The Cryosphere Discuss., 8, C208–C212, 2014
www.the-cryosphere-discuss.net/8/C208/2014/

The Cryosphere
Discussions
Open Access

Interactive comment on "Simulating the Greenland ice sheet under present-day and palaeo constraints including a new discharge parameterization" *by* R. Calov et al.

Anonymous Referee #1

Received and published: 13 March 2014

General comments:

This paper applies a coupled simple climate model (REMBO) and coarse-grid ice sheet model (SICOPOLIS) to past and present Greenland, including a new parameterization of ice discharge to the ocean through unresolved marginal flows and fjord systems. There is a reasonably extensive exploration of ice-model parameter space and validation against modern and some paleo observations, as summarized nicely for instance in Fig. 5.

The new discharge parameterization is a simple but novel attempt to capture marginal discharge in coarse-grid models that do not resolve streaming or fjord flows. It is tested

here through comparisons with the modern Mass Balance Partition (MBP, ratio of total solid discharge to the ocean vs. precipitation), and also through marginal extents with and without the parameterization, where it produces significant improvement in modern extents.

Most of the validation of the discharge parameterization in this paper is for the present (although its basic behavior is shown to be reasonable for the Eemian). Further work will be needed to test it in more subtly changing conditions, such as different basal or englacial hydrologic regimes, ice temperatures, and smaller changes in ice margins (future, LGM), perhaps by comparing results against higher-order and higher-resolution models. However, the current paper is useful as an initial presentation of the parameterization, which could prove to be a viable tool for long-term applications that require coarse grids for computational feasibility, yet still need a basic representation of marginal discharge fluxes.

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.

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)?

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).

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.

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.

Much of the analysis concerns the 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 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.

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.

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.

Technical comments:

pg. 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.

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".

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?

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

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" (?).

Interactive comment on The Cryosphere Discuss., 8, 1151, 2014.