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 flow 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.

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. 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. 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 profile remains linear, suggesting that this coupling has little to no effect. By the way, I am surprised by this linear profile. Given the temperature-dependent thermal conductivity specified in Eq. (15), I would expect at least some degree of non-linearity in the temperature profile. Am I wrong ?

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 $a(\phi)$ and $b(\phi)$, the resulting density profile would likely deviate somewhat from the measured 2009 distribution (Brondex et al., 2020). 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 ?

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. First, the fact that there is only a $\sim$ 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 $\sim$ 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. 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? 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.

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. 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. 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.

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). 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.

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.

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.

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.

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

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

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

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

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.

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

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.

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.

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.

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.

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

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

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

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).

p8 L136 You forgot to mention what is T_0.

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 $\rho = \rho_{\text{i}}$. For pure ice, the incompressibility assumption leads to $\operatorname{div}\mathbf{v} = 0$, 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., $\operatorname{div}\mathbf{v}$) tensors is required to close the constitutive relationship. This equation is:

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

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:

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

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).

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

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).

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.

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.

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

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

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

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

References

Brondex, J., Gagliardini, O., Gillet-Chaulet, F., and Chekki, M.: Comparing the long-term fate of a snow cave and a rigid container buried at Dome C, Antarctica, Cold Regions Science and Technology, 180, 103164, https://doi.org/10.1016/j.coldregions.2020.103164, 2020.

Gagliardini, O. and Meyssonnier, J.: Flow simulation of a firn-covered cold glacier, Annals of Glaciology, 24, 242–248, https://doi.org/10.1017/S0260305500012246, 1997.

Gilbert, A., Gagliardini, O., Vincent, C., and Wagnon, P.: A 3-D thermal regime model suitable for cold accumulation zones of polythermal mountain glaciers, Journal of Geophysical Research (Earth Surface), 119, 1876–1893, https://doi.org/10.1002/2014JF003199, 2014.

Licciulli, C., Bohleber, P., Lier, J., Gagliardini, O., Hoelzle, M., and Eisen, O.: A full Stokes ice-flow model to assist the interpretation of millennial-scale ice cores at the high-Alpine drilling site Colle Gnifetti, Swiss/Italian Alps, Journal of Glaciology, pp. 1–14, https://doi.org/https://doi.org/10.1017/jog.2019.82, 2019.

Lüthi, M. and Funk, M.: Dating of ice cores from a high Alpine glacier with a flow model for cold firn, Annals of Glaciology, 31, 69–79, https://doi.org/10.3189/172756400781820381, 2000.

Vincent, C., Vallon, M., Pinglot, J. F., Funk, M., and Reynaud, L.: Snow accumulation and ice flow at Dôme du Goûter (4300 m), Mont Blanc, French Alps, Journal of Glaciology, 43, 513–521, https://doi.org/10.3189/S0022143000035127, 1997.

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, Annals of Glaciology, 45, 29–37, https://doi.org/10.3189/172756407782282543, 2007.