Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-29-RC1, 2020

Interactive comment on "The Zone of Influence: Matching sea level variability from coastal altimetry and tide gauges for vertical land motion estimation" *by* Julius Oelsmann et al.

Alvaro Santamaría-Gómez (Referee)

alvaro.santamaria@get.omp.eu

Received and published: 18 June 2020

This paper addresses the methodology of estimating vertical land motion (VLM) from the combination of satellite altimetry (SAT) and tide gauge (TG) observations. The work by J. Oelsmann, M. Passaro et al. builds upon earlier studies concerning the selection of the most suitable SAT observations that show high temporal correlation with high-frequency TG observations. In addition, and contrary to past studies, they use dedicated coastal "retracked" along-track observations to reduce the VLM differences with respect to co-located GNSS VLM estimates, which are taken as ground truth.

My feeling is that this paper is a significant technical contribution to the estimation of

coastal VLM from altimeters and tide gauges, which is a relevant topic for the journal. I generally agree with the authors that advances in this field call for better consistency of the sea-level observations from tide gauges and altimeters. To reach this goal the authors focus on using altimeter observations closer to the coast and high-frequency tide gauge data. The authors show that areas of high consistency between both datasets, which they call "zone of influence" or ZOI, can be defined based on different statistical criteria. The comparison of SAT-TG VLM estimates against GNSS VLM estimates is improved, but the typical differences between both VLM estimates are still much larger than their respective formal errors. This indicates that there are still missing pieces to be accounted for in the VLM estimates from SAT-TG or GNSS or, likely, in both.

Below a few minor comments that hopefully will improve the quality of the paper:

Abstract: ZOI should be defined in the abstract, if space allows it.

L24&59: Many thanks for citing my 2017 paper, but there is no need to add it twice to the reference list. 2017a/b should be 2017.

L62: change accuracy by precision

L271-273: "To confine the ZOI, we select subsets of the data containing the best-performing statistics (i.e. highest correlation, lowest RMS SAT-TG or residual annual cycle) above the Xth-percentile according to the distribution of the statistic S in a 300 km radius around the tide gauges." » It is not clear whether the TG and SAT series were detrended and deseasoned before comparing them. If the TG and SAT series were not deseasoned and the seasonal variation is prominent in the series, then there is the risk that the three metrics are telling us almost the same thing, that is, the impact of the amplitude and phase differences between the seasonal signals in both series. The correlation and the RMS of the differences may be more representative from deseasoned series, as done in past studies. In addition the authors fit the seasonal variation together with the linear trend in the SAT-TG series, i.e., the residual seasonal variation may play a minor role in the estimated VLM and its uncertainty. I'm not sure if this is what the authors intended. It may not have a significant impact on the selected ZOI areas, but the authors would be at least using more independent criteria.

L367: The authors take the GNSS VLM as ground truth, and that is fine, but are the formal VLM errors similar among the TG and GNSS stations, respectively? Formal errors can provide valuable information for the VLM validation and this should be accounted for when assessing the VLM differences (WRMS instead of RMS for instance).

L377-380: the differences between the SLA trends in ALES and AVISO are quite significant. From Table 2 it appears that the median SLA value from AVISO is 1 mm/yr higher than that from ALOS (also seen in Fig. 4). Is this correct? If so, how would you explain this difference?

L386: "absolute median bias of trend differences" can be confused with the median of absolute VLM differences. I suggest changing this by "median of the VLM differences" or similar here and elsewhere.

L400-401: Some comments on results shown in Table 2: the unweighted RMS from GESLA is smaller than that from PSMSL, but this is probably because the median value is closer to zero. The weighted standard deviation or any other measure of dispersion (interquartile range) that does not include the mean/median value would be more appropriate here. Also the formal VLM rate uncertainties are higher with GESLA than with PSMSL, even with a spectral index slightly closer to zero. This means the noise (especially the power-law variance) of the residual series (trend and seasonal variations removed) in the GESLA VLM series is larger than that from PSMSL or that the GESLA series are significantly shorter or less complete. In that case, the choice of using GESLA instead of PSMSL would need better argumentation. There may be also a TG trend bias between GESLA and PSMSL of around 0.3 mm/yr. This is probably not significant, but it may be worth discussing.

L421-422: the power-law variance may have also changed, maybe producing a significant improvement of the VLM formal errors.

L429-436: The discussion in this paragraph is not very clear and would require improvements. We only need a single SLA series to estimate VLM from SAT-TG. This SLA series can be obtained using different strategies as the authors have discussed: spatial averaging/filtering of SLA data, the single most correlated SLA series, the single closest SLA series, etc. The more similar the selected SLA series is to the TG series, i.e., the smaller the SAT-TG differences (again excluding the seasonal variations that are captured by the model fitted to the SAT-TG series), the more precise VLM will be obtained in terms of formal error. This is a metric very easy to interpret. Note that interpreting how the SAT-TG VLM values compare to the GNSS VLM is much more complex and is, in general, not a strong criterion given the large differences between both. The smaller quantity of averaged SLA should not be blamed if they represent increasingly consistent SLA series with respect to the TG series. A different explanation for the bad results with >80% thresholds could be that the RMS metric is not telling us whether the SAT and TG series are more similar, especially if the seasonal signals were included in the RMS as per my comments above. In addition, the RMS alone is not directly tied to the autocorrelation of the SAT-TG series (i.e., the spectral index), which is another important metric to assess the consistency of the SLA and TG series.

L466-478: the discussion here is interesting, but an important point should be stated more clearly. The optimal ZOI in Fig. 5 a&c was retained by assessing the consistency between SAT and TG series. On the other hand, the local optimal ZOIs in Fig. 5b are defined by comparing SAT-TG and GNSS VLM estimates. It is therefore not surprising that different optimal local ZOIs are obtained and that there is not an optimal threshold that fits well all sites. In addition, imposing the GNSS VLM as a criterion to the computation of the SAT-TG VLM would remove its independent nature.

L487-488: I guess you derived the 3 to 4 inflation factor from the color range in Fig. 6, but Table 2 actually shows that using different SLA data does increase VLM uncertainties by less than 10% (comparing ALES-PSMSL to AVISO-PSMSL).

Alvaro Santamaría, June 2020

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-29, 2020.