We thank referee #1 and #2 for helpful comments and for reviewing the first revision.

To referee #1

In the revised manuscript titled “Chronostratigraphy of Larsen blue ice area in Northern Victoria Land, East Antarctica, and its implications for paleoclimate”, the authors made substantial changes and added new materials to the draft. In particular, a section estimating the site temperature and accumulation rate made the paper more interesting and worthy of publication. Most of my concerns and suggestions to the previous version of the manuscript have been properly addressed. Therefore, I am happy to recommend its acceptance by The Cryosphere after some suggested revisions listed below have been resolved. These suggestions mostly strive to improve the clarity and the accessibility of the paper.

Please note that the line numbers are from the finalized manuscript (version 3), not the one with Author Track Change

Line 3: the institution number for Christo Buizert needs to be changed if you move that author forward.

- Line 3: Institution number is changed to “5”.

Line 17: “chronostratigraphic studies” should not be complicated? Either you mean that the chronostratigraphy is complicated, or the efforts to study the chronostratigraphy is challenging (“owing to fold and fault structures”).

- Line 19: We changed “complicated” to “challenging”.

Line 27: what does the “± 5” mean? Is it 1σ or the 95% Confidence Interval? Please specify.

- Line 28: We added “(1σ)”.

Also in Line 27: why do you attribute the deglacial warming to the retreat of the AIS? It seems to suggest that AIS retreat is the whole reason for the observed warming. However, according to Section 3.7, the ice sheet retreat and possible elevation changes might only be responsible for the warming in excess of the 7.1 ± 2 deg C (observed at Talos Dome). The current sentence is somewhat misleading and some clarifying statements are needed here.

- Line 28: We deleted the description of the reason about temperature and accumulation increase in the abstract. The new relevant text is as follows:
  - A tentative climate reconstruction suggests a large deglacial warming of 15 ± 5 ℃ (1σ), and an increase in snow accumulation by a factor of 1.7–4.6.

Line 34: “archives” and “records” are synonyms. You could just get rid of “records”.

- Line 35: We removed “records”.

Line 82-83: here I think you made a good case for using Kr-dating as a complementary tool. Perhaps you could say something like “given these uncertainties we opt to use correlation of … with … as the primary dating tool.”

- Line 92: We added a sentence as reviewer #1 suggested. The relevant text is as follows:
  - Given these uncertainties, here we opt to use correlation of atmospheric gas records with existing well-dated ice core records as the primary dating tool.

Line 89: it may be helpful to highlight that this approach exists before the advent of absolute dating (e.g. Petrenko et al in 2006 vs Bender et al in 2008).

- Line 98: We added a statement. The relevant text is as follows:
  - CH_4 and O_2 are well mixed in the atmosphere (Blunier et al., 2007), and conducting correlations of both CH_4 concentration and $\delta^{18}O$ of O_2 ($\delta^{18}O_{atm}$) is a well-known strategy for establishing chronologies of blue ice, which have been used even before the advent of absolute dating method (Petrenko et al., 2006; Baggenstos et al., 2017; Yan et al., 2021).

Line 95-99: the description of Kr-81 dating seems out of place. It may be better if you move it to the earlier paragraph (around Line 80). Also, what’s the most basic principle of Kr-81 dating?

- Line 84: We moved the description of ^{81}Kr dating to line 84. We also added the basic principle of ^{81}Kr dating. The relevant text is as follows:
  - ^{81}Kr is a cosmogenically-produced radioactive isotope with a half-life of 229 ± 11 ka, decaying to ^{81}Br via electron capture. It is mixed in the atmosphere within 1–2 years and has no significant sources or sinks, which makes ^{81}Kr as an ideal tracer (Oeschger, 1987; Zappala et al., 2020). Oeschger (1987) suggested the potential of ^{81}Kr for radiometric ice core dating. However, at that time, 10^5–10^6 kg of ice was required. Owing to the development of Atom Trace Trap Analysis (ATTA), the required ice has continued to decrease (Lu et al., 2014; Tian et al., 2019; Jiang et al., 2020; Crotti et al., 2021).

Line 114: can you justify the use of the new analytical framework? The superiority of the new method is not self-evident and needs to be declared so readers will understand why.

- Line 119: We added statements. The relevant text is as follows:
  - In contrast to the previous studies, which used the Herron-Longway model (Herron and Langway, 1980; Baggenstos et al., 2018; Menking et al., 2019; Yan et al., 2021), we applied a recently developed analytical framework, which does not require stable water isotope values to estimate past surface temperatures and accumulation rates (Buizert, 2021); stable water isotope values from Larsen BIA seems altered by fractionation during sublimation of ice (see Sect. 3.2).

Line 175-177: if I understand this correctly, did you calculate the average of the daily offsets first, and then subtract the average value from the measurement each day? If it's the former, please specify the average with 1 standard deviation (on top of the range of 2-20 ppb).

- Line 183: We calculated the average offset by measuring the standard air that was injected into four flasks containing bubble-free ice samples when doing measurement each day. The average offset varies day by day. So we present the mean of the daily average offsets. We also present the average of the intra-day standard deviation of the CH_4 concentration measured from the control group. We deleted the previous statement. The new relevant text is as follows:
  - (average of the daily average offsets: 11.4 ± 3.2 ppb (1σ); average of the intra-day standard deviations of the control group: 3.7 ± 1.2 ppb (1σ))

Line 233: "$\delta O_2/N_2$,gravcorr of around −30 ‰ indicates that the ice is poorly preserved" seems to suggest that all your samples have −30‰ O_2/N_2 ratios. However, this is not the case. In fact, there is only one sample (#301) that is characterized by such a negative value. Maybe you could just describe the O_2/N_2 data first, and then say there is one very negative $\delta O_2/N_2$ sample (<−30 ‰), which you decided to reject.

- Line 241: We replaced the sentence as reviewer #1 suggested.

Line 238: either in this chapter or earlier (around line 80) you need to briefly describe the principle of Kr-81 dating. For example, what is the process producing ^{81}Kr? What is the decay product? What is the half-life time (given that you mention its uncertainty)?

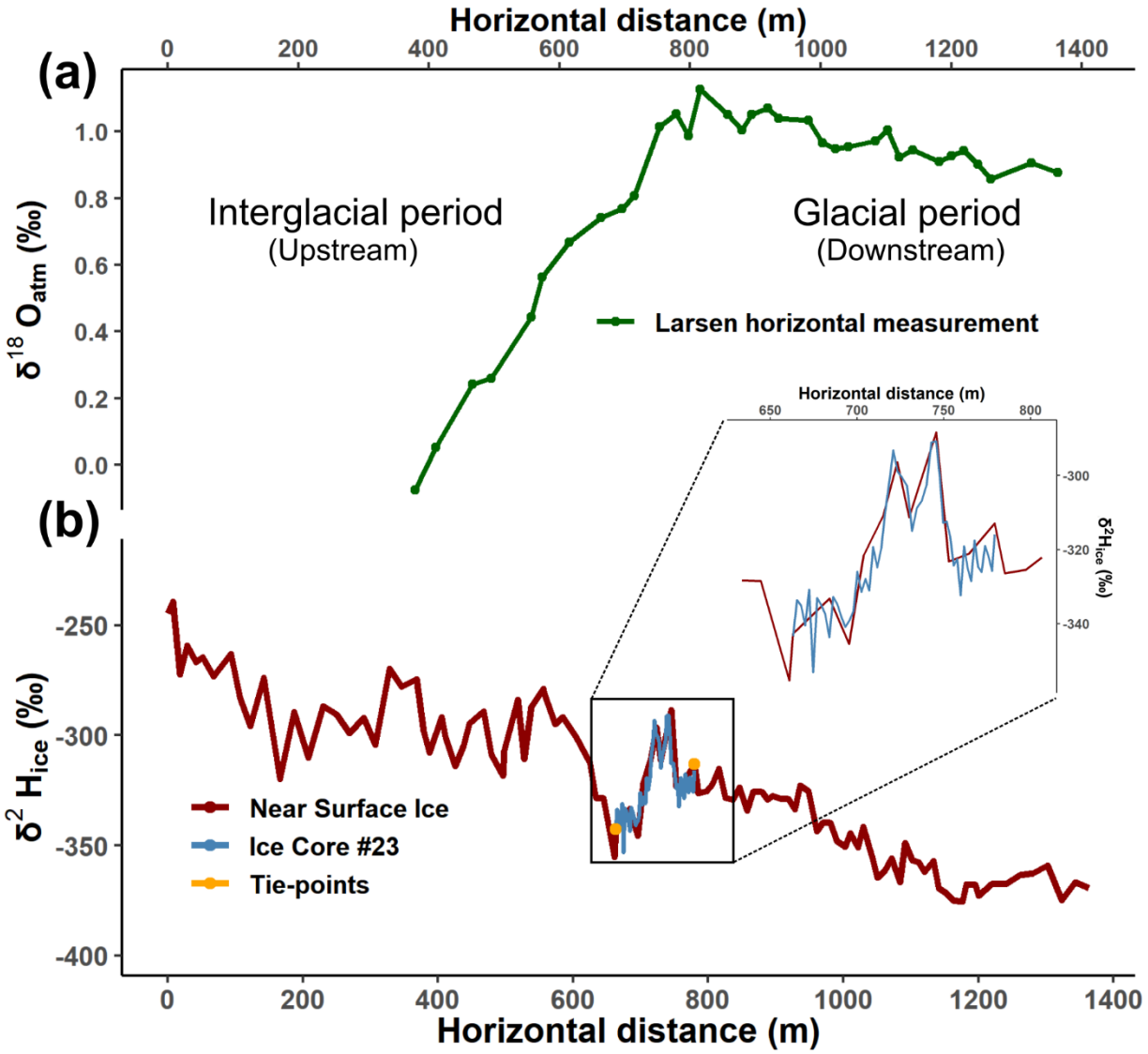
- Line 84: We added the basic principle of ^{81}Kr dating.

Line 250: this table belongs to “Results and discussion”, as you have presented the actual data measured on the ice.

- Line 384: We moved the table and the relevant sentence to section 4.1.

Line 305-307: “The average offset of the values of $\delta^{2}H_{ice}$ between vertical ice core #23 and the horizontal record is 9.8 ‰ and 0.6 ‰ for $\delta^{18}O_{ice}$. We assume that the average offset is the uncertainty (1σ) of stable water isotope values of the Larsen ice.” I don’t understand what this means. Do you imply an offset in the stable water isotopes between the horizontal samples and the shallow cores? Fig 4b does not show this offset (or that the offset has been corrected).

- Line 312: We changed “offset” to “difference”. “Offset” was not the right word.
- Line 319: We added a magnified figure in Fig. 4 to help readers’ understanding.



Line 353: Section 3.4-3.7 could be separated from the Sections before, and be listed as Discussion because you are using the measurement data to make further inferences. Existing Section 3.1-3.3 can be the new Results section. Table 1 can be moved to here (as a Section 3.3.3. Krypton isotopes) as well.

- Line 268: We separated the section as reviewer #1 suggested.
- Line 384: We moved the table and the relevant sentence to section 4.1.

Line 360: since you have the Kr-81 results you could be more confident here.

- Line 382: First of all, we wanted to constrain the age without using the ^{81}Kr result and then finally used the ^{81}Kr as an independent dating method. The ages of ^{81}Kr are mentioned in Line 382 (now more confident). The relevant text is as follows:
    - Finally, we confirmed the ages of the Larsen ice with ^{81}Kr dating, indicating 9–41 and 14–43 ka for ice from the TF and #23 cores, respectively (Table 2), and concluded that Larsen ice covers the Last Glacial Termination (LGT, T1).

Line 388: gas and ice age could be split into two sections. This way you could combine the ice age of Larsen ice with the Appendix C.

- Line 403: We separated the section.
- Line 442: We moved Appendix C into the new section 4.3.

Line 443: an orphan period.

- We deleted the orphan period.

Line 455: I am not sure about the length requirement but it seems that the discussion on uncertainty can be moved to the appendices (or supplement).

- We moved the section to the supplement.

Line 519: again please specify the statistical meaning of the ± 5 deg C.

- Line 494: We added "(1σ)".

Line 541: change "highly constrained" to "well constrained"

- Line 520: We changed "highly" to "well".

Line 547: this is an interesting idea worth exploring more. Is it possible to have an array of ice cores along the Transantarctic Mountain? The deglacial retreat of the RIS might be reflected in the timing of the accumulation rate increase.

- We added new statements about the accumulation rate changes at Taylor Dome as well as that at the TALDICE site because they are located near the Transantarctic Mountains.
- Line 514: We added a new statement. The new relevant text is as follows:

- During the same time period, the accumulation rates at Taylor Dome and TALDICE site increased by a factor of ~15.4 and ~2.4, respectively (Veres et al., 2013; Baggenstos et al., 2018). Based on the three records (TALDICE, Larsen blue ice, Taylor Dome), the accumulation rate has increased more at the Southern Victoria Land than the Northern Victoria Land.

- Line 526: We added a new statement. The new relevant text is as follows:
  - The accumulation rate at the Taylor Dome during 14–21 ka BP shows lower value than that from TALDICE and Larsen ice (Fig. 11). However, the accumulation rate increased by a factor of ~5.2 at Taylor Dome, while those at the Larsen and TALDICE increased by a factor of ~2.1, and ~1.8, respectively. Overall, it appears that as RIS retreats, the accumulation rate at the Southern Victoria Land increases more than the Northern Victoria Land. This interpretation is in line with the previous studies (Morse et al., 1998; Aarons et al., 2016; Yan et al., 2021) and may help studies for reconstructing past atmospheric circulation associated with the retreat of RIS.

Line 559: this is a very long paragraph that is hard to follow. Splitting it into two or three shorter paragraphs may help.

- Line 541: We split the paragraph into three shorter paragraphs.

Line 601 (Figure A3): Figure A3 and A4 aim to achieve the same goal. You could merge them into a figure with two panels to save space.

- Line 572: We merged them into Fig. A3.

Line 611: you could merge Appendix C with the discussion on ice age in the main text (after it has been separated from the gas ice).

- Line 442: We moved Appendix C into the new section 4.3.

In supplement:

Line 6: this information is quite important to the main conclusion of the paper. I suggest you move it to the main text as an Appendix. The supplement will be

- Line 583: We moved it into Appendix D.

Line 13-14: the word “constant” is broken into two lines.

- The document was corrupted. We solved the problem.

Line 16-17: you mean "… depth at all times"?

- The document was corrupted. We solved the problem.

Table S1-S7: I commend the authors for attaching the data here. My only suggestion is that those data could be uploaded to a public depository for easier access.

- Line 602: We added "Data availability" section.
- We corrected the uncertainty values in Table S3 and S11.

To referee #2

General comments

The new version of the manuscript has been greatly improved, with an elaborate discussion in section 3.7 and a concise conclusion. By adding paragraph 3.7, the implications of the analyses are illustrated and placed into context. I suggest moving the newly added paragraph 3.6 to supplementary materials, given its methodical/technical character. I have noted my specific suggestions per section below.

- We moved the section to the Supplement Information.

Specific comments per section

Line 1: The word “because” is out of place. I would rephrase as: “In BIAs deep ice outcrops, allowing for the cost-effective collection of large-sized old ice samples at the surface.”

- Line 16: We rephrased the sentence as reviewer #2 suggested.

Line 46: I do not understand what is meant by “deposited”. Also, in the current formulation it seems like ice is exposed on the surface everywhere. Please consider a formulation like “In BIAs, old ice is exposed on the surface. Normally, ice forms through the compaction of snow, and flows directly towards the margin of the ice sheet. However, in some areas, (basal) topographic obstacles redirect

the flow of the ice towards the surface, resulting in so-called blue ice areas. In these BIAs, surface snow is ablated by ….”

- Line 47: We rephrased the sentence as reviewer #2 suggested. The new text is as follows:
  - Once snow is deposited on the surface, ice is formed through the compaction of snow, and flows directly towards the margin of the ice sheet. However, (basal) topographic obstacles redirect the ice-flow towards the surface in some areas, resulting in so-called blue ice areas. In these BIAs, surface snow is ablated by katabatic winds and/or sublimation (Bintanja, 1999; Sinisalo and Moore, 2010).

Line 139: Dust lines are very beautifully visible in Google Earth indeed!

- Thank you!

Line 255: “due to” → “because of”

- Line 259: We changed “due to” to “because of”.

Line 274: please add “(subsurface)” before “crevasses, cavities…”

- Line 278: We added “(subsurface)”.

Line 277: “, which is a method of interpolation that provides unbiased prediction at unsampled areas (Oliver and Webster, 1990).” can be removed

- Line 281: We removed the statement as reviewer #2 suggested.

Figure 3a: Thank you for clarifying how the data is interpolated. As there is still no reference to or analysis of the data shown in panel a, I would suggest swapping the panel with figure S1 of the supplementary materials (so Fig. S1 becomes Figure 3a, and Figure 3a becomes Figure S1).

- We better show the distribution of ice thickness and bedrock elevation in Larsen BIA together. We briefly described the distribution of ice thickness and bedrock elevation in line 279–283.

Line 304: So, the interpolation is conducted to increase the resolution of the data? (Please specify

the motivation in the text)

- Line 309: We conducted linear interpolation to assign horizontal distances that correspond to the depths of core #23. The relevant text is as follows:
  - To match the two δ^2H_{ice} profiles (measurement of ice core #23 and horizontal measurement of near-surface ice), the depth of ice core #23 was converted to the horizontal distances by pinpointing the deepest result of #23 to the result of ice core #104. The horizontal distances for the rest of the depth of core #23 were assigned by conducting linear interpolation.

Line 330: Interesting, and nice overview in Table 2.

- Thank you for your comment!

Line 368: "which is in line with our observation of ~0.05 ‰ offset between core #23 and horizontal measurements at a depth of > 1.95 m (Fig. A2)." I do not see this offset in Figure 2A. For me it looks like the values of the core #23 fluctuate around the horizontal measurements (with indeed a value of approx. 0.05 ‰, but in my understanding an offset is something constant (so all values would be too low or too high). Possible correction: "which is within the same order of magnitude of the differences observed between core #23 and horizontal measurements at a depth of > 1.95 m (Fig. A2)."

- Line 376: We corrected the statement as reviewer #2 suggested.

Line 369: I do not understand to what age difference is referred (difference between ..?) and how that explains an offset.

- Line 377: We deleted "age difference because some". We also rephrased the sentence and changed "offset" to "difference". The new relevant text is as follows:
  - Due to the lack of a corresponding $\delta^{18}O_{atm}$ record to the CH_4 of core #23, the difference may also come from the $\delta^{18}O_{atm}$ records that were estimated by linear interpolation.

Line 421: By interpolating data no new information is added, so it does not per definition avoid a bias. I'd suggest to remove: "to avoid bias in the spline curve of the …" and just state that the horizontal resolution of the data has been increased through linear interpolation at 5 m intervals.

- Line 437: We deleted the statement as reviewer #2 suggested and stated that the horizontal resolution of the data has been increased. The new relevant text is as follows:
  - As described above, the horizontal resolution has been increased by interpolating the original $\delta^{18}O_{ice}$ records at 5 m intervals.

Line 445: The comparison to the vertical ice core resolution undersells your results: please emphasize that the ice is available in large quantities at the surface (in contrast to TALDICE).

- In the 1st revision, reviewer #1 suggested calculating the temporal resolution in the horizontal dimension and compare it with TALDICE. We think presenting this would be worthwhile.
- Line 475: We added "In addition, Larsen BIA, allows for collecting large quantities of ice from the surface".

Line 447: In the previous version a different number was mentioned (156 yr/m) – is this correct?

- Yes. Previously we used 156 yr/m. We corrected the number.

Table 2/Conclusion: "Larsen Glacier" is not the same area as the "Larsen BIA". Please change all occurrences to "Larsen BIA" to avoid ambiguity.

- We changed "Larsen Glacier" to "Larsen BIA".

Section 3.6: Although it is always valuable to discuss uncertainties, the added paragraph is mostly describing a rather complicated method, without discussing implications of the estimated uncertainties. I'd suggest moving the paragraph to supplementary materials and indicating the estimated uncertainties in the main text/figures. Moreover, the description of the used method should be clarified/simplified.

- We moved the section to the supplement.
- We clarified the description.
- We corrected the uncertainty values in Table S3 and S11.

Section 3.7: very nice addition to the paper!!

- Thank you for your comment!

Line 528: “deposition site” means “upstream accumulation area”?

- Line 502: Yes. We rewrote the statement for the readers’ understanding. The relevant text is as follows:
  - We suggest that this enhanced cooling may have affected the original deposition site (upstream accumulation area) of Larsen BIA.

Line 535: Not only the tie points would be incorrect but matching the two records would not be very sensible. However, given the good match this seems not to be the case: maybe reformulate to something like: “Given the reliable results in matching the collected samples to existing ice core data (ref to figure/paragraph), the $\delta^{18}O_{ice}$ features used for the matching are likely climatic in origin and are not strongly influenced by local effects (such as sublimation intensity, and accumulation controls by surface slope).”

- We agree that the ice age is not significantly incorrect. However, because matching the ice age was relatively difficult with only using $\delta^{18}O_{ice}$, ice age must be improved by matching ion concentrations. Also, it is likely that $\delta^{18}O_{ice}$ value was altered by fractionation during sublimation because the d-excess value indicate negative value (please see section 3.2 and also Hu et al., 2021). Hence, we are cautious to say directly that $\delta^{18}O_{ice}$ value is climatic in origin and that the ice age is reliable.