

Clim. Past Discuss., referee comment RC4
<https://doi.org/10.5194/cp-2021-7-RC4>, 2021
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Review of Yan et al.

Anonymous Referee #3

Referee comment on "Enhanced moisture delivery into Victoria Land, East Antarctica, during the early Last Interglacial: implications for West Antarctic Ice Sheet stability" by Yuzhen Yan et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-7-RC4>, 2021

General comments:

Yan et al. present a reconstruction of the accumulation rate for the S27 ice core in the Allan Hills Blue Ice Area over the Last Interglacial and investigate the moisture evolution and implication on West Antarctic Ice Sheet during this time period. New gas measurements in S27 ice core are presented and combined with previous data from Spaulding et al. (2013). A refined gas chronology has been developed using $\delta^{18}\text{O}_{\text{atm}}$ synchronization with the EDC record and improved using greenhouse gases concentration measurements. Together with the gas and ice ages, the authors used firn densification inverse modeling to estimate the accumulation rate.

Based on this record indicating exceptionally high snow accumulation over the early LIG, the authors proposed several hypotheses to explain this peculiar event as well as associated consequences on the stability of the WAIS. The paper is well-written and easy to read. This study is interesting and worth to be published in Climate of the Past. However, I do have comments which I hope can help improve the manuscript.

The authors made the hypothesis that gas loss affects the $\delta^{18}\text{O}_{\text{atm}}$ in the same proportion in both fractured and non-fractured ice. The gas loss correction uses then the same values over the entire record. It is however not clear if such a difference exists or not in the record. For example in the Figure S4 the $\Delta\delta^{18}\text{O}_{\text{grav}}$ vs $\Delta\delta\text{O}_2/\text{N}_2$ relationship is shown but we don't know if the data are for fractured, non-fractured ice or both. The slope could then maybe be differentiated in two different slopes if the two zones are represented and if a difference between them is observed. In the results part, differences between EDC and S27 $\delta^{18}\text{O}_{\text{atm}}$ are observed for samples older than 145 ka (and especially between 202-210 ka) and the core quality is proposed as explanation. This is supported by the younger ice age than the gas age at similar depth interval suggesting large gas loss. Could a different slope for gas loss correction for the deeper part of the core reduce these differences?

The discussion of possible hypotheses to explain the increase in moisture during the early LIG is convincing but would deserve further comparison with proxy records used to infer changes in moisture source and shift of evaporative regions (in relation with ice-sheet extent, shift of atmospheric/oceanic pattern...) such as δ -excess, T_{site} or T_{source} . Such data exist for other Antarctic ice cores. A comparison to these records could maybe support the proposed hypotheses.

I appreciated the uncertainties part in the Supplementary but I missed some values about the final uncertainty associated with the new gas chronology (even if they are given in the Supplementary Data Table). Few lines could be added to explain the final gas age uncertainty, i.e. $\pm x$ years, how it varies along the record and maybe compare it to the AICC2012 gas age uncertainties (and also refer to it in the main text). Similar comment can be done for the ice age uncertainties. The uncertainties of EDC are assigned to S27 age points during the synchronization of the ice chronology without any estimation or discussion of the range of associated uncertainties and tie-points previously used to develop the ice chronology. This could be developed a bit more.

Other comments:

Line 19: Write "during the LIG maximum".

Line 25: Remove the s in "insS27".

Line 43: Specify that it is for both past and future simulations.

Figure 1: Add WAIS and Taylor Dome to the map.

Line 57: Remove "that of".

Line 59: Detail the time period covered by the new record.

Line 71: Figure S1 instead of S2.

Line 72: Rephrase the sentence. I guess the missing peak in $\delta^{18}\text{O}_{\text{atm}}$ is only because no measurements have been done at these depths.

Line 73: It is not clear what the $\delta^{18}\text{O}_{\text{atm}}$ sampling strategy was. Improve the resolution? Complete missing intervals?

Line 75: Specify that new CO_2 and CH_4 measurements are used to improve the gas chronology between 105-147 ka.

Line 79: Change to "circulation changes and ice shelf / ice-sheet stability during the LIG".

Line 88: Figure S2 instead of S1.

Line 102: Add to the end "to prevent contamination from exchange with ambient air".

Lines 105 and 141: Give the temperature in $^{\circ}\text{C}$ (for consistency).

Lines 110-113: Use " $\delta^{18}\text{O}$ of O_2 ".

Line 113: Give the equation for gravitational fractionation correction.

Line 114: Remove "paleo".

Line 145: This sentence suggests that there are also fractures in the ice above 151 m. Are they numerous? Is there an influence on the $\delta^{18}\text{O}_{\text{atm}}$?

Line 150: Specify between 115-255 ka.

Line 198: Remove the extra parenthesis for δD_{ice} .

Line 205: Wrong units, $\text{kg}\cdot\text{m}^{-3}$.

Line 228: Give the value for h_{diff} .

Line 242: Correct "samples".

Figure 4: Change "per mil" into "‰". You also compare in the main text the $\delta^{18}\text{O}_{\text{atm}}$ variations to orbital variations. Maybe add the insolation curve on the figure.

Figure 5: Add the tie-points and anchor points used for the chronology on the figure. In the caption, precise that CH_4 data are from EDC, the CO_2 is a composite record and the timescale is AICC2012.

Line 260: Remove "at this peak differ from by 2 ppm".

Line 271: To conclude this part on the gas chronology I missed a sentence on the total uncertainty associated with this new chronology. How much is it?

Line 281: "Figure 2" not usefull here.

Lines 321-324: I don't know if we can say that the accumulation rate at S27 is comparable to Vostok and EDC. The trend is similar yes but the absolute value not. And how is the 0.02 m.yr^{-1} value defined?

Lines 338-339: Could you support this hypothesis using model comparison?

Line 341: Remove the values of the accumulation rate, not usefull.

Line 344: Delete "apparently".

Lines 345-350: TALDICE's accumulation rate starts to increase earlier than S27 (and is more similar to Vostok and EDC). As for the magnitude, it is much larger for S27 than for TALDICE. The S27 site is already pretty coastal so I would rather say that the peak in accumulation rate at 128 ka reflects more open-ocean conditions than a transition into a coastal site.

Figure 9: I would have removed the Greenland temperature record and drawn instead the variation in mean ocean temperature from Shackleton et al. (2020). It could also be good to add an insolation curve to have an orbital context to refer to in the discussion. Change "(g) Relative sea-level vs present day".

Line 414: Remove "(~80 km)".

Line 422: Correct "with a high sea-level stand".

In the supplementary:

Line 16: Remove "age".

Exchange the Figure S1 and the Figure S2 to match the order of citation in the main text.

Figure S2: Remove "paleo"

Figure S3: The $\delta\text{O}_2/\text{N}_2$ equation for depth > 148 m has to be corrected in $\delta\text{O}_2/\text{N}_2 = -0.205 \cdot \text{depth(m)} + 24.26$. In the caption, give the exact number 0.0067 for the slope.

Figure S4: It is not clear if the data presented here are only for non-fractured ice or for both non-fractured and fractured ice. Please indicate if this is non-fractured ice or differentiate the data with two regression lines for the two zones.

Figure S6: We don't see the peak in the C.I. modelled $\delta^{15}\text{N}$ at 128 ka. Adjust the y-axis.

In Figures S2, S3, S4, S6, change the "per mil" to "‰" for consistency with the main text.