Reply on Report #1

Dear Dr. Martin Lüthi,

We are very grateful to you for the second review of our manuscript, your positive assessment of the revisions done, and recommendation for publication after final revisions. We consider your comments on the revised version of the manuscript to be very significant and have taken them into account. Below, we provide the text of your review, our responses, and the corrected or added fragments of the manuscript.

Sincerely,
Gleb Chernyakov, on behalf of all the authors

Dear colleagues

This is the second time I read the manuscript. This version is a well-done improvement on the first version of the paper. My recommendation is to publish the article once the small comments below have been taken into account.

Sincerely,
Martin Lüthi

General comments

What is the rationale for using Point "a" for the distributions, but "b" for the data? Shouldn't be all evaluated at "b"? At lines 95ff some of this rationale is given, but it should be more prominently advertised, and be re-formulated such that it is clear from the beginning, why these positions are different. Just start the paragraph starting on line 93 with: "We chose to evaluate the model results at position "a" instead of position "b", where the bore hole was located, for the following reasons."

> *Thank you for the suggestion! To emphasize from the outset that the change in position is due to a discrepancy between different data, we have supplemented the paragraph with the introductory phrase you suggested:*
>
> We chose to evaluate the model results at position *a* instead of position *b*, where the borehole was located (see Fig. 2), for the following reason. According to the DEMs of the surface and bedrock, obtained by interpolation of the GPR data profiles the ice thickness at the 2009 drilling site location is 6.1 m greater than the ice core length. This difference is within the accuracy of the GPR data but creates the discrepancy in modeled ice thickness.

In Equation (2) you describe a purely advective problem. How was this solved, they very often lead to numerical instability. Why did you not use particle tracking from the borehole back to the surface (as was done in e.g. Lüthi (2000)).

Equation (1) with boundary condition (2) is meant here. We discuss the approach to solving this problem in the last paragraph of the Numerical methods section: "The equation was discretized via discontinuous Galerkin method and the resulting linear algebraic systems were solved by a direct method for the thermomechanically coupled model and by BiCGStab with ILU(1) preconditioner for the purely mechanical model". The computations converged and numerical instability was not observed. Regarding the backward trajectories, there was no necessity to explicitly track them for modeling the age field, since we applied the well-functioning advection–reaction Elmer solver. However, we previously performed backward trajectories simulation for this borehole in order to account for the upstream effect in the reconstruction of the accumulation rates at the Elbrus Western Plateau (Mikhalenko et al., 2024).

The use of an enhacement factor E is somewhat unsatisfactory. This very large discrepancy from literature values should be discussed in detail. It appears, that the outflow boundary condition has a very strong influence on the resulting dating (Figure 4 clearly shows that). (Similar to what we found for Colle Gnifetti, Lüthi (2000)).

We agree that the issue is insufficiently clarified. The text was revised.
For your knowledge, we came across this problem on different mountain glaciers when ice/firn fluidity is greatly overestimated in the uncorrected case (E = 1), and a strong reducing coefficient is need. To investigate the reasons for this we varied the geometric and physical parameters of the modeled domain (using another mountain glacier, but a similar model). Our modeling experiments showed that the average flow velocity is strongly dependent on glacier geometry (thickness, slope, domain size), but that density (replacing firn with pure ice), bedrock topography, and the presence/absence of lateral outflow have only a minor effect (also, Figure 4 in the manuscript corresponds to a case without lateral outflow). These results are still preliminary, so we would prefer to refrain from speculating on this topic in this article.

We have added the following comment to the Discussion section:
The flow enhancement factor values selected as a result of model calibration turned out to be less than 1, which is not typical for glacier dynamics models (Greve and Blatter, 2009) and indicates that without appropriate correction the flow model overestimates the ice/firn fluidity. Similar results were reported previously for a crater glacier in Kamchatka where the value of the flow enhancement factor was also less than unity (E = 1/3) (Zwinger et al.,2007). Further analysis will be required to identify the causes of the atypical shift in the value of this parameter.

Specific comments

92 "wall"); *and * a part...

The sentence has been corrected.

347 Among all... (strange sentence, reformulate)

We have reformulated the sentence as follows:

From all the simulation results, we determined the range of modeled age–depth curves corresponding to the 2009 ice core.

References

Greve, R. and Blatter, H.: Dynamics of Ice Sheets and Glaciers, Springer, Berlin, Germany, 2009.

Mikhalenko, V., Kutuzov, S., Toropov, P., Legrand, M., Sokratov, S., Chernyakov, G., Lavrentiev, I., Preunkert, S., Kozachek, A., Vorobiev, M., Khairedinova, A., and Lipenkov, V.: Accumulation rates over the past 260 years archived in Elbrus ice core, Caucasus, Clim. Past, 20, 237–255, https://doi.org/10.5194/cp-20-237-2024, 2024.

Zwinger, T., Greve, R., Gagliardini, O., Shiraiwa, T., and Lyly, M.: A full Stokes-flow thermo-mechanical model for firn and ice applied to the Gorshkov crater glacier, Kamchatka, Ann. Glaciol., 45, 29--37, https://doi.org/10.3189/172756407782282543, 2007.