

The Cryosphere Discuss., author comment AC2
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Reply on RC2

Elise Kazmierczak et al.

Author comment on "Subglacial hydrology modulates basal sliding response of the Antarctic ice sheet to climate forcing" by Elise Kazmierczak et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-53-AC2>, 2022

In the manuscript, the authors study the sensitivity of the Antarctic Ice Sheet model to the choice of subglacial hydrological model and to the values of power exponent in the Weertman/Budd sliding law. The authors conduct two series of numerical experiments, considering extreme and realistic environmental forcings in the ABUMIP and ISEMIP6 setups, respectively. One of the novel findings presented in the study is the increased sensitivity in case when the subglacial model depends on the subglacial water pressure.

The paper in question is definitely of scientific interest, is well-written, and I would recommend it for publication after minor revisions. I have two general comments, detailed below, followed by specific comments/questions.

We thank the referee for the effort in reviewing our manuscript and for the positive comments. Below we answer the specific comments in more detail (in bold italic).

General comments:

- The subject of the study is the sensitivity of the sliding laws and various subglacial hydrological approaches. However, the sensitivity is not formally defined in the text. This makes it difficult to follow the discussion and to reason about the results of the paper. I therefore suggest the authors to define the sensitivity quantitatively and to use that definition throughout the text in a consistent way. An additional figure presenting the summary of the sensitivity study for the ice sheet scale would also simplify the interpretation of the results.

Thank you for this pertinent remark. Indeed, we could have done a better job in properly defining what is meant by sensitivity. Our sensitivity metric is defined as the global sea level contribution (volume above floatation) from the Antarctic ice sheet compared to present-day. A higher sensitivity then means an overall larger mass loss for the same given forcing. We propose to add a summarized results table at the beginning of the discussion to permit a better understanding of it and define sensitivity in the introduction.

2. One of the factors that determine the dynamics of the ice sheet is the basal sliding coefficient A_b first used in the Eq. 1. In the paper, the spatial distributions of A_b are obtained through the optimization procedure for every combination of the power exponent m and the model for subglacial hydrology. Are these values of the basal sliding coefficient constrained in any way, e.g., in order to be within physically plausible ranges? How these values depend on the choice of m ? I would recommend providing the figure(s) presenting the spatial distributions of A_b at least for some representative problem setups and discussing how the values and spatial variation of A_b influence the response of the ice sheet both on large scale and basin scale.

For each value of m , the range of values of A_b is of course different, as the coefficients A_b are a multiplier to the sliding law. Nevertheless, we try to avoid overfitting and let the coefficient A_b evolve over maximum 4 to 5 orders of magnitude, leading to basal sliding velocities that are within the physical range (from mm/a to hundreds of m per year). The pattern of A_b is broadly consistent for different values of m and/or different subglacial hydrological approaches. The highest values are encountered in outlet glaciers and ice streams and along the Siple Coast; the lowest values are within the interior of the East Antarctic ice sheet. The pattern is very similar to what is presented in Pollard and DeConto (2012), where the optimization method was presented initially. In the discussion we added a subsection on the effect of A_b on the results. In short, there is definitely a large difference in response if one starts from a uniform distribution of A_b underneath the whole ice sheet (leading also to a different ice geometry compared to the observed ice sheet) than if one starts from an ice sheet close to the observed geometry and with a spatial distribution of A_b . However, for each of our experiments, grounding line retreat occurred preferentially in the same areas. The magnitude of grounding line retreat (and retreat rates) remains therefore only a function of the applied sliding law and/or basal hydrology.

Minor comments/questions:

Below we answer those questions that need some explanation. In the revised manuscript we will take care of the corrections and typos that are asked.

- 1, l. 7 - please define "RCP" before first use;
- 6, l. 118 - how Q_i is calculated?;

Q_i is calculated as the incoming flux plus the basal melting rate corrected for the unit width of the cell or the subglacial water speed multiplied by the subglacial water thickness. For more details on the method, see for instance LeBrocq (2006) where the same method is used for determining balance fluxes of ice.

- 6, l. 125 - "and the subglacial water flux, i.e.," - change to "and the subglacial water flux ϕ , i.e.,";
- 6, Eq. 10 - please define A_0 , e.g., "and A_0 the initial value of A_b , obtained through a nudging method described in Section 3";
- 6, l. 131 - "the effective pressure N is considered constant for SWF" - what is the value of the effective pressure N ? Does this value influence the results?;

In the model it is taken constant (used as a scaling factor). However, for the manuscript it is better to remove this statement and just state that for SWF, the effective pressure is not considered in the sliding law.

- 7, l. 137 - please define the "yield stress" of what is discussed;

It is the yield stress defined in the equation (1). We could modify the sentence by « A fixed fraction of ice overburden equal to one implies an effective pressure and consequently a yield stress equal to zero (Equation (1)) ».

- 7, l. 145 - W instead of W_{til} ;
- 7, l. 153 - " δp_o is the lower bound on N, taken as a fraction of the ice overburden pressure." - I suggest changing it to " δp_o is the lower bound on N, taken as a fraction δ of the ice overburden pressure p_o " for better readability;
- 15, l. 258 - "between difference" should read "between different";
- 16, Fig. 9. - the TIL model seems to be dramatically different from other models for $m > 1$, especially for the Enderby Land basin (Fig. 9c). It would be useful to see an explanation for this.

It should be noted that the y-axis has a different scale than in Pine Island and Thwaites and in Wilkes basins. The main reason why the TIL model is less sensitive in Enderby Land is because of the lack of saturated till in this basin (fig. 4). Furthermore, the TIL model is generally less sensitive than the other approaches.