Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-29-RC2, 2020

Ocean Science
Discussions
Open Access

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.

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Received and published: 21 July 2020

This manuscript by Oelsmann et al provides a contribution to the improved determination of vertical land motion (VLM) at tide gauge locations using satellite altimetry. The work advocates for a refined approach to selecting valid altimeter data in a variable 'zone of influence' around a specific tide gauge to improve the accuracy and precision of VLM estimates. The team make incremental advances using a coastally retracked altimeter dataset and tide gauge data with various temporal resolutions. The focus of the paper has a rich history in the literature and this technical contribution provides a worthy contribution to progressing the method and ultimately achieving improved VLM estimates around the coast. The demand for improved VLM is clear – VLM is a vexing issue with manifold impacts across the scientific and the broader community.

The manuscript is well presented, well crafted and generally presents a compelling and comprehensive case worthy of publication in this outlet. Noting my support for this manuscript, I have a few substantive issues that I feel would benefit from consideration and discussion/revision in the paper. I present these issues below, followed by a list of technical clarifications and minor suggestions/corrections that may benefit the manuscript.

Main Issues:

Central to the manuscript is the comparison of various altimeter minus tide gauge differences against a single 'ground truth' GPS solution. Significant differences in the solutions derived from different variants (e.g. Fig 4b) are evident, highlighting the sensitivity of the problem. This suggests that fundamental issues remain to be understood in one or more of the constituent components used (i.e. any one or all of ALT/TG/GPS). Relating to this point and notwithstanding my remarks above regarding the relevance of this work, my sense from reading the manuscript is that the authors pay too little attention to discussing a few key issues that I argue should otherwise appear in a paper such as this:

1) Despite the technique itself originally emerging at the time investigators were using tide gauges (and thus VLM) to validate the stability of the altimeter measurement system, the authors do not seem to acknowledge the potential for regionally correlated error within the altimeter – brief mention of the importance of this point in the heritage (and current use) of the technique is also surprisingly lacking. Rather, the authors make the bold assumption of 'no instrumental drifts' (pg 3, ln 75) as the sole mention of possible systematic error in the altimeter record. I note the authors exclude TOPEX/Poseidon from the coastally retracked dataset given waveforms are presently unavailable, however, it is included in the AVISO product. The well-known issues as-

sociated with TOPEX (brought to light using this technique and later confirmed) put the issue into focus. Further, inspection of solely orbit differences in a regional context places even greater emphasis on the issue (e.g. Couhert et al, ASR (2015) who report on the significant challenge of achieving 1 mm/yr orbit stability at regional scales). The use of a consistent orbit across all missions in this current paper (which I assume are the GFZ Ver13 SLCCI orbits?), will assist to mitigate this effect but it is likely that mission-specific, spatially-correlated 'whole-of-system' drift will remain in the residuals and undoubtedly make at least a contribution to the differences observed in the current paper. I contend that 'no instrumental drifts' is somewhat of a misnomer in the context of whole-of-system altimetry at regional scales. Not mentioning these broad issues nor the heritage of the ALT-TG technique in this regard would be disappointing for a paper such as this.

2) A second issue of less significance relates to the treatment of the tides in the high frequency tide gauge data. The authors attenuate the tide with a 40-hour loess filter (pg 8, ln 217). Clarification and further defence of the filter strategy would be beneficial here (e.g. both are likely small, but I wonder what % of *non*-tidal variance is attenuated by the filter, and what % of *tidal* variance at longer periods is retained (but removed from the altimeter with its model-based correction)). The comment regarding the temporal resolution of the DAC used for the high rate TG data felt ambiguous here (ln ~219), and should be clarified with respect to consistency with the filtering approach. The magnitude of the difference in tides between (coastal) altimetry that uses a global model and at the tide gauge is an important issue that warrants at least a mention in the discussion.

3) Finally, the improvement observed between along-track altimetry v gridded altimetry is presented in the context of 'coastal altimetry' driving the improvement. I'm very pleased to see these results however I feel that conclusion needs some further defence. From my understanding of Fig 5d, the lower quartile of the data commences at just under 30 km from the coast (and increases from there). Discussion of Fig 5b in the text

(pg 22, ln 470) refers to a much smaller subset in which the mean distance was 33 km. Given the benefit of retracking is 'most pronounced in the last 20 km' the authors should further evidence what % of their data is within this threshold and thus further elucidate whether it is simply the influence of along-track data versus its retracked equivalent. To be clear, I am not asking for the former to investigated in the same way – the benefits of the retracking in general terms are clear – I just wish to ensure the conclusions are appropriately elucidated.

Technical Issues:

It is unclear throughout if the RMS statistics computed against the GPS time series take into account the GPS uncertainties? I expected to see WRMS mentioned rather than RMS.

In section 2.1, I assume the GFZ Ver 13 SLCCI orbits are used homogeneously for all missions, but this is only inferred. I suggest adding detail to Table 1 for clarity and citing the relevant Rudenko et al paper.

I was pleased to see the data has been cross calibrated using the MMXO approach (pg 6, ln 165). Some additional detail here would be beneficial: I assume this provides a location-specific and mission-specific radial bias, computed using the *identical* dataset (Table 1). Some further detail on this would be useful here. Further, some comment on the relative trends observed (and applied?) between the Jason-series and ERS/Envisat missions (using the MMXO approach) is warranted here.

Regarding the pole tide in Table 1, could the authors confirm they only apply the solid Earth component of the pole tide (noting the pole tide that is present in the GDR combines the ocean plus solid earth component, hence needs to be multiplied by h2/(1+k2).

Regarding the ocean tide in Table 1, it might be worth adding a paragraph to further discuss the long-period tides that are/are not considered in the altimeter data. The current treatment of the tide in the high frequency dataset at least should retain any

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long period constituents – confirmation that is the case with the altimetry would also be beneficial.

Regarding the cubic interpolation to the hourly tide gauge data, I'm assuming you don't use this across outages larger than some threshold?. Worth clarifying.

In Figure 2, this may be more beneficial to show the data in panel 3 over the data in panel 2. Then, in the now vacant third panel, show the same but with the annual and semi-annual terms estimated and removed. Note the caption reports "SAT minus TG" but the y-axes report "TG-ALT".

In Figure 4, I found Fig4b and d difficult to interpret. I feel they need to be introduced earlier and/or more comprehensively explained as I felt it took me a few reads to get the point.

Minor Issues:

The word 'frequent' is used throughout in the context: 'high frequent data'. I suggest this should be 'high frequency'.

The terms accuracies and uncertainties are used throughout when singular is arguably more appropriate. E.g. ln 11 in the abstract.

Suggest a search and replace for units – some (e.g. km) do not have a space beforehand after the quantity.

L15 p1. Perhaps increasing instead of progressing? Perhaps . . .the spatial scales of the coherency in coastal sea level trends increase.

L20, p1. . . .as the sea level rise signal itself. . .

L21, p1. . . .can regionally account for a large fraction of the. . .

L35, p2. I had expected TG instability to at least be mentioned here. I know it comes later, but consider a mention here.

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L40, p2. “global” trends range 1 to 3 mm/yr. Do you really mean “global”? If you do then I feel this could be made more specific to a time period of interest as it feels too vague.

L54, pg2. . . .solutions against those using measurements. . .

L59, pg3. “considerable” isn’t the right word here, consider change.

P3 – See main issue #1.

L80, p3. “synergistic applications” isn’t quite right, consider change.

L87, p3. . . .further develop. . . instead of “carry on” the latest progress? Suggest characterisation and quantification instead of detection?

L96, p4. “were achieved” c.f. what?

L108, p4. suggest: . . .however, they reported insignificant improvements of. . .

L126, p4. no comma needed after processes.

P6 – See main issues 2 and 3.

L190, p7. The PSMSL constitutes. . .

L191, p7. . . .for most sea-level research.

L222, p8. low latitude maybe ambiguous for some readers. Do you mean nearer to the equator or poles? I note you use high-latitude later.

L240, p8. I’m unsure that “matching” is the correct word. “Differences formed using” perhaps?

L252, p9. . . .the capability to compare. . . no comma after altimetry.

L264, p9. duplicate as

L310, p12. performance not performant

L330, p14. “next to SLA correction” could be significantly improved.

L∼340, p14. Some comment from the authors regarding whether they consider PL+WN more appropriate than GGM for example would be useful.

L349, p14. needs “ellipsoid” inserted after TOPEX/Poseidon.

Figure 3: The distribution is unavoidably clustered. Does residual VLM show any spatial patterns? L370, p15.is “also” needed?

L399 p16. Suggest improve “which stronger deviate”.

L402, p16. This is the opposite sign however. . .

L435 p18. assuming issues with e.g. tide model errors, the method presented will likely have a optimum threshold that still enables sufficient spatial averaging to mitigate that systematic effect as much as possible. See main issue 2.

L439, p18. Is bisector the correct term? 1:1 line?

L441, p18. Unclear what you mean by outstrips improvements induced by. . .. Clarify.

Figure 4 – Comment error bars are 1 sigma?

L447, p20. The residual annual cycle criterion. . .

L450, p20. I tend to agree with the statement, but what about the bias in the accuracy as arguably the low frequency component may lead to larger biases in deltaVLM (as well as scatter). A comment maybe useful here.

Figure 5. In the legend, unclear what AM is. I assume something to do with Annual, but needs clarification in the caption.

L455, p22. estimates do not always

L458, p22. See main issue #1.

L460, p22. Is average the best word here. I understand your point, but statistically speaking? Perhaps denote metrics of performances derived from the global dataset?

L463, p22. at every coastal site considered.

L469, p22. “large” in inadequately defended. See issue #3.

Section 5.1 was challenging to read, Fig 4b and d were difficult to interpret. I suggest some further elaboration.

L494, p23, no comma needed after we note.

L503, p24. I understand your point here but I feel more emphasis should be placed on the sensitivity of the ALT and TG to the same phenomena. If both were sensing 100% the same signal, the time span is not as critical. Again, I come back to main issue #1 here.

L535, p26. Quantify “much of” and “close vicinity to the coast”. See main issue 3.

L538, p26. . . .not be unequivocally

L560, p26. Are these statistics medians?

L565, p26. . . .and dedicated coastal altimetry. . . confine ZOIs and increase. . . . . .the global coastline which improved uncertainty.

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