Reply on Referee Comment 2

Dear Dr. Martin Lüthi,

We are extremely thankful to you for such a great effort in reviewing our manuscript, as well as for many constructive comments that served to improve our work, and for useful ideas for future research! We are also very grateful for the overall positive assessment of our work! We have tried to take into account the proposed revision as fully as possible and have prepared a new version of the manuscript. Below, the text of your review is highlighted in blue, our responses are in black, and the corrected or added fragments of the manuscript text are in purple.

Dear collegues

This is an interesting and important analysis of the flow behavior of a firn-covered large glacier, where an ice core for climatic analysis has been drilled. The paper is nicely written and comprehensive. The discussion needs some more consideration of the shortcomings of neglecting bubble close-off and a static climate.

My recommendation is to publish the manuscript after taking into account the comments below.

General comments

- The abstract is quite long and should be considerably shortened. It contains repetitions and too many details.

We have shortened the abstract. The revised text is below:

The glaciers of Mount Elbrus (Caucasus) contain paleoclimatic and paleoenvironmental information representative of a vast region. Negligible seasonal melting in the near-summit area of Elbrus ensures excellent preservation of climatic signals. In 2009, a 182.65 meter long ice core was obtained from the glacier on the near-summit Western Plateau (WP) of Elbrus. The upper part and basal samples of the core were dated. In this work, a three-dimensional (3D) steady state thermomechanically coupled Stokes flow model for a cold glacier with a rheological law accounting for firn densification, calibrated based on the ice core dating, was applied to model the velocity field and the corresponding distribution of the age of the ice in the central part of the WP. We performed multiple model runs, varying boundary conditions (BCs), ice viscosity, and the inclusion of thermomechanical coupling. The Elmer/Ice software was used for numerical simulation. The model quite accurately reproduces the age of the ice according to ice core data to a depth of 150 m (up to 170 years). Below, the age of the ice increases sharply and the discrepancies in dating between different modeling scenarios become larger. Overall, the simulated ages fell within 68.2 % confidence intervals for the ages of near-bottom ice samples (mean radiocarbon age 1–2 ka). The model is not applicable for dating the lowermost ice layer (3–4 m thick). Future model improvements should focus on accounting for potential melting and identifying areas containing the oldest ice.

- "full Stokes" does not exist (but seems to be marketing jargon of Elmer Ice) -> these are just the Stokes equation from fluid mechanics. There exist "reduced" equations, omitting some terms due to scaling arguments.

Yes, it is possible to omit the word "full". By the way, the term "full Stokes flow problem" is used, for example, by Greve and Blatter (2009) even outside the context of Elmer/Ice. Also, we have taken into account all the comments below on this topic.

- "ice age" usually means a geological epoch. Better replace this term everywhere with "the age of the ice"

Agree. Corrections have been made everywhere.

- citation style should be adapted: Often \citet or \citep!

Done.

- Section 3 is out of place, and is also partly repeated in Section 4. Maybe convert to a table, or relate it better to the rest of the manuscript.

In our opinion, Section 3 is a logical continuation of the description of the study area and field data. In addition, the information presented in it is necessary for understanding the operation of the model. We appreciate the idea of converting it to a table, but Section 3 probably contains too much verbal information to be presented in tabular form. Therefore, we would prefer to keep Section 3 as it is. Also, to avoid repetitions and to clarify the wording, we have changed the beginning of Section 4. The revised text is presented here:

Ice/firn flow modeling with subsequent dating was performed in a 3D domain (Fig. 2). The domain is limited by that part of the glacier on the WP, for which DEMs of both the surface and the bed are available. The computational domain is bounded by three surfaces: a part of the glacier surface; the lateral surface of the domain (the vertical "wall"); a part of the glacier base.

- also in section 4 and 5 information is not given in a logcial order. You should rater describe, in this order:

- geometry, density, temperature etc
- numerical approach & solver etc
- discretization
- boundary conditions

Partly agree. We think that it would be most logical to first fully present the analytical formulation of the problem and then move on to the numerical implementation of the model. Therefore, we would prefer that the subsection "Boundary conditions" precedes the numerical approach. Also, in our opinion, discretization logically precedes the methods for solving linear systems, etc. At the same time, we agree that the existing sequence of presentation is not optimal. We have moved the information on discretization from Section 4 "Spatial structure" to Section 6 "Numerical methods".

To obtain a reasoable age of the ice at the base, the air bubble pressure after close-off has to be taken into account. This was pioneered by Pimienta, and implemented in e.g. Lüthi & Funk (2000). Without that effect, the ice at the botton cannot be dated correctly, and the age is much too old. Also, varying basal melt could easily be used to control the age of the ice at the bottom. If it is too old in the model, just slightly increase the melt.

Overall, this is a very nice and conclusive study on the important topic of dating a climatologically relevant ice core. The study has a few shortcomings, especially wrt. the model. These problems are mostly discussed, and are largely due to the lack of data to constrain the model. These include flow velocities and climate data to constrain the long-term thermal and dynamical evolution of the glacier. I think that at this stage it is not useful or necessary to implement bubble close-off in the Elmer Ice code base (but this needs urgently be done for subsequent studies).

The discussion should not only list, but also clearly work out the effects of neglecting in the model the effects of bubble close-off, and of assuming a high and constant basal heat flow, as well as assuming steady climate conditions.

Agree. We discuss these issues in the comments below.

Specific comments

22 If the area is so large and encompasses North Africa and Asia, it is not representative for any of those.

We agree that the wording should be clarified. It was meant that this paleoarchive for each of these regions contains some proxy of atmospheric phenomena (precipitation, dust, etc.). We corrected the sentence as follows:

The glaciers of Mt. Elbrus offer a unique **paleoclimate archive that traces signals from a large region,** including the North Caucasus, the Black Sea region, Southeastern Europe, North Africa, and the Middle East (Mikhalenko et al., 2024; Kutuzov et al., 2019a).

34 does the part after "while" refer to measurements? Or the model?

This part refers to the model results. Here is the corrected phrase:

According to an estimate based on a two-dimensional (2D) analytical model of Salamatin et al. (2000), the age of the ice at the bottom of the drilling site does not exceed 350–400 years and the age of the basal ice in the deepest part of the glacier (more than 250 m deep) is about 660 years (Mikhalenko et al., 2015).

39/45/54 and other places "full Stokes" -> Stokes

Done.

40 "the accumulation record"

Corrected.

49 "Mt. Dôme du Goûter" -> leave away Mt.

Corrected.

50 " Blank," -> Blanc

Corrected.

61 the glaciated area

Corrected.

62 a stray parenthesis

Corrected.

74 Better put the information on Pleiades in the Acknowledgements (does not fit in the main text)

Agree. We have moved the sentence with information about providing the Pléiades DEM to the Acknowledgments.

79 Here, I was expecting the Pleiades DEM for the surface

In fact, this includes the Pléiades DEM for the surface. We have corrected this data list item as follows:

1. DEMs of the glacier surface and bedrock with a cell size of 10 × 10 m, both obtained in 2017.

79 How do you get a 10x10 m bedrock from a radar survey? Was this somehow gridded?

Text revised:

The results of a series of ground-based radar surveys at a frequency of 20 MHz in 2005, 2007, and 2017 show a significant ice thickness and a crater shape of the underlying bedrock. The maximum depth is 255±8 m at the central part of the plateau, with minimum values of about 60 m near the edge. **The GPR survey used in this study was conducted in July 2017. The ice thickness map was completed using empirical Bayesian kriging interpolation (Kutuzov et al., 2019b).**

Figure 1, caption: Coordinates are NOT given in UTM, but this would be much more useful!

Corrected.

89 So, this is a triangulation of the domain. How was this done, with Triangle? You should also mention that you need this grid for the FE-model.

Text revised:

For finite element calculations, a flat computational grid (footprint) was created **using the mesh generator Gmsh** within the contour (shown in Fig. 1) of the area of the WP covered with DEMs.

92 Does this mean that you have prismatic elements, which are problematic for the incompressible flow due to LBB stability criterion?

Yes, we use prismatic finite elements. In our compressible formulation, convergence is achieved in all cases.

Also, the mesh resolution with depth is wildly varying. I can see the advantage of extruding a mesh in the vertical, but a TET4 mesh would likely be much more suited for the computations at hand.

Thank you for the idea for our future investigations! Indeed, the mesh resolution is strongly depth-dependent, and we have added the following information in the text:

The typical node spacing at the refined boundary is of order 0.1 m (less than 1 m), and at the coarser boundary it is of order 1 m (less than 10 m).

95 This argument is not clear. It would be trivial to shift the bedrock up by 5 m, and then retain the exact horizontal position of the borehole.

We are grateful to the reviewer for this suggestion! This is a very straightforward and correct idea. Our only doubt is that this approach is likely to underestimate (even if only slightly) the ice thickness in the model (assuming that the radar data are not subject to a systematic error towards overestimation of the thickness). Our experience of numerical simulations with a similar model, but other objects, shows that varying the ice thickness with other parameters unchanged has a very significant effect on the velocity values. Unfortunately, implementing this approach would require performing the full simulation from the very beginning. However, we will certainly apply this technique in further research.

This is crucial, since mainly surface slope drives ice flow, especially in delicate saddle-like topographies like the one modeled in this study. Point a is on a clear slope, while point b is not.

As for the simulations already performed, to compensate for possible errors caused by shifting the point of modeling in combination with heterogeneity of the subglacial relief, we have a calibration parameter (flow enhancement factor, discussed below), as well as the possibility to vary the lateral boundary condition and the type of model (purely mechanical/thermodynamically coupled).

Also, radio echo data was likely not correctly interpreted (vertical instead of perpendicular to the surface), and depends a lot on the velocity model assumed, which depends on the density structure & temperature.

Sorry, but your point is not clear enough for us. The methodology for processing GPR data and error estimation is discussed in the work of Kutuzov et al. (2019b).

99, Tab 1 "Mathematical model"   better: "Numerical model" or "Ice flow model"

We agree with the remark. We have replaced "mathematical model" with "ice flow model" in the title and the text of the section. Below is the corrected text:

Under the assumption of steady state, the velocity distribution in the glacier allows one to calculate the time required for each ice/firn particle to move from the glacier surface to its current position. The calculation of the velocity field was performed on the basis of a 3D stationary Stokes model with the rheological law of Gagliardini and Meyssonnier (1997) for a compressible nonlinear viscous medium (ice/firn). We have applied both purely mechanical (isothermal) and thermomechanically coupled ice flow models. Information on the quantities used in the models is given in Table 1.

Tab 1: there is way too much information in this table at the same time it is totally unclear what type of relations were used.

We believe that it should be convenient for readers to have all the numerous parameters of the model in a single table, which can be used as a reference. We also believe that indicating the functional dependencies in the table even without giving their explicit form (due to their cumbersomeness) should make it easier to understand the model. Of course, explicit relationships between the quantities of the model can be determined by analyzing the formulas in the text. Therefore, we would prefer to keep the table in its current form.

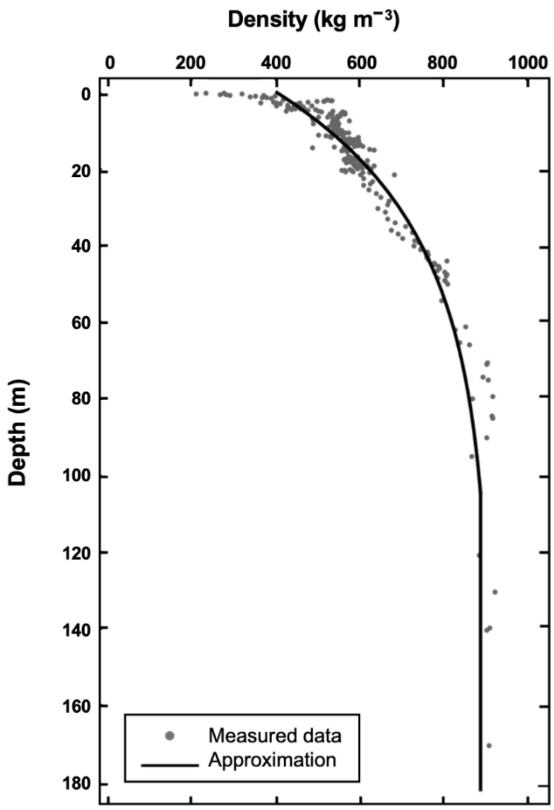
113 This "full Stokes..." appears here for the 3rd time. Rather describe it properly at some point.

Since there are no approximations to describe in our Stokes problem setting, we added a brief remark at the beginning of the section "Ice flow model":

The model is applied in its full version, without scaling and excluding any terms in the equations below.

120 please show the data and the approximations in a figure.

The following plot has been added to the text:



125 Show the equations that are solved (maybe in an appendix). Not everyone wants to look up these equations. Are these the exactly same equations? Then why show them. If not: what is different?

Yes, these are exactly the functions used by Gagliardini and Meyssonnier (1997). In general, we have chosen to write down explicitly all the formulas we use. In addition, it will really save interested readers from having to look for them in other works. Also, to be consistent in shortening the description of the model, we should exclude a number of other formulas, for example, the tensor invariant and the Arrhenius law, limiting ourselves to indicating references. We fear that this would make the presentation less clear.

130 The derivation of the tensor invariant makes no sense if it is not presented in the context of the flow relation used. Either show all, or nothing (but show in the appendix what exact equations were used).

We agree that this information is redundant. We excluded from the test the definition and decomposition of the strain-rate tensor. As for transferring the formulas to the appendix, we would prefer not to do this, since some elements of the model (for example, the flow enhancement factor) are mentioned in the presentation of the results, and readers would have to refer to the appendix to fully understand the content of the sections of the article preceding it.

130 It is confusing using "D" for several things, and also "T".

Partially agree. In our opinion, such notations have the advantage of being easily associated with the corresponding operations (deviator and transpose). In addition, when denoting these operations, we use different fonts than when denoting the strain-rate tensor and temperature. Therefore, we would prefer to keep the existing designation. Also, when correcting the text according to your previous comment, we excluded the only formula with transpose.

145 This is wrong in the context of a compressible flow relation. "p" is a Lagrange multiplier in a purely incompressible flow law. Here a compressible law is used, were isotropic and deviatoric parts are mixed. See e.g. Gagliardini & Meyssonnier (1997), Lüthi & Funk (2000)

Sorry, we may not understand you, but we do not see a mistake here. The implementation of the porous flow law in Elmer/Ice uses the same definition of pressure as in the incompressible case $p = -\operatorname{tr}\boldsymbol{\sigma}/3$ (https://elmerfem.org/elmerice/wiki/lib/exe/fetch.php?media=solvers:poroussolver.pdf). Thus, p represents the total pressure in the compressible fluid (Greve and Blatter, 2009). The p defined in this way differs only in sign from the pressure in the work of Gagliardini and Meyssonnier (1997). Also, $p = -\sigma_{\mathrm{m}}$, where σ_{m} is the mean pressure in Lüthi and Funk (2000). In addition, this definition of pressure is also used, for example, by Zwinger at al. (2007) and Brondex et al. (2020). In the text we have added an explanation regarding pressure:

After decomposing the Cauchy stress tensor into an isotropic and a deviatoric parts $\boldsymbol{\sigma} = -p\mathbf{I} + \boldsymbol{\sigma}^{\mathrm{D}}$, where $p = -\operatorname{tr}\boldsymbol{\sigma}/3$ we can write the rheological law in general form […].

147 This is Glen's flow law for INCOMPRESSIBLE ice. You should write down the proper Duva-Crow flow law!

Again, we may not have understood your point, but this relation is valid for both incompressible (for which $\mathbf{D}^{\mathrm{D}} = \mathbf{D}$) and compressible fluids. As for the Duva–Crow flow law, its analogue is obtained by substituting the formula for viscosity (and then other expressions) into this general flow law. It seemed to us that it is better to introduce the relations sequentially than to combine them.

150 References for the parametrizations of c and \kappa should be given. Is T in K or degC?

We have added the following source reference:

Ritz, C.: Time dependent boundary conditions for calculation of temperature fields in ice sheets, in: The Physical Basis of Ice Sheet Modelling, edited by: Waddington, E. D. and Walder, J. S., IAHS Press, Wallingford, UK, 207–216, IAHS Publication No. 170, 1987.

T is in K, as given in the Units column of Table 1.

Also, these values can be deleted in Tab 1. It is important to notice that both thermal quantities are strongly dependent on density.

We would prefer that Table 1 contained information on all parameters used. We also attempted to include the dependence of thermal conductivity on density to the model, as in the work of Zwinger et al. (2007). Added the following description to "Discussion":

Including the dependence of heat conductivity on density in the model was tried but did not lead to adequate results. A more effective approach was found to be to compensate for this simplified parameterization by choosing appropriate values of the flow enhancement factor.

154 This equation is wrong, it is k, not \kappa (diffusivity) Sorry, I see that \kappa is used for conductivity here, but it is convention to use \kappa for diffusivity and k for conductivity.

Regarding the notations, we mainly followed the work of Greve and Blatter (2009). The heat conductivity is denoted there by ‘kappa’. In general, we are used to seeing both designations for the heat conductivity. We replaced ‘kappa’ with ‘*k*’ throughout the text.

156 these are the conservation of mass equation (the volume is changing!)

We agree that the term we used is inappropriate and have excluded it. Also, as recommended by Reviewer 1, we have moved this equation to the "Constitutive relations" subsection.

165 Are these b.c. varying with depth? Already mention it here, I see Eq (26).

Indeed, the BC (25)–(26) is varying with depth in some model cases. In the first sentence of the section (line 165) we only intended to indicate the general structure of the BCs. It seems to us that there is no need to provide any additional information about the BCs right here, especially since all the information is presented below in the same section.

179 What is the rationale for a non-zero vertical velocity, but a zero horizontal velocity?

Indeed, an explanation is needed here. We set the normal velocity on the bed to be non-zero, and the tangential velocity to be zero. A comment has been added in the text after this boundary condition (no. 22):

Under the BC (22), ice/firn particles are modeled as moving from the glacier surface to the bedrock in finite time, which ensures convergence in the numerical solution of the dating problem (1)–(2). Also, such a small deviation of the basal velocity from zero apparently does not affect the dating results of the overlying ice/firn (except for a thin bottom layer), as indicated by the coincidence of the age fields obtained with the v_{b} increased and decreased by several orders of magnitude compared to the selected value.

198 This shape of the velocity profile is only reasonable below the firn-ice transition, as can be seen e.g. in Fig 4a.

We agree that further refinement of this boundary condition makes sense in the future. However, in the absence of velocity data on the glacier, this does not seem to be so significant.

207 This is not a reduced flow model, you solve the full flow model for a time-constant T-distribution.

We agree that such an interpretation is possible. We have excluded the section "Reduced model" (as recommended by Reviewer 1) and moved the information from it to the section "Model calibration". Here is the revised text:

All the simulation variants produced on the basis of the thermomechanically coupled model were also repeated by means of a purely mechanical model, i.e. a flow model with the heat transfer block switched off.

The simplification of the complete model is as follows. The ice/firn temperature is assumed to be constant […].

211 The viscosity must be dependent on the density. This is absolutely crucial. If you use Duva-Crow it is given by a(phi) and b(phi).

The viscosity is dependent on the density in our model (see Eq. (11)).

213 This section should be part of the Methods section, and most of the text appears here for the 4th time.

We agree in essence, although adding the Methods section would make the structure of sections and subsections perhaps too cumbersome. We have rewritten this section and, in particular, removed repetitions. Text revised:

Differential field equations are solved numerically via their transformation to a discretized variational form (Gagliardini et al., 2013). **In the numerical implementation, the constitutive relation (11) is interpreted as a field equation with unknowns v and p.**

For the thermomechanically coupled model the Stokes equation (17) (together with the Eq. (11)) and the heat transfer equation (18) are solved sequentially until convergence is achieved. On each step of this nonlinear iteration a system of linear algebraic equations arises and needs to be solved […].

230 What is the role of the flow-enhancement factor? Is this needed at all? How is it applied, to firn and ice simultaneously? This would lead to densification values that are not compatible with the measured densities anymore.

The enhancement factor is absolutely necessary – it is the only free parameter of the model, apart from the model type, the boundary conditions and the characteristics of the numerical implementation. It is a selectable constant and applies to firn and ice simultaneously, which, however, does not lead to contradictions, since it corrects the viscosity and not the density distribution. Also, we have made the following addition to the text:

Changing the enhancement factor results in a general increase/decrease in ice/firn velocities. The velocities, in turn, affect the age field and, in particular, the configuration of the age–depth curve at the studied location. For the purely mechanical model, we varied the enhancement factor in the range from 0.15 to 0.35; for the thermomechanically coupled model, in the range from 0.01 to 0.05. Choosing the value outside these ranges resulted in a significant discrepancy between the model and the empirical age–depth dependencies.

245 Figure 4C merits some attention. This temperature profile is truly exceptional with 18 K temperature difference on only 180 m! Such high temperature gradients and heat fluxes are truly remarkable. Why are they occurring? Is the assumed basal heat flux of 340 mW/m2 really realistic? In mountains, the vertical heat flux is often much reduced due to topography.

Here is a possible explanation: *"This value is 4–5 times higher than the average heat flux density for the Earth's surface and higher than the mean value for central Caucasus, and may be associated with a heat magma chamber of the Elbrus volcano"* (Mikhalenko et al., 2015).

251 Write out "Figure" everywhere in the text, and abbreviate it in parentheses.

When providing references to figures, we relied on the following author guidelines: *The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that..."* (https://www.the-cryosphere.net/submission.html#figurestables). However, we noticed and corrected a discrepancy with the above rule in line 245. The corrected version is:

In Fig. 5b, the dating of the upper 168.6 m section […].

255 Since we cannot see anything useful in Fig 4b, please indicate what deviation the age of the basal ice has. Is it too old?

Yes, it is too old. Now we have mentioned this in the text:

The age of the basal ice is formally equal to 10,000 years due to the upper limit specified in the numerical solution.

280 wrong parens

Corrected.

280 But they nicely agree for other sites, such as Colle Gnifetti and Col du Dome, with similar model setups (Lüthi, Liciulli, Gagliardini, ...). The problem with the deepest layers is that they might be remnants from a time when the geometry of the glacier was very different, the ice divides were shifted and the flow regime was altered.

Agree. The text will be supplemented with a review and discussion of the effect of bubble close-off.

Also, as mentioned above, neglecting the effect of bubble close-off (not yet implemented in Elmer-Ice?) strongly affects the age at depth.

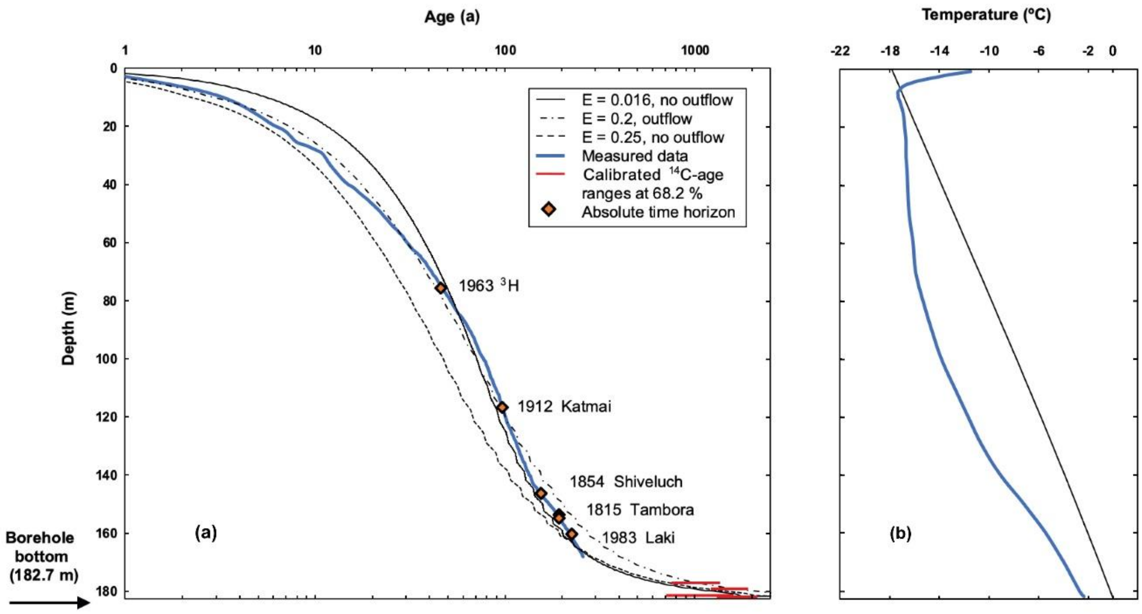
Unfortunately, we do not have the data to account for this effect in the dating problem. We are aware of only one attempt to implement the bubble closure effect in Elmer/Ice (Liciulli et al., 2020).

Figure 4b: There is nothing useful to see in this figure. Show the top and bottom parts separately. Additionally/alternatively, you could use a logarithimc age scale. Also indicate the reference dates (volcanoes) with dots.

Thank you for this very useful suggestion! This is done (see below).

Figure 4c: Please indicate 0 degrees, and also the pressure melting temperature at the base. From the plot one cannot determine the basal temperature.

The basal temperature according to the model turns out to be equal to the pressure melting temperature (–0.14 °C), which is now noted in the text. Since the latter value differs so little from 0 °C, it makes no sense to indicate it, in our opinion. Additionally, taking into account Reviewer 1's comment, we removed the velocity plot as it does not seem very informative in the context of our work. Below is the plot revised:



For a steady state model with density variation you will never get a straight line, since the vertical heat flux is constant, but you need a higher gradient in firn. The measured temperature profile also looks advective, which should have been captured by the model.

In fact, this profile is slightly nonlinear, it's just that its nonlinearity is very small. Indeed, thermomechanical coupling has little effect in our simulation. In general, for us it was just one of a series of numerical experiments. In our experience, in some cases, using a purely mechanical model to estimate the age of ice yielded better results in terms of matching ice core chronology, compared with the case of thermomechanical coupling. Namely, when scaling the rheological function in a coupled model, large corrections were required.

Finally, the spatially constant vertical heat flux b.c. in the model is not realistic in a mountain topography (e.g. Lüthi, 2001).

Since we have the heat flux value at only one point, the value of the vertical gradient of the heat flux would remain hypothetical. We will take this into account in further studies and consider it in more detail in the "Discussion".

Figure 4 caption: what is this stray "vertical a"??

We have corrected the caption to:

Figure 5. Modeled age (a), and temperature (b) **vertical profiles for the location *a*** (see Fig. 4) and empirical data.

Also, the number of this figure has changed.

Citation: https://doi.org/10.5194/egusphere-2024-3955-RC2

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