Review of « Dating the ice of Gauligletscher, Switzerland, based on surface radionuclide contamination and ice flow modeling » submitted to The Cryosphere by Jouvet et al.

In this study the authors use measurements of radionuclide activity in ice samples taken from the surface of Gauligletscher to calibrate an ice 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 geuss 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 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 centre line 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.

- 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 ?

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, doi:10.5194/tc-12-2167-2018
- ***Page 3 line 63****,* ref. to Gäggeler et al. Remove *« () »*
- ***Page 4 line 85****,* ref. to GLAMOS. Idem
- ***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.
- ***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 ?
  - ***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 ?
- ***Figure 3 and similar maps*** **:** please include a scale in the figures.
- ***Page 6, line 125*** **:** change the order of the sentence, i.e. *« ub is the norm of the basal velocity, \sigma and u are the basal shear stresses and basal velocities… ».*
- ***Page 7, Line 128*** **:** Provide the exact parametrisation used for the transition between cl and cu.
- **Sec 3*.1.3 :*** give units for fm, ri and rs
- **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
  - 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.
- ***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.
- ***Sec 4.2***
  - ***Line 225*** : « *By contrast all the samples collected in summer 2018 show very small activities (below 0.1 mBq/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.
  - ***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 ?

- ***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 ?.
    - ***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 ?
    - ***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.
- ***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, so it is not exactly the one shown in Fig. 10. Maybe, it could be interesting to show the 12 trajectories in Fig. 10 or in a new figure?