Review: Napoleoni et al. Radiostratigraphy and surface accumulation history of the Amundsen-Weddell Ice Divide, West Antarctica (Manuscript #: egusphere-2025-4670)

We thank the reviewer for their careful and constructive assessment of the manuscript. Detailed responses to each specific comment (italic and highlighted in grey) are provided below (non-highlighted text).

Summary and general comments

This MS describes the mapping and interpretation of newly traced radiostratigraphy from two CECs ground-based surveys across an unusually important and complex portion of the Amundsen-Weddell ice divide. I wasn't familiar with this survey but based on its location and breadth it's arguably one of the most important Antarctic ground-based radar surveys performed to date. The methods to map and interpret the 7 traced layers are close to the state-of-the-art. The interpretation in terms of accumulation rates is straightforward but very well contextualized and its relevance is fairly described. This MS could be published virtually as is, but I think there are a couple of substantive opportunities for improvement.

I am satisfied with how the layers were traced, the QC thereof given the crossovers (Figure 8), and the matching to the already dated layers (IRH 4/6). What it took me a little while to understand in the MS, and what concerns me slightly, is the use of the Dansgaard-Johnsen model and accumulation rate measurements/models to both loosely verify the dating of those two layers and to then also date the others.

First, the range of basal shear layer thicknesses seems too narrow, given the range of ice flow conditions covered by the survey. Compare, for example, the 20-30% range used to that considered by Waddington et al. (2005, 10.1130/G21165.1) for the Siple Dome ice divide region: 25–70%, and h/H is known to vary a lot across Greenland (MacGregor et al., 2016, 10.1002/2015jf003803). I understand that Bodart 2021 used that narrow range (20–30%), but that precedent alone does not seem sufficient to justify its use here.

We thank the reviewer for this thoughtful and important comment. We agree that the basal shear layer thickness (h/H) can vary substantially across ice sheets and between flow regimes, as demonstrated by studies in both Greenland (e.g. MacGregor et al., 2016) and Antarctica (e.g. Waddington et al., 2005).

In this study, the choice to explore a basal shear layer thickness range of 20–30% of the ice column was motivated by two considerations. First, our age–depth modelling is intentionally restricted to a single, carefully selected location near the Amundsen–Weddell Ice Divide that best satisfies the assumptions of the Dansgaard–Johnsen model: low horizontal velocities (~ 5 m yr^{-1}), a locally flat bed, and well-preserved, laterally continuous stratigraphy. In such divide-proximal, slow-flowing settings, previous work in West Antarctica has consistently

adopted comparatively thin basal shear layers (e.g. Siegert and Payne, 2004; Ashmore et al., 2020; Bodart et al., 2021).

Second, our primary objective is not to infer spatial variability in basal deformation across the survey area, but to ensure methodological consistency when comparing IRH-derived ages and accumulation rates with adjacent radiostratigraphic datasets that use the same modelling framework and parameterisation. Expanding the range of h/H to values representative of faster-flowing or shear-margin environments would reduce the comparability of our results with those studies, without being physically appropriate for the specific modelling site chosen here.

Importantly, we explicitly treat basal shear layer thickness as a source of uncertainty rather than a fixed parameter. Sensitivity tests show that varying h/H between 20% and 30% produces less than a ~10% change in modelled ages for IRHs shallower than ~70% of ice thickness, which includes all horizons used to constrain accumulation rates. We have clarified this point in Section 2.3.2 and Section 2.4.3, and now emphasise that uncertainties in accumulation rate dominate the age envelope relative to uncertainties in h/H.

To address the reviewer's concern more explicitly, we will revise the manuscript to (i) clarify that the selected h/H range is appropriate only for the divide-proximal modelling site, (ii) acknowledge that wider ranges may be required in faster-flowing settings, and (iii) state that our conclusions do not depend sensitively on the chosen basal shear layer thickness.

Second, this seems to be a real missed opportunity to use an existing method, quasi-Nye dating (MacGregor et al., 2015), to potentially better date the undated layers in a manner independent of the somewhat stronger assumptions imposed by the Dansgaard-Johnsen model. In quasi-Nye dating, the core assumption is that the vertical strain rate in the sandwich between or near a layer pair is uniform, i.e., it is not tied to the bed directly or indirectly. Given the layer depths under consideration relative to the assumed relative basal shear layer thicknesses (Figure 4), this seems like a satisfactory assumption. I would have started from the two independently dated layers and made my way through the other five that way. The code is out there; while set up for a more complex situation, it could be readily simplified to this interesting survey: https://github.com/joemacgregor/pickgui/tree/master/dating. The substantial benefit of this proposed approach is that the (hopefully) independently dated set of seven layers could then themselves be used to estimate past accumulation rates across the survey region, using e.g. the Dansgaard-Johnsen model, rather than only being able to "calibrate" accumulation rates using Dansgaard-Johnsen using the two independently dated layers.

We thank the reviewer for this insightful and constructive suggestion. We agree that quasi-Nye dating (MacGregor et al., 2015) provides a valuable alternative framework for propagating ages between independently dated internal reflecting horizons (IRHs), relying on weaker

assumptions than the Dansgaard–Johnsen (D–J) model by decoupling age propagation from basal conditions. Given the relative depths of the traced IRHs and their position well above the basal shear layer (Figure 4), the quasi-Nye assumption of a locally uniform vertical strain rate between adjacent layers is physically reasonable in this setting.

In the initial design of this study, our primary objective was to establish a consistent regional radiostratigraphic framework across the Amundsen–Weddell Ice Divide that could be directly compared with neighbouring datasets from Pine Island Glacier and the Institute Ice Stream (e.g. Ashmore et al., 2020; Bodart et al., 2021). For this reason, we adopted the D–J model as the primary age–depth framework, as it has been widely used in adjacent regions and allows direct comparison of inferred accumulation histories within a common methodological context.

Importantly, sensitivity tests presented in the manuscript already indicate that, for the depth range occupied by the IRHs, age estimates are only weakly sensitive to basal shear layer thickness, implying near-uniform vertical strain rates across this portion of the ice column. Under these conditions, differences between D–J and quasi-Nye age propagation are expected to be modest and largely predictable, affecting uncertainty bounds more than central age estimates.

Nevertheless, in response to the reviewer's comment, we will explicitly apply a quasi-Nye age propagation using the observed IRH depths at the modelling trace and the two independently dated horizons (IRH4 at 4.72 ± 0.28 kyr and IRH6 at 6.94 ± 0.33 kyr) as anchors.

We will add a short description of the quasi-Nye analysis and the resulting ages to the manuscript to demonstrate the consistency between the two approaches. This comparison will strength the robustness of our chronology while retaining the D–J framework as the primary method to ensure continuity with neighbouring radiostratigraphic studies.

Line	Comment	Response
L14-17	This last sentence of the abstract is a concise statement about the potential value of AntArchitecture, but it doesn't connect effectively to the work done in this study. I suggest revising to better focus on relevant future outcomes from this study.	We thank the reviewer for this helpful comment. We agree that the original wording of the final sentence in the abstract was too general and did not sufficiently emphasise the specific contributions of this study. We will changed the original sentence to: "These results extend dated radiostratigraphy into a previously unresolved sector of West Antarctica, supporting AntArchitecture goals by strengthening continent-scale IRH connectivity, improving constraints on Holocene accumulation variability, and providing new observational benchmarks for ice-sheet models."
Figure 1	1. In panel A, the red box and ice divides should be bolder. 2. In panel B, add a legend to define the white star rather than resorting to the end of the caption. 3. Label all panels with a summary of what the main field they are showing is, e.g., A. Regional map; B. Surface elevation, etc.	We thank the reviewer for these helpful suggestions to improve the clarity and readability of Figure 1. In response, we will (1) increased the line weight of the red bounding box and the ice divide traces in panel A to enhance their visibility; (2) add an explicit legend in panel B to define the white star; and (3) update all panel labels to include a brief descriptive summary of the primary field shown (e.g. "A. Regional map", "B. Surface elevation"). We believe these changes will significantly improve the interpretability of the figure.

| L206-9 | The rationale for using the accumulation-rate field from Arthern et al. (2006) needs to be improved substantially to justify its use in this study. That field is coarser than RACMO and nearly two decades old in a field of study that has evolved substantially since then. I don't think comparison with Arthern is adding much to the MS. | We thank the reviewer for raising this important point. The accumulation-rate field from Arthern et al. (2006) was deliberately included to assess the sensitivity of the inferred IRH depths to differences between earlier, widely used accumulation products and more recent regional climate model outputs.Our intention is not to argue that the Arthern et al. field is superior to RACMO2.3p2, but rather to evidence how the choice of accumulation-rate product - particularly one that has been commonly used in previous Antarctic radiostratigraphic studies - influences depth and age estimates. This comparison therefore provides context for the robustness of our results with respect to accumulation uncertainty. To make this rationale clearer, we will revise the text in the Methods section to explicitly state that the Arthern et al. (2006) field is used as a sensitivity test rather than as a preferred accumulation product, and we will clarify the limited interpretative role of this comparison in the manuscript. |

L232-4	A lot of work has now been done by the altimetry community to improve estimates of firn layer variability across both ice sheets. It would be good to take advantage of this to improve the estimate of the firn correction and its spatial variation, given that the rest of the methods used to estimate layer depths are at or close to state-of-the-art. For Greenland radiostratigraphy v2 (MacGregor et al., 2025, 10.5194/essd-17-2911-2025), I used the firn air thickness estimates from Medley et al. (2022, 10.5194/tc-16-3971-2022). Something similar can be done for Antarctica layer depths. This probably won't change the layer depths and their uncertainties much, but it is a process improvement that is relatively straightforward to implement.	We appreciate this constructive suggestion. We agree that recent firn air content and firn thickness products derived from altimetry represent a methodological improvement and are increasingly becoming state-of-the-art.In this study, however, our primary objective is to ensure methodological consistency when comparing our IRH depth and age estimates with two previously published datasets that apply the same firn-correction approach described in the manuscript. For this reason, we will retain the original firn correction in the main analysis to preserve comparability.That said, following the reviewer's recommendation, we will recalculate IRH ages using a more recent firn correction product. These results will not be used in the primary comparison, but they will be included as a reference for future work in this region. We will clarify this choice and its motivation in the Methods section (Lines 128–132).
Figure 6	In panel B, are the blue fills the *observed* depths of those subglacial lakes, or simply illustrated? If the latter, only a blue line seems appropriate. 2. The caption statements about layer drawdown over the lakes require more support to be convincing (they also don't really belong there anyway), because layers often dip down in steep troughs, independent of the nature of the bed there.	We thank the reviewer for these helpful comments. In the original version of Figure 6, the blue-filled polygons represented estimated lake geometries derived from seismic observations (Brisbourne et al., 2021), rather than directly observed radar depths. To avoid ambiguity, we will revise the figure so that only a blue surface line is shown. We also agree that the statement regarding layer drawdown in the caption was insufficiently supported and could lead to overinterpretation, particularly given that similar layer geometries may arise from bed topography alone. We will therefore remove this statement from the caption in the revised manuscript.
L22	a-1 -> yr^{–1}.	Thank you for detecting this oversight. This will be changed.

L48-50	This sentence perhaps inadvertently implies that radiostratigraphic calibration of ice sheet models had *already* reduced uncertainty in future projections, which I don't believe that it has. Suggest rephrasing.	We appreciate this perspective and agree that the original wording could be interpreted as implying that radiostratigraphic calibration has already reduced uncertainty in future projections. We will therefore rephrase the sentence to clarify that radiostratigraphy offers the potential to improve model calibration and reduce uncertainty, rather than representing an outcome that has already been achieved.
L326	What are the "subglacial gradients" referred to here? A confusing turn of phrase.	We thank the reviewer for pointing out this ambiguity. By "subglacial gradients" we refer specifically to steep gradients in subglacial bed topography. We will revise the sentence accordingly to clarify this and avoid confusion.
L467-470	I suggest reversing the order of this sentence, as it seems to unintentionally imply that the model is more reliable than the observations.	We thank the reviewer for this suggestion and agree that the original sentence ordering could be interpreted as implying that the model is more reliable than the observations. We will reorder the sentence to clearly foreground the observational constraints and to present the model as a supporting interpretative framework.