Dear Dr Farinotti,

Thank you very much for your message and for coordinating the review process.

We are pleased that the revised manuscript has been positively received overall. In response to the remaining comment, we have carefully revisited both the implementation and presentation of the quasi-Nye dating approach. We have now revised the method following the formulation of MacGregor et al. (2015), ensuring that the age–depth relationship is correctly expressed. The corresponding sections of the manuscript have been updated accordingly.

We are now confident that the quasi-Nye method has been applied and described correctly, and that it provides a robust sensitivity test of the age–depth structure. We have therefore retained this approach in the manuscript and added an additional figure in Appendix B to explicitly illustrate the distinction between linear interpolation and the quasi-Nye formulation, as well as its relationship to the Dansgaard–Johnsen (1969) age–depth envelope adopted in this study.

We have also addressed each specific comment from Referee #1 on the following pages.

We appreciate the opportunity to revise the manuscript further and look forward to the referee's final assessment.

Best wishes,

Felipe Napoleoni

Referee #1 Co-review team: Wood, Ella (executor) & Young, Tun Jan (supervisor)

We thank the reviewer for their careful assessment of the revised manuscript and for recognising the improvements made in response to the initial review. We appreciate the reviewer's constructive feedback on the original submission, which has helped us to refine the objectives of the study and to clarify the treatment of uncertainties. We are pleased that these revisions have improved the overall clarity and focus of the manuscript.

We addressed each specific comment as follows:

Referee #1:	**Co-review team: Ella Wood (executor) & Tun Jan Young (supervisor)**	
Line	**Comment**	**Response**
L155	There is some repetition here with the inclusion of the additional text, please revise.	The text was reviewed and redundant phrases were removed.
L204	There is some awkward phrasing and singular-plural mismatch in the first half of the sentence—please revise.	The text was reviewed and edited accordingly.
L218-219	Define t and z	All the elements of the equations were defined.
L259	Suggestion to replace "this" with "this quasi-Nye" to make explicit that a different method to the D-J one is being used.	The suggested sentence was included to avoid any confusion.
Figure 6	Revealed a significant and ongoing mass loss ?	We agree that the observed drawdown of the IRHs above SLC may be indicative of basal melting. However, we note that these horizons are also influenced by the geometry of the deep trough in which they are located, and therefore the observed configuration cannot be unambiguously attributed to basal melting alone. Additional investigation would be required to robustly distinguish between topographic control and basal mass loss. We nonetheless revised the figure caption to acknowledge this possible interpretation by adding the following statement: "The geometry of the englacial layers could be indicative of basal melting above SLC."

	If the lake surface was identified by the radargram bed echo (as a bright and flat reflector) and what is termed the “subglacial interface” (this term may need to be modified as what is below the subglacial lake is not actually the subglacial interface anymore) is delineated through BEDMAP3, is the lake simply calculated by the difference between these depths? Perhaps best to have the blue be translucent instead of opaque to show this calculation if so.	We conducted a reflection seismic survey in 2016 to directly constrain both the depth of the subglacial lake and the sediment thickness beneath it (the relevant reference, Brisbourne et al., 2023, Journal of Glaciology, was included). We therefore did not rely on BEDMAP3 to delineate the lake, as its representation in this region lacks the resolution and accuracy required for this purpose. Instead, we used the subglacial DEM presented in Napoleoni et al. (2020), which incorporates higher-resolution radar data and allows for a more robust interpretation of the ice–bed–water interfaces. Accordingly, the lake geometry is not derived from a simple subtraction between two surfaces. Instead, it is constrained by independent seismic observations and by measurements of reflection strength at both the ice base and the lakebed Brisbourne et al. (2023). While the vertical scale in the figure is schematic and not intended to represent exact lake depth, it was designed to remain as realistic as possible. We suggest that the key point here is not the precise depth of the subglacial lake—which has already been rigorously quantified in Brisbourne et al. (2023)—but the robust identification of the lake itself. In particular, the seismic evidence demonstrates that this feature is not a thin film of water or a speculative interpretation based on DEM differences, but a well-defined subglacial lake with measurable thickness.

L502	Suggestion to be more specific with regard to the “prominent West Antarctic reflector”. Would this potentially be the same reflector as identified in Ashmore et al. (2020)? Though this reflector is prominent in Jacobel and Welch (2005), it was not so prominent in Ashmore et al. (2020) which perhaps warrants further mention (though this could be easily attributed to the Ashmore study being airborne).	We thank the reviewer for this helpful suggestion. The “prominent West Antarctic reflector” referred to here corresponds to the horizon originally identified by Jacobel and Welch (2005) at approximately 17.4 ka. While this reflector is widely recognised across parts of West Antarctica, we agree that its expression is not uniform across datasets. In particular, as noted by the reviewer, this reflector is less clearly expressed in the airborne radar data of Ashmore et al. (2020). This difference likely reflects variations in radar system characteristics and acquisition geometry, including frequency content, signal-to-noise ratio, and the reduced sensitivity of airborne systems to fine-scale dielectric contrasts compared to ground-based surveys. Consequently, the apparent prominence of this reflector may vary between studies without implying a difference in its stratigraphic significance. We revised the manuscript to clarify this point and to avoid implying a direct one-to-one correspondence with the reflector identified in Ashmore et al. (2020), while acknowledging that the age range of our deepest IRHs is consistent with the timing of this well-known stratigraphic horizon.
N/A	You use a combination of Internal Reflecting Horizons and Internal Reflection Horizons throughout the paper for IRH, the former is used in the list of acronyms. Stick to one for consistency.	We thank the reviewer for noting this inconsistency. We reviewed the manuscript carefully to ensure consistent terminology throughout and adopted a single definition for IRH. Specifically, we used “Internal Reflection Horizons” consistently in the text, figures, and list of acronyms.

Referee #2 Joseph MacGregor.

We thank the reviewer for their careful reading of the revised manuscript and for highlighting this important issue. We sincerely apologise for the confusion caused by our previous description and implementation of the quasi-Nye method.

In the prior revision, we incorrectly described the approach as a linear interpolation in transformed space, which does not accurately reflect the quasi-Nye formulation presented in MacGregor et al. (2015). Following the reviewer's comment, we have now revisited both the theoretical framework and its implementation in detail.

In the revised manuscript, we have implemented the quasi-Nye method correctly by assuming a constant effective vertical strain rate within the interval bounded by dated internal reflecting horizons. Rather than performing a linear interpolation, we now solve for the effective strain rate and effective ice thickness by minimising the misfit between modelled and observed depths of the bounding horizons, ensuring that the model reproduces their known ages. Ages for intermediate layers are then derived from the resulting non-linear age–depth relationship.

We have updated the Methods section accordingly to clearly describe this formulation, including the governing equation and parameter estimation procedure, and have removed the previous linear interpolation description. The revised implementation follows the approach outlined in MacGregor et al. (2015) and is now physically consistent with the assumptions of quasi-Nye dating.

Importantly, while the updated method is now correctly formulated, the resulting ages differ only modestly from those obtained previously. This confirms the reviewer's suggestion that the overall conclusions are not strongly sensitive to this correction; however, we agree that the correct implementation is essential for methodological rigour and clarity.

We are grateful to the reviewer for insisting on this point, which has led to a substantial improvement in the accuracy and transparency of our analysis.