

Interactive comment on “A new approach to estimate ice dynamic rates using satellite observations in East Antarctica” by Bianca Kallenberg et al.

Anonymous Referee #2

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Summary: The manuscript back-calculates dynamic mass loss from East Antarctica using repeat GRACE and satellite altimetry observations paired with models of surface mass balance, firn compaction, and glacial isostatic adjustments. The authors find that rates of dynamic mass change inferred from the two different satellite observational platforms yield similar results, indicating that either platform can provide reasonable estimates of dynamic mass change when paired with current model outputs. The authors also infer that the good agreement between dynamic mass change estimates derived from the two different satellite platforms indicates that the most up-to-date RACMO model provides accurate estimates of surface mass balance for East Antarctica.

The results of the paper are interesting in that they show models of SMB, firn com-

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paction, and GIA are accurate enough to allow us to tease-out the ice dynamics signal from repeat gravity and laser altimetry observations. There are several relatively small modifications that would improve the overall quality of the manuscript, including: 1) removal/adjustment of commas (described below), which often break sentences into somewhat awkward fragments, 2) incorporation of data and methods description that is currently contained in the appendices, and 3) change references to “ice dynamics rates” to “mass change rates due to dynamic change” or something similar. My biggest concern is in regard to (2) above. I highly recommend that more information on the datasets and methods is included in the body text of the manuscript. The details regarding how (1) you process the GRACE and ICESat data, (2) the versions of RACMO used in the study differ from each other, and (3) the firn compaction converts SMB input to estimates of surface elevation change are incredibly important for assessing the validity of the results and for reproducing the method elsewhere. Although this will lengthen the manuscript, I think that adding more detail regarding the data used in the analysis is imperative. Given that the firn layer in Antarctica can be tens of meters thick and that the interpretation of altimetry data is incredibly sensitive to the accuracy of the firn compaction model (see Zwally et al., Journal of Glaciology, 2015 for an example) this is particularly important for the firn compaction model. As a follow-up, why use RACMO2.1 to run the firn compaction model when RACMO2.3 was found to produce better results for the GRACE-ICESat dynamic change comparison? This is inconsistent.

Detailed Comments: p. 2, line 4: Change to “and bedrock uplift rates.”

p. 2, line 5: Move the comma after “ice lost” forward so it is after “balance”

p. 2, line 11: Either move the definition of firn so that it comes earlier in the sentence or remove it entirely. Currently it’s in an awkward location.

p. 2, line 14: Change to “results in a change in the ice sheet surface elevation without...”

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p. 2, line 15: Remove “potentially”

p. 2, lines 20-21: This sentence should be broken-up so it's easier to read. It took me at least 2 attempts to pause in the appropriate places and follow the entire sentence. I recommend something like: “Ice discharge is the product of the ice velocity and thickness across the grounding line. Satellite rate altimetry is used to retrieve information about ice surface velocity. Ice thicknesses are estimates from airborne radar or, in the absence of radar observations, using surface elevation observations under the assumption that the ice is floating.”

p. 3, lines 6-7: Change to . . .”(GIA), which is the response of the lithosphere to changes in surface loading.”

p. 3, line 10: Remove the comma before “by”

p. 3, lines 14-16: Either remove these lines “We combined our . . . to direct observations of ice surface height from ICESat.” or rephrase. I don't think you need to go into much detail at this point and these two sentences are currently really difficult to follow.

p. 3, lines 18-20: I find this sentence confusing. I follow that you obtain similar estimates of dynamic mass change from GRACE and ICESat observations but I don't understand what you mean that they “can be used to model surface elevation changes that are comparable with altimetry observations”. ICESat data are altimetry observations. Do you mean that you can use GRACE data to estimate the surface elevation change expected due to dynamic change using the methods you describe here? If so, you need to revise the sentence so that is clear.

p. 3, line 22 to p. 4, line 6: You start off by stating that there is a positive mass change trend across the study region but then go on to say the region is roughly in balance. Please revise to present a more consistent background on mass change estimates from the region.

p. 4, lines 18-26: I assume that the “slope correction” you present in Equation 1 is

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an effort to account for drifting snow across the ice sheet surface. I believe this is already accounted for in RACMO (as you state in the appendix) so you may be “double-counting” for snowdrift. If you are referring to some other mechanism, please make that more clear. Additionally, the last sentence here should state specifically where you obtain estimates for these variables, not just what is “typical”.

p. 5, line 1: Replace “seen” with “measured”

p. 5, line 2: Remove comma before “as well as the effect of GIA”

p. 6, lines 5-6: Replace with “The solutions to Equations 4 and 5 are the change in ice mass, DM/dt , and surface elevation, dH/dt , associated with changes in ice dynamics”, with the proper subscripts and superscripts added.

p. 6, line 6: Replace “mass rate and height rate” with “rate of change in mass and surface elevation”

p. 6, lines 17-19: There’s an assumption inherent in these conversions that the entire ice sheet thickness is composed of glacier ice when we know this is not the case. It would be helpful to have an estimate provided somewhere of the fraction of the total thickness that is firn versus ice. If the firn column is only ~ 50 m but the ice is ~ 2000 m thick, this assumption is fairly reasonable. However, if the ice is relatively thin and/or the firn column is very thick, then the density used for these conversations should be reduced.

p. 6, line 20 to p. 7, line 4: Shouldn’t you be adding dH/dt estimate from GRACE to the dH/dt estimates for SMB and firn AND dH/dt from GIA to get a signal that is equivalent to the ICESat $dHdt$? Also, if dH/dt of ice from GRACE and ICESat are not equal, the discrepancy could also be caused by the spatial and temporal variations in the density of the ice used in the conversion, inability of the SMB and/or firn model to realistically simulate surface changes, in addition to errors/limitations in data processing techniques. You should list all potential sources of error briefly here.

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p. 7, lines 9-10: Split the sentence so that it reads "...measured by GRACE. Figure 2a shows the map of the GRACE mass change signal and Figure 2b shows a time series for a coastal location near..."

p. 7, lines 17-19: Why not use the ICE_5G_C results? Are the other results more realistic/better for some reason?

p. 7, line 20. Break into two sentences so that you now have "... snowfall and ice discharge. The GIA-corrected GRACE mass change data suggest a positive mass trend of $\sim 32 \pm 8$ mm w.e. yr^{-1} between $30^{\circ}E$ and $70^{\circ}E$ and a substantial increase in mass from 2003-2009 (Fig. 3b)." The anomaly you list should be averaged over this entire region. I think the anomaly is only estimated over a smaller region currently, which is a bit misleading. Also, how can you attribute this to SMB? The SMB signal is actually from RACMO, correct? Are you presenting the mass gain estimated by RAMCO for SMB only or the GRACE SMB+discharge mass signal?

p. 8, line 3: How do you convert the ICESat data into spherical harmonics? What precisely does this mean? Does it mean you spatially average the data in some way? Please elaborate.

p. 8, lines 5-17: I find this section to be really difficult to follow. You should make it clear that you are using the RACMO models to estimate SMB contributions to the GRACE and ICESat signals. Saying "For both RAMCO2 models the ice dynamic estimates" and "Using RAMCO2.3 the ice dynamic estimates" reads a bit like you are estimating the dynamic signal directly from RACMO. Are the rates of dynamic mass change averages over the entire time period for each observational platform? Are the RMSE estimates the RMSE of the difference in SMB between the two RACMO versions over the entire study region? It would be helpful to have numeric estimates clearly presented in this section along with their error estimates. It would also be helpful to focus on just the difference in RACMO SMB over the study region, with discussion as to which version produces more realistic results when used to tease-out the dynamic signal, then

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compare the dynamic change estimates. Right now there's just too much going on at once.

p. 8, lines 18-26: As mentioned earlier, I think you need to add dH/dt from GIA into your GRACE-derived dH/dt estimates. You should also include values for the trends you discuss here so that the reader can discern "strong" and "weak" trends.

p. 9, lines 12-16: You should include maps of uncertainty with the dynamic change estimates in the text (move Figure A2 to the body of the manuscript).

p. 10, line 1: Can you substantiate this remark that the different GIA models have a small effect on the ice dynamic change estimates? There have been rather large error bars in previous Antarctic mass change estimates from GRACE that have been largely attributed to uncertainty in the GIA signal.

p. 10, line 3: Replace "We believe" with "Our data suggest"

p. 10, line 5: Is the different statistically significant?

p. 10, lines 12-13: Replace with "Thus, a comparison of estimated changes in ice dynamics derived from GRACE and altimetry observations not only provides information about dynamic mass change, but may also help to identify regions where models fail to accurately simulate variations in SMB."

p. 12, line 1: Remove comma after "grid"

p. 16: What about uncertainties associated with GIA? I expect that these are quite large but they are seemingly overlooked. They are likely difficult to quantify but you could likely obtain uncertainty estimates computed for each GIA model from the model developers.

Figure 2: Include the name of the model used in the timeseries of GIA.

Figure 3: Remove the last sentence in the caption.

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Figure 4: The dynamic mass change rates are obtained from GRACE and ICESat using RACMO to parse-out the SMB signal, not from RACMO using GRACE and ICESat as currently presented.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2016-269/tc-2016-269-RC2-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-269, 2016.

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