The Cryosphere Discuss., 6, C2889–C2915, 2013
www.the-cryosphere-discuss.net/6/C2889/2013/

The Cryosphere
Discussions

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

V. R. Barletta et al.

v.r.barletta@gmail.com

Received and published: 1 February 2013

Authors reply to the two reviews and the short comment on paper "Variability ...."

We want to thanks the reviewers and Martin Horwath (MH) for reading our manuscript carefully and providing valuable comments. We give one comprehensive reply since several comments are common in the reviews and MH short comment.

The following list of answer does not reflect the order of the questions, which are indicated in the short description of each point (in bold). The reviewers comments are reported in italics.

1) Concerns about comparing trends using the regression index (MH main point; point G.3 Reviewer 1; last main point Reviewer 2).

We partially agree with these comments (MH and Rev.1) and we have now made a deeper reasoning about our comparison approach and have hopefully found a better way to explain its validity and its novelty. We are dealing not just with trends but with scattered time series, each of them supposed to be a "best solution" for a selected method and data set. Scattered time series could have differences, not just in the trend but also in the seasonal component and/or in the noise and we cannot discriminate precisely the noise from the true signal.

We aim at finding a way to compare scattered time series and to extract an optimal solution (the most accurate one). Assuming that the different time series represent the same phenomena, we have no reason to prefer one series to another but instead we want to estimate the accuracy error, i.e. the relative distance from the true solution, by quantifying their differences.

Much information can be extracted from a comparison of two time series. Two series can be compared through some of their properties, such as the trends, or by computing overall indexes like the correlation index, each providing a partial description. However, a comparison should account for more than one partial aspect, to be useful.

We chose an approach that allows us to extract information on both the overall similarity/difference between the time series, and point-like agreement/difference, and at the same time it also keeps these separate.

Let us assume for clarity that we have two time series that are extremely well correlated, but differ by a scaling factor. This is exactly what could happen when one compares GRACE time series coming from methods that differ only in the treatment of leakage, see for example the use of a scaling factor in some consolidated method (Velicogna and Wahr, 2006). We could look at the time series, and visually recognize that they are very similar, but differ by a scaling factor. How can this similarity/difference be described? If we extract the trends (and possibly other overall functions) and compare them, we would say that they differ in amplitude. The similarity is instead found in the correlation that is a normalized index.

An alternative way would be to find a way to match the overall features of the two time series, i.e. by scaling one with respect to the other (or both with respect to their average). Then, the rescaled time series would be directly comparable, and their difference (the residue) would describe the point by point (or time by time) agreement, while the scaling factor represents the overall agreement between the two series (the closer to 1, the better is the overall agreement). If the two series are extremely similar except for a scaling factor, the result of our method would be that the overall difference is large, while the point-like difference is negligible.

In our analysis the overall agreement is represented by the regression index m, while the delta (in Eq. 2 in our manuscript) represents the "time by time" agreement or how well the series are "correlated". For the estimate of the accuracy error (Eq. 3 and 5 in the manuscript), we remind that we do not know which is the closest to the truth. By comparing different/scattered time series we assume that we can estimate accuracy for the measurement of the mass changes at basin scale. The information about the overall agreement is the most important for the estimate of the accuracy error on the trend.

The reviewers suggest that we could make direct comparisons of the trend, but we do not believe that this information is sufficient and that it could be misleading in some cases. For example, assume that two time series are highly correlated (so they seem to portray the same phenomena) and have the same trend, but with very different amplitude in the seasonal contribution. By comparing the correlation we would say that they agree well. By comparing the trends, we could say that they agree perfectly for this part of the signal. But can the two series be considered in good agreement, if

they have a large difference in scale?

$$f1(x) = a + bx + A\cos(wx) + B\sin(wx)$$

$$f2(x) = a + bx + 3A\cos(wx) + 3B\sin(wx)$$

$$f1(x) \neq f2(x)$$

but the correlation is almost 1 and the trend is the same.

We think they cannot be considered as being in good agreement, so in this case our approach would conclude that the point-like features are very similar, but that the two series are not in overall good agreement, and that the apparent agreement of the trends alone is misleading.

In general the comparison between trends alone will also suffer from another problem, i.e. the time span for its computation, when we change the time span, the difference in the trends of two series can change.

Our strategy provides at the same time the information coming from an overall comparison (trend + other terms) and a correlation-like analysis. These two information are extracted consistently, and so is the difference.

We are aware that when the noise or the seasonal signal is larger than the trend, our regression index does not represent the differences in trends, and we have stated this already in our assumption RI (pag 3415 line 16-21), but we agree that it might be not very clear for the reader (as pointed out by MH), so we will explain this and the motivation for our comparison strategy better in the manuscript.

We also realize that our "trend comparison", i.e. the description of fig. 4 and 5 and the related discussion should better reflect our assumption. In fact in Fig 4 c, we show the differences of the regression index with respect to 1 (as we state in the text and in the caption), i.e. differences of normalized trends (with the regression index), which under our assumption RI almost coincides with the differences in trends.

The deviation of the regression index with respect to 1, represents the overall difference between the time series, and in some cases, if the monthly difference is small (Fig 4 c), it also represents the differences in trends. However, the monthly differences are relative to the average monthly error, so they don't give clear information on the scattering as the correlation coefficient would do. Moreover, those relative monthly differences are also shown in Fig 7.c, so in Fig.4.c (and Fig 5.c), but we will substitute the relative monthly difference with the correlation coefficient, to give a more clear overall picture.

In the case of fig 5, as MH noticed, the scattering is large and our assumption does no longer hold, and we can't say that fig 5 c represents the difference in trends (as MH noticed). Moreover also another assumption doesn't hold in this case, i.e. the RL05 is supposed to be less noisy and the solution obtained with the new release is likely more accurate than the one obtained with RL04. So only where the monthly difference is small, i.e. for basin nr 11 and 16 in Antarctica, the differences in regression index could represent also the differences in trends.

As suggested by MH, we have computed the difference obtained with the other regression index, and this showed that MH is right, since in many basins the differences in regression index have the same sign.

Moreover, we computed the actual differences in trends between RL04 and RL05 and do not find a clear indication of a systematic difference. So our conclusion about this will be changed in a revised manuscript.

Fig 5.c represents only the overall difference in regression index, which is large because of the large difference in noise content. Another observation is that, at basin

scale, the differences in regression index between RL04 and RL05 reflects quite closely the relative error (err/Trend obtained with RL04), so the differences in trends could likely reflect the same pattern, and be small where the trends are strong and clear (as in basin 21, 22 and 23). This confirms the analysis recently presented on the better performances of RL05.

2) Description about our calibration procedure (MH pag C1903 last point; Rev. 1, point S.10; Rev. 2 several point)

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

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

-P3410L23 and P3411L23: please specify details on how the smoothing parameter lamba 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'?

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

- Rev 2 (pag C2106 last paragraph) "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."

As suggested by Reviewer 1 we add more detail about the refinement of the inversion method and about our calibration in the supplementary material. In response to the questions by Reviewer 2, we try to be more specific on all of the details mentioned. However, the calibration process could be the subject of a technical paper in itself.

i) The number of disks does not change the result appreciably, if they are smaller than the input data resolution, but it does affect the computational costs. Moreover the disks are only a convenient representation, in fact point-like mass are actually inverted to find the mass distribution. The spacing of the point-like mass (which corresponds to the disk radius) reflects the distribution and it affect the results only if the points are separated by more than 300 km.

ii) The lambda parameter is related to the smoothness of the resulting mass distribution. In previous studies (Forsberg and Reeh 2007, Sørensen and Forsberg 2010) using the original inversion method, a range of the lambda parameters has been found which gives a good compromise between the ability to recover details of the mass changes and its smoothness. In this work (suing a new grid for the solution and an extra solution area) we find that the solution is much less sensitive to variation of the smoothing parameter. By the use of synthetic data, we find that a variation of 30% in the smoothing parameter (lambda) resulted in a variation in the solution of less than 3%.

iii) We have used different synthetic data sets, like uniform mass loss over the region of interest, mass loss with spatial distribution extracted from the real data, masked and scaled to have a fixed mass loss over the region of interest and zero in the rest of the world. We used the synthetic with zero mass loss over the region of interest and a small signal outside mimicking the ocean signal, and we also used pattern mimicking a known GIA correction.

iv) The weights on the 100, 200 and 300 km solution used in the conversion method follow a Gaussian curve with parameters (its centre and the sigma) that are are tuned with the calibration procedure.

v) The calibration procedure shows how the variations of the masking in the preprocessing and the distance of the auxiliary solution area affect the final results. However once one or both this leakage control are included in the processing (with an initial configuration reflecting the resolution of the data, i.e. from 250 to 400 km), the synthetic solution recover the correct amount within a range of less than 15% error, i.e.

variations in the parameters of the solution area have small effect on final results.

3) Comment on independency of calibrated methods - Reviewer 2 (point 3)

Reviewer 2 statement (in point 3) clearly implies that two techniques to measure the same phenomena or the same quantity cannot be independent if they are calibrated on the same test case (or benchmark).

We don't agree with this view, on the contrary it is quite easy to demonstrate that a useful and meaningful comparison cannot be performed without a calibration on the same benchmark. For instance we can imagine two tools to measure lengths, e.g. a simple "meter" and a laser ranging, and we want to use them to measure a certain distance. If we calibrate those two tools in different ways, i.e. with different benchmarks, then when we compare the results we cannot say if the results are different because the two tools have been calibrated with different benchmark. But if they have been calibrated on the same benchmark, when we obtain different results we know that the problem could be in the procedure that we used rather than in the tools, e.g. the scientist had some reading problems.

This is what we mean in the sentence at pag 3400 line 27 commented by the reviewer 2 *(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).*

Our two methods are independent and the comparison of results from those two is indeed a cross validation. They are independent because they rely on different assumptions, different mathematical and technical framework. Starting from the same input data, one method results can be compared and validated with the other method results and vice versa, and that makes a cross validation in case the results agrees within the errors.

Another way to see that calibration does not make mathod dependent comes directly from first calibration experiment (mentioned in the manuscript) that we did and the following necessity to refine the inversion method. As we say in the text (Pag 3411 line 8-11) the original inversion method need a strong assumption that the gravity signal is negligible outside the region of interest. With synthetic data we can reproduce that′optimal condition and recover almost all the mass (up to 99%), i.e. we build for example a uniform mass loss over Antarctica of 100 Gt/yr and the inversion method results using that data as input is about -99 Gt/yr.

However when we use synthetic with zero signal over Antarctica and around it and all over the globe we use the real one extracted from GRACE, the original inversion method recover about 54 Gt/yr of the mass increase.

Adding a mask and/or an extra solution area in the inversion method allowed us to recover almost the full signal and to drop consistently the leakage from the ocean in the second case. In fact, in that way the inversion could account for gravity generated outside the region of interest. However the result over the first (uniform) synthetic experiment remains almost the same. This is an example to show that calibration do not make method dependent because after calibration two different tool can still give very different results in some particular cases.

Moreover, by considering that the conversion method does not rely on the assumption that there is no signal over the ocean, the example also shows the big difference in assumptions and in the way they handle leakage.

With the above explanation we consider also answered the other Reviewer 2 points that ask us to remove the concept of independency of our method:

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

-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 [. . .]'

Answer to the rest of Reviewer 2 point 3.

We know and we also write that our conversion method is similar to Horwat and Dietrich (2009). Our inversion method is similar to other inversion method like Shrama and Wouthers (2011) and it has been compared quite thoroughly within the IMBIE experiment which has been published in Shepherd at al. 2012. After a calibration (partially the same that we used for this work) the trend and especially the monthly time series obtained with 6 different methods (especially: Wahr, Luthcke, Shrama, Horwath, Forsberg/Barletta) agree very well within their errors bounds.

Supported by the GRACE derived mass time series in Shepherd at al. 2012, this work does actually resolve most of the methodological uncertainties.

Rev 2: 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.

We assume that only in our Conversion method; that makes one of the differences in the leakage treatment. Our assumption can be incorrect, but if after calibration the overall results matches in most of the basins with the solution of the inversion method (which account for both leakage), it simply means that the leakage form the ocean is in most of the cases (basins) negligible.

We do include the coastal rim in our processing and our calibration also accounts for broader leakage produced by GIA, as we explain in the calibration procedurure in the revised version of the SM.

Rev 2: 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?

Once calibrated, both methods act as linear operators on input data, so any variation in the input GIA pattern linearly reflects in the output mass variations. However, we did test many more GIA models than the ones used to produce the averaged GIA correction which includes uncertainties.

However, the calibration could indeed change if it was optimized to deal only with GIA patterns of different amplitude (e.g. dependent from viscosity), but we find an optimal compromise in order to retrieve present day melting rather than the GIA correction.

It is not clear if the Rev. 2 with the last question in the point 3 is referring to the Riva2009 empirical GIA model. In that case of course we believe that until someone will prove to have found the right GIA correction we are allowed to use any reasonable GIA correction available possibly with uncertainties.

4) The word "variability" in the title and the text (MH pag C1903, Rev 1, pag C2092-C2093 last sentence and rev 2 pag C2108)

The Rev.1 suggest the word "scatter" and Rev. 2 suggest the word "uncertainty". And in other comment reviewer 2 suggest uncertainty or error:

-P3399L19: change 'variability' to 'error'

-P3399L28: change 'variability' to 'uncertainty'.

-P3404L22: change 'variability' to 'uncertainty'

We think that those two words are not really correct to describe our work. The word variability has been used in the GRACE literature mostly to describe time variation so now it seems associated mostly to time variations, but the word variability in principle is correct. Variability is the property of the variable of a function, and the variable can be time, space or any other parameter which make the solution to vary. We show, through error and uncertainties evaluations, the differences among many possible alternative solutions of the monthly mass changes, i.e. the variability of the solutions of

the monthly mass changes with respect to some of the input used in the process of deriving it, and the process itself.

However since it is important to give the right impression from the title, we are going to modify our title in this way:

"Scatter of mass changes estimates at basin scale for Greenland and Antarctica"

We are deriving mass balance only from GRACE (as noticed Rev 2), but only because for now those data are unique. Our analysis could also be applied to results obtained with other gravity mission (like the GRACE follow on). And when gravity changes data will be available from other mission our analysis could be either extended to the new dataset or performed only using the new dataset.

So we think is not the case to specify in the title that mass changes are derived from GRACE.

5) GAC correction (Reviewer 1, point G.2)

We need to show the difference in using the old AOD1B-RL04 and the new AOD1B-RL05 with the old GSM-RL04 in order to asses the possible bias embedded in previous estimate of mass chages derived with GRACE RL04.

By using the delta-GAC (GAC RL05 - GAC RL04) we obtain not only jumps but also trends which are impossible to discriminate from the real trends. So by analysing the GSM it is not possible to fully isolate the GAC error. We could make the empirical test that reviewer is suggesting by computing also the jump directly using the GSM-RL04 and then compare the amplitude of the jump in this case and with the delta-GAC to see how they differ. However, we mainly want to show whether the error in the old AOD1B-RL04 gives an appreciable contribution to the trend (when using the release 04) rather than identifying the fingerprint of this error into the data.

Moreover, in the GRACE processing the AOD1B are used to correct for Atmospheric and Ocean contribution to the gravity variations. The GAC (GAA and GAB) products are provided along with the GSM. According to the GRACE L2 documentation (Technical Note 04 and pg. 39 of AOD1B Product Description Document), the GAC product has to be used to restore the Atmospheric and Ocean contribution whenever necessary for the user.

This means that the GAC are provided as an adequately good approximation of the Atmospheric and Ocean contribution to the gravity variation, in the same format of GSM. Since they are meant to be simply added back to the GSM, no scaling or additional processing is required. Moreover the GAC are directly obtained as monthly average of the AOD1B (pg. 39, AOD1B Product Description Document) so they are not the result of the difference between the GRACE L2 processed with or without AOD1B.

According to the above reasoning GSM = Processing (L1data - model); and GSM + GAC = Processing (L1data) -> GAC = Processing (L1data) - Processing (L1data - model);

This would mean that the processing acts almost linearly on the inputs (data and model). If GAC comes directly from AOD1B, it means that at least to a first approximation (to the extent to which the GAC data can be used as prescribed), GAC are a good representation of the difference between the L2 data corrected for the AO1DB and the one not corrected, i.e. of the effect of GRACE processing on the model. So if the propagation of difference in AOD1B were not very closely 1:1 in the GRACE processing then also the GAC could not be used to restore of the Atmospheric and Ocean contribution.

The difference between GAC-RL04 and GAC-RL05 (slide 20 in Bettapur GSTM presentation) and the difference between GSM-RL04 and GSM-RL05 (slide 21) prove that part of the difference between GSM-RL04 and GSM-RL05 is certainly due to the correction in the AO1DB product, but we can't conclude that, when using only GSM-RL04, the difference in AOD1B does not propagate 1:1. In fact in the GSM-RL05 there might

be other processing features that attenuate the slope amplitude.

Moreover, in the IMBIE exercise (Shepherd et al. 2012) we did use the delta-GAC as correction to account for the error in the AO1DB-RL04. So the results in this paper are just showing the effect of that correction, i.e. if we didn't use that we would have obtained a different trend for Antarctica of about 20 Gt/yr which is not negligible.

5a) MH, comment on fig.1 showing the GAC correction

The orange line in fig.1 is the function, continuous with respect to time, which has been fitted to the difference between GAC-RL04 and GAC-RL05 (delta-GAC), the green curve. The jump is in the green line and it is not extremely sudden, and we approximate it with the fitted function, so the overall result look "smoothed" as noticed by MH. In some case the delta-GAC is not even a jump but a linear trend, so we clarify the text in the manuscript to make this point clearer.

6) Question about leakage between basins (MH last point pag C1903; point G.4 Rev. 1).

We don't directly consider this kind of leakage/error which is implicitly accounted for in the comparison of our two methods and so in our accuracy error.

In fact different methods should be affected by leakage between basins in different ways so the differences obtained using our two methods should also account for the error produced by this kind of leakage.

The leakage between basins is basically related to the resolving power of the tool that we are using to derive mass balance at basins scale, i.e. GRACE data and the method. It is not possible to reduce that leakage, i.e. increasing the resolution power, without introducing additional constraints to find our solution, and if we did this, the mass would not be inferred purely with gravity data.

The possible correlations among close basins reflect this limit in the resolution of the method, and we have to remember that basin definition is built from topographical and hydrological information and it should reflect the dynamic of the ice, while the mass loss can also reflect other kind of dynamics. So the basin definition is suitable for comparison of results obtained in other disciplines but it is not the optimal subdivision for local study of GRACE data. That is the other reason why the correlations in results for close basins cannot easily be avoided.

7) Replacing C20 in CSR RL05 and other corrections for low degree (Rev. 1 point G.5, e MH point 3 pag C1903).

We replaced the C20 time series with the SLR derived one provided in the technical note N5 for the CSR RL04. For the new release, we downloaded and processed the data in April 2012 and at that time there was not the note N7 with the associated SLR derived C20 time series.

So we are reprocessing our data which are now including that correction and we make this clearer in the text.

Moreover we are well aware that linear rates of the coefficients C20, C21, S21, C30 and C40 (according to IERS conventions) have to be restored only in the RL04, and we did all the computations accordingly. We clarify this point in the text.

8) The explanation of the deg1-sensitivity kernel. - Rev. 1 (s6) [linked to the point 1 Rev. 2]

Rev.1: *This explanation confused me first. The description appears to imply that the Tables 1/2 present the amount of mass, which when placed in 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.

The reviewer interpretation is correct, however we used another definition/explanation for that, i.e. we show how much mass we retrieve if we generate a gravity field caused only by the geocenter shift of 1-mm in each of the direction (X Y and Z).

We will add the alternative definition/explanation as suggested by the Rev 1 to the manuscript.

The Rev 2 asks how we derived this sensitivity kernel for degree 1 correction. We generate the gravity field due to the geocenter shift of 1-mm in each of the direction (X Y and Z) separately using the following relation between Stokes coefficients and geocenter coordinates in the CF (center of figure) frame is derived after Cretaux et al. [2002] (see also Swenson et al. (2008) and Chambers (2006)):

$$C_{11} = X/(a\sqrt{3})$$

$$S_{11} = Y/(a\sqrt{3})$$

$$C_{10} = Z/(a\sqrt{3})$$

where a is the mean radius of the Earth.

In this way we find the gravity field generated by the motion of the geocenter in each direction, and we use our methods to compute the associated mass changes in the same way we do with the monthly solution.

The difference by using one method with respect to the other could also be included in our error analysis but this would complicate the reasoning even further. Also, we find that this difference is much smaller than the one produced by using different geocenter motion time series. The agreement between our methods with respect to the use of different data set is shown within our work. So we chose to keep things simple and produce the sensitivity kernel only with our inversion method.

Other comment on geocenter motion from Rev. 2

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.

Right. The geocenter motion affects only the mass balance estimate derived with gravity data.

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

We have mentioned that (pag 3404 line 16) degee-1 of Swenson et al. (2008) is derived with GRACE data and an ocean model. We don't see which other detail of their method can be useful to understand the concept of variability presented in our paper.

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.

The SLR derived geocenter motion contains the full observed signal, seasonal and trend. The Swenson et al. (2008) contains the seasonal signal and the trend which already accounts for the GIA, i.e. they removed the GIA trend during their processing.

So the SLR derived geocenter needs to be corrected for the trend induced by GIA before it can be used with GRACE data, while the Swenson et al. (2008) one can be used directly as it is. We explain this concept better.

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. . .

Our sensitivity kernel is based on the above procedure and the simple relation between geocenter coordinates and the harmonic coefficient for degree-1. We clarify the concept as outlined above.

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.

Klemann and Martinec (2011) report a wide range but they do not say that the range of viscosities used are all equivalently realistic. So we chose for now to stick to the range that is used for global GIA model.

Moreover the purpose of the sensitivity Kernel is exactly to provide with a very simple tool to compute a new degree-1 correction using other values of different geocenter trends or variations. So the range given by the reviewer (0.1 to 1 mm/yr) translates easily in Gt/yr using our table 1 and 2, for the whole AIS for example from about -8 Gt/yr to -80 Gt/yr. And if we consider the whole range plausible we just obtain much larger error on trend estimate.

9) Global problem and leakage from regions in the far field - Rev. 2 (point 2)

and also Rev. 2 point: *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.*

We do not consider the degree-1 twice when using the GIA correction. In fact we remove the degree 1 from the GIA correction because in some geocenter time series, the GIA effect is already included, (Swenson 2008), and where it is not included we consider degree-1 GIA correction in Wu et al. (2011) and in Klemann and Martinec, 2011. We clarify this point in the text and in Table 3.

We state in the text (pag 3408 line 16-17) that we include degree-1 for GIA computation, which is true for the simulation, but then we remove it to compute the apparent GIA mass changes. That agree that this sentence is confusing so we change that to make it clearer.

We do account for the degree-1 also in our calibration process because it does alter the calibration. In fact we tried what the Rev 2 is suggesting at the beginning of our calibration process, before refining the inversion method. However since including the degree-1 in the calibration process is correct we don't need to prove again the possible effect of not including it.

The conversion method (tuned from J. Wahr 2002) uses the assumption that the mass variation is originating from a thin layer on the surface. After that assumption the resulting mass distribution is localized as much as the low resolution allows it, so each pixel obtained converting the gravity field in the mass distribution, accounts for a region which is as wide as the GRACE resolution, i.e. 300 – 400 km.

The geoid or the gravity field is sensitive to mass changes in the far field, but the converted mass distribution is not. And that is what we are using in our conversion method and in the pre-processing of our inversion method.

The inversion method without leakage control does indeed suffer from far field leakage and that is the reason why we refined the inversion method with a leakage control pre-processing and suitable solution areas. After adding such leakage control we are confident that also the inversion method is insensitive to mass changes in the far field.

For the GIA prediction global or local holds the same reasoning except for the sea level contribution. Our ICE-5G GIA models are global while the IJ05 GIA models are

regional, i.e. they contain ice history only for Antarctica. However, even if we are using global models, we vary the viscosity as it was representative only of the local viscosity.

Since we don't use the degree-1 prediction coming from those models, and we transform their related gravity field into equivalent mass changes, our GIA corrections are unaffected by far field GIA signal.

Moreover we should stress that in this paper we are not interested in finding the best Earth model for GIA prediction in Antarctica or Greenland but only good reasonable corrections with a likely range of variability, so we chose the models that better fits the Riva2009 empirical model. In this way the apparent mass of the GIA corrections has plausible values even if those models could be slightly biased.

Rev 2 (pag C2106 last lines) "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."

We are going to describe the effect of the leakage control in the SM where we explain the calibration procedure (see our point 2, this reply). The calibration procedure account for the degree-1 and this mean that the way the ocean is treated already accounts self-consistently for the degree-1.

10) Our choice for the range of viscosity for GIA correction- rev 2 (point 4)

And also Rev 2 point: *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 x 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.*

Our choice to lower the viscosity for Antarctica follows a reasoning described in the manuscript at pag 4307, but we will elaborate and be more precise on that.

We can summarize:

1) Riva et al. (2009) found for Antarctica is an empirical GIA which is much lower than the one predicted by traditional GIA. Also Wu et al. (2010) show similar findings.

2) Thomas et al. (2011) find uplift rate much lower than the one predicted by traditional GIA.

3) From point 1 and 2 we have many indicators that traditional models overestimate GIA signal in Antarctica.

4) The GIA model has two main components that can be changed in order to obtain better agreement with observations: ice history and viscosity.

5) We think that the ice history needs to be revised in many regions of Antarctica according to many recent evidences such as (Todd et al., 2010; Ackert et al., 2011; Mackintosh et al., 2011) and possibly it should account for a lower ice loss since the LGM.

6) Barletta et al. (2008) and then Gunter et al. (2009) show that by lowering the viscosity one obtains smaller GIA correction, i.e. smaller GIA signal over Antarctica.

7) So new GIA models need to have revised ice histories and/or lower viscosities.

When we performed our analysis we did not have access to revised ice histories, so we simply decided to use traditional ice histories with lower viscosity in the mantle.

We choose a range of viscosity that produces results having small differences (positive and negative) with the empirical GIA Riva2009.

Moreover our viscosity range is in agreement with that inferred by Ivins for the Shepherd et al. (2012).

The contribution of the Northern Hemisphere, as explained in the reply to Rev 2 point 2, does not affect our solution as our GIA corrections.

We could include the new empirical GIA correction only when the associated paper is published.

We can also reply here to reviewer 2 question on rotational variations:

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?

We do not estimate the contribution of any Rotational feedback in our GIA correction. However for the contribution of GIA in the degree-1 trend we used values computed in published papers, in some of those cases the Rotational feedback is included.

11) The sentence in the conclusion about the robustness of our results. (Rev 2, point G.6)

We are well aware that in our results there could be still some bias mostly in the trends, so it is true that our results might not be statistically centered on the true results especially for the trends. In fact we don't say that our results are statistically correct, just meaningful with respect to the state of the art of all the known corrections.

Moreover, many of the known and unknown biases are most likely related to trends while we mostly analysed the monthly time series and the accelerations which are not affected by such biases. We are going to change the sentence and make our meaning clearer.

12) Error bounds against previously published ones (Rev1 G.1 point).

Previously published error bound are usually not very clearly characterized in terms of the individual contributions. So a comparison in many cases is meaningless, if even feasible. In Scharma and Wouters (2011) there is a list of previous estimates on Greenland, for example, with different error bounds.

In the revised manuscript we are going to stress our error bound.

13) about underestimate of the source of variability or uncertainties in mass balance- rev1 (s2) and rev2 P3398L07):

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.

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.

Reviewer 1 is right by saying that source of variability has been all studied each in some dedicated work and recently combined together. However, we say in both case commented by reviewers that sometimes the possible sources of variability and uncertainties in mass balance have been underestimated or overlooked. We do not mean to say that these sources of variability have never been studied before, but just means

that there are some of recent and less recent papers on mass balance that do not account for many of the most known sources of variability in their error budget.

Moreover the variability of mass estimate caused by the particular choice of degree one has never been addressed (as noticed by Reviewer 2) as well as the error induced in mass estimate by the error recently found in the de-aliasing product.

14) On "the first time" of our systematic analysis - Rev 2 (pag C2018, P3398L07)

P3398L07: change 'for the first time systematically' to "systematically". The former is not true; see for example Schrama Wouters (2011).

In Sharma and Wouters 2011 it is only mentioned in two lines the use of one single value of -8 Gt/yr as the correction due to the degree-1 with no error, in the case we want to compare their results with others, like Velicogna and Wahr 2005 which do not account for the degree 1. Sharma and Wouters 2011 do not account for possible errors in background models as the dealiasing products, and they use only 2 GIA corrections. Moreover they are not showing the impact of each source of difference in their errors, and especially not in the monthly solution. Sharma and Wouters 2011 contains an analysis about the sensitivity of their method and they use different GRACE solutions as input, but they don't do a systematic analysis on the different kinds of sources that can give difference in the monthly solution.

Also Horwath and Dietrich (2009) did an analysis of the errors for their different methods especially looking ar the leakage treatments, and they show the impact of many components in their error budget. However the diversity of degree-1 solution available is not accounted for. They use GRACE data from one processing centre (GFZ), and only IJ05 ice history for their GIA correction.

Now there is a new paper about comparison of different method for deriving mass balance from GRACE, i.e. Shepherd et al. 2012, but it does not show how much the different method affect the final estimate with respect to the total error or the error on each technique (because it wasn't the purpose of that paper).

Systematic discussions about error induced by GIA uncertainties are in Barletta 2008 and Gunter 2009.

So there are some systematic but partial analysis, and therefore we believe that our sentence is true as we formulated it, i.e. that we assess the errors for each of the source of discrepancy in GRACE derived mass balance, and we do that systematically for the first time.

We are aware that the source of variability/scattering in mass balance estimate are known but their relative impact on the estimate (both trends and monthly solution) and the error budget is not, and we assess this systematically for the first time.

15) Minor comments

We change the manuscript according to the main points discussed and where is still applicable we take into account all the other minor comment and suggestion of the reviewer.

In the following we indicate some of them:

MH) Mentioning the period for the trend in the Abstract: we add this information in the abstract.

MH) We add in the manuscript the suggested (by MH) explanation and related reference for the improvements in the new GRACE release.

MH) We add in the manuscript the reference to the website about the GAC issue as suggested by MH.

MH) We change the Fig.3 in order to make more readable the basins definition of Antarctica.

MH) We add some more symbols in the text and we are referring them in our pictures as suggested by MH.

rev1, s1) Forward referencing. We fix the text trying to avoid as much as possible forward reference.

rev 1 -s3) Unclear explanation about GAC correction: We change the text to explain better this paragraph.

rev 1 -s4) Degree-1 in principle is not detected by GRACE because the satellites move in space together with the centre of mass of the Earth. However there are some arguments in favour of the thesis that degree 1 has a partial effect. In absence of a solid reference for this discussion, we change the sentence in this way: "they partially detect" with "they in principle cannot detect".

rev 1 -s5) We agree with the reviewer and we change the text to clarify that.

rev 1 -s7) With "after new consideration" we mean that we are going to explain our particular choice for the GIA corrections using also arguments that comes from recent papers.

rev1 -s8) We actually didn't say clearly which are the "two alternatives" there. We refer to the two most used ice models for Antarctica, i.e. ICE-5G and IJ05. We fix the text to say that clearly.

rev1 -s9) We change the first sentence of the section Methodology to make clear that we are referring to the method to derive mass changes from GRACE data.

rev1 -s11 and s12) These sentence will be completely changed in the revised manuscript in light of the discussion in point 1) of this reply.

rev1 -s13) We discussed the general point G.2 of the reviewer, moreover the RL04 is not going to be used anymore, so we change the text to say that we generate an alternative correction that can be used to correct old estimate derived with the RL04.

rev1 -T1, T3, T4, T5, T7) We change the text accordingly to rev comment.

rev1 -T2) We are considering removing the small introduction of section 2.

rev1 -T6) A GIA model could be also referring to the geopotential changes and uplift rate output of simulation using an Ice+Earth model. In fact those outputs should be unique for any given Ice+Earth model.According to this definition, Tellus does provide a GIA model. We try to better specify this definition in the text and that we will use the words "GIA model" with that meaning.

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