Reply to Reviewer 2

Please find our responses in bold italics below.

The paper entitled, “A thinner-than-present West Antarctic Ice Sheet in the southern Weddell Sea Embayment during the Holocene” by Small and colleagues presents new cosmogenic nuclide inventories from subglacial bedrock cores that elucidates Holocene ice sheet history in the Weddell Sea Sector. This work is very timely as grounding line retreat and re-advance in the Holocene has been found in both Amundsen (Balco et al., 2023; Nichols et al., 2024) and Ross (Venturelli et al., 2020, 2023; Kingslake et al., 2018) sectors of West Antarctica. Thus, the results presented in this paper are important because they prove the hypothesis of retreat and re-advance in the Weddell Sector originally posed by Bradley et al (2015) and demonstrates that this phenomenon occurred in **every** sector of West Antarctica. As a result, I believe this paper should be published with very minor revisions and I provide some minor comments and questions below that I hope will help to clarify what is already a very excellent contribution to the literature.

1. The feat of drilling and recovering 4 subglacial cores is really impressive. I found this to be a bit underplayed in the manuscript and suggest including a bit of context to highlight (a) how impressive/important this is and (b) how targeting blue-ice areas might be advantageous in future subglacial drilling areas given the results and successes herein. It could also be useful to add some context about the conditions at the ice-bed interface, given past challenges with sediment-laden basal ice in non-blue-ice-areas described in Braddock et al. (2025).

We are happy to add some extra detail on our drilling experiences in the methods section. The clean transition at the interface was mentioned but we will expand. We also drilled in a blue-ice area in 23-24 (Pensacolas) and encountered (some) sediment-rich basal ice, speculatively perhaps geology is a key factor here with certain lithologies producing more debris rich basal ice. That said we agree that the experience of all drilling campaigns is useful for future efforts and will include some extra context.

2. I realize that some of this information is in the supplement, but can you provide a bit more information about quartz preparation for in situ ^{14}C samples? With the work of Nichols & Goehring (2019) showing that quartz isolation/preparation methods may impact the validity of the in situ ^{14}C result, adding some specific details about how long successive leaches were carried out, if any other separation techniques were used (e.g., frothing or magnetic separation), etc. would be useful for the interpretation of these results into the future.

We will add the extra information on leach durations etc. Samples were initially cleaned at SUERC. After delivery to UOW/ANSTO one was randomly selected for purity testing. Although a small difference was noted between the reported and measured purity, it remained within the expected analytical range.

We will also clarify that no froth flotation was undertaken however previous experience in preparation of samples for in situ 14C extraction suggests that differences in quartz purification protocols have minimal impact on in-situ ^{14}C results until the final 1% HF/HNO_3 etch is applied. Consequently, froth flotation remains a reliable and preferred method for producing clean quartz aliquots. All samples in the present study were extracted using an updated and more rigorous protocol than that published in 2019 by Fülöp et al. For this reason, we report intercomparison data CRN (three samples using the same protocols as our unknowns) with standard deviation of 14%. Full details of the revised protocol will be presented in the forthcoming paper by Fülöp et al.

3. In addition to quartz preparation: I do have some specific questions about the in situ ^{14}C data:

- Can you provide some more information about the long-term blank value and how that is derived? With the methods paper being cited coming from ~6 years ago, I do think more information is needed here. Specifically, I would like to see the values of individual blanks analyzed during the measurement window of the core samples presented herein. This would help to not only understand the long-term blank value, but also blank variability during the measurement of these samples.

  As with our reply to reviewer 1, we thank the reviewers for their comments concerning the in-situ ^{14}C measurements and the performance of the UOW/ANSTO extraction line. In response, please see the attached figure which shows the blanks associated with the samples from this study, listed in measurement order. The mean blank value (+/- s.d) for the past six years is 1.5 5± 0.94×10^4 atoms. The 15 blanks processed alongside the current core samples are indistinguishable from this long-term average. Given this reproducibility, and the fact that none of our samples overlap with any blank measured during the relevant period we do not consider additional blank modelling to be necessary. The long-term blank produced

by the UOW/ANSTO extraction line is discussed in more detail in a forthcoming paper by Fulop et al.

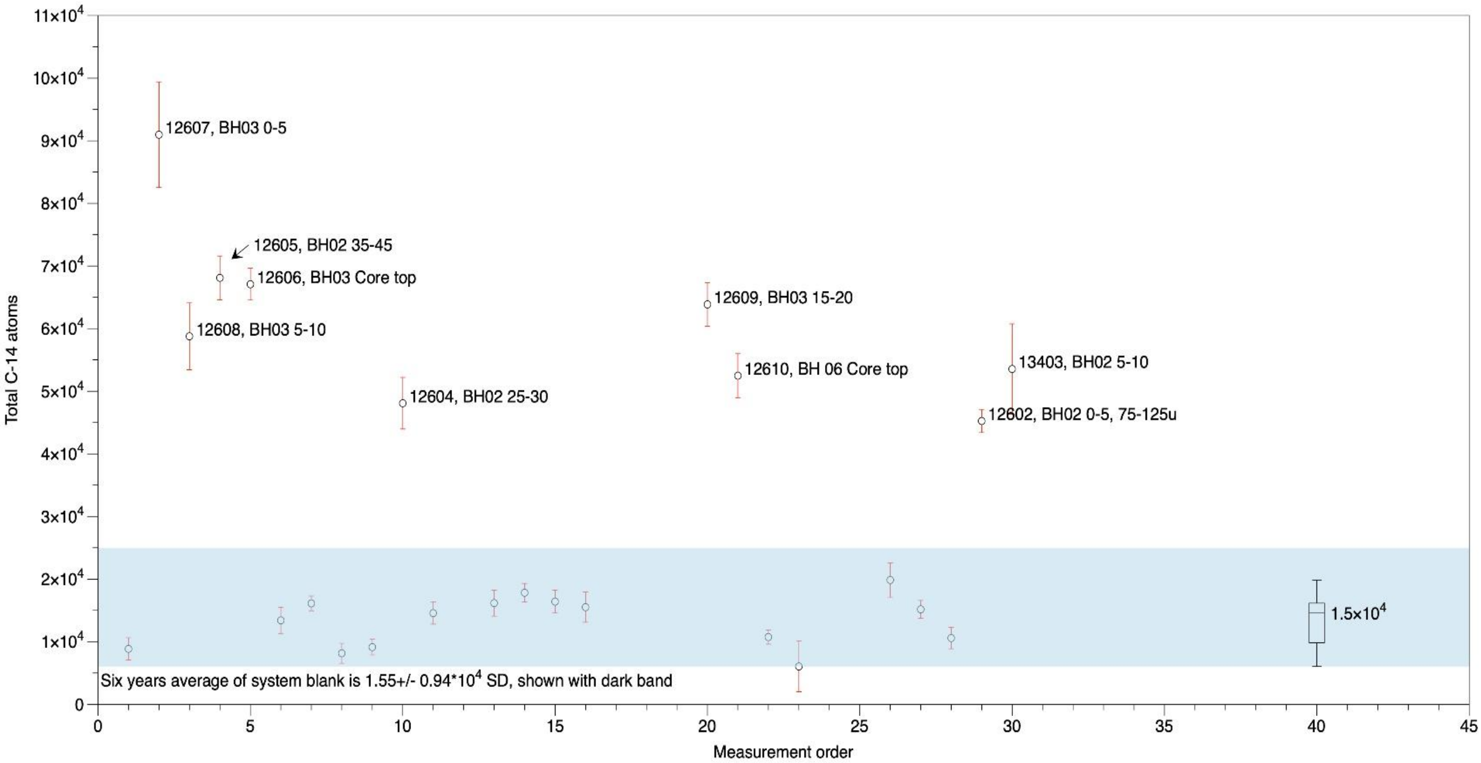

Figure A1. Total ^{14}C nuclide inventories of core samples and process blanks in measurement order. The six-year average (± s.d.) of blanks produced at UOW/ANSTO is shown with blue shading. The box and whisker plot summarises the distribution of the 15 process blanks analysed along with the unknowns.

- I am curious about the very low CRONUS-A concentration (5.85 x 10^{5} ^{14}C atoms g^{-1}) presented in the supplement. The consensus value for CRONUS-A is 6.93 ± 0.44 ×10^{5} ^{14}C atoms g^{-1} (Jull et al., 2015), which has been maintained in the literature as new laboratories come online (Lupker et al., 2019; Fulop et al., 2019; Lamp et al., 2019; Lifton et al., 2023; with the exception of similarly low values from Goehring et al (2019)). The authors state that an in vacuo cleaning step at 600°C was used to remove meteoric ^{14}C—how long was this step carried out? Lifton et al (2023) showed that holding a sample at 600°C for >1 hr has the potential to remove higher temperature/in situ ^{14}C, so it is possible that this step is removing some of the in situ component here too. These methodological details are important as they have implications for the interpretation of ice history from the core samples, because concentrations presented might not reflect the full period of exposure, but rather a concentration accumulated during exposure minus the removal during the 600°C step. If this explanation can not be used to justify why the CRONUS-A value is so much lower than the consensus value, I do think some justification and details on the value presented are needed (How many measurements of CA were done? Was there any variability? Why doesn't the value presented here match previous values from this lab (e.g., Fulop et al., 2019).

We agree that the CRA value quoted here will be of interest to the wider community however in the first instance we would note that the intended purpose of quoting this value was to emphasise that we considered it unlikely that we were systematically overestimating inventories (cf. Reviewer 1's comment on false positives). The value is derived from a significant body of work (representing significant effort as I am sure the reviewer will appreciate) and to go into substantial depth on, the overall distribution of values as well as potential explanations for the observed values is beyond the scope of this reply (or paper). That said it is important to point out some key methodological differences between the approaches described by Lifton et al. and by Fülöp et al. which make comparisons complex. These differences fall into three main categories: flux use, oxygen availability, and dilution strategy.

1. ***Flux versus no flux.***
   Lifton et al. employ lithium metaborate flux, which melts at 845 °C and reduces the effective melting temperature of quartz to ~1200 °C, allowing complete melting. In contrast, Fülöp et al. heat quartz to ~1650 °C without flux, inducing a phase transformation rather than melting the mineral. It is possible that quartz softening in the Lifton et al. system begins at lower temperatures (~600 °C), particularly in the presence of additional oxygen, but this has not been directly demonstrated.
2. ***Role of oxygen in fluid-inclusion degassing.***
   In the Fülöp et al. method, samples are heated in vacuo, without oxygen addition. Quartz does not release the major fluid-inclusion component until temperatures exceed the α–β transition at 573 °C, consistent with results presented in Fülöp et al. (Goldschmidt 2019; Radiocarbon 2022) and there is no loss of in-situ C-14 signal. By contrast, the introduction of oxygen may enhance degassing and potentially alter the extraction pathway, although the presence and magnitude of this effect remains uncertain.
3. ***Blank dilution and carrier strategy.***
   Lifton et al. (and Goehring et al.) routinely dilute samples and blanks by factors of 10–40. Under these conditions, even small sources of contamination can become masked by the blank gas, making it difficult to distinguish laboratory contributions derived ^{14}C. Their flux also contributes to the procedural blank, as shown in Lifton et al. (2023). Furthermore, no synthetic quartz is used to replicate the matrix behaviour.
   In the Fülöp et al. approach, synthetic quartz is processed through the same extraction pathway, and solely from the extracted quartz the gas remains within the uncertainty of the pressure transducer unless a solid diluent is introduced before extraction-not after. This enables more direct monitoring of the blank contribution.

Regarding the CRA reference material. Supplementary data in Balco et al. (2023) show that the Tulane laboratory reported an average CRA value of ~5.88 x 10^5atoms which is statistically indistinguishable from the UOW/ANSTO value. For other laboratories, meaningful comparison remains difficult due to methodological differences such as those highlighted above, and any discrepancies should be properly evaluated once the consensus value reported by Jull et al. (2015) is systematically revisited by the community. We support such an effort, however we don't consider this paper the most appropriate place to tackle such an in-depth and technical question. We can add some text either in supplementary info or the main body highlighting the issue but pointing out, importantly that it does not change the key finding of this paper, namely that ice must have been thinner at some point during the Holocene. Finally, a forthcoming paper (Fülöp et al.) will discuss CRA values from the UOW/ANSTO lab in significantly more detail than is possible in this paper and we are reticent to pre-empt this paper and its peer review.

- The slightly higher value at depth in BH02 and BH03 is a pretty cool observation, and it's interesting that the increase in concentration occurs at different depths. I think this is a bit overlooked in the text, and some context about how much muogenic production would be needed to produce the concentrations observed would be of value to the in situ ^{14}C community.

Prolonged muon production would, presumably, result in broadly similar concentrations in lower core samples. Our sense is that while we can be confident that in situ 14C is present in the deeper BH-02 and BH-03 samples (cf. blank values) their measured 14C concentrations are low enough to be influenced by variability within the blank value. That is perhaps the correction is "too large/small" on some samples and the produces an apparent increase in concentration at depth. We are thus reluctant to speculate on the apparent increase observed in the deeper BH-02 and BH-03 samples.

4. The authors invoke dynamic thinning to explain that grounding line retreat would be expected alongside the magnitude of thinning observed from this work. This argument could really be strengthened by the addition of some simple calculations to demonstrate the likely magnitude of grounding line retreat associated with the thinning observed at the site. Such a constraint would be a valuable addition for constraining ice sheet models, and provide constraints on future sub-ice drilling efforts for grounding line retreat signals (e.g., Venturelli et al., 2020).

This certainly crossed our minds however we decided against this for several reasons. Firstly, models show that grounding line retreat is focussed over the Robin Subglacial Basin which lies some distance from our drill site. We suspect thinning at our site is most likely linked with GL retreat in Hercules Inlet. Thus a quantification of the magnitude would be very specific to this small section of the wider GL and probably not reflect the (potential) major GL retreat (>300 km in some models) across the RSB. Secondly, it was not

obvious to us how to extrapolate upstream thinning to GL retreat in a relatively slow flowing area. Konrad et al. (2018) provide a value to link metres of thinning to metres of grounding line retreat (110 metres of retreat for 1 metre of thinning) in fast flowing ice streams (800 m a^{-1}) but it is unlikely that this value is appropriate for areas for slower flowing areas such as between our site and the grounding line (<20 m a^{-1}). We think this comment links to a point made by reviewer 1, specifically that we (the community) probably need to figure out how to better link the sort of data presented here to the former grounding line positions. Instead of speculating about the magnitude of retreat (for reasons above) we propose to make this point with reference to these reviews.

Again, I believe this paper is a valuable contribution to the literature and the suggestions and requests above should be viewed only as minor suggestions. I really enjoyed reading the submitted manuscript, and I commend the authors on the excellent work presented herein.

Thank you, we appreciate the review and suggestions which will improve the paper.

Proposed list of changes

- **Add extra text on drilling experiences.**
- **Add extra detail on sample preparation.**
- **Include addition figures showing individual blanks measured alongside samples.**
- **Include comment on CRONUS A values (our choice would be to do this in the supplemental).**
- **Include additional point about linking data to magnitude of grounding line retreat.**