Reviewers' comments are *in italics*, while our replies are in roman text. Serif font indicates our action.

Reply to Reviewer 1

We thank reviewer 1 for providing a detailed review.

General remarks

My first general comments concerns the data used to validate the model. We don't expect the model to reproduce the exact value but to belong in the error interval associated to each value. Are these incertitudes available for all dataset? How the incertitude associated to each dataset can be incorporated in the method should be discussed. For example, when it is said "The root mean square error (RMSE) is 43–46 ma^{-1} (Table 1), having a similar magnitude as obtained in a data assimilation study (38 ma^{-1}, Price et al., 2011)", this should also be compared to the mean error associated to the data themselves. What is the influence of having a dataset which have been acquired along a quite long period in comparison to the total period, as for example the surface speeds which include speeds measured from 2007 to 2010. How can we account for this. Is it negligible? Is this application using all the possible data? This point is tackled in the conclusion regarding isochrones, but it might be interesting to include a discussion listing all available data, their limits, errors and how they can be used in future hindcast applications.

A formal, comprehensive analysis how uncertainties in input data propagate through a system, is very challening, and beyond the scope of our manuscript. In the revised manuscript, we now provide an outlook on how our work could be integrated into statistical frameworks: "Furthermore, hindcasting can be part of a concerted effort to validate ice sheet models. Other parts include formal sensitivity analyses to assess error propagation in forward models as, for example, carried out by Larour et al. (2012b,a). Ultimately, validation may be integrated in statistical frameworks to quantify uncertainties in ice sheet evolution due to different sources of model and observation uncertainty (c.f. Steinschneider et al., 2012, for an example in hydrologic modeling)."

Informally speaking, at the current stage of ice sheet modeling, we are trying to reduce the mismatch between observed and simulated quantities, while realizing that agreement within errors associated with the observed quantities may not yet be achievable for some quantities (e.g. surface speeds). We could add a statement like "The RMSE is more than an order of magnitude larger than the error associated with SAR-processing.". However we are unsure about the value of such a statement without knowing how such an error progagates through the system (see above).

The RMSE is not particularly sensitive to the combination of year of simulation and observation. We added the following text to the discussion "RMSE ranges from 43 to $48\,\mathrm{m\,a^{-1}}$ when computed for six different combinations of year of simulation and observation (Supplement Table 4.2)." and we updated the Supplement accordingly by adding Table 4.1. Also we now mention errors associate with observations, where available.

Regarding use of available data sets, we clarified p. 5074, l. 8, by replacing

"The hindcasts are then compared to observations"

with

"The hindcasts are then compared to observations listed below. Our study does not use all available observations, but the one used allow drawing robust conclusion. Additional observations that could be used are mentioned in Section 5."

Listing all observations that could be used for validation, however, would rest on the assumption that we know all available observations. This is a claim we do not dare to make. Moreover, the number of observations is constantly growing, and such a list might be already outdated at the time of publication.

I understand that it is not the main objective of this paper, but I would have like to see a deeper discussion about which of the three initialization is working the best. Just saying "this initialization works well with these data and not well with these ones" doesn't allow to really conclude which initialization is the best here. Can the method be push further by calculating a pondered mean error to give a global mark to each method using all datasets available? This might be done using a cost function as for control methods. Moreover, this cost function could then account for data errors. Or may be we cannot conclude which initialization method is here the best, but this should be discussed.

As outlined in our general reply, it is not the intend of our paper to identify a preferred initialization procedure. While in our study the "constant-climate" initialization appears to perform best, this may not necessarily true for other combinations of ice sheet models and boundary forcing. We thus hesitate to make any strong statement. In the revised manuscript, we discuss the current limitations of hindcasting as a method to asses model performance, and conclude that a quantitative assessment is currently not possible.

Nonetheless, a cost funtion is a great idea. Through a recently funded project, we are commited to explore this topic further.

I would have like to see which differences in term of sea level contribution you then get running the three different initializations forward in time over a century or so. I agree this is somehow beyond the scope of this paper, but since hindcast has the objective to validate the initialization of the model before running a forecast simulation, the last part of the exercise is missing.

As outlined in our general reply, this is not the scope of our paper, and would require a whole new set of forcing fields that are different from the ERA-Interim forced downscaling product used in our study. We agree with the reviewer that it is a natural part of the exercise and we are indeed addressing this last part in a companion paper by Aðalgeirsdóttir and others (manuscript in prep.)

Other minor remarks

p. 5071:, **l. 7:** *We validate these ... changes. Not very clear if it is a general statement or a statement related to this particular application. In that case, I would suggest to move this further in the introduction. Moreover, should write: We validate these initial states using observations of ice thickness, ice discharge, surface elevation changes, surface speed, and time-series of mass changes.*

As outlined in the general reply, we rephrased the introduction. The new version does not include this sentence anymore.

p. 5071:, **l. 20:** *Morlighem et al. (2010) paper is not applying inverse method to a whole ice-sheet. In Gillet-Chaulet et al. (TC, 2012) inverse methods were applied to infer the basal friction below Greenland and the discharge of individual catchments was used to validate the initialization.*

The reviewer is correct that Morlighem et al. (2010) did not apply an inverse method to the whole Antarctic ice sheet but to Pine Island Glacier. As outlined in our general reply, we rephrased the introduction. The new version does not contain this sentence anymore, but we added a citations to Gillet-Chaulet et al. (2012) to p. 15, l. 21:

"Properties at the ice sheet bed cannot be directly observed. Ice sheet models may achieve a close fit to observed surface speeds by using data assimilation techniques that invert for a field of basal parameters, for example adjusting the value of the basal traction at each grid location (Morlighem et al., 2010; Price et al., 2011; Gillet-Chaulet et al., 2012)."

p. 5073:, **l. 3:** *can you be more precise about what is the last part of the simulation.*

Changed to "the last 2,000 years of the initialization."

p. 5075:, **l. 6:** *The root mean square error (RMSE) of the surface velocities...*

Changed to "The root mean square error (RMSE) of the surface speeds"

p. 5075:, **l. 21:** *this last sentence about ice-stream velocities should be before (line 8)? Why the ice-stream speeds are underestimated should also be discussed here.*

We agree that such a discussion is useful, however we consider the Discussion section as a more appropriate place to keep results and their discussion strictly seperated. We added a paragraph to the Discussion section:

"Surface speeds of most major outlet glaciers are under-estimated by all three initial states relative to the 2006–2009 average. First, simulated surface speeds of initial states should be compared to the observations from 1989, but observations for this year are not available. Between 1989 and 2006 surface velocities of many outlet glaciers have experienced large increases (Joughin et al., 2004; Luckman and Murray, 2005; Howat et al., 2005). Therefore initial states seemingly underestimate speeds in these outlet glaciers. Second, a different explanation is required for the 'Northeast Greenland Ice Stream' because the inland part has not undergone significant changes in flow speed (Joughin et al., 2010) and no initial state reproduces the fast-flow pattern. For the onset region of 'Northeast Greenland Ice Stream' Fahnestock et al. (2001) estimate a geothermal flux 15 to 30 times higher than continental background. Such a feature is not present in the data set by Shapiro and Ritzwoller (2004) used as the basal boundary condition for the conservation of energy equation. Finally, our choice of the three spatially-uniform scalar parameters (Supplement), controlling ice dynamics and basal processes, attempts to minimize the root mean square error. Because 92 % of observed surface speeds are smaller than $100\,\mathrm{m\,a^{-1}}$, our parameter choice favors slow flowing ice."

p. 5077:, **l. 10:** *By assuming the same equal split... This paragraph is not really clear.*

Why simulated values cannot be compared directly to the observed cumulative mass change of -1695 Gt?

We replaced

"Trend under-estimation is expected because of the absence of ocean forcing that could lead to an increase in ice discharge."

with

"As mentioned earlier, observations show a rapid increase in ice discharge since the late 1990s, which was attributed to changes at the ocean boundary. Our model does not include ocean forcing that could lead to an increase in simulated ice discharge. Therefore, under-estimation of the simulated mass loss trend is expected."

This should explain why we expect that our simulations are expected to under-estimate the mass loss.

Table 1: the RMSE of which quantity? Should be specified.

Clarified.

The legend of Figs. 4 and 5 should be smaller and included in the frame of the figure.

We apologize. This due to an oversight in the production phase of the paper. Our intention is to have figure 4 in one-column format, and figure 5 slightly smaller than a full two-column figure, such that font sizes will be the same for all figures.

Caption of Fig. 6: the sentence MODIS mosaic in the background is courtesy of M. Fahnestock. is missing. By the way, I wonder if it is necessary to repeat it in each caption?

We agree. We removed the sentence from all figures, and moved it to the acknowledgements instead.

Supplementary Information: Enhancement factor E is not defined. May be you should write Glen's flow law? Either the caption or the table 4.1 itself is wrong, but I cannot see where the numbers for the different grid sizes are.

We updated the Supplement, the enhancement factor is now defined in terms of effective viscosity. Mass loss rates are now given for all grid resolutions in Table 4.2 (former Table 4.1)

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