Response to Reviewer 1

We are grateful to the reviewer for the constructive review containing valuable remarks and suggestions for our present and future work. The suggestions will greatly improve our present paper and stimulate our forthcoming work.

Reviewer’s comments are in indented blocks and italic fonts.

Response to general comments

The reviewer highlights the improvement in our simulations of the modern ice extent yielded by our novel ice discharge parameterization and its use for long-term simulations with coarse resolution ice sheet models, which still need treatment of marginal ice discharge. Our efforts in exploring the parameter space and validating the model are appreciated. Indeed, we made more extensively use of present-day data for the validation of the ice discharge parameterization than for other climates. Additionally, we used Eemian proxy data to constrain the model parameters. We agree that further work is needed to test the model behaviour for more subtly changing conditions. There is a body of ongoing work at PIK on the explicit treatment of Greenland’s outlet glaciers in an ice sheet model, which will allow a more detailed representation of the physics (longitudinal and lateral stress), as well as an improvement of the horizontal resolution of the cryospheric model components. This will allow us, in forthcoming publications, to perform a much more extensive comparison with higher-order and higher-resolution models on a variety of time scales. As the reviewer noted, the present paper is an initial presentation of the ice discharge parameterization, which is very promising for long-term simulations. We will include these perspectives on future work in our discussion. This work will target the suggestion by the reviewer of comparing our results with higher-order and higher-resolution models.

Response to specific comments

Just an observation: A general difficulty in interpreting the results is that in many of the observational tests, errors are due to a combination of REMBO’s climate forcing and the ice-sheet model physics, with little done to distinguish between them. This applies especially to the regional discharge amounts in Fig. 10 and discussed on pg. 1169. Nevertheless, the main point that the discharge parameterization significantly improves the results is probably robust, and does not seem to be due to cancelling errors with the climate model.

Indeed, we did not discuss this problem. Of course, discrepancies between observed and modelled GrIS also arise from the imperfectness of the REMBO climate forcing. However, we made extra simulations using observed precipitation from Bales et al. (2009) (not shown in the paper). Prescribing observed precipitation slightly improved the present-day sectoral ice discharge for most of our basins, but this did not solve the main problem: without discharge parameterization simulated GrIS is too large, irrespectively whether we use simulated or observed precipitation. Since using of present-day climatology for different climates and different GrIS shape (like during Eemian) is not
justified, we always prefer the use of the simulated REMBO climatology in spite of its biases. In summary, we did perform work to assess the role of REMBO’s climate biases for the simulated GrIS, but decided, for the sake of simplicity, not to present these results in this paper.

The comparisons of the model’s MBP and total discharge with data use somewhat older observational papers for discharge (pg. 1157, line 16). The very recent observational results of Enderlin et al. (2014, GRL online) should also be included to the extent they are relevant, for totals and geographic sectors. Enderlin et al. report large trends in recent years (towards greater surface runoff vs. discharge) – is this a concern for the results here, analogous to the concern regarding the recent decline in total GIS volume (pg. 1171 lines 1-6)?

We will include total ice discharge by Enderlin et al. (2014) in our comparison with observations (Fig. 10). Kindly, Ellyn Enderlin will provide us sectoral ice discharge. We will discuss the changes in runoff and ice discharge mentioned by Enderlin in the context of the trend in ice volume during recent years. Thank you for the hint.

If the discharge parameterization is to be a useful tool in other coarse-grid applications and models, it would be good to know if it depends on grid size, i.e., would the best-fit values of \( c_d \) in (3) change for different grid sizes (20 km here)? A dependence on grid size seems likely from the nature of the parameterization, in particular how “d” mimics lateral velocity yet is used as a surface wastage rate (Eqs. 2 and 5; pg. 1158 lines 24-26).

Indeed, the optimal values of the parameters of our ice discharge parameterization can be somewhat resolution-dependent. We do not expect that this dependence is strong, because this parameterization is applied to a large number of grid cells. In any case, we cannot test this dependence, because we do not have a set of models with different resolution. However, we agree that it would be good to test if there is such dependence. We will mention this in the discussion.

Just an observation: The two-parameter space explored here (\( c_m \) and \( c_d \)) is adequate as a first cut, with \( c_m \) and \( c_d \) representing the two most relevant model processes of surface melt and discharge. But it is still worrisome whether the main conclusions would hold if other parameters were included. As noted on pg. 1168 lines 1-2, this is an important area for further work.

We fully agree that the parameters \( c_d \) and \( c_m \) represent the most relevant model processes. Of course, taking into account the uncertainties of other parameters may somewhat affect the range of GrIS contribution to Eemian sea level highstand. Therefore, we phrased our conclusions very carefully. We are going to undertake such analysis in the future.

Related points: lines 21-22 on pg. 1157 state that the powers \( p \) and \( q \) in the discharge parameterization were chosen to be 1 and 3, based on an ensemble of simulations (separate from the ensembles shown, and not discussed further).
Some further information could be provided, perhaps including the physical meaning if any of the "3" value. It might be interesting in future work to explore generalizations of the discharge parameterization, so that discharge depends on a more general function of the 2 values (i) minimum distance to ice-free land and (ii) minimum distance to ocean.

Indeed, some information on the choice of the powers $p$ and $q$ could be provided, including the physical meaning of $q=3$. In general, the physical meaning of the parameters $p$ and $q$ is discussed on page 1158, lines 15-23. We will move these sentences to the beginning of section 3. Then later in that section, we will give more detailed information on the choice of the values of the parameters. Additionally, we made simulation in the entire $(c_m,c_d)$ parameter space for $p = \{1,2,3\}$ times $q = \{1,2,3\}$. We found an increase of $\text{err}(H)$ for small $p$ and an decrease of $\text{err}(H)$ for large $q$. For $q$ larger than 3 the error did not improve too much. For simplicity, we decided for integer numbers in the powers. The physical interpretation of $q=3$ is that our ice discharge depends on the high inverse power of 3 on the outlet glacier density (minimum distance to ocean). The reviewer proposed to include as additional parameter the minimum distance to ice-free land. We suppose that this could mimic how far fast ice flow affects the interior of the ice sheet. Partly, we already have such a parameter, which is the parameter $\Delta H$, see Eq. (2) in our paper. Together with the inverse distance to the coast $1/l$ this reduces the simulated ice discharge for positions farther inland of the ice sheet. The proposed dependence of minimum distance to the ice margin sounds promising and we will test it in forthcoming work. We really acknowledge this proposition.

Much of the analysis concerns the $\text{err}(H)$ measure (Eq. 7), the average of local ice sheet thickness errors normalized by average ice thickness, which cannot be reduced below _18% (pg. 1162, line 17). But this type of error could well be due to other errors in the ice-sheet model, notably the distribution of basal sliding coefficients – in other modeling studies these are often deduced by inverse methods, which greatly reduces modern thickness errors (e.g., Price et al., 2011, PNAS; Larour et al., 2012, JGR). Without this kind of inversion procedure here, the $\text{err}(H)$ measure is not very meaningful for the purposes of this paper, especially because the parameters explored here ($c_m$ and $c_d$) primarily affect near-coastal regions, and have little ability to reduce thickness errors in the interior.

We agree that in the interior of the ice sheet, a large portion in thickness biases are related to other model deficiencies, which cannot be healed by our parameterization. This explains why we cannot reduce $\text{err}(H)$ below 18%. Still our parameterization enable us to reduce $\text{err}(H)$ from more than 30% to 18%, which indicate that this metric of model performance is not entirely useless. At the same time, it is rather simple and can be used to trace further model improvements. Therefore, we opted for applying $\text{err}(H)$ to the entire Greenland ice sheet, because this is the metric which we would like to minimize, at least for the near future of our research. Among other things, the cumulative $\text{err}(H)$ is a very good measure of the adequacy of the initial conditions for future sea level rise.
projections, for which regions affected by melt can increase considerably. Of course, we are aware that using of an inverse approach including additional parameters would allow us to reduce err(H) much farther, but there is no guarantee that in this case the right result is achieved for the right reasons.

A related point: I think the sentence in lines 20-21 on pg. 1162 ("This supports the latter value...") is misleading. I would say, or add, that the latter value (20%) indicates that other model errors such as basal sliding coefficients are causing the errors, and are not accessible to the parameters explored here.

Agreed. We will change the text accordingly.

It would help to add difference maps (model minus observed) for (i) ice surface elevation and (ii) marginal areas where the ice extents differ, to augment Fig. 9 and the differences discussed on pg. 1169.

Basically, the three panels of Fig. 9 already contain the main points discussed on page 1169. The difference plot would not provide too much additional relevant information, given our simple initial approach. Therefore, we would like to restrict the figure to the three panels.

Response to technical comments

gp. 1156: Mention that REMBO and the melt formula (Eq. 1) have seasonal cycles. That information is in Robinson et al. (2011), but is important here, otherwise readers may think Eq. (1) is crudely based on annual means.

Good point. We will provide this information.

pg. 1157, line 6: "nearest ice-free land surface point". Presumably, this also means "...or nearest ocean point if no nearby ice-free land points exist".

Yes. This is correct and it is implemented as such in the code. Thank you for the catch. We will complete the sentence.

pg. 1163-1164: It is unclear to me what is shown in Fig. 6, i.e., what "all possible ice margins consistent with the different constraints" means (pg. 1163, line 28). Do the constraints come from the corresponding individual panels in Fig. 4, and if so, how are they determined? Also, in Fig. 6 caption, it is mysterious to say "with p=1 and q=3". Why say that here, when those are the only values used in the paper?

We will improve that sentence. Indeed, the constraints correspond to those in Fig. 5. The motivation for this figure was to demonstrate the impact of every single constraint on the ice margins. Only the respective constraints are applied, e.g. for Fig. 6a, the mass balance partition applies. We will make the paragraph clearer. We can change the sequence of the
panels of Fig. 6 such that they correspond with the Fig. 5 panels. We will erase “p=1 and q=3” from the figure caption.

pg. 1164, lines 15-16: For clarity, it might help to say "increasing (less negative)" and "larger (less negative)".

We will include "(less negative)". Indeed, the negative sign of $c_m$ could be overlooked.

pg. 1167, lines 18-19: This is slightly unclear, and would benefit by adding 1 more sentence to describe what was done here (and in Calov and Ganopolski, 2005). Perhaps: “...imposed a range of uniform temperature increases (or insolation increases in the 2005 study), ran the GIS model to equilibrium, and looked at the amount of GIS decay from the modern control” (?).

Yes. We will clarify the method for threshold determination.

Additional References