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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 #2

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The paper describes a sub-grid scale parameterization for ice discharge into the ocean in a coupled ice-sheet / SMB model (SICOPOLIS/ REMBO). Dealing with the discharge term at the marine boundary is indeed a challenging task in coarser-grid numerical models as it often occurs through narrow outlet glaciers and the mechanics of calving and marginal thinning are badly understood, let alone the coupling with water temperatures in the adjacent fjords. The paper is also very well written.

A parameterization of ice discharge would indeed be very useful for ice-sheet models of this kind. However, I believe the parameterization developed here is not appropriate and intrinsically highly problematic. In fact, ice discharge is already implicitly included in SICOPOLIS anyway – as all ice that reaches the ocean is removed passively. The

calving flux could easily be calculated from the ice flux at ocean-bordering gridpoints. If the scheme of the model conserves mass well, this flux should add up with SMB to give a zero total mass balance for a steady state.

The parameterization proposed here in fact does something else. It is effectively an additional surface mass balance term that has no direct relevance to the process of calving. The thinning is applied some distance inland irrespective of whether the ice sheet is in contact with the ocean or not. As such, the additional thinning merely masks intrinsic model shortcomings at the margin related to the boundary condition (presumably zero ice thickness for the margin, be it ocean or not) and/or the way the ice fluxes at the ice sheet margin are calculated. Therefore there must be a double counting of 'ice discharge' in case the ice sheet still reaches the ocean. Here, the authors are effectively imposing a zero SMB at the ice sheet surface for balanced conditions (a steady state). In case parts of the ice sheet still border the ocean, the total mass balance is then negative in their model (because the passive calving flux needs to be subtracted). In case the ice sheet has retreated on land, there cannot be any ice discharge in reality, however their parameterization will still impose this additional SMB forcing – dubbed ice discharge. That seems intrinsically wrong, and explains why the parameterization does not perform well for the southwestern part of the ice sheet that is already nowadays largely land-based. When applying the model to the Eemian or future warmer climates, this artefact of the method will only become stronger. In summary, the model seems to violate mass conservation, there is an issue of double counting and the process of ice discharge is disconnected of what happens at the ice-ocean boundary. In my eyes a more credible parameterization of ice discharge should focus on thinning at the contact point between ice sheet and ocean. How this affects inland ice dynamics is part of the ice-dynamic model.

Instead of focusing on the amount of ice discharge (which is in fact disguised as a surface melting term) and its fraction of the precipitation (MBP), it would help to show all mass-balance components and their evolution though time. Incidentally, do the

authors mean ‘accumulation’ when they refer to ‘precipitation’ or do they not account for the fraction of rainfall?

As a second point, I am surprised that the authors choose to vary only the parameter cm in the melt parametrization. The term ‘cm + lambda*T’ in eq. 1 is a linearization for the longwave and turbulent fluxes. If one changes cm one ought to change also ‘lambda’ and likely ‘tau s – transmissivity’ to have a reasonable fit to any mass-balance data. Changing only the linear term cm in the paramaterization is equal to add a systematic offset for the melting part of the SMB all over the ice sheet, hardly realistic as the effect of T on SMB.

Thirdly, I miss a graph showing the actual temperature forcing over the ice sheet during the Eemian in particular as this can be compared to the NEEM ice core record.

To conclude, I believe the paper cannot be published provided the authors are able to convincingly counter the criticism raised above.

Minor comments

GIS should be called GrIS throughout the manuscript to avoid confusion with the established meaning of GIS: Geographic Information Systems.

What is the origin of the bedrock data in their version of SICOPOLIS applied to Greenland?

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