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Interactive comment on "A high-resolution bedrock map for the Antarctic Peninsula" *by* M. Huss and D. Farinotti

M. Huss and D. Farinotti

matthias.huss@unifr.ch

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We would like to acknowledge M. Morlighem for the constructive comments that point out some important issues which we tried to more adequately address in the revised version of the paper. In particular, we provide a more detailed discussion of the limitations of our approach and stress the importance of regarding it as an "sophisticated interpolation scheme" rather than a full modelling study, thus addressing both major comments of the reviewer.

Below, we respond to all comments, and state how we account for them in the revised version of the paper. The responses (normal font style) to the reviewer's comments are written directly into the reviews (displayed in italic font style). The corresponding revised sentences in the manuscript are given in quotation marks, including line num-

bers in the revised manuscript.

The first issue that I see in the proposed approach is that mass is not conserved. The method is based on mass conservation principles (equation 1, page 1197), but many operations relax this constraint:
1. page 1199 line 23: unrealistic ice thicknesses become h_{max}
2. page 1199 line 26: the ice thickness map is smoothed to remove local noise
3. page 1202 line 21: a "correction map" is superimposed to the modeled ice thickness map in order to better fit OIB data
These procedures will affect the predictability of ice sheet models because they rely on mass conservation to compute changes in ice geometry (free surfaces and/or mass transport).

We absolutely agree with these comments. Although our methodology is in principal based on simple ice flow modelling (shallow ice approximation), results are corrected in a second step at locations where they become unrealistic or direct observations of thickness are available. We are aware that this procedure might violate mass conservation to a certain degree. However, our aim is to produce a best possible interpolation of ice thickness for the entire Antarctic Peninsula. We thus argue that forcing the final solution to fit all direct measurements (reviewer's point 3) is more suitable than forcing the solution to obey the mass conservation calculated through one particular model.

In the revised version of the paper, we include an additional paragraph addressing of the mass-conservation issue in the Discussion section.

"As the calculated ice thickness locally disagrees with direct observations based on OIB (Fig. 5) we apply a correction grid to tie our final bedrock map to the measurements (Fig. 6b). Although this procedure might locally violate mass conservation according to our simple modelling approach, we argue that forcing the final solution to fit all direct measurements is more suitable than forcing the solution to obey mass conservation calculated through one particular model."

In particular, we also stress the main goal of the study: The optimal *inter- and extrapolation* of the scarce ice thickness data by including all available information and combining the different datasets using simple ice flow modelling.

"Here, we derive a new high-resolution ice thickness and bedrock dataset for all glaciers of the Antarctic Peninsula north of 70° S. By combining simple ice flow modelling with a large number of direct ice thickness measurements from Operation IceBridge, gridded surface velocities and mass balances, local glacier thickness is calculated from characteristics of the glacier surface. This approach allows us to inter- and extrapolate the scarce thickness observations relying on physical relationships and thus to achieve a highly-resolved bedrock estimate."

"The combination of these observational data with considerations of ice flow dynamics allows us to extrapolate to unmeasured catchments based on physical relations. This is a significant advantage compared to direct extrapolation approaches and makes it possible to increase spatial resolution by one order of magnitude."

In order to address the reviewer's first point we evaluate the percentage of the area affected by the correction to h_{max}. The analysis shows that this correction only has an effect on a limited part of the study domain (less than 2% of the total area of the Peninsula). This is now stated in the paper.

The smoothing (point 2) does not result in a systematic effect regarding cross-sectional thickness and mass conservation, for example. As we use surface slope (among other variables) to predict local ice thickness, it is important however to ensure that spurious small-scale surface roughness does not affect the calculated glacier bed which justifies the applied smoothing.

The second issue is that the misfits between the calculated flow speed and the measured flow speed (relative error of 50%), as well as the OIB data themselves (RMSE = 255 m) are very large. The solution that the authors propose to reduce the error to OIB data is to create a "correction map" that is based on the difference between the calculated thicknesses and the OIB measurements, and to subtract this map from the calculated ice thickness map. It will indeed reduce the mismatch between the model and the measurements, but it sounds like sweeping the dust under the carpet. Why are these misfits so large? Does it come from the fact that only 2 parameters are optimized (f_{RACMO} and A_{f})?

Whether the mismatch of modelled and observed OIB ice thickness and flow speed is "very large" or not has to be considered in the context of the size and the complexity of the study region. The topography of the Antarctic Peninsula is highly complex and we cannot expect capturing all local details. The relatively large misfits are certainly related to the fact that only two parameters are optimized as suspected in the review. However, having more and regionally specified optimization parameters would not allow extrapolation to unmeasured parts of the study area in our opinion. We thus consider our choice of optimizing two factors as the most straightforward and robust option. The actual uncertainties enter our uncertainty map (Fig. 7) that we provide along with our bedrock topography in digital form.

We do not agree that the correction map applied *after (!)* the validation is "sweeping the dust under the carpet". The performance of the model is evaluated *before* the corrections given by the direct observations are applied, and the model result is thus independent from the observations at the local scale. The correction map allows including ice thicknesses measurements in the model-based bedrock topography. This step is highly important as the goal is to integrate the true and observed thickness values in the final product wherever this information is available.

In the new version of the paper, the Discussion has been extended with an additional paragraph addressing the drawbacks and the advantages of the correction map applied (see proposed text above). Another paragraph of the Discussion already deals with the potential problems of calibrating only two parameters and answers this part of the reviewer's comment.

"The model parameters are assumed to be constant over the entire study region, i.e. to not show any spatial variability depending on geographic location or local climate. This is, of course, a strong simplification. For example, ice temperatures, and thus the flow rate factor A_f, are likely to show variations along the glacier and to be different for the maritime northern tip of the Peninsula compared to the more continental south of the study region. Also f_RACMO is assumed to be the same for every glacier although the bias in the downscaled RACMO dataset is poorly constrained in the spatial domain. The apparent mass balance $\widetilde{b}$ (Eq. 1) is determined by the glacier's current dynamic imbalance. $\partial h/\partial t$ shows a high spatio-temporal variability over the Antarctic Peninsula (e.g., Berthier et al., 2012) and was only crudely approximated. As the unambiguous calibration of the model was only possible with reducing the degrees of freedom, the above effects were not taken into account. The regionally constrained calibration experiments however indicate that these simplifications have rather small effects on the final result."

Such large values also call into question some of the conclusions: calculated ice thicknesses reach more than 1500 m (page 1205, line 5), is this something that is supported by direct measurements from OIB data (in which case it should be mentioned), otherwise that might as well be an artifact of the model in a region that is not well constrained... p 1206 line 7, the authors claim that they capture "many features of small scale variability", but are they real given the uncertainty in some regions?.

We partly agree with the reviewer here. Some of the inferred variability might not be real but be caused by the utilized approach. However, this holds true for any model applied to any environmental problem. The large ice thicknesses in deep troughs are indeed

supported by direct measurements (clearly stated in the paper now). Our methodology allows us to detect morphologically similar situations for which comparable thicknesses are likely. Our statement mentioned by the reviewer ("many features of small scale variability ...") rather points to general issues related to the resolution of the dataset: A 1 km bedrock DEM is unable to reveal structures (subglacial valleys, rock outcrops) with a length scale smaller than 1 km. Such features are typical on the Peninsula and can be resolved by the new dataset resulting in a more realistic thickness estimate. This is now clarified in the manuscript.

"For some outlet glaciers, thicknesses of more than 1500 m are found (e.g. Crane, Flask, Lurabee, Fig. 1) and the bedrock is partly located up to 1000 m below sea level. This is also supported by OIB data (see Fig. 6a)."

"In comparison to Bedmap2 (Fretwell et al., 2013), our new bedrock for the Antarctic Peninsula provides a ten times higher resolution and significantly more details on the sub-surface topography. We capture many features of small scale variability that were lost in the 1 km grid of Bedmap2 such as narrow subglacial troughs or smaller ice thicknesses around rock outcrops."

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page 1193 line 5: spatially distributed ⇒ comprehensive coverage ?.

"However, ice flow modelling requires accurate bedrock data with a comprehensive coverage."

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page 1193 line 25: it has not been shown that the resolution provided by Bedmap2 is sufficient for large-scale ice sheet model (and I personally don't think so). I would add "might" to the sentence.

We agree and follow the suggestion.

"Whereas for large-scale ice sheet modelling the spatial resolution provided by Bedmap2 might be sufficient, ..."

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page 1193 line 28: use "projection" rather than "prediction".

Done.

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page 1194 line 20: the velocity map from Rignot et al. is available at a resolution of 450 m, how do you interpolate the data on your 100 m resolution grid?.

This information is already provided in the Methods section: No interpolation of the flow speed data to a 100 m grid is performed; the model results are resampled to the (coarser) grid size of the observations for comparison to the velocity measurements.

"Calculated surface velocity is resampled to the 450 m grid of the observations and a point-to-point comparison is performed ..."

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page 1195 line 18: is the accumulation rate in water equivalent or ice equivalent?.

Given in water equivalent (i.e. mass). The unit is replaced with kg m^{-2} yr^{-1}.

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page 1197 line 7-14: the SMB data is transformed by using an elevation-dependent

relationship. Is the total amount to SMB conserved?.

Yes, SMB is conserved within <1% over the model domain. This is now stated in the paper.

"The total surface mass balance is conserved within <1 % of the original RACMO dataset over the study region."

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page 1197 line 16: mention the unit of b (kg/m^2/yr, not m/yr).

The unit given in m yr^{-1} is dh/dt, and not b. According to Eq. 1, the unit of b becomes kg m^{-2} yr^{-1}.

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page 1197 eq 1: mention that basal melt is neglected.

Mentioned when introducing the basic concept.

"Based on surface mass balance, ice volume fluxes along the glacier are determined and are used to compute ice thickness based on Glen's (1955) flow law for ice deformation and assumptions on basal sliding. Contributions of basal mass balance are neglected."

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page 1198 line 1: is the linear decrease of dH/dt applied to the entire basin or only to fast-moving ice? Thinning is indeed most commonly present on ice streams (e.g. v 50 *m/yr), rather than stagnant ice on the sides.*

This is a good and justified remark and certainly is an effect that would need to be considered in a further development of our approach. In our current methodology, we however perform all calculations (Eqs 1-4) for a 2D shape of the glacier and derive the spatially distributed ice thickness only in a last step (see page 1197, lines 2-6, in the TCD paper). Thus, 3D effects, such as the enhanced lowering over fast-flowing ice, are not included. As our assumptions on surface lowering are very crude (there is a large variability, see e.g. Berthier et al., 2012, among individual glaciers that is not accounted for by our assumptions) we do not expect that neglecting these 3D effects has negative consequences on our mountain-range scale results. Detailed studies for individual glaciers might however need to consider them.

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page 1198 line 6: how do you constrain the ice thickness at the inflow boundary (or on the divides)?.

As we consider ice flow catchments, there are no "inflow boundaries" in that sense. Ice thicknesses for ice divides are not prescribed and are calculated in the same way as for all other cells, as explained in the Methods section.

"We then extrapolate calculated mean elevation band thickness from the simplified 2-D shape of the glacier (Fig. 2b) to the 100 m grid by inversely weighting the distance to the closest rock outcrop, and including surface slope α of each grid cell proportional to $(\sin\alpha)^{-n/(n+2)}$ (based on Eq. 2)."

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page 1200 line 2: derivate $\Rightarrow$ derive, or take the derivative.

"... we solve Eq. 2 for q_d and take the derivative."

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page 1200 line 16: I don't agree with the statement that using two independent datasets allows to determine unambiguously f_RACMO and A_f. It is probably true in this case (because these two parameters are assumed to be constant for the entire domain), but this would probably not be true in a more general case.

This is formulated more cautiously in the revised manuscript.

"It is therefore essential to utilize two independent sets of measured data to determine physically meaningful values for f_RACMO and A_f which are assumed to be constant for the whole domain in our case."

———

page 1200 line 27: I don't really understand the meaning of "flow speeds determined by grounding line dynamics". Do you mean that the Shallow Ice Approximation (from which you derive your simplified model) is not a valid approximation in the vicinity of the Grounding Line?.

Yes. Sentence reformulated.

"... and exclude areas close to sea level as the shallow ice approximation is not suitable to reproduce flow speeds in the vicinity of the grounding line."

———

page 1203 line 13: add a comma after "Peninsula".

Done.

page 1203 line 23: have you tried to increase/decrease dH/dt by ± 2 m/yr instead of a factor 2?.

No, this was not attempted for the following reason: Uniform dH/dt over the entire glacier elevation range (i.e. also above 700 m a.s.l.) does contradict direct observations (see Scambos et al., 2004; Pritchard et al., 2009; Berthier et al., 2012; Kunz et al., 2012). Changing dH/dt by ± 2 m/yr in the terminus region would however result in a much smaller effect for glaciers flowing into the Larsen A and B embayments than in our experiment, and would also strongly disagree with observations for all other regions. We thus do not change our sensitivity experiment which already considers relatively large uncertainties in this parameter.

page 1205 line 4: add a comma after "According to our dataset".

Done.

page 1205 line 10: why don't you use the firn layer depth provided by RACMO? an average density of 870 kg/m^3 sounds a bit low for the thickest regions of the Peninsula.

In response to this comment we now use firn-air content data on a 5.5 km grid based on RACMO and a firn densification model. Data were kindly provided by S. R. M. Ligtenberg (see Ligtenberg, 2014, chapter 5, for a description). We obtain average density for every gridcell from calculated local glacier thickness and the RACMO-based firn-air

content and thus also take into account the spatial variations in mean density of the ice column. The thickness-weighted average density is slightly smaller than the value used in the TCD paper (852 kg m^{-3}) but, as stated by the reviewer, the mean density of outlet glaciers is higher.

The update is described in the paper (including references) and has caused small changes in the calculated sea-level equivalent of Antarctic Peninsula glaciers (Table 1 was changed accordingly).

"Average glacier density for every grid cell is obtained by combining calculated ice thickness with results of a firn densification model (Ligtenberg et al., 2011) driven by RACMO at 5.5 km resolution (see Ligtenberg, 2014, for a description). Over the glacier volume analyzed, a mean density of 852 kg m^{-3} is found. For ice grounded above sea level we calculate the contributing ice mass by using local glacier densities."

page 1208 line 3: what is the agreement in meters (not in percent)?.

The absolute misfit in meters has been added.

"Median and maximal deviations are contained within 3.7 % and 11.7 % of the local ice thickness, corresponding to 27 and 45 m, respectively, indicating that OIB also captures the bottom of deep glacial troughs (Farinotti et al., 2014)."

page 1210 line 18: does TC allow to cite submitted papers?.

The paper is now in press and the reference has been changed accordingly.

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page 1217 figure 1: could you change the color of the green line to white? We might visualize it better.

We tried to display the green line in white (for Figure 2, which is probably meant here). However, it was considerably more difficult to visualize it than before. We now displayed the contour as a solid, dashed line in black to improve the figure.

Throughout the text: many commas are missing before "which".

Commas before which were introduced where necessary.

Additional reference:
Ligtenberg, S. R. M. (2014). The present and future state of the Antarctic firn layer. Ph.D. thesis, Utrecht University, ISBN 978-90-8891-834-6.

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