In this document, we respond to the comments of reviewer 2 one by one. Whenever some entirely new text has been added to the manuscript, it has been added in italics and in red.

The proposed revised with and without track changes is added as a supplementary .pdf file.

Reviewer 2

Lander Van Tricht and colleagues present the ice thickness estimations of the Grigoriev ice cap (Kyrgyzstan) collected in several field campaigns during August 2021 (summer) by using GPR technique. Then, the radar data was processed by applying the yield stress method and interpolated to produce an ice thickness layer. Finally, the authors evaluate if the global outputs resulting from 6 different experiments are able to capture the spatial patterns of the ice thickness at the local scale in the Grigoriev ice cap.

The manuscript is well structured, clear, and concise, making it easy to understand. I congratulate the authors because they have compiled a large amount of data with potential for scientific applications, however, they do not give enough detail on the statistical approach for demonstrating the unreliability of the global datasets, and it seems they remain in a visual description of the discrepancies.

The global ice thickness products were conceived as an approximation of the total volume of ice available on the Earth's surface, with its associated uncertainty. It is therefore logical to expect that their site-specific net representation will vary from site to site, depending on morpho-topographic conditions. In addition, several of the world's ice masses are inaccessible for logistical or risk reasons, in which case in-situ observations are simply not feasible. This does not seem to be the case with Grigoriev. Therefore, numerical modelling products can provide valuable complementary data to field measurements.

There is a methodological gap in this study and the authors need to work on major corrections before this manuscript can be published in TC.

We would like to thank the reviewer for the useful comments and suggestions which helped us to improve the quality and clarity of the manuscript. We have also expanded the study with a more thorough comparison with the other ice thickness datasets.

Detailed remarks

[RC2.1] L11. I am not sure how the under-representation of ice thickness in the global dataset demonstrates the importance of in-situ measurements. Please provide more evidence of the specific factors that may render the thickness data obtained by global models deficient, e.g. the role of basal topography. For example, it would be interesting to suggest methodological considerations that would improve model outputs.

We agree and added in the text at lines 16-21:

"The results highlight the limitations of these generic datasets primarily stemming from the subdivision of ice caps into distinct glaciers, the adoption of a (calibrated) creep parameter value, assumptions regarding ice mass flux, and errors regarding surface velocity observations. These shortcomings especially emphasise the importance of integrating local observations to calibrate models to achieve precise representations of ice thickness, particularly when dealing with smaller or slow-flowing cold ice caps, such as the Grigoriev ice cap".

[RC2.2] L38. Grigoriev Ice Cap has a gentle topography, which allows most of the ice cap area to be covered by radar, but not so for other glaciers. Since data are available, please shed some light on the role of mass balance, dynamics and morphology in explaining such discrepancies. Also give the area covered by the ice cap.

We added more information on the role of glaciological variables on the discrepancies in section 4.2. We added the area of the ice cap at line 43.

[RC2.3] L45. I don't think 'range' is the right word, but if it is, then provide a range of mass balance or thermal profile max/min values.

We agree and we modified this into “such as”.

[RC2.4] L74. The error estimate is not clear. Is it 8m or 5%? In the location, in the profile or in the interpolated area? Provide a detailed description of how do you arrive at these values or a reference.

The error estimate of the point measurements concerns 8 m + 5% of the measured ice thickness. We followed the method described in Van Tricht et al. (2021) to arrive at this value. We added a reference to this study.

[RC2.5] L77. Give a description of the photogrammetric process. Did you perform a geodetic adjustment? If so, give the mean error in the horizontal and vertical residuals. How many ground control points did you use? Are GCPs located in the off-glacier areas?

We agree to add this information and added in the text at lines 93-100:

“In addition to the radar measurements, a DJI Phantom 4 RTK drone was used to capture > 1000 images to reconstruct the surface elevation of the ice cap using the photogrammetry workflow in Pix4D (Van Tricht et al., 2021c). During the drone surveys, a total of 42 orange plastic squares of 30x30 cm were strategically distributed as ground control points (GCPs) across the glacier’s surface, and at some exposed bedrock sites near the ice margin. Accurate positions of these GCPs were established using the GPS device and subsequently utilised for georeferencing and validation purposes. The validation yielded a root mean square error (RMSE) of 0.06 m horizontally and 0.09 m vertically indicating a very high accuracy of the 2021 DEM.”

[RC2.6] L78. 0.2 m is the nominal uncertainty of the GPS or has the adjustment error been reached, please clarify

This indeed concerns the nominal uncertainty in the vertical. In the horizontal, an uncertainty of 0.1 m can be reached. We have clarified this in the text at line 93.

[RC2.7] L90. Please show some radar profiles.

We added 7 radar profiles in Figure 2c.

[RC2.8] L111. Instead to interpolate τy why not only compute a bedrock surface model taking advantage of the high resolution data you have.

We added some additional points, using the yield stress approach, to fill up areas where we did not perform measurements. In general, the influence of these additional points is limited because i) their number is small and ii) we have many direct measurements. Not using the additional points would for some areas lead to too low ice thickness values (interpolation errors).

[RC2.9] L128 and L133. Text is repeated

We removed the sentence.

[RC2.10] L136. I do not understand why you stick to visual inspection when you can make a robust statistical comparison of the two spatial datasets. This involves checking the spatial distribution, patterns, and correlations between spatial locations of ice thickness once the data are standardized.

We aimed to keep the analysis for this brief communication concise; however, we acknowledge that the comparison lacked sufficient depth. As a result, we have substantially expanded Section 4.2 and now include the root mean square error (RMSE) and mean error (ME) between our ice thickness measurements and the independent reconstructions from the global models."

[RC2.11] Section 4.2. In view of the comment made by referee 1 about the misunderstanding of the approach used by Milan et al. and Farinotti et al., this comparison should be reviewed and adjusted to obtain a reliable interpretation.

We agree and reviewed/adjusted the analysis for a proper comparison. We now consider the Millan dataset to be valid for 2017-2018 and corrected it for ice thickness changes between 2018 and 2021 to compare it with our reconstruction.

[RC2.12] Conclusions. If data are available, please shed some light on the role of mass balance, dynamics, and morphology in explaining such discrepancies. It could be interesting to identify the reasons for such discrepancies between datasets and to suggest approaches to resolve such discrepancies. For example, I would like to see the authors propose some alternatives for adjusting global products based on local observations or evaluate the representativeness of the global products in terms of their applications for estimating the future evolution of ice masses or runoff in the context of a changing climate.

We agree and we added more in-depth information throughout the manuscript (especially section 4.2) and in the conclusion. We believe this has improved the quality of the manuscript.

Apart from modifying the text in the conclusion, at lines 299-305 we added:

"Moreover, for ice caps, improved ice thickness estimates near ice divides could be achieved by avoiding ice mass subdivision. Additionally, incorporating supplementary information, such as accurate surface ice flow velocities, surface mass balance gradients or a creep parameter adapted to the thermal regime of the considered ice mass, could enhance the reliability of ice thickness estimates, as many methods rely on ice flux estimations. In summary, it thus remains crucial to recognise that the adoption of global ice thickness datasets can have significant implications, especially at the local scale, for projecting future ice volume and the associated evolution of runoff"