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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.

Anonymous Referee #1

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Title: Variability of mass changes at basin scale for Greenland and Antarctica

Authors: Barletta, Sorensen and Forsberg

GENERAL COMMENTS:

When deriving mass variability estimates for a specific geographic region from the global gravity field variability estimates from GRACE, a number of processing steps, ancillary data, and error correction methods must be used. At each step, there are several choices available to the analyst. In this paper, the authors examine the impact of several such choices on the estimates for rates of mass loss from regions of Antarctica and Greenland ice-sheets. The parametric study forms the basis for deriving a best-estimate for ice-sheet mass loss rates, and their error bounds.

The specific "parameters" examined in this paper include: Available time-series of geopotential harmonics; choice of degree-1 (geocenter) correction; choice of the GIA correction; choice of methods reducing the geopotential observations to mass anomalies; and a specific feature in the background model changes between two revisions of the GRACE data product time-series.

For the selected parameter space, the paper contains a complete discussion of the methodology, outcomes and the conclusions.

The paper has a lot of information - it is not easy to grasp the full scope of the analysis. However, the relative rank ordering of the different "parameters", and the final error bounds are clearly captured.

G.1 - The authors should compare - both in the abstract and in the conclusions - their error bounds against the previously published error bounds. While the estimates themselves might not be directly comparable, the error bounds can be compared.

G.2 - In general, I disagree with the authors' approach to one specific experiment - that of assessing the effects of change from AOD1B_RL04 to AOD1B_RL05. All changes in the background model do not propagate one-to-one into the gravity field estimates from GRACE. The effect depends on the spatial-temporal character of the process. The authors have not considered the effect of GRACE data processing on the changes in the background models. The necessary information around the jump in 2009 is well within the data span already considered in this paper, and the authors should re-examine the change in the GRACE estimates in coastal Antarctic region between RL04 and RL05.

G.3 - I have concern about the authors approach to comparing trends as outlined on Page 3415. It is not clear why one of the time-series must be corrected for regression parameters (m,q) before the trends can be compared. Each time-series is supposed to be providing the "best-estimate" of variability, so why not simply directly compare the trends derived from the two time-series? While I cannot estimate the effects for

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this particular experiment, I have found that estimates of the parameter "m" tend to be very susceptible to a few outliers in either series. The authors should verify their trend difference estimates from this method, for a few cases, with simpler, direct trend comparisons.

G.4 - The estimates of mass for basins will be statistically correlated, even if the geopotential spherical harmonic coefficients "data" are un-correlated. These correlations will have a tendency to inflate the uncertainty of the estimates for each basin. Is this effect captured in the analysis?

G.5 - On Page 3402, the authors state that the C20 harmonics were not replaced with their SLR determined values. This is not correct when using either RL04_CSR or RL05_CSR time series. The various meeting presentations, user guide and the project website clarify that only GFZ_RL05 may be used without SLR values. The authors should replace the RL05_CSR values of C20 with SLR-determined values of C20 provided in the GRACE project Technical Note 7 (TN-07). This replacement can have a significant effect on the Antarctica mass rates.

G.6 - The authors state in the first sentence of Conclusions that "In the light of the consistent and systematic error analysis that we have performed, the results presented in this study are statistically meaningful". I agree that the study is consistent and systematic. However, several of the causes of scatter in the estimates are not random. This may be particularly so for the GIA models, where the variations are traceable to specific choices having to do with history or Earth parameters or physics. There is no assurance that the ensemble of estimates of tested GIA carries the "true" GIA at its "center" - it is more likely that the GIA choices will bias the mass estimates from GRACE. Also, several systematic differences between the results from processing centers were presented at the last GRACE science team meeting in September 2012 (presentations are available online from the meeting site). So the data input need not average around the truth either. Rather than a statistically meaningful error estimate, these results provide a realistic estimate of the range of scatter among the possible estimates. This "scatter"

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is what the authors mean by the word "Variability" in the title - not the time-variability of the signal itself.

SPECIFIC COMMENTS:

S.1 - There is a lot of forward referencing in the paper - in other words, comments and conclusions are written down well before the particular aspect was put through the data analysis. This is very disorienting, and must be corrected. Specific examples include the following:

S.1-a: Page 3403, Lines 20-21: The word "use" and the phrase "but in our final estimate" is looking too far ahead

S.1-b: Page 3406, Line 13: Phrase "each of our methods" - your data processing experiment design is not yet presented.

S.1-c: Page 3409, Line-17: Phrase "In our final preferred trend" - the description of your methodology and results are yet to come.

S.2 - On Page 3399, Line-28, you have mentioned that the literature has underestimated the importance of some of the errors. This reviewer disagrees. All the sources of variability considered in this paper feature prominently in any GRACE discussion - though the virtue of this paper is that several of these are being systematically considered in one paper.

S.3 - Page 3403, Line 20-25: This explanation is unclear, particularly given that the analysis methodology is not yet described.

S.4 - Page 3404, Line-2: What does the phrase "partially detected" mean here?

S.5 - Page 3404, Line-6: The importance of degree-1 corrections is not an open issue - the best choice of the numbers/values to use is an open issue.

S.6 - Page 3404, Last-para: This explanation confused me first. The description appears to imply that the Tables 1/2 present the amount of mass, which when placed in

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a specific bin (zero outside the bin), would lead to 1-mm geocentre shift in a specific direction. It took a while to figure out that the table provides that part of a global mass distribution (arising from 1-mm geocentre shift) which is confined within the basin. The authors should consider clarifying this.

S.7 - Page 3407, Lines 14-16: What does this mean, particularly the phrase "after new considerations" ?

S.8 - Page 3407, Line 26: What are the "two alternatives"? The transition to the content of the next para is confusing.

S.9 - Page 3409, Title & first sentence: It is not immediately obvious that the "method" here refers to the process by which gravity field estimate is reduced to a mass estimate. A sentence stating so would be useful. If that is so, I agree that the treatment of leakage is the cause of major differences.

S.10 - Page 3411, Lines 4-23: Is this calibration study documented in more detail? If not, it should be documented as supplementary material - it is important to the findings of the paper.

S.11 - Page 3418, Line 6: Please provide a quantitative assessment of this difference.

S.12 - Page 3422, Lines 8-13: Please provide specific numbers.

S.13 - Page 3422, Lines 24-27: The authors have the data necessary to more completely examine this question, in light of the General Comment G.2 in the previous section.

TECHNICAL COMMENTS:

T.1 - Please drop the word "official" from the paper, when referring to the various processing centers - "GRACE project centers" or "GRACE Science Data System Centers" is a more appropriate designation.

T.2 - Page 3401, Lines 11-15 (introducing Section 2) are not necessary.

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T.3 - At several places, SLR (=satellite laser ranging) has been written as SRL.

T.4 - Page 3405, Lines 20-21 - please give units

T.5 - Page 3406, Line 8: Perhaps "data DDK3 filtered" should be "DDK3 filtered data"

T.6 - Page 3406, Line 21: The Tellus site does not provide a "GIA model" - I think it only provides the geopotential harmonic rates predicted by a GIA model (i.e. in the sense that a GIA model would include the ice-sheet history and the adopted physics and parameters)

T.7 - Please increase the text font size for all notations and legends in all the figures. Most of the images are difficult to read, even on my large screen. On paper, it is impossible to decipher.

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