Author's response
to comments from Anonymous Referee #1, posted on 05 Sep 2023

Date 02.10.2023

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Dear reviewer,

we appreciate you having taken time to review our submission and send valuable comments on our manuscript. Find below a detailed list of your comments and our response along with references to the manuscript describing changes we made.

Reviewer comment:

My first concern is if the authors have made a global search during step (I) to step (III) in section 2.3 for all the potential positions (CX, CY), the peak outflow velocity and the heat source width R. You take UZ0=−1250 m/year in step (I), and you get an optimized location, which is used as input for step (II). If you take a different value of UZ0 in step(I), you would possibly get another different optimized location, which may have impact on the subsequent results. From Table 1, run 06-09, we can see that for a fixed location of (CX, CY) and changing UZ0, the resulting RMSE is sensitive to the change of UZ0. Therefore, global search for optimized values in the space of 3 parameters are needed. I cannot see how the present scheme in the paper to do so.

Authors response:

There seems to be a misunderstanding on how the parameter grid search has been carried out in our study, partly caused by us omitting runs that have been carried out but not analyzed in detail. To clarify the matter, we have extended Table 1 to include all runs performed in the study with all performance measures calculated. Given the computational cost of one simulation as well as the clear indication, that cauldron off center axis heat source locations lead to significantly worse performance(cf. 3rd paragraph, section 2.3), we think that we have searched the parameter space of CX, CY, R, UZ, and sigma extensively enough to come to the conclusions presented in the paper.
However, we thank you for pointing out that the manuscript text is also somewhat unclear on how the global parameter search has been carried out (following point 1.-4. in the end of section 2.2 instead of (I)-(III) in 2.3) and added explanatory text to the end of section 2.2 as well as the section 2.3.

Reviewer comment:

The numerical model in Section 2.2 is not completely or correctly described. I have a few questions. (1) The boundary condition for the ice flow model is not completely described. Do you assume zero Cauchy stress at the ice surface? Your domain is a part of the ice cap. What is the lateral boundary for the side walls?

Authors response:
We will unpack theses comments step by step and reply individually.
The ice flow model BCs are indeed not fully described in the manuscript and thus we have added additional text to section 2.2 describing all BCs. A brief answer here is that yes we do use a zero Cauchy stress BC at the surface and use lateral walls (**v**=0) at the domain boundaries.
We took this review comment also as an opportunity to streamline our notation in the manuscript and now refer to ice velocities as **v** = (v_x, v_y, v_z) throughout the manuscript.

Reviewer comment:
(2) You take the outflow velocity as basal ice velocity, right?

Authors response:
Yes we do and we have clarified the text in section 2.2 and 2.3 to make this clear to the reader.

Reviewer comment:
(3) Is it a steady state simulation or prognostic simulation? It is not very clear. As you move the ice surface, I assume it is a prognostic simulation. Then what is the timestep used in Eq. (3)? In Line 88-89, 'a predefined time step (cf. Sect 2.3)', but I do not see the predefined time step in section 2.3. Maybe you refer to sect 2.4. And how many timesteps do you use?

Authors response:
It is a steady state simulation and we have added a note on that to section 2.2. As described by equation (3), we can move the ice surface forward in time by a moving mesh approach, thus the simulation can predict a new ice surface geometry after a given time interval/time step. However as we only do this for one time step, we refrain from calling the simulation prognostic. Either way, the ice dynamics simulation is steady state, thus we think this is the better term to describe our overall simulation.
Thank you also for spotting the wrong reference, it should indeed refer to 2.4 and we have corrected the manuscript.
As has already been described and justified in the beginning of section 2.4 (around line 130 now) we only use one time step.

Reviewer comment:
(4) The steps shown at the end of section 2.2 is not a cycle. What will you do after step 4 when you find bad match between the modelled ice surface geometry with reference data? Will you change the more iterations to make a better fit?

Authors response:
We have not claimed that the process described in section 2.2 (point 1-4) is an iterative cycle, nor does it have to be one. On the contrary, we have stated clearly that steps 1-4 are there to study the suitability of a prescribed basal heat flux distribution. If step 4 results in poor performance measures, we manually change the model configuration and redo steps 1-4 for a new result. How we proceed to find a well performing configuration is outlined in section 2.3, steps (I-III). We do think the manuscript is sufficiently clear on these procedures.

Reviewer comment:
(5) The outflow velocity is a vector (see Eq. (1)). However, when you mention it in section 2.3, it becomes a scaler, for instance, UZ=−130 m/year. It is wrong. It is not consistent. I guess UZ is only the vertical component of outflow velocity? You need describe it correctly.

Authors response:
That is correct and Eq (1) has been somewhat misleading. We have updated equation (1) to reflect our conversion of heat flux to a vertical ice outflow component. Also the notation on what is a vertical ice velocity component was not completely consistent, as you have stated, thank you for pointing this out. We have corrected the manuscript to reflect that the basal ice outflow velocity has only a vertical component.

Reviewer comment:
The surface elevation change is caused by the surface mass balance (SMB) and the ice motion (Eq. (2)). Have you compared them? It would be helpful to show them. You used two approaches to calculate the SMB. How does the spatial distribution of SMB and its uncertainty compare with the elevation change caused by ice flow transportation? From Fig. 2, we can roughly guess the surface lowering caused by surface velocity is 3-6 m/year (blue area in Fig. 2). Could you make a plot of SMB distribution? Then we can know what role they play in surface elevation change.

Authors response:
We only do have three SMB survey sites for the study area and period (see Figure 1 in the manuscript). All of section 2.4 in the manuscript is dedicated to describe our estimation efforts for SMB and how we correct for SMB effects in the input data. As we do not have a SMB distribution for the study area we unfortunately can not fulfill the request of rviewer #1.

Reviewer comment:
The ice flow model is named Elmer/Ice, see its website. So please change all the Elmer-ICE to Elmer/Ice.

Authors response:
Done, thank you.

Reviewer comment:
Line 59, as I understand, you assume the ice is temperate everywhere, so you used a constant value for Glen's rate factor. If so, you did not consider the coupling between stokes model and heat transfer equation, it is just Stokes equation, you cannot say you solve the Full-Stokes equations.

Authors response:
It is correct that we assume temperate ice and thus a constant rate factor. The term "Full-Stokes" however refers to models that solve for the full stress tensor in a Stokes flow model for a given numerical solution scheme, contrary to simplified models that use a simplified stress tensor (e.g. shallow ice or shallow shelf models). We are not aware of any publication in which the meaning of "Full Stokes" has been changed to mandatorily include any treatment of heat transfer processes within the ice since Full-Stokes models have been developed around 2007-2008, e.g. Icetools (Jarosch A. H.,

2007 https://doi.org/10.1016/j.cageo.2007.06.012) as well as the Elmer/Ice (Gagliardini et al., 2013) or ISMIP-HOM (Pattyn et al., 2008,https://doi.org/10.5194/tc-2-95-2008).

Reviewer comment:
Line 71. I got confused. It is said here that "Hence spatial variation in heat flux can be simulated". What do you mean? The heat flux is given or simulated? It should be an input data for an ice flow model. Forward ice flow model cannot simulate heat flux.

Authors response:
Now line 75. Indeed this is confusing to a certain degree and we have rephrased that sentence.

Reviewer comment:
Line 82, Eq. (3), the second plus symbol should be times ×.

Authors response:
Done. Thank you, that is important.

Reviewer comment:
Line 94. What is 'the cauldron', K5 or K6? Pleaser clarify it.

Authors response:
It is K5, and we have updated the text in the manuscript. Thank you.

Reviewer comment:
In Table 1, the UZ in the 1st row should be UZ0. They are different, see Eq. (4). The abbreviation of sim. nr. should be defined somewhere.

Authors response:
True, thank you. We have updated the table and sim. nr. is now defined in Line 109.

Reviewer comment:
Figure 2 and its caption need to change or improve. Is the scale bar for both plots? In the caption, you need mention the domain of left plot is the modelled region, and refer to Fig. 1a for its location. You can change the background to white rather than grey. It is better to change 'velocity z' on the colorbar to 'vertical velocity'. The right plot is the vertical outflow velocity at the base. It is not basal outflow velocity distribution as written in the caption, which is 3D. Please change the caption. The caption is not complete. Please also add the location information of the basal outflow velocity distribution. Please consider to add a plot for the horizontal basal outflow velocity. Besides, please consider to add a subplot to how the modelled basal velocity for the whole study area – as the left plot. Also add marker for K6 and K5. What is run 004? The number is not consistent with Table 1.

Authors response:
Thank you for this valuable input and we have updated Figure 2 to include now two scale bars, changed labels for the velocity fields as well as have introduced a white background. The location of the basal outflow distribution is included as a orange circle. Outside the prescribed basal vertical

velocity distribution representing a heat flux, all velocity components are equal 0 as given by our basal boundary condition, so we do not see the need to add an additional subplot.

Reviewer comment:
Figure 3 caption. Please refer to Fig. 1a for the domain you show here. Is it modelled area or focused area? Please add a marker for K6. Similar for Fig. 4, 5, 6.

Authors response:
Good point, thank you. We have updated the figure captions of Figures 3-6 to refer to the focus area in Figure 1. We did not add a marker for K6 as the whole maps show just K6.

We would like to thank you for taking the time to review our manuscript and all the valuable comments you gave.

Kind regards,
Alexander Jarosch on behalf of the authors.