values (but only if we have a large sample and we assume the VLM estimates in 50 km follow a Gaussian distribution, which may not). I would suggest using the interquantile range instead of the range to evaluate the dispersion of the different approaches.

P11L6-7: In relation to my comment before, these global estimates of spatial variations of VLM were given as 1 sigma standard deviations. You would have to multiply them by 5 or more to obtain something close to the range of the extremes (for instance, by 10 in areas with strong GIA gradient). On top of that, a global figure will never fit all locations which will be underestimated or overestimated.

P11L11-16: Table 3 shows the VLM differences at 70 TGs between using the closest ULR5 value and 8 different approaches with the NGL velocities. It is surprising that the RMS of the differences is the highest for the closest NGL value (approach 3), which will use the same GPS station as in ULR5 for many TGs, whereas it is minimum for the median of the NGL values 50 km around the TG (approach 2). The WRMS of the differences between ULR6 and NGL is about 0.7 mm/yr. You are using ULR5 and not ULR6 here, but the RMS for the closest NGL station is two times larger and appears unreasonable to me. It may be due to the VLM errorbars not being used. Also the ranking of the methods in this table and that in Figure 7 matches exactly as if the ULR5 velocities were providing the same benchmarking information as the ALT-TG trends. Is this coincidental?

Figure 4: Change “reduction” by “change” or invert the sign of the scale for consistency (positive reduction is good, otherwise is bad).

Figure 7: It would be easier to read the legend if the mean RMS of each line, with fairly constant values, is added on the right of the figure, for instance.

P16L6-8: Please explain how the median takes into account the standard deviation of the GNSS trends as in a weighted mean (approach 7). Also, any approach using more than one GNSS trend in 50 km around the TG is filtering the spatial variations in VLM, including the variance weighting (weighted mean) approach. From these lines on, it is decided that the median approach is the best candidate, but I'm not fully convinced and I would suggest adding more discussion on these results. For instance, the fact that a simple median provides better results than the more complex approach of including distance and uncertainty information needs better discussion. The combination of the distance and errorbar information is not trivial and may depend on the TG location, so this may have flawed this approach. However, even the weighted mean is using additional relevant information, but it is ranked after the median and the mean. This makes me think whether the evaluation using the ALT-TG trends is the best benchmark. For instance, the ALT-TG VLM uncertainties are probably large as well, with important variations among the TGs (correlation, length of the series, etc), and it seems to me that they were not used for the benchmarking either. On the other hand, the alternative explanation would be that the trend uncertainties of the NGL solution are not providing a useful value of their precision. For instance, it is known that there are trend biases not explained by their formal uncertainty and caused by a combination of the time series length and non-linear effects like seasonal signals, discontinuities, interannual deformation, transients, etc. Different processes would also bias the ALT-TG trends (orbital error, altimeter bias drift, etc.).

P2L20 and elsewhere: the correct reference for the ULR5 solution is Santamaría-Gómez et al 2012 Glob. Planet Change.

I fully agree with the last sentence and I would add that, whenever possible, one should always inspect the data being used. A much extended (and faster) practice is always using the trend uncertainties together with the trends, because they (should) carry relevant information on the linearity of the observed series.

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