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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.

Anonymous Referee #1

Received and published: 13 December 2017

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,

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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.

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.

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.

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).

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. p.3,

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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?

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.

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.

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

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

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

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?

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.

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

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.

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

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

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

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or. . .).

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.

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

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

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

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

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

p.20, Line 2: Strictly speaking, "observations" is not appropriate (estimates? data?)

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