The Cryosphere Discussions

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Comment on tc-2022-49

Anonymous Referee #2

Referee comment on "Gas isotope thermometry in the South Pole and Dome Fuji ice cores provides evidence for seasonal rectification of ice core gas records" by Jacob D. Morgan et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-49-RC2, 2022

This manuscript deals with paleothermometry based on new measurements of gas isotopes in the South Pole ice core. Some measurements from the Dome F ice core are also presented. The authors are presenting a very thorough description of the methods and present improvements in the precision of d15N and d40Ar measurements which is impressive. Using firn densification modeling combined with the series of measurements of d15N and d40Ar over the South Pole ice core, they propose a reconstruction of the firn thickness and temperature gradient between the top and the bottom of the firn over the period covering 30 to 5 ka. The interpretation of the variation of the temperature gradient in the firn is not easy. Several scenarios are proposed and the authors conclude with the existence of a seasonal bias affecting the gas isotopes record.

This manuscript is well written and details the different steps of the methods and of the reasoning. It should be published within TC. I still have several comments which I think should be addressed before publications.

General comments:

- I suggest to remove the whole section focused on Dome F. It is a bit disconnected from the study of the SP DTz and DCH. This section is also difficult to follow since it is not enough documented (the d15N and d40Ar data are not shown nor the origin and associated uncertainty for the DTz modelled curve). Moreover, if the Dome F data are shown, we may also wonder why we can not have the same for other sites ? showing or not a seasonal rectifier effect.
- The results displayed here raises doubts on the classical interpretation of d15N-excess (Kobashi et al., 2007; Kobashi et al., 2011) in term of surface temperature variations. A discussion revisiting these previous studies should be included here as well as clear recommendations on how to use or not the d15Nexcess for reconstructing past surface

temperature variations.

Comments along the manuscript:

l-25 : Precise which « temperature difference » you are speaking about.

l-41: The authors were aware that the spatial temperature – isotope relationship was a surrogate for the temporal relationship and always tried to check if this was true. So I suggest to replace "thought" by "assumed"

l-136: I do not understand why the 30 minutes delay is important for the reference gas only ? Should it not be also the case for the sample gas ?

Table 1 and associated text: I am not sure how relevant it is to compare the d15N results between "Orsi Ice" and "This study SPC". Indeed, "Orsi" and "This study" obtain the same results on air measurements and the improvements mentioned in the methods section apply on both air and ice. What could explain a better precision only for ice then ?

We can thus wonder if the difference is not simply due to a poorest ice quality in "Orsi" ?

Also, it should be noted that the replicates number by "Orsi" is much larger (169) than for this study (14) which makes the comparison questionable. Can you also provide the numbers of replicate fo the LJA analyses by Orsi ? By the way, given the length of the record presented in Figure 1, I am surprised that Table 1 presents only 14 replicates. This should be better explained. It would also be useful to give the number of replicates for Kobashi's data.

Section 5.2 (first paragraph): The arguments developed here are a bit complicated to follow after the previous section where you explained that DCH is controlled by accumulation rate itself influenced by topography. And here, you say that we expect a link between accumulation rate and temperature. I thus suggest to rewrite this paragraph so that it is coherent with the findings of the previous section.

Section 5.2.1 – you mention that you are using the Dage to make the reconstruction of

temperature and accumulation rate but the Dage model – data fit is not shown (nor any Dage data) and it is thus difficult to follow this discussion. Moreover, when looking at the DTz (REF), it seems that the shape of the record does mainly depend on the d18Oice – can you explain better this reconstruction of temperature ? It is important to show which data are used to constrain the shape of the temperature evolution when DECOUPLE and REF disagree. I also expect that the shape of the Kahle reconstruction is mainly imposed by the water isotopes so actually it is expected that both Kahle and REF scenarios have the same shape. This resemblance should not be taken as a strong argument to discard the surface temperature influence on the DTz scenario.

Figure 6 – is there a way to add the DTz data so that the reader sees immediately that there is a mismatch

l-424: the mechanism is not clearly explained – this part should be rewritten.

- Figure 7: I am confused since the different data seem not 100% coherent with the provided explanations so probably more explanations are needed. If there is a temperature rectifier effect as suggested by the mismatch between model and data on the top 16 m, we expect a difference between d15N and d40Ar at the bottom of the firn which would then lead to a d15Nexcess signal due to seasonal rectification. Here, we see a difference but at 16m depth. Moreover, the d15Nexcess profile shown on the figures 1 and 3 does not show any 15Nexcess signal in the bubbles for the recent period, suggesting no difference between d15N and d40Ar at he bottom of present-day firn. Could the authors then better explain how they link their observation on the firn and the obesrvations in the air bubbles.

l-19 and 20: The addition of the Dome F data are confusing and not helpful in this manuscript. It is a different site (much lower temperature). We have many details on the technique for measuring d15N and d40Ar but the data are not show (only firn data and only in the supplement), only DTz from d15N – d40Ar and the DTz from Buizert method but without much explanation on how it is calculated (from which data, with what kind of uncertainties ?). I suggest removing this section which does not add anything to the manuscript. On opposite, figure 7 can be helpful (but need to be shown over the whole firn depth) and is adapted to this study focused on South Pole (see however previous comment).

References:

Kobashi, T., Severinghaus, J. P., Brook, E. J., Barnola, J. M., & Grachev, A. M. (2007). Precise timing and characterization of abrupt climate change 8200 years ago from air

trapped in polar ice. *Quaternary Science Reviews*, *26*(9-10), 1212-1222.

Kobashi, T., Kawamura, K., Severinghaus, J. P., Barnola, J. M., Nakaegawa, T., Vinther, B. M., ... & Box, J. E. (2011). High variability of Greenland surface temperature over the past 4000 years estimated from trapped air in an ice core. *Geophysical Research Letters*, *38*(21)