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

We very much appreciate the insightful comments from reviewers (italic and highlighted in grey) and for their constructive and helpful reviews of our manuscript. Below we respond (non-highlighted text) to the comments.

Summary and general comments

This manuscript extends the age-depth radiostratigraphy of the Amundsen-Weddell Ice Divide through the incorporation of 2000 km long of Centro de Estudios Cientificos (CECs) ground-based RES survey. In this dataset, seven internal reflection horizons dating to the last ~17.6 kyr were traced of which two were linked to wider mapped radiostratigraphy datasets in this region.

Overall, this paper is well written and clearly structured with nicely presented figures showing interesting findings on the accumulation history of the Amundsen-Weddell Ice Divide. We have minor comments to improve the clarity of certain points and emphasise and contextualise the significance of the findings.

General comments

1. In general, the contextual scope of the manuscript is currently positioned between a paper describing a dataset and an analysis of the larger flow stability of the region—these were defined in the study aims (L74-75). Though the former aim was clearly accomplished and detailed, the latter aim was only discussed in a broad scope in Section 4.2, with regards to the overall lack of disturbance of ice flow in the wider region of study. We suggest redefining the secondary aim to be more focused and reframing Section 4.2 in terms of tying in where this study extends in interpretation from the previous work done in the region.

Given the revised (narrower and more targeted) scope, we will change L73-75 to be more focused as it follows:

"By tracing and correlating Internal Reflection Horizons (IRHs) across the Ellsworth Subglacial Highlands, we link existing radiostratigraphies from the Amundsen and Weddell Sea sectors and extend dated stratigraphy into a region that was previously poorly constrained. Our specific objectives are to: (1) characterise the geometry, continuity, and age of IRHs across the divide; and (2) use these dated IRHs to reconstruct spatial variations in Holocene accumulation and assess their implications for local divide-proximal ice-flow stability, thereby extending previous regional interpretations."

Similarly, we will reframe Section "4.2 Ice dynamics" to emphasise interpretation derived specifically from our new stratigraphy and accumulation reconstructions. This section will now be "4.2 Constraints on Divide-Proximal Ice-Flow Stability".

2. Though the statement of a stable ice divide is indeed evidenced across multiple past studies (an addition to the list would be Conway and Rasmussen 2009 for WAIS Divide), there is little mention of how this study extends / expands beyond the understanding of complex flow dynamics present in certain sections of the regions, particularly at the boundaries of the Institute-Moller ice streams. Here, there will be benefits in contextualising the study and positing interpretations where appropriate in relation to previous work done in the region (in particular Siegert et al. 2019, Ross et al. 2020). Potentially, addressing this point will also in part address comment (a).

We appreciate the scope of this comment, and we agree on the benefits of contextualising the study. We will modify the section as it follows:

"Our findings are consistent with previous evidence that the inland Amundsen–Weddell divide has remained comparatively stable throughout the Holocene (e.g., Conway and Rasmussen, 2009; Bentley et al., 2010; Ross et al., 2011; Hein et al., 2016b; Siegert et al., 2019; Small et al., 2025). The expanded spatial coverage provided by our new radiostratigraphy allows this interpretation to be refined by identifying where regional stability coexists with localised dynamical complexity. Reduced ILCI values and deformation of deeper reflections along the Institute–Möller system and within the CECs and Ellsworth troughs (Figs. 5 and 7c) indicate enhanced strain and tributary interaction linked to complex basal topography, consistent with previous inferences of dynamically variable flow in these onset regions (e.g., Siegert et al., 2019; Ross et al., 2020). These low-continuity zones coincide with tributary convergence and topographic steering, where inflow into confined troughs promotes shear and reduced preservation of coherent stratigraphy. Importantly, this disturbance remains spatially restricted and does not extend far upstream toward the divide, where layering remains well preserved."

3. For someone less familiar with the region, the use of the CECs abbreviation was at times confusing in that it was sometimes referring to the region and sometimes to the source of your radar data. It would be beneficial to add a sentence early on to clarify these differences. Occasionally the final S in CECS is also capitalised (line 84, figure 1 caption), I assume this is a typo. The reference to the Subglacial Lake CECs (SLC) was slightly confusing, consider adding a short statement to clarify that the lake has been previously identified and also why it is referred to as CECs.

We apologise for this misunderstanding and the laxity regarding the capital S in the word CECs. We will include two short statements to clarify this confusion, and we will amend the word CECs.

Introduction (L64): "The ground-based RES data used in this study were acquired by the Centro de Estudios Científicos (CECs) during campaigns in 2006 and 2014. Throughout this paper, "CECs" refers to the institution, and as "CECs surveys" to the associated radar datasets. We note that two geographic features within our study region, the CECs Trough (Napoleoni et al., 2020) and Subglacial Lake CECs (SLC; Rivera et al., 2016), are also named after the institution; to avoid confusion, we explicitly refer to these by their full geographic names."

4. Figure 8 is only very briefly mentioned in the text, perhaps this figure doesn't need to be in the main text body or could be discussed more detail.

The figure will be move into Annex B.

5. Paragraph (L385) here you talk about high and low accumulation scenarios. This is not previously mentioned, please provide further information to clarify what is meant by this and why contrasting high / low scenarios are relevant here.

Thank you for bringing this to our attention. We will add two short statements. The first statement will be in the 2.3.2 Age-depth modelling section (L179): "To account for uncertainty in past accumulation, we consider a range of plausible accumulation scenarios derived from Noel et al. (2023), Arthern et al. (2006), and snow stake rates. We refer to these as "low-accumulation" and "high-accumulation" scenarios, representing the lower and upper bounds of the accumulation estimates (Tables A1, A2 and A3). These bounds are used to explore the sensitivity of age–depth relationships to accumulation variability."

The second statement will be included in 3.3 Estimated and modelled ages for IRHs section (L381): "To bracket the ages of IRHs lacking independent tie-points, we used a range of accumulation-rate estimates derived from snow stakes and regional modelling approaches. The contrasting "high" and "low" accumulation scenarios correspond to the upper and lower bounds of accumulation estimates from Noel et al. (2023), Arthern et al. (2006), and stake-derived rates (Table 1). These scenarios are relevant because, within the D–J framework, accumulation rate exerts a strong control on layer residence time and therefore has a first-order influence on the resulting age–depth structure, particularly at greater depth".

6. Table 3 showcases significantly larger minimum / maximum bounds for Area 3, especially in comparison with bounds of corresponding IRHs in the other two Areas. Although it is understandable that the lowest IRH (7) will have substantially large bounds, there is scope to contextualise why these errors are so much larger in the discussion.

We agree there is scope to discuss this further. We will add a short complementary text addressing these errors in 4.1 Radiostratigraphy and chronology section(L434)

"Within the D-J framework, accumulation rate exerts a first-order control on downward advection and vertical thinning. Lower prescribed accumulation results in slower descent of layers, longer residence times at a given depth, and consequently substantially older inferred ages relative to higher-accumulation scenarios. This effect becomes increasingly pronounced at depth, where vertical velocities diminish and the age–depth relationship becomes strongly non-linear. As a result, uncertainties in prescribed accumulation rate and basal shear-layer thickness accumulate over millennial timescales, amplifying curvature in the age–depth profile and increasing the sensitivity of the deepest englacial layers to small perturbations in model parameters."

Specific comments

Line	Comment	Response
L7	Satellite derived values	We will change it to "values"
L13	New linkages or a new linkage	We will change it to "linkages"
L19	Revealed significant and ongoing mass loss - no a?	I have checked the spelling and everything seems OK
L22	“a-1” à a-1”	We will change it to "a-1”
L23	Geological records indicating	We will change it to "indicating"
Figure 1	Suggest visualising the locations of the three sites of study (potentially here or in Figure 5): CECs Trough, (Ellsworth?) Subglacial Highlands and Alpine Terrain, and Ellsworth Trough. These sites are included in Figure 2 but it is difficult to georeference these locations alongside physical features, in particular the Ellsworth Trough which is not actually pinpointed.	Figure 1(c) already contains the topographical features referred in the document
L129	“internal layer continuity index”	this will be changed to Internal Layer Continuity Index
L132	“as a consequence of—for example—topography…”	commas will be changed to dashes

L136-7	Quantitatively constrain what defines the "upper part" and the "lower end" of the ice column—from Eq. 1 this is defined as the surface to 200 m and 400m to the bed, but then Figure 3 suggests the lower bounds to be much deeper. Regardless—these definitions need to be much more explicit and clear.	We thank the reviewer for highlighting the ambiguity in our definitions of the "upper" and "lower" parts of the ice column. We will clarify that these terms refer exclusively to the depth intervals excluded in the calculation of the Internal Layer Continuity Index (ILCI), as defined in Eq. (1). Specifically, we will clarify that the upper excluded interval (**U**) corresponds to the upper 200 m below the ice surface; whilst the lower excluded interval (**L**) corresponds to the basal 400 m above the ice–bed interface (i.e., the lowest 400 m of the ice column). Thus, **L** is referenced to the bed rather than to the surface. This will clarify that therefore, the lower bound of the analysed interval occurs at different absolute depths below the surface depending on local ice thickness, which explains the apparent discrepancy noted in Figure 3. In addition, we will emphasise that these depth thresholds are used solely for the ILCI analysis and should not be confused with the basal shear layer thickness used in the Dansgaard–Johnsen (D–J) age–depth modelling, which is parameterised independently as 20–30% of the total ice thickness measured upward from the bed. The manuscript will be revised to make these definitions explicit and to clearly distinguish between ILCI depth exclusions and the D–J basal shear layer. .
L184	Suggestion to elaborate on the constraints in which Nye-style modelling was found to yield comparable results to D-J modelling. Such conditions are implicitly given in L192-193 but could be made much more explicit.	We thank the reviewer for this helpful suggestion. We will made the relevant constraints explicit in the revised manuscript, clarifying that the agreement between Nye-style and D–J modelling applies in settings characterised by slow and steady flow, limited lateral variability in accumulation, and smoothly varying vertical strain rates, such as central East Antarctica. Furthermore, we will included in the supplementary data the age calculation of the IRH using a simplified version of quasi-Nye model (MacGregor et al., 2015)
L192-202	This paragraph seems to repeat several points multiple times—it could be condensed to be much more precise and succinct.	Thank you for this constructive comment. We will revise the paragraph to remove repetition and present the rationale more succinctly. The updated text will now focus on the key criteria for site selection to clearly explains the balance between flat bed conditions and minimising ice velocity.
L248-251	A remnant clause still exists in this paragraph that will need to be removed.	Thank you for noticing this. We will delete the remnant.

L263	Equation reference to be pointed to Eq. 7 (I think, but am not sure). Potentially some additional explanation needs to be provided as to exactly how this specific uncertainty component has been defined and propagated.	We apologise for this mistake. The equation we were referring to is number 7. We will clarify how this uncertainty component is defined and propagated in the revised text. We will also explicitly state that the independently estimated IRH depth-picking uncertainty is propagated using the local age–depth gradient, yielding a formally defined additional age-uncertainty term (δt_depth) analogous to Eq. (P7).
L303	Suggest also providing additional offsets between IRH4 and H2, or if H2 and R2 are equivalent, then make this explicit.	We will explicitly report the additional offsets between IRH4–H2 and IRH6–H3 and clarify the equivalence between the corresponding radar horizons in the revised manuscript (L303). We will also emphasise that these offsets remain within previously reported vertical uncertainty bounds.
Figure 7	Suggest to mark IRHs on this figure. Figure caption mentioned a red arrow in panel f that doesn't appear in the figure.	We thank the reviewers for this suggestion. The missing red arrow in panel f has now been added and is clearly visible in the revised figure. With respect to marking the IRHs directly on Figure 7, we carefully considered this recommendation. However, Figure 7 is intended to illustrate spatial patterns of internal structure and disruption at a regional scale, rather than to highlight specific reflector geometries. Superimposing all traced IRHs would substantially increase visual clutter and reduce readability, particularly in panels where layering is already complex. The IRHs are explicitly shown and labelled in Figure 3, where their geometry and continuity are the primary focus. To maintain clarity and avoid redundancy, we therefore retain Figure 7 as a structural context figure without additional IRH overlays.
Table 3	Could include headings for much better signposting to indicate the upper and lower bounds for each IRH and area. Are the alpha values representing snow-equivalent or (as referenced in L402) ice-equivalent depth-rate estimates? For L402, is this then the midpoint between the bracketed alpha values shown in Table 3?	Table 3 will be reformatted to explicitly distinguish lower and upper age bounds (Age_min and Age_max) and to clarify that accumulation rates are ice equivalent.

L403-4	I am unsure as to how matching IRH4 to the equivalent age-depth reflector in Bodart et al. (2021) will produce accumulation rates—would this not be determined independently through the D-J model?	Thank you for raising this point. We will clarify the wording to make explicit that the age of IRH4 is taken from the Bodart et al. (2021) chronology, and this age is then used within a rearranged D–J framework to solve for accumulation rate at the observed depth. The revised text will explain this procedure more clearly.
L427	"At least two" As far as I am aware, only two reflectors (IRH4 and IRH6) were discussed, so I am not sure there were more than two that were actually of note.	Thank you for this observation. We agree and we will amend the text to state explicitly that two reflectors (IRH4 and IRH6) are confidently correlated with previously dated horizons, removing any implication that additional IRHs were involved
L473	Typo Arthem et al. (2006)	the reference will be correctly cited
L476	Statement is quite vague "rates vary over short distances". Can you quantify magnitude and distance in some way to make this statement more explicit.	We agree that the original statement was too vague. We will revise the text to quantify both the magnitude and spatial scale of accumulation variability based on the empirical semivariograms of snow stakes and RACMO2.3p2. Specifically, we will state that snow-stake accumulation variability is characterised by correlation lengths of approximately 15–20 km and point to point differences of ~0.03–0.04 m ice-eq. yr^{-1}, whereas RACMO exhibits much smoother variability over longer length scales.
L486	Consider including reference for broader WA climate trends.	We will add references to broader West Antarctic climate and accumulation trends and expand the text to place our findings in this wider regional context, citing Fudge et al. (2016) and Bodart et al. (2023).