

Interactive comment on “The imprints of contemporary mass redistribution on regional sea level and vertical land motion observations” by Thomas Frederikse et al.

Anonymous Referee #1

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Summary: The manuscript presents an original approach to estimate trends in vertical land motions (VLM) and relative sea levels (RSL) due to the present-day mass redistribution (PDMR) occurring during the GRACE (Gravity Recovery And Climate Experiment) satellite mission (March 2002 – June 2017). PDMR changes are evaluated using the JPL (Jet Propulsion Laboratory) GRACE mascon solutions [Watkins et al., 2015] and separated into a cryospheric and terrestrial water storage components, depending on the geographical location of mascons. This analysis is completed with an estimation of VLM and RSL trends due to Glacial Isostatic Adjustment (GIA), using the ensemble of GIA models from Caron et al. [2018]. The predicted VLM trends due to changes in the cryosphere, TWS and GIA are then compared to observations,

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using a subset of the GNSS data from the University of Nevada [Blewitt et al., 2018] matching the GRACE observation period. Finally, the authors discuss the impact of different VLM corrections on secular sea level rise averaged using a subset of 13 long tide gauge records.

Major comment: The manuscript provides a robust analysis of RSL and VLM trends due present-day ice melting, terrestrial water storage changes and glacial isostatic adjustment during the GRACE era. The link towards longer time scales is however not convincing. The major issue associated with the extrapolation of VLM corrections on secular time-scales is that we only have a very limited observation window (Figure 4, p7) and strongly non-linear processes. The problem is well stated by the authors (e.g. L10-12 p1), but, for several reasons, I doubt that their approach is appropriate to answer the issue, as it is claimed throughout the manuscript (e.g. L13-16 p1, L32-34 p2, L1-2 p16, L30-34 p17).

(i) VLM observations are decomposed in a cryosphere, TWS, GIA and residual components (eq. 3 p7), which except for GIA (and even this is arguable), all mix linear and non-linear processes. Therefore, non-linear processes in the residual VLM, due to local groundwater depletion, seismic deformation or other processes (e.g. L23-24 p2), are still present in the VLM correction and will generate a bias when extrapolated at longer time-scales.

(ii) PDMR observations cannot fully account for the non-linear processes related to the cryosphere and TWS, given the limited time-span and spatial resolution of GRACE observations. These unmodelled processes will also end up in the residual VLM, be extrapolated at secular time-scales, and bias the correction applied to tide gauge observations.

(iii) The authors have tested their approach on a very limited subset of tide gauges, comprising only 13 sites. It is difficult to believe that this sample is statistically significant. RSL changes arise due to a complex mix of processes, that can be easily aliased

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with linear or non-linear VLM on such a small subset. The PSMSL database comprise many tide-gauge records that are significantly longer than VLM observations (with at least 370 tide gauges with more than 50 years of observations), that can be used to test the validity of VLM corrections. Besides, given that the main issue here is the extrapolation of non-linear processes in time, it would be good to show the impact of these corrections in a time-series analysis. Sole the mean and standard deviation of RSL trends are provided here, which is insufficient to assess the temporal and spatial variability expected from tide gauge measurements.

Further analysis is therefore required to validate the assumption of the authors (i.e. their approach allows to get rid of the bias when extrapolating a VLM correction, based on the analysis of GNSS data, on secular time-scales). The paper could be limited to the analysis of recent (March 2002 – June 2017) mass exchanges between the continents and the oceans and their impacts on barostatic sea level changes, vertical land motions and relative sea level changes. The results would have to be put in context (the tool developed is not adapted to solve the issue brought forward here) and bring some clarifications on the following points.

Other comments and questions:

General comment on methodological aspects: The “Data and methods” section is rather difficult to read. It would help to have a small paragraph and/or flowchart giving an overview of the method, that would link the various observational datasets and modelling approaches that are used together. The equation 3 at p7 is quite helpful to understand the author’s approach but should come sooner in the paper.

Section 2.1 GIA model

p3: Is the ensemble of GIA predictions extracted from Caron et al [2018] only applied as an a-posteriori correction to the GRACE mascons solutions? Please, confirm or correct in the manuscript.

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Section 2.2 GRACE etc.

p5 L3-4: “Each . . . mascon” How? What noise model is used? (+ typo in measurement)

p5 L6: “The uncertainty in the trend is dominated by GIA uncertainty”: What are the other sources of uncertainty and how are they estimated?

p5 L19-21: Can you clarify how the separation between the cryosphere and TWS is done in mixed mascons?

Section 2.3: Solid earth deformation etc.

p6 “we solve the SLE using the pseudo-spectral approach (Tamiesia, 2010)”: If I’m correct, this requires to express the load in spherical harmonics. How was this done? At which order? Please, also precise what load model is used (GRACE-derived PDMM mascons?)

Section 2.4: GNSS etc

p7 L7-9: This stresses the issue of the record length, which is too short to account for non-linear changes in VLM, and extrapolate them on longer time-scales.

p8 L2-6: “Using the trend in zresidual (t) . . . from the record”: I do not understand the logic here. Once again, z residual is not supposed to be linear in any way, it is probably the largest source of error in the extrapolation of VLM correction at longer time-scales.

Section 3: Results

p8 last line: How do you explain a net land mass gain in TWS?

You do mention later in the conclusion (p16 L20-22) that the global sea level rise due to PDMM (1.58 mm/yr Table 1) disagree with other estimates, that are usually higher. Why is the cryosphere contribution smaller with your approach?

p 11 L6-7: Can you quantify the acceleration terms in ice mass loss? Is it comparable with other estimates?

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p 11 L20 : What is your indicator of “smooth temporal variations”?

p 13 L9 : Because GRACE resolution and mascon geometry is not adapted near the coast? Following that train of thought, it is unlikely that GRACE-derived TWS changes can account for the strong local variability evidenced at tide gauges.

p 14 L10: this can easily be estimated with the application of a spatial filter on the observations (e.g. Pfeffer et al., 2017).

p 14 L15 to 18: can you provide some quantitative information about the agreement/disagreement between VLM observations and predictions (root mean square error, coefficient of determination, bias, maximum differences, distribution of the differences etc.)

p14 L20: why do you limit your analysis to these 15 stations? why such a restrictive selection?

p15 L7 to 8 “For the full model . . . GIA RSL trend” Why? How does that help with the extrapolation of non-linear processes in time?

p 15 L10 “assuming a power law spectrum” to describe temporally correlated noise?

p 15 L11: Why do you choose the T16 subsample? How does it help to validate your approach?

p 16 L1-2: How are these issues resolved?

p16 L9: Yes, the “sampling bias” is a huge problem that needs to be addressed. More observations are available to start with.

Section 4: Conclusion

p 16 L20: Why this disagreement? This should be discussed earlier in the manuscript.

p 17 L6: How do you estimated uncertainties that are not related to GIA?

p 17 L 23: please provide metrics to illustrate the agreement/disagreement between

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VLM observations and predictions.

p 17 L 33: The authors did not convince me that their approach avoids the bias, due to the extrapolation of non-linear VLM. It does not logically follow their assumptions and has not been evidenced in the results. However, they provide insights on the mechanisms driving recent (GRACE era) sea level changes and solid earth deformation, which is, I think, useful results.

Details:

Abstract:

L1 to 3: rephrase the first sentence to be more readable (less “and”, please)

L8: “the temporal variations affect GNSS-derived VLM”: temporal variations of what?

L13 to 16: This is very confusing. Separating VLM observations in GIA and PDMR components does not separate linear from non-linear components (see major comment).

Introduction:

L32 to34: see major comment

Data and methods:

L10: “GIA affects GRACE observations . . .”: replace by causes changes in the gravitational potential observed by GRACE satellites or equivalent to keep the same structure for each proposition of the sentence.

L27 p3: “we bin the quantity of interest”: not sure what bin means

eq 3: why using two different variables (R and z)?

p 7 L14: use of the term “uplift”: replace by vertical displacement of the earth’s surface (or equivalent)?

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Results p10 L8 and 9: “considerable” means how much?

Conclusion p16 L14- 17: please reformulate to clarify

Figures: It is difficult to read the color scales of figure 7, 8 (bottom row), 9 and 10. Is possible to provide a bit more contrast to have a better idea of the range of variations of RSL and VLM trends? Otherwise, provide numbers in the legend or in the text.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-128>, 2019.