

Rebuttal - reviewer 1

We would like to thank the anonymous reviewer for the thorough review of our manuscript. We update the manuscript such that it answers the questions and implements the recommendations of the reviewer. Below we wrote a point-by-point response to the reviewer comments.

General comments:

Accurately determining vertical land motion at tide gauges is an important scientific issue with crucial societal implications associated with future relative sea levels at the coast. The study by Kleinherenbrink et al builds upon the most recent estimates of vertical land motion from GNSS data analyses and the combination of satellite altimetry and tide gauge data. The authors perform a detailed and honest critical review of the estimates available from the literature, while they provide ways to overcome some of the limitations. For instance, wherever there is no permanent GNSS antenna at the very top of the tide gauge (co-location), but multiple GNSS receivers are in the vicinity, they explore different methods to deal with this situation. In addition, they delve into the details of the best possible way of deriving estimates from the combination of satellite altimetry and tide gauge data with insightful outcomes too.

The manuscript reflects a sound scientific approach. The methods applied are clearly outlined. Some minor technical details are missing, however, and require clarification (see below). The results are discussed in detail, and overall the results and discussion provide a substantial contribution to the area of research on determining vertical land motion at tide gauges. In addition, the manuscript is well structured, clear and concise, and the conclusions are supported by the data. A somewhat negative note is that I miss that the authors are not providing their best estimates on vertical land motion (with the error bars) in a supplemental material. Similar to the studies they build upon, they should provide their estimates for future investigation. Perhaps this can be considered by the authors for the final version. In conclusion, my suggestion is a minor revision before publication.

We were already planning to make the data publicly available. The vertical land motion estimates for all altimetry-tide gauge correlation settings and the median GNSS approach are now provided in the supplementary material. The aforementioned sentence is adjusted accordingly.

The technical details and other comments are discussed below.

Other (minor or technical) comments:

p.1, Title: The term “weighting” does not correspond to several of the approaches examined in this study. See also 1st and 10th lines in the abstract). In addition, I would change “derive” to “estimate” to underline that behind the scenes the results from these methods are based on an estimation procedure, not directly observed.

We changed the title to: “A comparison of methods to estimate vertical land motion trends from GNSS and altimetry at tide gauge stations.” The text is adjusted as well, so ‘approach’ is used.

p.1, Lines 2-3: It should be clarified that these methods are considered to deal with the situation of multiple GNSS stations nearby a TG.

Two sentences are added at the beginning of the abstract and the existing text is adjusted accordingly. "Global Navigation Satellite System (GNSS) are usually not co-located with Tide Gauges (TGs). Therefore trends from neighbouring GNSS stations are combined to estimate a VLM trend at the TG. This study compares eight methods to estimate Vertical Land Motion (VLM) trends at 570 TG stations using GNSS."

p.1, Line 20: Ostanciaux et al. did not established the magnitude that can reach the GIA effect. I suggest to quote an original early reference such as Gutenberg, in Bull. geol. Soc. Am. (1941).

The reference to Ostanciaux is replaced with the one to Gutenberg.

p. 1, Lines 21-22: The statement that trends at TGs are affected by erosion is not obvious to me. Please, quote a reference that demonstrates this relationship.

We changed the sentence. Erosion and gas extraction are removed. So now the sentence reads: "including water storage, postseismic deformation and anthropogenic activities (references)."

p. 3, Line 26: For the sake of consistency, I wonder why Hector is not applied for the GNSS trends too. Can you develop the §with your arguments, please?

We wanted to have the best possible GNSS trends for which we do not have to apply any screening. The trends are strongly affected by jumps in the time series. Based on the Blewitt et al. (2016) their MIDAS method has the smallest equivalent step size detection. Therefore we selected this method. We cannot apply the same method to the altimetry-tide gauge time series, since the time series cover only ~200 months, which is rather short for the MIDAS approach. We rephrased several sentences in Sect. 2.1.1 to clarify the reasoning.

p.3, Line 31: The issue is primarily that the differential land motion between the GNSS antenna and the tide gauge is not monitored locally, for instance via repeated levelling campaigns. Thus, a lack of information.

A sentence is added that addresses this issue.

p4. Line 7: I guess "However" is not correct here. Considering revisiting this since the decrease in accuracy is not associated with the use of the software and its advantages.

We rephrased the sentence and split it up in two separate sentences. "The software is also able to estimate and detect discontinuities that occur due to earthquakes and equipment changes. Even though a large fraction of the trend estimates have formal accuracies better than 1 mm/yr, undetected discontinuities might significantly bias the estimated trends (Gazeaux et al., 2013)."

p4. Line 11: the term measurements is not appropriate here, the positioning time series are outcomes (estimates) of the measurements analysis.

The term measurements is replaced by estimates.

p.4, Line 12: Please, develop how the scaling is performed (what is its origin).

We added an equation and a reference to Wilcox (2005).

p.4, Line 12: typo in "devations", should be "deviations"

Updated.

Section 2.1.1: did you screen the GNSS time series for apparent transient processes that would impact (question the validity of) the linear trend estimation?

No, we did not screen the time series, because we use the pre-computed trends from MIDAS. Any non-linear behavior might bias the trend, but it will also inflate the error bars as described in the section. We now clearly state that we do not apply any screening.

Section 2.1.2: See above my comment on the term “weighting”. Within this section you use the term “approach” which is definitely more appropriate. The term weighting is either replaced by ‘method’ or by ‘approach’ throughout the manuscript.

p.4, Line 17: at some point (here or later in the manuscript) you should discuss this vague statement “a record long enough”.

We changed the sentence, such that: “currently only a few have a record that ensures a trend accuracy of 1 mm/yr or less”.

p.5, Lines 3-5: You detailed the “obvious” relationship of method [7], you should detail that of method [8], which is less obvious to me.

An equation is added for method [8].

p.5, Line 7: Holgate is published in 2013 (not 2012). See also reference list (p.22, Line 33).

The reference is updated.

p.5, Lines 22-25: Please, rephrase. I had to read the sentence several times. Consider splitting it into two sentences.

The sentence is rephrased.

p.5, Line 30: Please, develop the rationale for 250 km (why not 200 km, or 270 km, or ...).

Outside of the equatorial regions and the continental shelves, ocean correlation scales are below 250 km (Ducet et al. 2000; Roemmich et al. 2009), so we do not expect significant improvements if observations outside of the 250 km range are included. We could probably find some long-shore correlation along the shelves over longer distances, but it would not be appropriate to take those observations into account, since long-term trends do not have to resemble anymore, i.e. large-scale signals like GIA trends are not equal to the TG location anymore. On top of that, at least one track of the altimeters is always passing through the 250 km region. Making the radius smaller, reduces the number of observations substantially, especially at lower latitudes.

p.7, Table 2: The information conveyed by this table is too technical. Consider moving it to an Appendix or Supplemental material. Clarify what are these differences (related to J1? TP-J1, then J2-J1?). In addition, add error bars to the parameter estimates, and/or say if all these parameters are statistically significant at the 95% level.

We moved the table to the appendix.

The caption is extended, to clarify what the differences mean.

Since we do not apply a full error propagation on these values, it is difficult to determine whether they are statistically significant or not. If we use the variances (σ^2) of the residuals to compute the, then the errors for the coefficient $c = \sigma^2 (A^T A)^{-1}$, several of coefficients are not statistically significant (primarily in the equatorial regions for TP-J1). This is primarily, because we average

the altimetry differences per latitude band (1/8 degree wide) first and then compute the polynomials. The degrees of freedom (~10 dof) is therefore rather small to estimate proper statistics for the equatorial regions. We therefore stick to the polynomials as used in the Ablain's paper, which is referenced to in the text.

p.8, Line 4: “are computed” should be “is computed”.

Updated.

p.9, Lines 6-7: The sentence has a problem. I don't understand, please rephrase.

The sentence is rephrased.

p.9, Line 15: What is the rationale for the 50km radius. Please, develop.

Most studies involving sea level include observations within radii of 10-100 km. We took the radius right in the middle, but we could have increased or decreased the radius. A radius of 100 km would include observations with errors due to local VLM of more than 0.5 mm/yr on average (Santamaria-Gomez et al., 2017), while taken a small range reduces the number of trends substantially. Tests, however, demonstrated that similar results are obtained for 30 and 70 km, but with slightly less or more trends estimates, respectively. Several sentences are added in the methodology section.

p.10, Table 3: Consider adding a mnemonic keyword (after the number) to designate the approach, for instance “closest”, “longest”, etc.

We added keywords in the table.

p.13, Lines 7-8: Can you quantify the amount of reduction using equation (4)?

Yes, we can. The median of the spectral indices (for the same stations) is closer to zero for higher correlation settings. We added a line with the statistics.

p.20, Line 2: Strictly speaking, “observations” is not appropriate (estimates? Data?)

We guess this should be line 7. The term observations is replaced with data.