Preprint egusphere-2025-5467: Assessing the potential for an ice core in the southern Antarctic Peninsula to elucidate Holocene climate history

We thank referees Frédéric Parrenin and Peter Neff, community-commenter Michael Sigl and editor TJ Fudge for valuable and insightful comments. This letter explains how we have revised the paper in response.

The largest change we have implemented in the revision is the addition of a new section (2.4.4 Constraining thinning history at ABW) which introduces the new age-depth model we use to investigate the history of ice thickness more quantitatively than in the previous edition of the manuscript. In writing this section, this created additional results (Figure 4 and 5), and extra discussion points. We hope, however, that the addition of this model adds extra clarity to where we had previously ignored ice thinning at ABW.

The second largest change is where we have updated IRH age assignment based on suggestions by Michael Sigl. For clarity, we add Table 2 to show how the new ages in the revised manuscript relate to ages used in previous studies for the same IRHs in other regions of West Antarctica.

Other revisions have been of a minor nature, and are detailed below responses to each referee.

Here, we provide two versions of the manuscript, one with tracked changes from the previously submitted manuscript, and a clean version. In our responses below, the line and section numbers in our responses refer to the tracked changes document.

Response to Referee #1 (Frédéric Parrenin)

Review of "Assessing the potential for an ice core in the southern Antarctic Peninsula to elucidate Holocene climate history" by H. Davis et al.

This article investigates the potential for an ice core in the Northern Ellsworth Land, at a triple ice divide point between the Amundsen, Bellinghausen and Weddell seas (so called ABW site, 1,200 m ice thickness). This is done with a conjunction of age modeling, radar observations and shallow ice coring. Two IRHs could be traced down to ABW, dated 2.62 and 4.72 ka. Another IRH dated at 6.94 ka could be traced elsewhere in this region but not down to ABW. The model of Martín et al. (2015) was used to evaluate the age-depth relationship at ABW and elsewhere along the radar profiles. It is a 1D model with a steady velocity profile but with a transient surface accumulation forcing. The model is actually able to invert the surface accumulation rate needed to fit some age markers. The accumulation is therefore inverted at ABW for the last ~5 ka with a linear by parts assumptions (there are actually two segments) and before that, it is forced with the Wais Divide scenario. It is found a very strong decrease in accumulation since ~5 ka ago, but the authors also suggest a possible ~600 m Holocene ice thinning explaining these age observations. The maximum age of the ABW profile is also evaluated depending on various estimates of the basal melt rate. It is found that the ABW record probably extends back to at least the onset of the Holocene and possibly back to the Last Glacial Period (LGP), with an acceptable vertical resolution. A spatialisation of this basal age estimate is done along the available radar profiles.

The manuscript is well written and I enjoyed reading it. The figures are generally pleasant and informative, the structure is clear, the references are appropriate.

Thank you for your positive comments on and interest in the manuscript.

Major comments

The modeling part is based on the inverse model by Martín et al. (2015). While I appreciate the quality of this model, I think it is only half appropriate in this study. Indeed, as the authors point out, there are two possible explanations of this un-steady age-depth profile: either a change of surface accumulation rate or a change of ice thickness (or a combination of both). While the Martín et al. (2015) model well explores the first option, it is not appropriate to explore the second option. A rough 600 m estimate of a possible ice thickness change is done by keeping the same a-dH/dt term but assuming a is constant. But this is not accounting for the coupling of ice thickness change with ice flow! I put it as a challenge to the authors if they can come up with a more quantitative estimate of ice thickness change, possibly with a figure illustrating possible scenarios.

Thank you for this challenge!

To address the thinning history at ABW we have added a new section to the manuscript (Section 2.4.4, Lines 195-207) in which we introduce a normalised elevation term and rewrite our age-depth model (Equation 5). As a result, we have updated Table 3 (Table 2 in previous manuscript) to detail the characteristics of our four modelled scenarios – Scenario 4 representing the new scenario.

Minor comments

- l. 39: extra question mark
- Figure 4, legend: remove "correspond to"
- l. 281: SINCE the mid-Holocene

Thank you for these minor comments. These have been corrected.

Response to Referee #2 (Peter Neff)

The authors present a compelling case for the ABW divide site to provide a full Holocene and late glacial climate record for Ellsworth Land and the Bellingshausen and Weddel Seas (both bodies of water providing primary moisture delivery to the site, per Thomas and Bracegirdle, 2014). The modeling approach here is very reasonable and well-supported by the IRH tracing building on previous work by Bogart and coauthor Bingham, in addition to the existing shallow F10 ice core results. All the data assembled here make a compelling case for the site, although I will note the extraordinary effort required to collect more than 1000 meters of Holocene ice to access a scant ~150 meters of Glacial ice (albeit with annual layers indeed likely interpretable for some part of the very late Pleistocene ice). The authors are appropriately conservative in interpreting depth-age results near the bed, given uncertainty in geothermal heating, melt rates, etc. at depth.

Thank you for your comments on the manuscript.

I assume no other potential ice core sites were discussed due to the number of constraints at the F10/ABW site, but it is worth noting that the local triple-dived nearer to the Amundsen Sea (~150km east of Canisteo Peninsula)—although compelling due to its proximity to the fast-changing Amundsen Sea—is home to a subglacial tephra layer visible in 2018 IceBridge RDS data. The presence of such a subglacial feature likely complicates preservation of interpretable snow stratigraphy, at least for a brief period. The authors may wish to note this in their manuscript, and also consider the likelihood and utility of chemical evidence of this layer being identifiable at the ABW site.

Link to 2018 NASA OIB RDS data with (presumed) tephra:
https://data.cresis.ku.edu/data/rds/2018_Antarctica_DC8/images/20181116_02/20181116_02_035_1echo.jpg

Thank you for sharing this radargram and highlighting the tephra layer visible in the OIB IceBridge RDS data. Our initial investigation at ABW was from a glaciological/geophysical focus, utilising the abundance of existing airborne and ground-based radio-echo sounding data, to expand our dated radar stratigraphy archive across the WAIS. It was only once we incorporated the radar layers into the model at the triple-divide at ABW that we discovered this exciting shift in the accumulation regime since the mid-Holocene.

We can clarify that the (presumed) tephra layer in the OIB RDS 20181116_02_035 profile is not visible in the OIB data around ABW (e.g. flightlines 20181116_02_38 – 20181116_02_ 43). Although we acknowledge thick tephra layers will complicate the preservation of interpretable snow stratigraphy, the NASA OIB RDS data at ABW do not indicate that a layer such as the one seen in other OIB profiles closer to the Canisteo Peninsula is present at ABW.

Therefore, while we thank you for raising this interesting point, and we have considered it, our inclination is not to add this discussion into the revised paper.

I hesitate to self-reference, but one important point relating to feasibility/utility of an ice core at ABW is expectations for ice core quality below 400 m given past BAS ice core drilling experience in this zone of increasing bubble pressure and brittle ice fracture. Not only will the top 1000 m of an ABW ice core be restricted to the Holocene (yes, valuable to constrain recent thinning and decreasing snow accumulation) it will also likely exhibit some brittle fracture from 400 or 500 m depth to the bed (e.g. Neff, 2014). This will reduce core quality and the quality of many chemical analyses if mitigating ice core handling procedures aren't implemented, and then some impacts may still remain. While this isn't an ice core drilling logistics paper, it is a key factor relating to ice thickness that will degrade the quality of the record.

Thank you for raising this important issue which we have now flagged in the paper. We have added a paragraph to Section 4.2 (Lines 383-389) which details the challenges with a brittle ice zone and how a proportion of the ice at ABW sits in a region where brittle ice was found at WAIS Divide. We refer the reader to literature describing specific core handling procedures that can be used to mitigate the impacts of brittle ice (Lines 388-389).

Additionally, the authors might consider providing a rationale for the particulars of the ABW site with respect to reconstructing past atmospheric composition. What will the characteristics of this site make advantageous for resolving regional discrepancies in greenhouse gas records from Antarctic ice cores? The snow accumulation rate at present may be relatively similar to WAIS Divide, but the shallower ice thickness alters the preservation depth and presence relative to the brittle ice zone so may make ABW more/less advantageous for gas analyses.

While we appreciate the comments made here by the referee, we feel that these discussion points are beyond the scope of this paper and would be more appropriate in an additional paper tackling the potential ice core analyses in more detail.

I have no specific edits to the paper, I think the methods employed to consider possible depth age distribution for the ABW site are very reasonable and just encourage some broader considerations for the site.

Thank you for your comments on the methodology of manuscript.

Response to Community Comment #1 (Michael Sigl)

Thank you for this valuable contribution, to which I would only like to add a brief comment. I previously made a related comment to two corresponding authors (J. Sutter and J. Bodart) of the Bingham et al. (2025) review paper during the open discussion phase, though not interactively.

These comments were not included in the final published paper, so I reiterate the main message here.

In line 79, you rather cryptically state that the IRH arise from 'variations in electrical conductivity related to paleo-accumulation events'. Setting aside the fact that the ice sheets themselves are the product of paleo-accumulation events, here you miss the opportunity to mention that these variations are caused by the deposition of volcanic acids across the polar ice sheets, and that some of these volcanic layers can be detected and dated very precisely and accurately in ice cores across the entire Antarctic, as well as the Greenland ice sheets (Sigl et al., 2022). Not only do the ages of these volcanic layers differ (in both directions, though still within uncertainties) by around 140–240 years in state-of-the-art ice-core chronologies such as WD2014 compared to estimates based on remote sensing used in this manuscript (see Figure 1, below), but the uncertainties in the ice-core ages are also an order of magnitude smaller (see Table 1 and 2, below). The volcanic sulfate anomalies that form these IRH layers are among the largest of the late Holocene, and the climate effects caused by the respective eruptions can be traced in tree-ring records over the past 5,000 years (Salzer and Hughes 2007). This allows the eruption dates, and thus the age of the IRH, to be pinpointed to within a few years.

Applying the updated ice-core ages to your Figure 4c (see Figure 1, below) shows that the ice-core ages would align much more closely with your optimized model ages, while also adjusting the layer thickness by up to 15 % (i.e. a 400-year difference between two time markers separated by 2,500 years). Since you also constrain your forward model with the accumulation profile from WDC (on *WD2014), wouldn't it make sense to use the time markers on the WD2014 chronology too?*

Thank you for these useful comments.

We have amended the text describing IRHs to state the probable volcanic nature of these layers more explicitly (Lines 79-83 and Lines 85-87).

Following your suggestion on updating the IRH ages, we have followed your suggestion to assign the ages more directly to the known sulphate peaks in WD2014 (Lines 86-89) and added Table 2 to clarify how the new IRH ages correspond with those assigned by Bodart et al. (2021).

As a result of updating the ages of our IRHs, the outputs for all scenarios changed, minorly, but the conclusions of the manuscript still remain the same as the previous version. Figures 3,4, and 5 all now use the updated outputs that are all available at (https://github.com/harryjoedavis/ABW_ice_core).

I am convinced that my queries will not affect the main conclusion of this manuscript -- that this site likely contains a continuous Holocene record. However, I would appreciate it if you could clarify the volcanic nature of the radar layers, as well as the possibility of using ice cores and other evidence to better constrain their ages.

In Figure 1 and in the caption, you call the ice-core site WD2014, which is the name of the latest chronology. The site of the deep ice core is typically abbreviated with WDC (sometimes WD) and the deep ice core with WDC06A.

The authors thank you for noting this. We have corrected the label in Figure 1 to note the ice-core site as WDC.

Response to Editor pre-review comments from 12th November (TJ Fudge)

Technical correction:

The WAIS Divide accumulation rate history is archived here:

https://www.usap-dc.org/view/dataset/601004

The proper references are:

0-31 ka (annual record): Fudge, T. J., B. R. Markle, K. M. Cuffey, C. Buizert, K. C. Taylor, E. J. Steig, E. D. Waddington, H. Conway, and M. Koutnik (2016), Variable relationship between accumulation and temperature in West Antarctica for the past 31,000 years, Geophys. Res. Lett., 43, 3795-3803. (doi:10.1002/2016GL068356)

31-67 ka (firn-based): Buizert, C., K.M. Cuffey, J.P. Severinghaus, D. Baggenstos, T.J. Fudge, E.J. Steig, T.A. Sowers, E.J. Brook, R.H. Rhodes, H. Cheng, L.R. Edwards, M. Sigl, J.R. McConnell, and K.C. Taylor (2015). The WAIS Divide deep ice core WD2014 chronology - Part 1: Methane synchronization (68-31 ka BP) and the gas age-ice age difference, Clim. Past, 11, 153-173. (doi:10.5194/cp-11-153-2015)

The authors cite Sigl et al. 2016 and Koutnik et al., 2016 repeatedly. Sigl et al. refers to the annual timescale, while Koutnik et al. addresses the divide and thickness histories.

The authors thank you for noting this. The in-text citations have been updated to reference the correct articles.

Gas records. No delta-age (the ice age gas age difference) has been presented. The dating of the core will likely require using methane, and potentially other gases, so discussing the gas records is an important component. Why this core would allow novel atmospheric gas reconstructions would make it stronger.

We considered this but have concluded that given the mainly glaciological scope, such details of ice-core analyses would be more appropriate in an additional manuscript which gives more space for detailed discussions.

Brittle ice. The authors propose an ice core where most of the interesting records will be in the brittle ice, roughly 500-1300m at WAIS Divide. This will impact the ability to date the core with annual layer interpretation as well as inhibit continuous flow analysis.

Please see response to Referee #2 on this same point.

The discussion states "To resolve climatic changes accurately at ABW, and confirm the signal as an ice-thinning, accumulation rate decrease, or somewhere in between, we need an ice core." Yet there is no discussion of what ice core records will disentangle what thinning histories. The SkyTrain ice rise work relies on both water isotopes and aerosols (sea salt if I'm remembering right), yet these are not discussed. The challenge at ABW is arguably more pronounced using these records given the influence from multiple moisture pathways.

Our intention was to present the glaciological suitability of this site, rather than evaluate the potential proxy records that the core could provide.

Referee #1 presented a challenge to model the thickness history at ABW, which we accepted, to attempt to address the thinning history at ABW from a glaciological scope. We feel this approach is more fitting for this particular manuscript to disentangle the thinning history at ABW. While we appreciate the comments made here by the editor about which ice core records could aid this estimation, we feel that these discussion points are beyond the scope of this paper and would be more appropriate in an additional paper tackling the potential ice core analyses in more detail.

The citation of Kingslake et al. 2018 doesn't seem fully accurate. The emphasis of that paper is Holocene re-advance, implying thickening at ABW during the time period identified in the paper for thinning. To me, this makes for a fundamental conundrum - is interior WAIS thickening or thinning from the mid-to-late Holocene? - and a much more compelling reason to drill an ice core at ABW.

This is a great point. To remove ambiguity with the Kinglsake et al., 2018 citation we have re-worded the text preceding to make note of the observations of readvance and thickening during the Holocene (Line 312).

The problem that we are faced with in this manuscript, however, is that our observations only go back to ~5 ka, so we cannot resolve the history of ice thickness at ABW (with the current model without making several assumption) through the entire Holocene. Moreover, in the period where we can constrain our model we observe significant ice thinning (Figure 4 from scenario 4) of a similar magnitude to Grieman et al., 2024, and so in our manuscript we often refer solely to thinning as a result of this, despite the work in Kingslake et al., 2018 implying mid-to-late Holocene thickening.

In Lines 328-334 we refer to this conundrum of was the interior WAIS thickening or thinning from the mid-to-late Holocene, and detail how an ice core would benefit our current understanding which, with our current observations, we are only able to constrain to 4.86 ka.

References:

Grieman, M.M., Nehrbass-Ahles, C., Hoffmann, H.M. et al. Abrupt Holocene ice loss due to thinning and ungrounding in the Weddell Sea Embayment. Nat. Geosci. 17, 227–232 (2024). https://doi.org/10.1038/s41561-024-01375-8

King, A. C. F., Bauska, T. K., Landais, A., Martin, C., and Wolff, E. W.: Ice core nitrogen isotopes archive dramatic changes in West Antarctic Ice Sheet thinning, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-3305, 2025.

Kingslake, J., Scherer, R.P., Albrecht, T. et al. Extensive retreat and re-advance of the West Antarctic Ice Sheet during the Holocene. Nature 558, 430–434 (2018). https://doi.org/10.1038/s41586-018-0208-x