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Interactive comment on "Dating the ice of Gauligletscher, Switzerland, based on surface radionuclide contamination and ice flow modeling" *by* Guillaume Jouvet et al.

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We would like to greatly thank you for your comments on our manuscript.

In this study the authors use measurements of radionuclide activity in ice samples taken from the surface of Gauligletscher to calibrate anice flow model. Radionuclide activity anomalies are typical of the nuclear weapon tests conducted in the 1950s and 1960s and provide a constraint on the ice age.The model is then used to update the trajectory of an aircraft which crash-landed on the glacier in 1946 and parts of which have recently reappeared. The predicted trajectory appears to be more consistent than that given by a previous study.In addition to the radionuclide data, 2 DEMs and a velocity field are used to constrain the model parameters (3 parameters for ice dynamics (A,cu,cl) and 2 parameters for surface mass balance (Cp and CM)).This is a very original study, the results are convincing and the paper is well presented. I have listed below some general and detailed comments that deserve some considerations by the authors :

General comments:

- The parameterization used in this paper for the basal friction differs from the previous study since here 2 distinct coefficients are used, for the upper and lower parts of the glacier. The friction coefficient for the upper glacier is calibrated using the radionuclide measurements and appears to be consistent with what we could guess as it leads to low sliding in the upper part of the glacier. Thus I wonder what is the real contribution of the radionuclide data for the calibration of the model, i.e. is it not possible to calibrate this parameter without these data and/or what would the model give if we assume, for example,no sliding above the equilibrium line?

  ⇒ The lack of velocity flow data in the accumulation area (snow-covered regions show hardly no features to track in comparison to ablations areas) makes challenging the calibration of the ice dynamics in glacier upper parts. Furthermore, our sliding parametrization is global in space (only two parameters) and in time (no time variation at all), and we have no information at all about former ice flow magnitude. Therefore, we need data that are global in space and time as well for calibration. While our observations are mostly available in the ablation area and in recent times, radionuclide data have the advantage to be global in space and time, which is a remarkable feature. The only global and past data we have are the DEMs, and even if there are key data, there are not sufficient to constrain our set of 5/6 model parameters. Radionuclide data are especially interesting as they include data from the past while being observable today (which is obviously not the case for DEMs). **The following sentence "In particular, Radionuclide contain data from the past seven decades while being still observable today." was added to the conclusion to emphasize this.** To our knowledge, there is no analogue data that have the same nice property.

⇒ It must be stressed that "fewer sliding in the upper part than in the lower part" is our own assumption based on physical considerations, and is not a result of radionuclide data (see i) in Section 4.3). Our results show that sliding can be reduced in the upper part but not necessarily suppressed. Assuming no sliding at all in the upper part might be too restrictive considering that over-estimated sliding in the upper part could possibly compensate (flux-wise) for underestimated ice deformation in this region as we assumed a constant viscosity parametrization (i.e. constant rate factor A).

- The performances of the model are presented only in terms of RMSE, as the parameterizations used are quite simple I think it could be interesting to show some error maps (speed and elevation) in order to discuss the robustness of the model. Notably only the speeds and ages along the centreline are used for the calibration, which corresponds roughly to the aircraft's trajectory. However the ages on the sides of the glacier are less well reproduced by the model and it would be interesting to show error patterns. ⇒ **We added an appendix on "Error pattern between modelling and observations", where we display the error pattern in terms of DEMs for the best guess model after 33 and 63 years of model simulations, and shortly discuss this additional result in the paper.** It is however more difficult to produce a similar map for velocities as we only considered 14 control points. Lastly, our choice of Figures 5 and 8 was made so that one can visualize partially the observation-modelling misfit in terms of age of ice in both directions: cross-flow (Fig. 5) and flow (Fig. 8).

- Only the uncertainty on the bedrock is discussed to justify the differences on the edges of the glacier. Could part of these differences also be due to a too simple parameterization with spatially uniform coefficients and be due to the calibration which uses velocities only along the central line? ⇒ Thank you, this is a very good point. Indeed, the inaccuracy of the model to reproduce the age of ice along the lateral direction can also be due to an inaccurate parametrization of basal sliding. **We have included this second possible cause along the bedrock data throughout the paper (section 4.3 and fourth paragraph of the discussion).**

Minor comments:

- **Page 2 line 46:** "the location of the oldest ice in Antarctica". Add also reference to Passalacqua et al. (2018)? Passalacqua O., M. Cavitte, O. Gagliardini, F. Gillet-Chaulet, F. Parrenin, C. Ritz and D. Young, 2018. Brief communication: Candidate sites of 1.5 Myr old ice 37 km southwest of the Dome C summit, East Antarctica, The Cryosphere, 12, 2167-2174 ⇒ **Done**
- **Page 3 line 63**, ref. to Gäggeler et al. Remove "()" ⇒ **Done**
- **Page 4 line 85**,ref. to GLAMOS. Idem ⇒ **Done**
- **Page 5 lines 106-107** "to iteratively compute the ice flow velocity field and the mass balance" and **Sec. 3.1**: Clarify that you compute the evolution of the glacier

free surface and give the equation. ⇒ **A section on "mass transport" has been added in the model section to provide further details. However, we do not give the equation itself for the safe of conciseness. Instead we refer to an equation in one of the Elmer/Ice core paper.**

- **Sec 3.1.1 Data: Bedrock topography:** as the uncertainty in the bed is mentioned as a potential explanation for the discrepancy between the model and the data on the sides of the glacier(sec. 5 page 19),it would be interesting to give more details on the method to get the bed and show the radar measurements profiles in Fig.3? ⇒ **As mentioned above, we now propose both uncertainties – in the bed and/or on the basal parametrization of sliding – in the revised manuscript.** Details concerning the bedrock derivation from radar profiles and a Figure showing the profiles are given in Supplementary material of our early study by Campagno and al. published in Frontiers in Earth Sciences, 2019, (https://www.frontiersin.org/articles/10.3389/feart.2019.00170/full#supplementary-material). **We added two references to this material in our revised manuscript.**

- **Sec 3.1.1 Data: Page 6, lines 114-115:** "and an update of observed velocities based on the 2015-2019 observations" is that not was is derived from the Sentinel-2 orthoimages mentioned in the previous sentence? ⇒ **"Sentinel-2" was added for clarity.**

- **Figure 3 and similar maps:** please include a scale in the figures. ⇒ The scaling is indicated by a the grid of the topographic map, however, it was not mentioned in caption. **This is now recovered. Concerned figure captions starts read "Topographic map of Gauligletscher with 1 km grid spacing ..."**

- **Page 6, line 125:** change the order of the sentence, i.e. "ub is the norm of the basal velocity ..., σ and u are the basal shear stresses and basal velocities... ⇒ **Done**

- **Page 7, Line 128:** Provide the exact parametrisation used for the transition between cl and cu. ⇒ Giving the exact parametrisation is somewhat cumbersome for an unimportant detail. **Instead we added the approximative width of the transition band, which is the main information to retain.**
- **Sec 3.1.3:** give units for fm, ri and rs ⇒ **Done**
- **Sec 3.1.5:** justify the fact that you calibrate cl only against 14 points along the central flow line and not against the whole velocity map shown in Fig.3 ⇒ As we only have one parameter to tune for sliding in the lowest area, our optimization problem would be highly under-determined if we were fitting the entire velocity field. Therefore, we restricted the misfit function (RMSE) to only a few uniformly selected points with the (more modest) goal to get the global velocity magnitude as good as possible. In that perspective, it makes sense to focus on the highest speeds, which are obtained approximatively along the central flowline. **For clarification, the following justification sentence was added: "The choice of considering only 14 points distributed along the central flowline was made to reduce under-fitting in the minimization procedure, which optimizes a single parameter."**
- **Sec 3.1.5:** It would be clearer to explain that you select 3 values for A and 4 values for Cp then calibrate the remaining parameters for the 3*4=12 sets ⇒ We agree that one could have fix other parameters than A and C_p, and optimize the remaining ones. Here have 5 parameters, and 3 constrains, so 2 parameters remains free, and we have to make a choice. Any other choice would give other "discretization points" of the parameter space but should yield to the same results. As our choice was more practically-motivated than anything else, we don't see the point to justify it in the text as it has no implications.
- **Sec 3.2.2:** As most of the readership will not be expert in radionuclides it would be interesting to give some description of the values that are expected and the

possible interpretation of the different nuclides in this section ⇒ **We added a paragraph at the beginning of Section 3.2.2 to introduce and motivate this section, as well as to give some expected values from the literature to ease the interpretation for non experts.**

- **Sec 4.2: Line 225:** "By contrast all the samples collected in summer 2018 show very small activities (below 0.1mBq/kg in 239Pu, results not shown). Therefore, only 2019 sampling results were considered in the following.". As I'm not an expert in radionuclides, at this point,it was not clear for me why the 2018 samples show small activities and were disregarded. I understood only later that this is because the 2018 line didn't sampled the right section so that the ice was younger and thus not contaminated. ⇒ **That's a good point. The sentence was completed with "since the ice sampled in 2018 was mostly likely too young for being contaminated (Fig. 5)" for the sake of clarity.**

- **Sec 4.2: Line 244-245:** "We associate it also with a ± 2 year uncertainty, which corresponds to the distance between the two peaks."We read above that the two maximums correspond to 1958 and 1963 so the distance between the peaks is 5 years and not 2? ⇒ Here we mean the interval $[-2.5, 2.5]$ or ± 2.5 has a length of 5 y (1958-1963). We rounded 2.5 to 2.

- **Sec 4.3, line 274:** "Parameters A and cl were found to match observed surface velocities of the ablation area over the 2015-2019 period".Please clarify this sentence. In fact cl is calibrated against velocity measurements for each value of A, and it seems that every combination give similar RMSE?. ⇒ **We rephrased "For each A, parameter c_l is ...". Yes, the RMSE is not very different between optimized couples (A, c_p) so that we can hardly use this constrain/data to exclude parameters.**

- **Sec 4.3, lines 276-278:** "This is due to the new dataset of observed velocities (Appendix A), which showed faster ice in the ablation area than the dataset used formerly.". Is it the only reason?As I understand the former study used a unique value for the friction coefficient, so we could imagine that the best value was a compromise between high friction in the upper part and low friction in the lower part? ⇒ Yes, this is the main reason. As observed velocities are anyway only available in the ablation area, the misfit function can only be used to assess c_l, and not c_u. So the former study simply assumed that the tuning made in the ablation area can be extended to the accumulation area. Here we get rid of this assumption and tune c_u using new radionuclide-based data.

- **Sec 4.3, Line 280:** "Thus we explored a stronger mass balance vertical gradient, i.e., higher precipitation and higher melt scenarios, as no direct measurements were available there." Not sure why the gradient will be stronger, if you increase precipitation and melt by the same amount the gradient should remain the same? You explore a larger set of mass balance scenario?Explain the relation with the gradient. ⇒ Here we meant precipitation and melt range or amplitude (controlled by C_P and C_M) over the full glacier altitude, making the gradient naturally stronger or weaker, considering that melt is highly controlled by altitude. **For clarity, we changed "higher precipitation and higher melt scenarios" into "higher precipitation and higher melt amplitudes".**
- **Figure 11, caption:** "Distance traveled by the Dakota main body along the trajectory line drawn on Fig. 10"I assume that each parameter set results in a different trajectory, soit is not exactly the one shown in Fig. 10. Maybe,it could be interesting to show the 12 trajectories in Fig. 10or in a new figure? ⇒ When preparing the manuscript, we actually first drew all trajectories, but found that i) it was very difficult to distinguish them ii) it was no very informative. Additionally, Fig. 11 already gives key information on all 12 trajectories, namely the end positions projected on a flowline. Therefore, we did not include an additional figure showing the 12 trajectories in plan-view to not overload this already long paper.

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