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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 #2

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The paper aims to develop estimates for deformation due to GIA and present-day mass redistribution with uncertainty estimates. The deformation patterns are explained and used to correct tide-gauges. It is an original idea to address a highly relevant problem. It is shown that this correction improves consistency between sea-level trends from tide gauges and sea level reconstructions, and offers potential for better regional sea level projections. I see no problems in the methodology. Therefore I recommend the paper to be published after addressing two main comments below, and the long list of textual comments connected to these.

Correcting tide gauges only holds for the time period of the data, or the time for which

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the model is valid, while one of the reasons for decomposing the relative sea level rate is to make projections outside the data range. The corrections are necessarily based on limited time span and also do not include regional deformation processes which can vary in time. This is an important limitation of the paper that is not discussed well in section 3.4 and in the conclusions, see specific comments below.

2. A second problem in the paper is the way it is written. Logic is sometimes hard to follow, procedures are not described clearly or are not explained, the use of words that should have a precise meaning is a bit sloppy (words such as trend or linear trend, deformation, model, relative sea level or just sea level). Several examples are given below. A thorough revision of the text is necessary because it now guessing is required from the reader in several places to put the pieces of the puzzle together.

A pdf is attached with small textual comments or typos.

Specific comments

The abstract is a mix of describing the processes and the methodology. I suggest to move the methodology to the last paragraph where the methodology is now partly described

Introduction The introduction is difficult to follow because it is a mix of a background and methodology, and the description of the objective is scattered. I suggest to reorder the introduction to more clearly separate background, problem statement, methodology and application separately. Also the first part of the conclusion and the introduction should be better aligned.

Page 1 Suggest to add Wu and Peltier (1984) to Milne and Mitrovica (1998)

Page 2 - line 3 I get the impression that sea level and relative sea level are not used consistently according to their definitions, please check - line 15 and further: use trend or linear trend consistently - line 34: “to avoid this possible bias” Please make clear what bias you refer to and what part you aim to remove. If the bias refers to the

(local) processes in line 21 and further then correcting for PDMR and GIA alone is not sufficient. If this bias refers to the bias due to PDMR alone (line 28 and further) it is not clear why you would remove GIA as well. Also the time-period seems relevant because you can not use the computed PDMR induced deformation beyond the period of the data.

Page 3 - Line 6: the text contains ‘estimates of GIA’, ‘GIA solutions’,’model ensemble’. Please make it more consistent. - Line 9: title of section 2.1 , I suggest to use something like ’prediction ensemble’ because you do not actually discuss the model - line 15: It seems to be partly circular reasoning when you use model ensembles scored according to fit to GNSS data to correct GNSS data. Please address this in the text.

Page 4 - Line 5: please explain why ocean bottom pressure changes are not used, because they are a form of PDMR - Line 6: please discuss the possible effect, if any, of this filter on the final estimates, as the main interest is in deformation along the coast.

Page 5 Potential uncertainty from determining deformation due to ice sheets, glaciers and TWS should be addressed here

Page 6 - Line 13: It is more useful to say what is neglected: viscous effects due to PDMR changes before and during the period of interested, and where these effects could occur - Line 15: please add a reference as there has been discussion about the methodology

Page 7 Line 14: how is the height anomaly defined?

Section 2.4 It would help the reader if you explain what result you are after in this section before describing possible corrections.

Page 9 line 7-15: There are probably existing studies on TWS and land mass changes that these results can be compared too.

page 11 section 3.2, the last paragraph in the section needs a conclusion, which could perhaps be moved here from section 3.3 - line 18: do you mean the solid earth defor-

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mation trend from section 3.2? How do GNSS observations play a role there? - line 20: the temporal variations are not shown in figure 11

page 13 - line 3: It should be explained what kind of cryosphere changes could cause these kind of uplifts

page 14 Line 2: discussion of the uncorrected trend could be moved before the statement that the observations will be corrected.

Line 8: “even considering the uncertainty.” This is ambiguous. Please make clear whether you mean that the ensemble mean is stronger or not, or whether you talk about a statistically significant increase Line 20: “partially repeat the analysis”. Describe the analysis because now it is not clear what you are doing different and why in what follows.

Page 15 line 1: specify what you mean by “in the vicinity” From line 5 onwards the text is very hard to follow. You need to explain that the goal is and why certain choices are made. The only explanation is that the analysis of Thompson et al. (2016) is partially repeated. Please add intepretation of the figure to line 12, now it is left to the reader. Several comments and questions on this section can be found in the pdf.

Page 16 line 1: I don’t agree that both issues are resolved (same for page 17 line 32). Regional deformation such as given in page 2 line 22 will also not follow a constant trend so you can not use GNSS data or models with a shorter period than the tide-gauge period and expect that extrapolation of deformation models or data works fine, or am I missing something? Line 10: “the gap discussed by T16” specify which gap for readers that have not read that paper

Page 17 Line 25: “uncertain GIA contribution” explain if this means that the uncertainty in the GIA models ensemble is underestimated

Please also note the supplement to this comment:

https://www.solid-earth-discuss.net/se-2018-128/se-2018-128-RC2-supplement.pdf

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

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