

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

Anonymous Referee #2

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Review of Barletta et al. “Variability of mass changes at basin scale for Greenland and Antarctica”

The authors present regional mass change time series of the Antarctic and Greenlandic mass balance derived from GRACE measurements, along with broad assessment of related uncertainties. For the error assessment, the authors compare the differences obtained by two GRACE releases, two inversion approaches and various GIA corrections. In addition, various (observed and modeled) time series of geocenter motion are used to estimate the effect and uncertainty of the degree-1 GRACE coefficients in present-day mass balance estimates. For the application by the reader, a Kernel is provided for transforming geocenter motion to basin-scale ice-mass changes.

The paper represents an interesting and systematically presented assessment of un-
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certainties in GRACE mass balance estimates. The most new and relevant content of the paper is pointing towards the unresolved issue of the poorly known geocenter motions, and the assessment of the GRACE releases RL04 and RL05 for their impact on the mass balance estimates.

However, I do have four major issues with the paper that I would demand to resolve before publication:

1) The Kernel for calculating the mass change due to a shift in the geocenter motions is provided without derivation and without validation that the mass change estimates are independent of the gravimetric inversion method 1 and 2 applied. Therefore, I would recommend providing an equation that relates the degree-1 potential coefficients to the geocenter motion (for elastic processes), so the user can perform its own gravimetric inversion.

2) Considering degree-1 terms makes the gravimetric inversion a global problem. Table 1 and 2 (please check columns of table 2, they are mixed) gives an estimate of the leakage of Antarctic mass changes to Greenland mass changes and vice versa. Therefore, both regions should be considered simultaneously. I guess, but I am not sure, this is the case for the results presented here. In addition, exchanging the Antarctic GIA component in global GIA predictions will also influence the degree-1 (e.g. Klemann and Martinec, 2011). The authors should state very clearly which GIA predictions are global, which are regional, and how they are completed, and how they are knit together; for example, is ICE-5G-LV a global model, or merged together from predictions with VM2 (not Antarctica) and LV(Antarctica). This will certainly influence your degree-1 terms. Please make clear that no ‘double corrections’ are applied, for example, by calculating SLR-GIA_2 (after Klemann and Martinec, 2011) and then again applying a GIA correction of apparent mass change based on Antarctic GIA predictions. Also, the author should explain how altering the solution area (forcing ocean to be zero, including a ‘belt’ around the coast of the solution domain) (P3411) influences their results. Again, I would assume that including or not including e.g. the ocean in the solutions

domain will influence how degree-1 propagates to the mass balance estimate. This may be somewhere hidden in the calibration process. Have the author tried calibrating the inversion by neglecting degree-1 terms and then performing the inversion on GRACE coefficients starting from degree and order 2.

3) The authors state that “a cross-validation” is done based on ‘two independent methods’ (P3398L11 and 12). However, both methods appear to be calibrated with respect to the same synthetic data, making their independence questionable. In addition, method 2 is a very heuristic special case of the basin averaging method used by e.g. Horwath and Dietrich (2009). It would be much better if the authors showed their agreement with other methods, e.g. Schrama and Wouters (2011) or Luthcke et al. (2006) or weaken their statement that they have resolved methodological uncertainties in the GRACE estimates. I think it is not correct to assume that leakage is only from the land into the ocean (P3412); GIA will have a significant signal along the coastal rim and the associated apparent mass will differ whether this region is included in the solutions domain. Did the authors test the sensitivity of the method 1 and 2 parameters w.r.t. the GIA contribution? Is it correct to apply the GIA correction estimates to mass balance estimates used with other approaches?

4) GIA is an open issue for Antarctica. The authors are very un-precise about what makes them reduce the upper-mantle viscosities. Based on what observational evidence? GPS uplift rates? Or based on revised models, e.g. Whitehouse et al. 2012a,b? The authors need to provide some quantitative argument why ICE-5G-LV is preferred over ICE-5G-VM. In the same context, the authors should also explain if the GIA correction provided in Table 4 encompasses the Northern Hemisphere contribution or not. Again, there is the danger of ‘double-booking’.

Please also follow the recommendation of Martin Horwath (TCD, short comment) to compare trend values of RL04 and RL05 not the regression coefficients, which is recognized as a bad measure for noisy data. This may also be more informative to the reader. Having mentioned all these points, I would like to emphasize that the paper is

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– among others (e.g. Schrama and Wouters, 2011) – a thorough and valuable analysis of uncertainties in GRACE mass balance estimates, covering several new aspects. I do recommend a publication, once more convincing arguments for the methods (details on calibration procedure, selection of parameters), as well as the consistent use of the global GIA corrections, i.p. for the degree-1 coefficients are provided. I apologize to the authors that I can be more positive about the material in the current state. Below are numerous minor comments to the text.

Title

The paper presents mass balances inferred from GRACE only. In addition, uncertainties of the mass balance estimate are the main focus, not the variability of mass change. I would therefore change the title to “Uncertainty of basin-scale mass-change estimates for Greenland and Antarctica from GRACE” or something similar. There are several places in the text, where it should better say ‘uncertainty’ than ‘variability’, e.g. in the Conclusions P3422L06.

Figure SM 6: Method + release comparison is missing for basin 25. Monthly solutions + error estimate missing for basin 24.

Table 2: Columns for basins and GrIS values are mixed up.

Abstract: P3398L04: exchange ‘, and analyses on the [...]’ to ‘...owing to differences in the applied methods, data sets and time intervals used’.

P3398L07: change ‘for the first time systematically’ to “systematically”. The former is not true; see for example Schrama & Wouters (2011).

P3398L08: change ‘mass inference method’ to ‘gravimetric inversion method’

P3398L09: better exchange ‘background models’ to ‘correction models’. GIA predictions are no background model in the GRACE sense.

P3398L10: Remove sentence ‘We find a very good [...] cross validation.’ As far as I

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understand it from the text, method 1 and 2 are calibrated with the same data set; the comparison of two results also shouldn't be called a cross validation.

P3398L12: change 'rather than different methods' to 'rather than our two different methods applied.' There are many more inversion schemes, which you do not cover.

Main text:

General comment

P3398L21: 'clear' → 'statistically significant'? (please determine this)

P3399L04: remove 'In 2005'. Clear from citation.

P3399L11: change 'GRACE provides a DIRECT measurement' to 'GRACE provides a differential measurement', if you wish. Or remove 'direct'.

P3399L12: change 'without the need to [. . .]:' to 'without the need to rely on volume-to-mass conversion estimates.'

P3399L14: change 'mass balance' to 'mass anomalies'

P3399L16: please specify what kind of tides

P3399L17: 'removed in a second stage in order' please add '...stage by the users in order'

P3399L19: change 'variability' to 'error'

P3399L24: GIA not only show a positive mass trend. Please correct the sentence.

P3399L28: change 'variability' to 'uncertainty'.

P3399L29: Please put reference after 'ways to infer it'.

P3400L01: Most melting in Antarctica is below the ice shelves undetected by GRACE. Please generalize statement.

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P3400L04: Remove sentence 'Therefore, mass balance estimates [. . .] motion'. It is clear from the context and additionally pointed out online P3400L08.

P3400L09: Please include additional references to Wu et al., 2012.

P3400L10: Geocenter motion is a source of variability in the mass balance ESTIMATE. Not – or probably not significant – in the mass balance itself governed by accumulation and discharge.

P3400L12: specify 'raw data'.

P3400L21: please add 'difference between RL04 and the new RL05 with the application of determining ice sheet mass balance'. There have been numerous assessment of the RL04 and RL05 by the processing centres and users not published but still very relevant. Remove 'to our knowledge [. . .] literature'.

P3400L24: please add 'Sasgen et al. 2010'; this forward modelling approach is different from the ones listed.

P3400L27: I don't understand the meaning of this sentence on the calibration and cross validation. Please explain more clearly what you mean or remove it.

P3401L03: change 'net mass balance' to 'mean mass balance'.

P3401L07: change 'different' to 'two'; remove following sentence.

P3401L13: remove 'some of which have [. . .] overlooked.' And narrow down the statement to the degree-1 issue. Influence of GIA, inversion method, GAB/GAC, GRACE releases, hydrological contamination – all has been done and is pretty standard. The focus on the potential uncertainty caused by degree-1 on ice-mass balance estimates appears to be the most novel.

P3401L23: change 'GRACE solutions [. . .] Stokes coefficients.' to 'GRACE gravity-field solutions issued as fully normalized spherical harmonic coefficients called Stokes coefficients.'

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P3402L09: add '... (Cheng and Tapley, 2004), as recommended by the GRACE processing centres.' And reference the GRACE user handbook.

P3402L14: change 'at an early stage [...] fields' to 'at an early stage for short-term atmospheric and oceanic pressure variability.'

P3402L20: please mention the reduction of the geometric bias in the K-band ranging data (Horwath et al., 2011).

P3402L24: remove 'small'.

P3402L25: Section 2.2, §1 is a bit confusing description of the AOD1B product. Please revise this section.

P3404L08: Please explain already here how Swenson et al. (2008) constructed his degree-1 terms.

P3404L12: Confusing. I would understand that Swenson et al. (2008) and the SLR are consistent in the sense that both contain GIA degree-1 coefficients. Please reformulate.

P3404L22: change 'variability' to 'uncertainty'.

P3404L24: The authors are asked to give details here how the sensitivity Kernel is constructed, because this is novel and most interesting part of the paper. I would assume that it is based on Eq. 7 of Klemann and Martinec (2011) and elastic surface load Love number theory...

P3405L06: change 'apply a GIA correction' to 'apply a GLOBAL GIA correction'

P3406L20: These GIA corrections are very similar. Klemann and Martinec (2011) report a much greater range for the magnitude of geocenter motion (0.1 to 1 mm/yr) coming from different ice histories/viscosity distributions. The mean and standard deviation may not be a good representative of the geocenter motion.

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P3405L22: please use: 'The monthly GRACE solutions [...]']

P3405L25: change: 'The latter are due to systematic [...]'] to 'The latter represent systematic errors related to [...]']

P3406L14: what is the 'proper calibration'? The authors need to provide details on the calibration procedure of their methods.

P3407L20: change 'a GIA model' by 'a global GIA model'.

P3407L15: The formulation 'revised proper GIA corrections, after new considerations,' is imprecise. The authors should please make clear based on what evidence and how they revise Antarctic GIA corrections. What makes them chose upper mantle viscosities of 0.1 to 0.2×10^{20} Pa s? I would assume that this mimics newer GIA corrections like IJ05_R2 or W12 (Whitehouse, 2012a,b), but then this should be mentioned. Also, the authors should mention that changing the viscosity profile will change the spatial and temporal response of the solid Earth, and is therefore a completely different approach than adjusting the forcing load. There is also a new GIA correction model available on TCD, which satisfies GPS uplift rates; the authors should consider including this in a revised version.

P3408L18: Rotational variations produce via the mass redistribution governed by the sea-level equation variations in degree-1. This effect is maybe small. Maybe not. Can the authors provide an estimate?

P3408L20ff: The authors should to be very precise, which GIA correction is global, which regional and how in the regional corrections, remaining GIA regions are treated. Considering degree-1 makes the gravimetric inversion a truly global problem.

P3408L24: 'GIA estimate' not 'GIA fingerprint'.

P3408L25 and L3408L26: change 'low viscosity' to 'lower upper-mantle viscosity'

P3409L11: remove sentence 'Due to [...] time series.' An exponential function on

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centennial to millennial time scales is not what is commonly understood by month-to-month variations.

P3410L10: change 'are different and independent [...] ways.' to 'different, in particular in the way they treat the leakage problem.'

P3410L19: you could give an equation here how $\Delta(g_k)$ is related to the (filtered) Stokes coefficients.

P3410L20: how do the mass-balance estimates change w.r.t. the number of discs?

P3410L23 and P3411L23: please specify details on how the soothing parameter λ is chosen and how sensitive the mass balance estimates (or calibration outcome) is to the choice of this parameter.

P3411L11 and P3411L22: again, please give details on the 'synthetic data'?

P3411L16: abbreviation CSA not needed.

P3411L13: How the weights for the 100, 200, 300 km inverse solutions defined?

P3413L01: change 'error related to the methods alone' to 'each associated with each single method'

P3416L11: it would be helpful to mention here again the number common months and time period

P3417L05: change 'we perform a cross validation of the two methods' to 'we perform a comparison of the two methods'. Then, in the sentence after, 'we investigated all the [...]'

P3421L25ff: The acceleration terms in Fig. 10 and 11 are mentioned here for the first time with almost no discussion. The discussion needs to be extended; alternatively, I think it is also possible to Fig. 10 and 11 and text P3421L26-P3422L02 without an impact on the main message of the paper.

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References

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