Reply on Referee Comment 1

Dear Reviewer,

We are very grateful for the work you have done, and we sincerely appreciate your thorough reading of our manuscript and provided fair and constructive comments. In the response below, your review comments are shown in blue, our responses are in black, and the corrected or added parts of the manuscript are highlighted in purple.

General comments

The paper "Ice/firn age distribution on the Elbrus Western Plateau (Caucasus) inferred from ice flow model" by Gleb Chernyakov, Nelly Elagina, Taisiia Kiseleva, and Stanislav Kutuzov presents a modeling study that employs a thermo-mechanical ice/firn glow model to analyze the age-depth relationship of an ice core drilled in 2009 on the Western Plateau of Mount Elbrus (Caucasus). The study explores multiple model runs, varying the outflow boundary conditions (BC), ice viscosity, and the inclusion of thermodynamical coupling. Among these runs (which are not all presented), the authors identify three configurations that are said to define an envelope within which all other simulated age-depth curves lie. A figure seems to show that the three selected cases yield age-depth relationships consistent with available measurements.

However, these measurements are represented as a continuous line, whereas discrete data points would be expected (see below).

Regarding novelty, this study is not the first of its kind and appears rather minimalistic compared to previous works (e.g., Zwinger et al., 2007; Gilbert et al., 2014; Licciulli et al., 2019). The only aspect that distinguishes it from existing studies is the choice of the study site.

Indeed, we did not set out to develop new methods. We were mainly interested in the object. This work is the next step in a long-term series of studies of the glaciation of Elbrus and, in particular, the Western Plateau.

Regarding the methodology, the thermodynamical model appears to be quite simplistic. It is formulated in terms of temperature rather than enthalpy, effectively excluding the possibility of melting/refreezing. This assumption may be reasonable given the stated "negligible seasonal melting," but it should at least be explicitly mentioned and justified.

The text has been revised accordingly (see our response to specific comment p2 L62). Also, in the "Discussion" section we have already noted the following:

[…] a study of the WP temperature regime (Mikhalenko et al., 2015), based on the method proposed by Salamatin et al. (2001), suggested basal melting occurs at depths exceeding 220 m, with rates not exceeding 10 mm water equivalent per year.

In addition, the model neglects the dependence of firn thermal conductivity on density, as evident from Eq. (15) and the linear temperature profile shown in Fig. 4c. This is a questionable assumption, and studies such as Zwinger et al. (2007), Gilbert et al. (2014), and Licciulli et al. (2019) have all accounted for this dependency.

We discuss this issue in response to specific comment p9 L161.

The surface Dirichlet boundary condition (BC) is set at a fixed temperature of −18 C, which is acceptable given that the simulations are steady-state. Similarly, the bottom Neumann BC assumes a uniform geothermal heat flux over the entire modeled domain, which is reasonable given the lack of data. At least, these two last assumptions are (very briefly) discussed in the last Section of the paper.

Overall, the relevance of this thermodynamical model is unclear. The only two-way coupling with the mechanical model would be through the advection term, yet the simulated temperature prole remains linear, suggesting that this coupling has little to no effect. By the way, I am surprised by this linear prole. Given the temperature-dependent thermal conductivity specified in Eq. (15), I would expect at least some degree of non-linearity in the temperature prole. Am I wrong ?

In fact, this profile is slightly nonlinear, it's just that its nonlinearity is very small. We agree that thermomechanical coupling has little effect in our simulation. In general, 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. E.g., when scaling the rheological function in a coupled model, large corrections were required.

Apart from the weak constraints on outflow velocities, the mechanical model appears reasonable, with one major exception: the mass conservation equation is never solved. This implicitly assumes that the 2009 density distribution applies uniformly across the entire modeled domain. Again, this is a strong assumption that should be explicitly discussed. A more rigorous approach would be to solve the Stokes equation and the mass conservation equation sequentially until a steady density profile is obtained, as done by, e.g., Gilbert et al. (2014) or Brondex et al. (2020). Naturally, given the uncertainties in the parameterization of functions a(ϕ) and b(ϕ), the resulting density profile would likely deviate somewhat from the measured 2009 distribution (Brondex et al., 2020).

We agree that this issue deserves special attention. We have added the following to the "Discussion" section:

Condition 5 is a significant simplifying assumption. A more rigorous approach is to calculate the density field based on the solution of the continuity equation together with the Stokes equation. In 2018, two more ice cores were obtained on the WP – 150.3 m and 119.8 m long. These cores are not dated, but the ice/firn density measurements showed close agreement with the density distribution in the 2009 ice core (Mikhalenko et al., 2020). This suggests that the actual deviations of the density values from the distribution used are moderate.

Again, I am surprised by the velocity field magnitude shown in Fig. 3. If I understand correctly, this result corresponds to the simulation where the outflow lateral boundary condition imposes zero normal velocity. If that is the case, how can such high (relatively) velocities be observed at the southern boundary ?

This is due to the relatively high tangential (to the vertical 'wall') velocities at the southern boundary. The lateral boundary condition in this simulated case sets the normal velocities to zero, while the tangential velocities remain unconstrained.

Another point regarding the methodology is the choice to evaluate the simulated profile at a location 50 m away from the actual drilling site. While I am fairly certain this does not significantly affect the results, I find the justification somewhat unclear.

We discuss this issue in response to specific comment p5 L93-98.

First, the fact that there is only a ∼ 6 m discrepancy between the length of the core and the ice thickness evaluated by differencing the bottom and surface DEMs is relatively good, especially considering the precision typically achievable with GPR measurements for an ice thickness of around ∼ 180 m. Second, while I am not an expert in remote sensing, I would expect that, although the mean annual surface elevation has likely been stable in recent years, there could be significant sub-annual variability in surface elevation, which could also explained part of the discrepancy.

Thank you for these ideas. In general, as noted below, the data obtained in different years relate to approximately the same season (July–September).

The surface DEM used to construct the computational domain seems to be from 2017, but was it retrieved at the end of summer to match the period when the drilling was done?

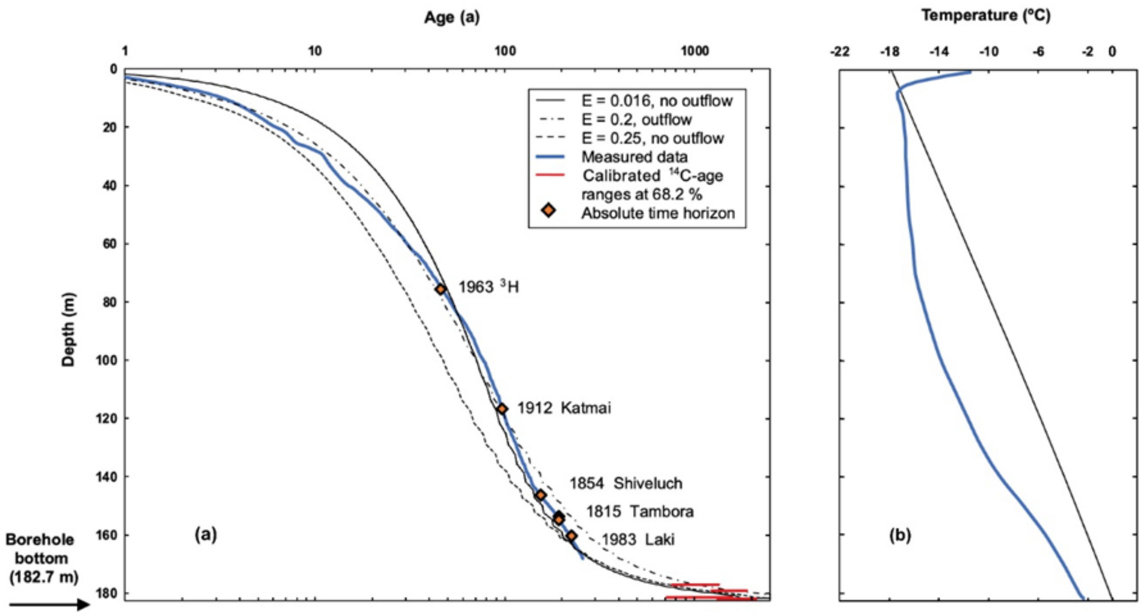
Yes, it matches quite well. The drilling was carried out in in August–September 2009, and the surface DEM is from 8 September 2017. See also our response to p2 L73.

In my opinion, instead of shifting the point of interest by 50 m, it would have been more appropriate to verify that the altitude provided by the DEM at the drilling site aligns closely with the field measurements taken in 2009. My general point is that, ideally, the depth 0 of the core should correspond as closely as possible to the surface altitude of the computational domain.

The difference between the elevation obtained using the 2017 DEM at the drilling site (5135.42 m above sea level) and the elevation determined during deep drilling in 2009 (5115 m above sea level; Mikhalenko et al., 2015) appears to be large, probably due to an assumed error in determining the latter. This makes comparison difficult.

Regarding the results, I have the impression that the figures are presented to the reader without being adequately described in the text. For instance, the purpose of showing velocity magnitudes in Fig. 4a is unclear, except perhaps to illustrate that velocities are essentially vertical, as suggested by the quasi-symmetry of the figure. However, this is not discussed anywhere in the text.

Indeed, the purpose of presenting this plot was to show how the vertical and the horizontal components of velocity are related, as well as typical magnitudes of velocity. We agree that this plot is not very informative in the context of our work and have decided to remove it. Also, in accordance with the recommendation of Reviewer 2, we presented the age dependence plot on a logarithmic scale and marked the reference horizons on it:



Additionally, I don't understand why the measured ages are plotted as a continuous line in Fig. 4b, while I would expect discrete points corresponding to the ages listed in the "Proxy date" column of Table 2.

This is mainly due to interpolation used in plotting the measured age (as well as the measured temperature). We have added a mention of interpolation to the text. In addition, the chemical dating of the upper part of the core has a resolution of about a year, and due to densification of firn at relatively large depths, neighboring dated points are close to each other, which even without interpolation gives the visual effect of a solid line in the age–depth plot.

One of the motivations of the study is stated as being to "reconstruct the age of the intermediate section of the 2009 core, which had not previously been dated by other methods." However, this outcome is not at all evident from this figure.

Agree. We have tried to fill this gap by adding the following description to the text:

The age of the ice in the intermediate part of the core (depth range 168.6–177.11 m), which has not been previously dated by other methods, can be estimated using modeling. For each horizon in this section, the model yields an age range depending on the selected parameters. The lower bound corresponds to a thermomechanically coupled model without lateral ice outflow and a flow enhancement factor of 0.016, while the upper bound corresponds to a purely mechanical model with lateral outflow and a flow enhancement factor of 0.2 (Fig. 5a). The difference between these estimates increases with depth, from 130 years at 168.6 m to 386 years at 177.11 m. At a depth of 168.6 m, the modeled age range is 1591–1721 (likely underestimated, as the corresponding age based on annual layer counting is approximately 1750). At 177.11 m, the modeled age range overlaps with the 68.2% confidence interval of the radiocarbon date (the overlapping range is 981–1245; see Table 2). It should also be noted that these age estimates refer to a location offset from the actual drilling site.

The discussion is also quite limited. The main assumptions of the model are only briefly addressed, with minimal justification provided (and some assumptions are even omitted, as noted earlier).

We have expanded the "Discussion" section and added the following points to the list of assumptions:

- *Density distribution:* The density distribution is not simulated and depends only on depth.
- *Heat conductivity parameterization:* The dependence of thermal conductivity on density is not accounted for.

There is a short mention of basal ice ages that "fall within or near the 68.2% confidence intervals of radiocarbon-dated ice core samples." However, given the large uncertainties associated with both the radiocarbon dating and the model at these depths, it is difficult to draw any meaningful conclusions.

Additionally, a discussion on the "calibration procedure" would have been appreciated. As it stands, one might get the impression that the authors ran thousands of simulations, discarded 997 of them, and kept only the three that gave the best fit to the available measurements. While I am sure this is not the case (since the authors mention that the three selected simulations bound the ensemble of depth-age curves) more information on this process would have been useful.

To describe the calibration procedure in more detail, we have added the following 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.

Otherwise, the English is good, and the quality of the figures is relatively good.

However, many citations are presented in parentheses when they should not be, especially towards the end of the introduction.

Corrected.

In my opinion, the paper cannot be published in its current form, and major revisions are required. Below, I list my specific comments.

Specific comments

p2 L26 It would be helpful to mention whether the 2009 drilling reached the bed. We understand later on that it did but mention it from the introduction would be preferable.

We agree with your comment and have added the following clarifications to the text:

Glaciochemical investigations of a deep ice core **(182.65 m long – from the glacier surface to solid rock)** drilled in 2009 include […].

[…]

According to DEMs ice thickness at the 2009 drilling site location is 6.1 m greater than ice core length. **Possible reasons for this discrepancy include: time difference in obtaining the DEMs of the surface and the base (8 years); errors in constructing the DEMs; inaccuracy in determining the coordinates of the drilling site; probable presence of a solid inclusion in the ice above the glacier bed, which could become a mechanical obstacle to drilling to the bed (Stiévenard et al., 1996).**

p2 L34-35 It is somewhat unclear what is meant by 'basal ice' and 'deepest ice'. I understood 'basal ice' to refer to the ice located at the bottom of the drill site, whereas 'deepest ice' seems to refer to thicker ice found elsewhere on the plateau, where the glacier is deeper. Clarifying this distinction would help improve readability.

We call the ice adjacent to the glacier bed 'basal' (regardless of its depth). By 'deepest ice' we mean the basal ice in the area of the glacier with the greatest ice thickness. To improve readability, we have changed the text as follows:

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).

p2 L47-59 Many citations are given in parentheses when they should not. Please check and correct here and elsewhere in the manuscript.

Done.

p2 L49-50 Remove "Mt" and replace "Mont Blank" by "Mont Blanc".

Corrected.

p2 L50-51 Gagliardini and Meyssonnier (1997) are actually dealing with the same study site as Vincent et al. (1997).

Agree.

p2 L50-51 Similarly, Licciulli et al. (2019) actually investigate the same study site as Lüthi and Funk (2000). I would suggest restructuring the paragraph to create a clearer logical flow. Currently, it shifts from one study site to another, then circles back to the first site. An alternative approach could be to structure the paragraph chronologically, highlighting the progress made over the years from basic models to increasingly complex ones. I believe this might be your intention, but at present, it does not come across clearly.

Indeed, we intended to describe the progress in model development. Text revised:

Ice flow models have been repeatedly used to study the age distribution of ice in mountain glaciers and, in particular, ice cores. **Dating models have undergone a natural evolution – from 2D analytical solutions to 3D numerical approaches that take into account the mutual influence of mechanical and thermal processes. The main stages of this development appear to us as follows.** 2D purely mechanical (Vincent et al., 1997) and thermomechanically coupled (Salamatin et al., 2000; Shiraiwa et al., 2001) analytical models were developed […].

p2 L62 I guess it is the yearly averaged isotherm (averaged over which period ?).

Text revised:

Glaciers cover an altitudinal range from 2680 to 5642 m a.s.l. **Above 5200 m a.s.l. temperature stays negative throughout a year and no melting occurs (Mikhalenko et al., 2020).**

p2 L73 The year and the time of year when the DEM was produced should be specified here, as this information is important for context and interpretation.

We have added the date of the Pléiades image to the text:

For the information about topography, we used the Pléiades digital elevation model (DEM) from **8 September 2017** with the vertical uncertainty between ±0.5 m and ±1 m (Kutuzov et al., 2019b).

p3 L78-79 It is not entirely clear whether both the surface and bedrock DEMs are from 2017. As currently written, it seems that only the radar surveys, and therefore the bedrock DEM, were conducted in 2017.

Text revised:

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

p4 Fig. 1 Even if explained later, I believe the definitions of 'a' and 'b' should be specified directly in the caption. I would also recommend explicitly mentioning that the top-right figure is a zoom of the black dashed square.

Agree. The caption is corrected as follows:

Figure 1. Study area on Mt. Elbrus. **The vertical distributions obtained in our study (Fig. 5) refer to location *a*; the 2009 drilling site is marked with *b*. The top-right figure is a zoom of the black dashed square.** The elevation bands are based on the Pléiades DEM of 2017. The coordinates are presented in the WGS 84 (UTM 38 N). The SPOT 7 image obtained on 20 August 2016 is shown as a background.

p4 L92 I think it is always helpful to specify the typical node spacing at both the refined boundary and the coarser boundary when working with refined meshes.

Agree. The following addition has been made to 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).

p5 L93-98 I find this argumentation confusing. See my general comments.

We have expanded the argumentation:

In our opinion, comparison of age–depth distributions (one modeled and one ice core based) for locations at a glacier with different ice thicknesses makes little sense. **Also, the glacier surface elevation at the drilling site (5135.42 m a.s.l.) obtained using the 2017 DEM does not align closely with the glacier surface elevation at the drilling site determined during deep drilling in 2009 (5115 m a.s.l.; Mikhalenko et al., 2015), probably due to an assumed error in**

determining the latter. This makes problematic another possible interpretation of the original data, namely, matching the zero depth of the ice core with the surface altitude of the computational domain.

To represent the vertical profiles of ice/firn age and temperature and compare them with ice core data, we chose one of the points closest to the drilling site with similar ice thickness (182.67 m according to DEMs) and topography of surface and bedrock.

p5 L100-101 I would recall that there is an assumption of steady state.

Agree. We have added this to the phrase:

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.

p7 L116 "Next, the constitutive equations of the model are subsequently introduced" → I would remove this sentence.

Agree. The sentence has been removed.

p8 L126 For a and b, you are not using the original parameterization proposed by Gagliardini and Meyssonnier (1997) but the corrected ones proposed by Zwinger et al. (2007) and then re-used succesfully by, e.g., Gilbert et al. (2014), Licciulli et al. (2019) or Brondex et al. (2020).

We are very grateful to the referee for pointing out the inaccuracy we made here. Now, when introducing the functions a and b, a more appropriate reference is given – Zwinger et al. (2007). Also, the history of modifications of the rheological law is described in a little more detail in the Introduction:

Gagliardini and Meyssonnier (1997) adapted the rheological law of Duva and Crow (1994) for a cold glacier with a thick firn layer and implemented it in a 2D dynamical finite element model for Dôme du Goûter. Further, the firn rheological law of Gagliardini and Meyssonnier (1997) was applied in 2D and 3D finite element models for Colle Gnifetti glacier saddle (Monte Rosa, Swiss/Italian Alps) in the work of Lüthi and Funk (2000). Zwinger et al. (2007) modified the rheological relations of Gagliardini and Meyssonnier (1997) for 3D thermomechanically coupled Stokes flow model implemented based on Elmer/Ice and applied the model to a crater glacier at Ushkovsky Volcano.

p8 L136 You forgot to mention what is T_0.

T_0 is mentioned in Table 1.

p9 L13 and below The way you are presenting the constitutive laws/field equations is kind of strange to me. Equation (13) alone is sufficient for pure ice, as it reduces to the usual Glen's law when $\rho = \rho_i$. For pure ice, the incompressibility assumption leads to div $\mathbf{v} = 0$, which implies that the strain rate tensor is purely deviatoric. However, when dealing with compressible firn, an additional equation for the spheric parts of the stress (i.e., p) and strain rate (i.e., div $\mathbf{v}$) tensors is required to close the constitutive relationship. This equation is:

$$p = -\frac{1}{b}(2A)^{-\frac{1}{n}}\gamma^{\frac{1-n}{n}}\,\mathrm{div}\,\mathbf{v}\,. \tag{1}$$

I agree that combining this equaiton to your Eq. (11) results in your Eq. (17), but in my view, this is a constitutive law (a relationship between stresses and strain rates) rather than a field equation. I am aware that Zwinger et al. (2007) also presents your Eq. (17) as a field equation. Conversely, the missing field equation in your formulation, in my opinion, is the mass conservation equation:

$$\frac{\partial\rho}{\partial t} + \mathrm{div}\,\rho\mathbf{v} = 0. \tag{2}$$

The way the problem is presented implies that the unknowns are (u, v, w, p) and the corresponding system of four scalar equations is given by the three scalar equations in your Eq. (18) together with the scalar Eq. (17). However, this formulation neglects the fact that ρ is another unknown, and thus Eq. (2) is necessary to close the system. Assuming that ρ is known from core measurements and can be applied uniformly across the domain is a strong assumption that should at least be mentioned and discussed (see my general comments).

We agree with your remark that it is more correct to classify this equation as a constitutive relation. Thank you for pointing this out. The corresponding correction has been made in the text. Another thing is that at the level of numerical solution, as in the works of Zwinger et al. (2007), Gagliardini and Meyssonnier (1997), this relation actually acts as a field equation (instead of div $\boldsymbol{v} = 0$ for pure ice). In our case, the fields sought are $\boldsymbol{v}$ and p, and in the full formulation also T. We discuss the absence of the mass conservation equation in our formulation of the problem in the responses to your general comments.

p9 L149 A reference to justify this parameterizaiton would be welcome.

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.

p9 L151 Same as above. I also find it strange that the dependence of the thermal conductivity of firn on its density is not accounted for (see my general comments).

The above reference also applies here. We discuss the independence of thermal conductivity from density in our model in the response to your next comment.

p9 L161 A thermodynamic model that operates in steady state, does not account for variations in thermal conductivity with density, and is unable to handle melting and refreezing (as shown from the fact that it is expressed in terms of temperature instead of enthalpy and lacks a latent heat source/sink term) calls into question its relevance for the present study. Please refer to my general comments.

Stationarity is a general and largely unavoidable (due to insufficient data) assumption in our study. We also attempted to include the dependence of thermal conductivity on density to the model, as in the work of Zwinger et al. (2007). The absence of ice melting in the model is discussed in the response to general comments. We added the following comment to the "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.

p10 L179 I don't get this boundary condition. Normally at the bed, a non-penetration condition applies and the normal velocity is forced to zero.

Indeed, an explanation is needed here. A comment has been added in the text after this boundary condition (no. 21):

Under the BC (21), 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.

p11 L205-211 I am not sure this explanation desserves a full paragraph and I think that the last sentence could be removed.

Agree. We have excluded this section and moved the information from it to the section "Model calibration". The last sentence has been removed.

p11 L218-220 The description of the numerical implementation is a bit unclear. From my understanding, the mechanical and thermodynamic problems are solved sequentially using a first-operator splitting approach until convergence is achieved (referred to as the 'steady state iterations' in Elmer). Independently of this coupling, the Stokes equation (Porous Solver in Elmer) is non-linear due to $n = 3$ in the constitutive law and needs to be linearized. The same approach applies to the heat equation, which is non-linear due to the T-dependence of heat capacity and thermal conductivity. In each non-linear iteration, a linear system is obtained, which can be solved using either direct or iterative methods.

We have tried to clarify this description. 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 […].

p12 L226 "a precalculated velocity field" → does that refer to the velocity field calculated by the mechanical model (I guess it does) ? Please, clarify.

Yes, it does. We changed the sentence to:

The dating equation (1) is solved in a final step based on **the velocity field calculated by the mechanical model.**

p13 L248 The last two samples do not appear in Table 2. Why ?

We see no point in comparing the dating of these samples with the modeling results because of the large discrepancies. The unsuitability of the model for dating bottom ice is noted in the Discussion section. We also provide the revised text:

Table 2 compares the dates obtained from our simulations with the 2009 ice core dating for reference horizons and the two upper basal ice samples. **The two lower ice samples are not included in Table 2 because the model significantly overestimates their ages.**

p16 L282-283 This is a bit of an overstatement. Your modelled temperature profile is actually quite far from the measured one.

Text revised:

The vertical temperature distribution, simulated using the thermomechanically coupled model, **is almost linear and overestimates temperatures (Fig. 5b). Thus, the mechanical coupling is weakly manifested.**

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