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The Cryosphere Discussions

Interactive comment on "Variability of mass changes at basin scale for Greenland and Antarctica" *by* V. R. Barletta et al.

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This is a very detailed analysis on the various influences that cause differences between GRACE-based time series of Antarctic and Greenland ice mass changes on a drainage basin level. Such an analysis is very important and the present manuscript presents many interesting observations.

I also have a major concern. As a prominent conclusion it is stated that the Release 5 (RL5) linear mass trends have smaller absolute values than the RL4 trends for the majority of the basins. Relative trend differences easily amount to 25% and more (Fig. 5c). This result is remarkable. It should be checked carefully, since it could have an important impact on the general appraisal of any previous (RL4) or future (RL5) GRACE results.

Unfortunately, closer inspection suggests that the trend comparisons are biased by the chosen methodology. The conclusion that RL5 shows smaller trends than RL4 (and possibly some more results) are not substantiated.

A glance on Fig. 5a quite convincingly suggests that the GFZ_RL5 trend is larger than the GFZ_RL4 trend. Contrarily, Figure 5c states that the GFZ_RL4 trend exceeds the GFZ_RL5 trend by $\sim$20%. The problem of those results lies in the simple regression that does not treat the two time series symmetrically. The regression is illustrated in the inset of Fig. 5a and formally expressed on page 3416, line 11. In the Fig. 5a inset, the red line minimizes the sum square of vertical differences to the red dots. If we would change the role of RL4 and RL5 and minimize the sum square of horizontal differences, the regression line would look differently (as you may try). To say it in the formal framework of Section 3.5: Assume that M1(t) and M2(t) contain the same (small) signal but different (larger) noise on top of the signal. Then the regression factor m_12 would tend to be smaller than 1. If we would change the role of M1 and M2 and calculate the respective regression factor m_21, then m_21 would again tend to be smaller than 1, and not 1/m_12 ! (One may try this by synthetic experiments.) That means, by changing the role of M1 and M2, the (unjustified) conclusion would be that for most of the basins RL5 trends exceed RL4 trends. The regression factors thus calculated are just not an appropriate measure for the ratios of linear trends in both time series. The authors seem to address doubts about the suitability of their regression approach in the text following Eq. 2, but I do not understand their argument and conclusion.

It is not clear why the authors did not follow the most obvious, and easily interpretable, approach: Fit a linear trend individually to each time series. Then compare those trends in an absolute sense. This approach would also guarantee that the contributions from the different corrections (e.g. for GIA) really add up linearly, which is by no means guaranteed with the present approach. I suggest that the method of trend comparison has to be revised (with an option to make it simpler) and the respective analysis has to

be re-done.

I add some minor comments in the following:

- I was initially misled by the title, understanding "variability" in the sense of "natural variability". The manuscript rather deals with the uncertainty of mass changes estimates from GRACE. This might be something to consult with a native speaker.

- In the abstract, when quoting trends, mention the exact time period.

- Section 2.1. Some wording about the low degree coefficients might be misleading. For RL4, linear rates (according to IERS conventions and repeated in every GRACE solution file) have to be added to the monthly values. In RL5, the monthly values already contain those linear changes. This is not a matter of solution quality (as suggested in line 21) but just a matter of convention. Concerning C_20, CSR still recommends to replace the RL5 values by SLR-based values (see presentation of S. Bettadpur in Proc. GRACE Science Team Meeting 2012).

- Section 2.1: A major improvement stems from the instrument/star tracker alignment calibration (see presentation by B. Tapley in Proc. GRACE Science Team Meeting 2012), which was prompted by this peer-reviewed article: M. Horwath, J.-M. Lemoine, R. Biancale, S. Bourgogne: Improved GRACE science results after adjustment of geometric biases in the Level-1B K-band ranging data; Journal of Geodesy, 85(1):23-38, DOI: 10.1007/s00190-010-0414-2.

- Section 2.2: After submission of this manuscript, a GFZ website on the GAC issues has appeared (www.gfz-potsdam.de/aod1b), which you might cite.

- Fig. 1: The orange line does not seem to make a step but a smooth transition, in contrast to what is stated in the text.

- Section 3.1: The description of leakage assessment could be a bit more specific. What synthetic dataset was used? Formulations like "recover up to 99% of the mass" seem to hide the lower limit of recovery. The patterns in Fig. 2 appear rather smooth.

Is there a quantification, how much of the mass changes in, say, Antarctic basin 21, is mis-attributed to the neighbor basins?

- Fig. 3: It is hard to distinguish some of the colors (e.g. 9, 10, 11 in Antarctica). Maybe put basin numbers directly into the map.

- Fig. 4-5, 7-9: It needs some time to grasp what is exactly shown, since so many magnitudes of error characterisation are discussed in the main text. You could precisely define the quantities in the text by symbols and refer to those symbols in the figure.

Interactive comment on The Cryosphere Discuss., 6, 3397, 2012.