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The Cryosphere Discussions

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.

Jouvet and colleagues make use of Uranium and Plutonium tracers deposited in the 1950's and 1960's on the Gauligletscher (Switzerland) due to the fallout of nuclear weapons tests. They use these tracers to identify isochrones and benchmark their high resolution glacier model against them. They argue, that using these isochrones

as a data benchmark to tune against provides a more stringent constraint on model parameter uncertainties compared to traditionally used tuning targets such as DEMs and surface velocity maps and their results make a compelling point. The manuscript is well written, with high quality figures and a logical structure which is easy to follow. Its content fit well into the scope of The Cryosphere as they show convincingly, that their methodology can be used to improve the parameterisation of glacier models significantly.

I congratulate the authors on a really nice manuscript and only have some minor comments/questions followed by stilistic/spelling edits.

- Given the large improvement of the overall model performance due to parameter optimisation a natural question would be how projections of the glacier's evolution would change compared to the old parameter set. Maybe the authors could speculate as to how they think the new parameters might change the expected mass loss in the future or whether they plan to carry out further simulations in this direction

  ⇒ This is a good point, that we have not discussed in our original manuscript. **We added the following sentence in conclusion about the challenge of well calibrating key parameters for modelled glacier projections: "Yet tuning accurately ice flow and mass balance parameters is essential to reliably model the future evolution of glaciers with a small uncertainty range, especially in the context of global glacier retreat in a changing climate regime."** However, we do not plan any specific simulations of Gauligletscher in the future.

- To illustrate the effect of the model optimisation an additional figure showing the spatial expression of ice thickness and surface velocity model mismatches with respect to observations (percent, relative change) for the old and new

model setting would be nice ⇒ **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 simulation, and shortly discuss this additional result in the paper.** A similar figure was produced for the former model in the in Supplementary material of the first 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). However, one can not see a clear improvement looking at the DEM error patterns between the former and the revised model because DEMs alone are not sufficiently constraining (see also our first block of answer to referee # 1). Say differently, one can obtain a reasonable ice surface elevation at the end of the modelled period while underestimating or overestimating simultaneously internal (ice dynamics) and external (mas balance) ice fluxes, the two compensating each other. Last, we have not produced a similar error pattern map for velocities as we only considered 14 control points, and not the full glacier surface as for DEMs.

- It is interesting to see in Figure 7, that the parameter optimisation seems to have a moderate effect on RMSE (thickness) compared to the change in observed and modelled glacier length. However, this might be a misinterpretation. As mentioned in the comment above, a 2D figure of the model mismatch for old vs new model would be helpful.

  ⇒ One always needs to be careful with interpreting the fit between observed and modelled glacier lengths since a slight and local change in melt parametrization at the tongue might induce strong change in the glacier length, which is computed along a given central flowline. This explains why the RMSE in terms of glacier length has a wider range that any other RMSEs. For this reason and because there are more global, RMSE in DEMs are – to our opinion – more

reliable metrics to assess model quality.

- on page 16 you mention that uncertainties in the bedrock topography could be causing the exaggerated flow asymmetry. This raises the question as to what extent the bedrock topography can be used as a "tuning" parameter to improve the velocity pat-terns. It probably depends on whether there is a systematic uncertainty in the bedrock topography which would offset the flow asymmetry? In general, modifying the bedrock topography to match flow patterns would be inadvisable unless one knows the actual bedrock topography. But then you would use the corrected bedrock topography in the first place. You re-iterate this point in the discussion by stating that "Here we have shown that our bedrock is likely too shallow on the north-east side and too deep on the other side" However, to me it is unclear how you get to that conclusion. The poor model prediction regarding line 1 and 5 might have other causes? Also bedrock uncertainties are to be assumed for the whole glacier area, why should they be especially relevant in case offline 1 and line 5? Please elaborate

  ⇒ We agree that optimizing the bedrock is far from obvious. Our statement remains quiet general i.e. one can accelerate (slow down) the flow by making the ice thicker (thinner), however, we are aware that the "how to" is a own field of research, and there is a community using proper inverse modelling dealing with this problem. **To answer your concern we tempered our statement "Here we have shown that our bedrock is likely too shallow on the north-east side and too deep on the other side" into "Here our bedrock data might be too shallow on the north-east side and too deep on the other side to explain the mismatch between modelled and observed isochrones."**. Also, the inaccuracy of the model to reproduce the age of ice along the lateral direction could also be caused by an inaccurate parametrization of the sliding (which is constant in the lower part here) as suggested by the other referee. **Therefore,**

we have included this second possible cause along the bedrock data throughout the paper (section 4.3 and fourth paragraph of the discussion).

- on page 17 you mention the interesting fact that the model isochrones shift from a U-(after 1970) to a V- shape (before 1970) without giving an explanation. Isn't this just due to the narrowing channel the glacier is pushing through? I would intuitively assume that the glacier moves more homogeneously across a given horizontal transect in the upper part compared to the lower part (where glacier flow is confined on both sides)?A short interpretation as to why the shape changes would be helpful.

  ⇒ Thank you very much, your interpretation makes perfect sense to us. **We added the following sentence: "Here we can reasonably assume that the downstream tightening of isochrones follows the narrowing of the channel, which hosts the glacier tongue."**

Minor edits: Most of the suggestions from the referee were followed. Comments with answers from authors are reported below only in the case the suggestions were not or not entirely followed.

- **Figure 7:** for the sake of readability i suggest to make the small markers (discarded runs) semi-transparent. Same fro Figure 8. ⇒ We tried to make small markers partly transparent, but it did not improve the readability.

- **p17, l325:** I suggest to rephrase to: "Ice with radionuclide contamination above 0.25mBq/kg has the same pattern and mostly appears to be band-wise (Fig.

5)" ⇒ We left the original sentence to keep both radionuclides in the same first introductory sentence before being specific on each.

- **p19,l1:** running an ice flow model

  ⇒ 'tuning' sounds more accurate than 'running' to us.

- **p19,l366:** Furthermore, snow-covered ...

  ⇒ We kept the original formulation as 'Indeed' sounds to us a more appropriate connection word.

- **p19,l1:** p22, l457: this part is a little implicit. Why is it remarkable? I would assume (correctme if I am wrong) that the climatic conditions of Aletschgletscher changed similarly to the Gauligletscher, so the correction factors should be similar. What are the qualitative differences of Aletschgletscher compared to Gauligletscher (except for the size)which would make a similarity of parameters surprising? I agree with the authors, that isochrones should be utilised to constrain model simulations. However, in this case"traditional" tuning targets for the Aletschgletscher seemed to have produced similar results compared to the fine tuning against isochrones for the Gauligletscher? Or was the 2011 model also optimised with respect to isochrones?

  ⇒ Aletsch 2011 model did not involve any observed isochrones for tuning, but instead used traditional data similar as Gauli (DEMs, surface velocities, ...), however, we had more data in past times making the calibration further reliable. Aletsch and Gauli are sufficiently different (especially size-wise) that we do not necessarily expect tuned parameters to be close to each other. As both key models (ice flow) and (mass balance) still rely on simplifications (the most

complex, the most generic), specific re-calibration is required for each glacier, especially if the size differs. **However, we decided to remove noticing the match of the two parametrizations from the conclusion as we can not make a strong point based on a good match of only two glaciers, and it is likely that the range would get larger if we were able to add glaciers to the statistic.**

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