Climate of the Past Discussions

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Comment on cp-2022-65

Anonymous Referee #2

Referee comment on "The new Kr-86 excess ice core proxy for synoptic activity: West Antarctic storminess possibly linked to Intertropical Convergence Zone (ITCZ) movement through the last deglaciation" by Christo Buizert et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-65-RC2, 2022

This manuscript presents a compilation of isotopic composition of krypton which has been obtained over the past years in different ice cores from Greenland and Antarctica.

Using the present-day understanding of the drivers of gas repartition in the firn as well as correlation with ERA products, the authors propose that the 86Kr_excess can be used for synoptic activity.

The manuscript is generally well written and well documented. The authors also explained in details the numerous limitations associated with this interpretation both in section 3.3 as well as in the supplements.

Because of the limitations in the interpretation of the 86Kr_excess, the authors should be more cautious in the proposed interpretation. While the scientists in the filed of ice cores will get the limitations and use the results with caution, it may be different for people who do not understand the complexity of processes affecting air elementar and isotopic repartition in the firn. I thus suggest to modify the abstract and the conclusion to insist on the speculative interpretation of the 86Kr_excess and on the additionnal measurements to be done to better quantify the effect of gas loss, thermal diffusion (including rectifier effect) and possible existence of a convective zone.

- 146 : it is strange to refer to Fig 6 here. Moreover, it is strange to prefer one or the other since it is shown later that gas loss and thermal effect are the most important corrections to take into account (d40Ar being sensitive to gas loss and d15N being the most sensitive to thermal fractionation). There is no obvious reason to prefer one notation compared to another.

From l. 223 : the preparartion of the samples is different for DE08-OH than for the other samples. May this explain the different slopes associated with gas loss in figure A1-B.

- 241 : How are the samples flagged for drill liquid contamination ? How is it possible to detect the drill fluid contamination ? From which measurements ?
- 245 – 247 : Can you explain the error propagation explaining why the 2 sigma is larger for 86Kr than for 86Kr_excess ?
- 253 : I do not see why it is useful to present these data to remove them immediatly after. In this case, the 86_Kr data from EDC samples affected by drill fluid should also be displayed with an explanation on how they were discarded.

Section 3.1 : The Phi parameter exhibits strong seasonal and interannual variabilities and I do not understand how this variability is taken into account in the « calibration » of the 86Kr_excess. Such sensitivity should be studied or implemented in Figure 3 since this is crucial for the interpretation of the 86Kr_excess proposed here.

Section 3.3 : this section is interesting in providing the limitations of the interpretation of 86Kr_excess and strongly suggest that further study should be performed for a robust interpretation such as firn air pumping study at different site with a correct determination of the thermal gradient (it is really surprising to find such temperature gradient at DE08 and EDC) + analyses of ice not affected by gas loss, etc… this is the reason why the authors should be much more cautious in their conclusions and better suggest concrete perspectives on how to progress with such proxy if it is reallt promising. Actually, the concluding paragraph of section 3.3 should also be summarized in both the abstract and conclusion of the manuscript to clearly state the limit of this interpretation which is now speculative.

In section 4, I feel that a discussion on the seasonal variability and its possible impact is missing.

Section 5 : I understand that the authors do their best with the poor data quality but it would be nice to comment on the strong scattering for the data at « present-day » ? Can this scattering be used to estimate the uncertainty as the authors mention that « no true replicate to assess the reproducibility » …

- 20 and 21 : the discussion is quite long for such speculative interpretation. I would suggest shorten it to stay on the safe side of the interpretation.
- 725 : I am not sure that the authors really « calibrate » the proxy – let's say that this is a first proposition of interpretation. A calibration would require more dedicated studies as mentionned in the concluding paragraphe of section 3.3.

Figure 3 : What is the origin of the uncertainty bars for the different sites ? Do the sites with more data have more scattering hence a larger uncertainty bar ? It would be useful to mention the number of points used for each sites in this calibration and how the error bar is calculated. A table may be useful to exactly describe the number of samples for each site, depth range, conditions of storage, etc…

Figure A2 : The displayed results show very depleted samples in dO2/N2 and dAr/N2 – are these results really relevant for this paper ? What is the origin of these samples ? core top ? Bottom ice ?

Figure B1 shows that there may be a large scattering with depth of 86Kr-excess. I am sure that this is taken into account in this study but it would be nice to explain a little bit more how it is done (also for the other cores). Probably again a table explaining the number of samples considered for present-day for each core, the depth range and individual values would help.