> We appreciate the reviewers for investing their time and providing constructive comments on our manuscript. Overall, we revised the manuscript according to their suggestions. Below, we explain the changes we have made and present our reasoning for the suggestions we didn't follow. We hope these revisions are satisfactory and the revised manuscript meets the journal's criteria. Our responses are tabbed and in blue and follow individual comments. The line numbers we refer to are those of the marked manuscript.

RC1: 'Comment on geosphere-2025-2037', Frédéric Parrenin, 08 Sep 2025

Review of "A new coastal ice-core site identified in Dronning Maud Land, Antarctica, for high-resolution climate reconstructions to the Last Glacial Maximum" by Goel et al. This manuscript presents a study of several ice rises in the Dronning Maud Land region, all being accessible from the Indian Maitri station. While the DJU and LEN ice rises are briefly mentioned, the focus is then made on the more promising KAM and VER ice rises. For these two rises, a detailed radar survey has been performed. These radar surveys are used to map the SMB from a shallow horizon and firn cores at the summits. The deeper horizons show Raymond bumps, characteristic of stable ice rises.

A simple 1D age model is then fitted onto observed isochrones dated by a Lliboutry-type 1D model at the flanks where the flow is better known. From this 1D model, a 3D mapping of the age can be done and shows that KAM is the most promising site and should hold LGM ice at an acceptable resolution. I enjoyed reading this manuscript and I think it is an important contribution for glaciology and ice core science.

In my opinion, this manuscript should be accepted after a few minor corrections and improvements.

> It is great to hear that the reviewer enjoyed reading our manuscript and found it worthy of publication. As described below, we revised the manuscript as guided by the reviewer's suggestions.

General comments:

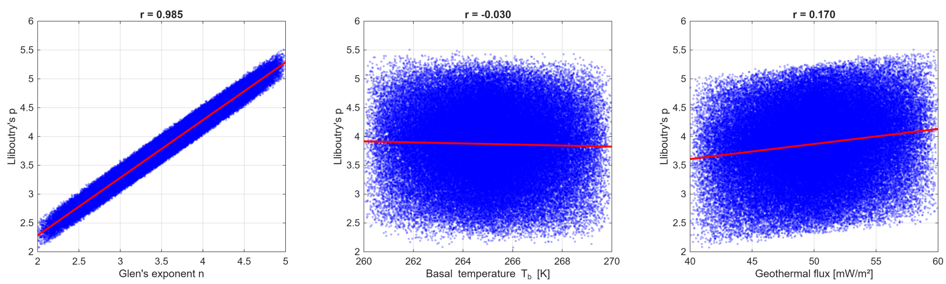
- The age model used is said to be pseudo-steady, but I have the impression that it is just steady.

  The difference between the two just comes from a change of the time variable based on SMB variations (see Parrenin et al., JG, 2006 and Parrenin and Hindmarsh, JG, 2007 for details). Here, the temporal variations of SMB could be taken into account by using, e.g., the EDML SMB temporal variations. This would affect the age and resolution of the deepest layer, close to the LGM, where SMB was probably ~2 times smaller. Numerically, this is really easy to do so I suggest to do it if it has not been done.

  > Thanks! We agree with your assessment. As implemented, our model is in steady-state. We considered using time-varying SMB input but since there are no reasonable continuous SMB records that could be used as an input to such a pseudo-steady state model, we instead tested multiple steady-state scenarios with scaled SMB values (±10% and ±25%). The amount of scaling was based on the few available coastal SMB records. Please refer to section 5.2 para (L327) for these discussions:
  >
  > > *"For our analysis, we use a simplified 1D ice flow model and assume a steady-state mass balance scenario. We use the radar-derived SMB representative of the recent decades and assume that it represents the longer time scale of the study. The longest accumulation record closest to our sites is from the Derwael Ice Rise, further east in DML, spanning 266 years. SMB averages over 30-year intervals vary by about ±17% (standard deviation) from the long-term mean SMB of the ice rise. Longer records from inland DML plateau (cores B31, B32, B33 from Oerter et al., 2000), spanning about 740 years, show ±6% variability for comparable 30-year averages. An even longer 2-ka spanning SMB record exists from the Law Dome ice rise in Wilkes Land, East Antarctica, which shows a variability of ±4% for the 30-year averages. Thus, to test the long-term sensitivity of our results to the single 30-year averaged radar-derived SMB for KAM, we re-run*

> > *the model by scaling the input SMB by ±10 and ±25%. The prior cases are more plausible, while the latter is likely a more extreme case over the target 20 ka period. We find that at the summit location, for lower long-term mean SMB values (-10% and -25%), the 20-ka age ice is found at 71 m and 60 m above the bed. For higher long-term mean SMB values (+10% and +25%), the 20-ka age ice is found at 85 m and 100 m above the bed."*

- The value of the Lliboutry exponent p is taken between 2 and 4. I am not sure where these values come from, so proper references would help. From the original 1979 Lliboutry article, an estimate of p can be done using the Shallow Ice Approximation and an estimate of the temperature gradient at the base. If I remember correctly, the p value for Vostok is more around 8. Not sure what is the temperature gradient at the base here, so the value might be different.

> Thank you for raising this point. You are correct that p values at deep ice core sites like Vostok are considerably higher (~8). However, our sites differ significantly in their glaciological conditions.
>
> Following Lliboutry (1979), p is calculated as:
>
> $$p = n - 1 + k \cdot G \cdot H$$
>
> where $k = Q/(R \cdot T_b^2)$ with Q = 60 kJ/mol , R = 8.31 J/(mol·K) , and T_b is the basal temperature [K]. The temperature gradient $G = q_{geo}/\kappa_{ice}$, where q_{geo} is the geothermal heat flux [W/m^2] and κ_{ice} = 2.1 W/(m·K) is the thermal conductivity of ice. H is the ice thickness.
>
> For our relatively thin ice rise (H ~ 525 m) with a frozen bed, we used geothermal flux values of 40-60 mW/m^2 and estimated basal temperatures of 260-270 K. Under these conditions, the thermal term $k \cdot G \cdot H$ is only slightly greater than 1, resulting in p values close to n (typically 3-4 for ice). In contrast, the thick ice at Vostok (H > 3700 m) with a temperate bed produces much larger thermal terms, hence p ~ 8 as you highlighted.
>
> Using the above stated values and a wide range of n between 2 and 5 we estimated p as shown in the figure below:
>
> 
>
>
> In our study we use a p range of 2-4. Given the expected range of n over ice rises, this range covers the most likely p values and is adequate for assessing sensitivity in our age-depth model, even if not optimally centered. We rephrased the text in the manuscript as (L150):
>
> > *"For thin ice rises with frozen beds, typical p values lie between 2–4 (Lliboutry, 1979). This range is notably lower than deeper ice-core sites like Vostok (p ≈ 8) with significantly thicker ice and warm basal conditions (Parrenin et al., 2007)."*

- From the radargrams, it seems Raymond bumps are surrounded by troughs, at least on one side. Parrenin and Hindmarsh (JG, 2007), showed that horizontal advection of the ice can create these troughs. Not sure it is the correct explanation here, but at least it could be worth mentioning.

> Indeed. We agree these “troughs” (or synclines) observed on both ice rises as you point out, could be a result of horizontal advection effects among other possibilities (localized SMB changes, divide migration). We have now updated the discussion of this syncline in section 5.2 at (L351):
>
> > *"The model can reproduce the IRHs well in the divide region in the modelled profile across KAM’s summit (Fig. 6d). However, on the western flank, the model cannot reproduce the observed stratigraphy well. The observed IRHs here show a syncline feature at the side of the divide. We do not observe any significant asymmetry in the bed topography across the divide that could explain the feature, with similarly smooth bed and similar surface slopes on either side. The observed mismatch on the western flank thus could be a result of a more complex 3D flow. Parrenin and Hindmarsh (2007) demonstrated that spatial variations in velocity profiles, particularly an abrupt transition from dome to flank flow regimes, can generate synclines flanking Raymond arches. A second possibility could be a gradual and systematic change in the spatial distribution of SMB with time, which is possible and has been speculated on other ice rises in the coastal DML region (Goel et al., 2018, 2022). However, both these possibilities act locally and thus should not affect our age-depth estimates near the summit.*
> >
> > *A third possibility could be a rapid divide migration towards the east, resulting in the displacement of the Raymond Arch towards the west (Nereson and Waddington, 2002). However, the observed IRH patterns do not match those predicted by established divide migration models (Martin et al., 2009b), making this scenario less likely to explain our observations."*

- By the way, using a flow tube model like in Parrenin et al. (GMD, in press) and Chung et al. (TC, in press) could be a possible perspective for the modeling exercise to take into account horizontal advection.

> Thank you for this suggestion. A flow tube model approach could indeed be a next step to include more physics while keeping the computation costs low as intended in this work.

Minor comments:

- l. 66: Maybe introduce the “LEN” notation here.

> Added.

- l. 134: Not sure your model is really pseudo-steady, see comment above.

> Updated to “steady state”

- l. 142: “isn’t” -> “is not”

> Corrected.

- l. 144: “p values lie between 2-4” -> see comment above

> Please see our response to the earlier comment.

- section 4.1: I am not sure to understand the comparison of this section. It is said in the beginning that the comparison will be done at 0.2H over the bed. Then the comparison is made at 0.12H for DJU and VER and 0.16H for KAM.

> Apologies for the confusion. We are making two separate comparisons for age and resolution. For age we pick a fixed depth of 0.2H, while for resolution, we pick a fixed age of 20 ka. We have now revised the text to be more clear about this.

> Line 258:
>
> > *"Here, we estimate the age and the resolution of a hypothetical deep ice core if it was drilled at the summits of these three ice rises. For age comparison, we make the comparison at a depth of 0.2H from their beds where the expected resolution is still practicable while the ice is sufficiently old (Fig. 5a)....."*
> > *"For resolution comparison, we compare the expected temporal resolution of retrieved ice from the Last Glacial Maximum (LGM; 20 ka), regardless of the depth at which this age is reached (Fig. 5b). "*

- Figure 2: I would rather use dark colours for troughs and light colours for highs.

  > It is indeed the case in the figure. Since the colorscale is in "Bed elevation (m below WGS84 ellipsoid)', the lighter color indicate shallower regions (highs) and darker colors indicate the troughs. The figure is thus unchanged.

- l. 407: Should not is be Fig. 1a and 1b instead of Fig. 1b and 1c?

  > We want to refer to the maps in the figure1 focusing the individual ice rises. Fig 1a is the regional map, while 1b focuses on KAM and 1c focuses on VER. We have left this caption unchanged.

- Figure 3: The labels of the sub-figures do not seem to correspond to the legend. They are also ordered from top to bottom, which is not consistent with Figure 2.

  > Thank you for pointing this out. We have now revised the figure and the associated references in the manuscript.

- Figure 6d: There is a model-obs discrepancy, which could be due to horizontal advection (see comment above) or to non-steady features such as varying accumulation pattern.

  > Correct. As mentioned in our response to the previous comment, we have discussed this discrepancy in section 5.2 (L351).
