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Dear Editor,

My co-author and I resubmit with this letter a previously submitted manuscript (***EGUSPHERE-2022-236*** entitled *'Geothermal heat flux is the dominant source of uncertainty in englacial-temperature-based dating of ice-rise formation')* for consideration by *The Cryosphere*.

First of all, we would like to thank each Reviewer and Editor for their time with this manuscript. In the revised version, we have carefully addressed every single comment provided by each Reviewer. Overall, the new version of the manuscript now addresses more aspects of temperature profile sensitivity (including the choice of vertical velocity approximation and thermal parameters of ice and bedrock). The revised manuscript contains ~9,250 words (including references, methods and figure captions), seven figures and a table in the main text, as well as a supplementary figure.

As requested in the Editor's decision letter from August 16th, we outline how each comment has been addressed in the revised manuscript, point by point. The detailed summary can be found below in this cover letter. It contains description of the changes made in response to the Reviewers' comments, which have been highlighted in green and *italised*. Along with the new version of the manuscript, we also attach a change-tracked version of the manuscript in our resubmission to facilitate the revision evaluation process.

We look forward to your feedback on the revised manuscript and ultimately seeing our work published in *The Cryosphere*. We shall look forward to hearing from you in due course.

Sincerely,

Aleksandr Montelli

Co-author: Jonathan Kingslake

EDITOR

Comments to the author:

Dear A. Montelli and J. Kingslake,

Thank you for submitting your list of planned revisions for your manuscript.
Based on your response, I have the following additional comments:

Please include more information on the parameters used in the Dansgaard-Johnson model and test the sensitivity of your results for different kink heights.
We use Lliboutry approximation for our inversion (now clarified in the text), but we ran further forward modelling sensitivity tests for different Dansgaard-Johnson kink heights (Fig. 6).

I agree that 2D modelling is beyond the scope of this paper.
This comment has been taken into account.

It is fine to keep the results in Figs. 5 and 6 for thicker ice rises but it should be mentioned in the text what the "typical" ice rise thickness is. Similarly for accumulation rate.
To address this comment, we have completed additional data analysis and extracted distribution of accumulation rates, heat flux and ice thickness specifically at over 100 ice-rise locations in Antarctica (histograms are now included in the updated Fig. 1). We also discuss further implications of these data further in the discussion.

Please explore the sensitivity of your results due to the possible range of values for thermal properties.
Sensitivity of our inversion to thermal diffusivity of ice and bedrock has been tested. We ran another synthetic inversion; the extended results are presented in the Supplementary Fig. 1, which in effect represents the extension of Fig. 3 with two more unknown parameters. We also include description of these results in the main text.

Best,
Nanna B. Karlsson, TC Co-editor-in-chief

REVIEWER 1

Review of Montelli and Kingslake: "Geothermal heat flux is the dominant source of uncertainty in englacial-temperature-based dating of ice-rise formation

Montelli and Kingslake use an inverse approach to estimate the uncertainty in determining the age of grounding at ice rises from the englacial temperature profile. They apply this technique to Crary Ice Rise – the primary location where this technique of dating the grounding of the ice shelf has been applied. The authors conclude that the uncertainty in geothermal flux is the primary source of uncertainty; given the uncertainty in geothermal flux, particularly in the Ross region, this is an important conclusion. The technique is similar to previous work using borehole temperature profiles to infer past temperature but is instead oriented towards inferring the timing of grounding in the ice- sheet to ice rise transition. Therefore, the past histories are assumed to be defined, and the modern parameters are assumed to be unknown – essentially a reversal of the assumptions used for in the borehole thermometry work. The manuscripts does a good job of exploring many sources of uncertainty, although quite a few more remain. The work is a useful contribution and improves our understanding of the limitations of ice-rise grounding from englacial temperature profiles.
We thank the Reviewer for their helpful comments throughout the manuscript, addressed below.

The paper is mostly well organized with clear descriptions and illustrations of the model set up. However, the choice of vertical velocity profile is not clear. For instance, what is the Dansgaard-Johnson kink-height being used? And I think a value of "n" of 3 is being used for the Lliboutry approximation, but this is often employed because different choices of "n" allow a wider range of vertical velocities than the D-J approximation (e.g. Kingslake et al., 2014). The shape of the vertical velocity likely can vary a lot for these types of scenarios – an ice shelf should have a nearly linear vertical velocity while an established divide will be highly non-linear, and typically represented with "n" significantly less than 3.
We ran further forward modelling sensitivity tests for different Dansgaard-Johnson kink heights and ice thickness (Fig. 6). The vertical velocity used in inversion was based on Lliboutry approximation, based on phase-sensitive radar results from Kingslake et al. (2014; 2016). We also show that the significance of the choice of vertical velocity approximation decreases with ice thickness (Fig. 6).

The paper also seems primarily focused on Ross-like locations. Most ice rises have accumulation rates much greater than 0.1 m/yr. I think the paper would benefit from specific discussion of how well englacial temperature profiles would constrain timing of grounding in other locations than the Ross and Filchner Ronne – where the accumulation rates are greater. This would provide guidance of where it is promising to obtain englacial temperature profiles and where it is unlikely to be useful. My guess is that higher accumulation rates shorten the timescale that regrounding can be inferred for.
We thank the Reviewer for this useful suggestion. To address this comment, we have completed additional data analysis and extracted the distribution of accumulation rates specifically at ice-rise locations in Antarctica. This provides better context and we included the extracted distribution in

the Fig. 1. We also added information in the last paragraph of the Discussion about the potential for ice rises within the Ronne Ice Shelf and elsewhere to store information about timing of grounding in their temperature structure. We added a description of the effects of higher accumulation in the Section 3.1 of the Results.

The paper should also discuss the impact of horizontal advection on the temperature profile. When can the effects of the upstream flow – starting with thicker ice, colder temperatures, and different accumulation rates (West Antarctica has weird accumulation gradients so it's not obvious that being higher and colder also means less accumulation) – be safely neglected. A few forward runs based on Mercer Ice Stream and Crary would be useful to discuss this potential uncertainty qualitatively. It would be beyond the scope of the paper to include this in the inversion.
As mentioned by the Reviewer and the Editor previously, 2D modelling would be out of the scope of the paper.

I also wonder if the forward modeling in Figures 5 and 6 could be restricted to the thinner ice sites. Are there examples of 2500m thick grounding events (L405 suggests ice rises are general much thinner).
Thick ice sites were included in the discussion to demonstrate wider implications for future temperature profile inversion in non-ice rise areas where ice is thick (e.g., central Greenland and Antarctica). The Reviewer is correct to point out, as far as the authors are aware, there are no known examples of such thick ice divides forming through grounding in the recent past. However, these forward simulations may be relevant to other related questions, such as how well can we reconstruct past temperature from temperature profiles. Therefore, we chose to leave these thicker simulations in the paper.

The paper should also be clear about the thermal properties being used. Regarding Table 1: I'm a bit confused by the values the authors have chosen for ice which I think is because the heat capacity and thermal conductivity values are switched. The heat capacity is chosen as 2.3 J/kg/K yet Cuffey and Paterson (2010) give the range of values between 0 and -50C as 2097 to 1741 J/kg/K. The factor of 1000 not withstanding, the chosen value does not fall within this range. Similarly for the thermal conductivity, the authors give 2000 W/m/K yet the Cuffey and Paterson range is 2.1 to 2.76 for 0 to -50C. It seems like the authors switched the values. The heat capacity of bedrock (assuming that the thermal conductivity of bedrock is actually the heat capacity) seems too low as well.
The mentioned typos have been corrected – the values have been indeed accidentally switched in this table of the submitted manuscript. They remain correct in the code.

Also, the authors should at the very least discuss the uncertainty of these values and the uncertainty of not treating them as temperature-dependent. It seems like this will be an important enough source of uncertainty to include in a paper about the uncertainty of dating ice rise formation with the englacial temperature profile, but maybe it isn't.

Sensitivity of our inversion to thermal diffusivity of ice and bedrock has been tested. We ran another synthetic inversion; the extended results are presented in the Supplementary Fig. 1, which in effect represents the extension of Fig. 3 with two more unknown parameters. We also include description of these results in the main text.

Overall, this manuscript is a valuable contribution that improves inferences of past ice sheet behavior. That the code for the inversions will be made publicly available is an asset that can be applied beyond this specific application (it didn't look the like the github link was active yet, but I wouldn't expect it to be before the paper is accepted).

We thank the Reviewer again for their feedback. The code should be available upon publication of the manuscript.

Minor comments

L294: The example of accumulation is not surprising. Most analyses will use a percent change in accumulation rate rather than a fixed amount. This better reflects the variability in climate.

We chose to change accumulation by a fixed amount to clearly demonstrate the variable net effect (see Fig. 5A)

Figures 3 and 4 – It would be good to label all of the axes even if all of the axes in a column or row are the same.

All axes were labelled where space allowed

Fig. 5 caption is long and very repetitive. It seems like this 5 scenarios text could be shared with only the differences articulated each time.

The caption has been made more concise and clear

Fig. 5 C – check values. Most heat fluxes are given in W m-2, so I think you should stay consistent with that.

Figure edited accordingly for consistency

Update Neuhaus reference

Reference updated

L 388-390 – It might be worth mentioning here that in addition to cooling effects from groundwater, you might also expect heating effects from friction during the regrounding. There is likely to be a very complicated and stressful (pun intended, sorry) time period when grounding occurs.

This additional limitation has been mentioned in the revised version of the manuscript

L412-416 – ApRES vertical velocity constraints were used at Dome C to improve inferences of past temperature change. It's pretty buried in a complicated paper, but it highlights the point being made: Buizert et al., 2021, Science, "Antarctic surface temperature and elevation during the Last Glacial Maximum"

The abovementioned reference was included

L445 – delete "comprehensive" or modify it with "most comprehensive yet

Word 'comprehensive' was removed

REVIEWER 2

Review of Geothermal heat flux is the dominant source of uncertainty in englacial-temperature-based dating of ice-rise formation

Authors present an analysis of contributions of multiple parameters when inferring the timing of the grounding of ice rises using temperature-depth profile measurements as data. The method is applied to synthetic data, and to borehole measurements from the Crary Ice Rise. Furthermore, they perform a detailed sensitivity analysis of their forward model, showing how the changes in different forcings (accumulation rates, heat flux, thinning rate...) influence modelled englacial temperatures. Authors identify the heat flux as the main source of uncertainties as their key result, making this paper important for any future studies focusing on ice rise grounding. The manuscript is clear in its purpose, well written and requires only minor updates.

We thank the Reviewer for their helpful comments throughout the manuscript, addressed below.

Major comments

State the boundary conditions applied in the Methods section. I understand they can be found in the Dahl-Jensen et al. (1999), but it would make it easier for the reader to follow as they are important part of the presented setup.

Boundary conditions are explained in detail in Section 2.1.2. "Spatiotemporal domain and boundary conditions".

Try to separate the forward Monte-Carlo and the inverse MCMC simulations more clearly, mainly in the Results and Discussion sections. Maybe refer to forward Monte-Carlo simulations as sensitivity experiments to avoid confusing the two.

We reworded sentences that mention sensitivity in the inverse MCMC section to make a clearer distinction between this section/method and forward Monte-Carlos sensitivity tests.

In most Figures you write A. B. C. instead of (a), (b), (c) – please recheck with the rules and adjust accordingly.

Numbering for all figures has been changed to the bracketed lower-case format.

Figures 3 and 4 – These two figures are very hard to follow. Either break each into at least two figures or simply put some plots in the Appendix and show us just the ones you discuss in detail. Colorbar is placed a bit strangely. Also, Figures 3 and 4 have different figure captions but show the same type of plots, and some plots are not described in the figure captions. In general, try to make all of your figures and figure captions as clear as possible.

We believe it is a rather important feature of this figure that it shows every parameter combination in its present form. We therefore chose to retain its initial structure; but also introduce a Supplementary Fig. 1, which in effect represents the extension of Fig. 3 with two more unknown parameters. We also checked the captions for clarity.

In Section 3.2 you mention the accumulation change, case of 2 km ice thickness, yields large effect on resultant temperature when accumulation is low. Could you in the same place report what would that change be for a 500 m thick ice?

We ran additional experiments for lower ice thickness - Fig. 5 and text in Section 3.2 have been updated accordingly.

Expand the discussion on the effect of uncertainties in accumulation rates, surface temperatures and other parameters on your results with respect to currently available data over Antarctica. Concerning the sensitivity study (forward Monte Carlo), it would be useful to discuss how well the range of values chosen (accumulation, thickness...) represents what we see in reality. For example, are more ice rises in the range of 500-1000 m ice thickness or thicker than 2000 m. Are accumulation rates over most ice rises in the lower range (0.05 m/yr) of tested values or higher (0.25m/yr)?

We thank the Reviewer for this comment. To address this, we have completed additional data analysis and extracted the distribution of accumulation rates, ice thickness and basal heat flux specifically at ice-rise locations in Antarctica. Histograms showing these distributions are now shown on the Fig. 1(b)-(d). We also ran additional experiments for lower ice thickness - Fig. 5 and text in Section 3.2 have been updated accordingly.

Minor comments

Methods

2.1.1. Model equations – First sentence is a bit off, compared to the rest of the text. I would reword it.

First sentence reworded accordingly

Equations (3) and (4) – ws is not defined

Ws is now defined in equation description.

Section 2.1.3 Do you need the word prior in the title?
The word ‘prior’ was removed from the title of the subsection

Table 1. This table should either be expanded with numerical values of your forward and inverse modelling runs, or another table should be added with those values as well.
The parameters of forward and inverse simulations are described further in the ‘Results’ section

Results

Section 3.1 Inverse modelling has Subsections 3.2.1 Synthetic data and 3.2.2 Crary Ice Rise borehole measurements. It should be Sections 3.1.1 and 3.1.2.
The numbering has been edited accordingly

Line 265 – use proper signs for less than or equal to instead of <= Line 280
The ‘<=‘ signs were changed to ‘≤’ as suggested

There is no Section 3.3, should it be Section 3.2?
Reference to the Section 3.2 was corrected

Results 3.2 Englacial temperature profile sensitivity – maybe separate steady state and transient runs into two subsections to avoid reader confusion
We chose to retain the current structure of the Section 3.2

Figure 5 The figure caption can be shortened here, no need to repeat the same words for each plot. Word timing after E., and geothermal after F. should be capital. Why did you choose the max accumulation example to be 0.25 m/yr and not more? Or is 0.25 m/yr the highest accumulation reported over an ice rise?
The caption has been shortened accordingly. The value of 0.25 m/yr was chosen based on the accumulation rates in the vicinity of Crary Ice Rise.

Discussion

Figure 7A - the w should be capital in x label (Heat flux, mW/m^2).
Corrected as suggested

Line 350 typo, there’s an extra d following “corresponds to ice grounding” GitHub link is not active but that is understandable as the paper is still in review.
Typo corrected

References

Please make sure that all of the references are in the same format. Sometime year is in brackets, sometimes not. Add DOI to more references if possible. Also recheck them for typos, i.e., Millero (1978) and Neuhaus et al. (2020) have typos in the title.

References have been checked for formatting consistency and typos.