Comments on “The new Kr-86 excess ice core proxy for synoptic activity: West Antarctic storminess possibly linked to ITCZ movement through the last deglaciation”

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Preprint text cited in this comment is cited in orange color.

Several occurrences: WDC $^{86}Kr_{xs}$ is sometimes noted WD $^{86}Kr_{xs}$.

Page 2 Line 40-41: The abstract should precise whether subpolar jet of Northern Hemisphere or Southern Hemisphere is discussed, as both Greenland and Antarctica are previously mentioned.

Page 5 Line 138 (and 142): in the equations $^{86}Kr_{xs}$ is written as the difference to a thermally corrected $\delta^{40}Ar_{corr}$ (or $\delta^{15}N_{corr}$). While this mirrors the deviation from the gravitational fractionation, it covers the fact that both $\delta^{40}Ar$ and $\delta^{15}N$ are used in the thermally corrected data. I agree that this expression emphasizes the pumping-induced deviation from a gravitational settling, but it should be noted in the main text that three pairs of isotopes are necessary to express the $^{86}Kr_{xs}$.

Mathematically, the notation “per meg ‰$^{-1}$” could be simplified to “‰” and requires clarification. From my understanding the authors want to emphasize that it is normalized by the gravitational fractionation. Perhaps, a mention to why “per meg ‰$^{-1}$” is used should be given along with the “The rationale for including a normalization in the denominator is discussed below.” (**Line 140**)

Page 5 Line 146: Fig. 6 is called before any other figure

Page 11 Lines 346-348: Authors write “Firn models predict that the gravitational disequilibrium effect in elemental ratios (such as δKr/Ar) should be proportional to that in isotopic ratios. However, the observations suggest that the former is usually smaller than would be expected from the latter. We do not have an explanation for this effect.” Does the same reasoning that was done for the $^{86}Kr_{xs}$ apply for elemental ratios? The authors have written just above “krypton is more readily adsorbed onto firn surfaces retarding its movement” (**Page 10 line 333**). Retarding krypton movement could lower its effective diffusive column height, leading to lower gravitational enrichment relative to other elements.

I also have a more open question: Could we theoretically compute a Kr-excess equivalent derived from elemental ratio of Kr/Ar rather than the isotopic ratios of $^{86}Kr/^{82}Kr$, supposing we can discriminate between elemental gas loss from pore closure and elemental ratio changes from the active mixing of firn gases (in addition to thermal and gravitational effects)?

Page 12 Line 389: the authors write: “the gas age distribution at the depth of bubble closure has a width of several years” to discard the influence of sub-annual variations on Kr isotopes. The Kr-86 excess is used as a proxy for deviation from the gravitational equilibrium, which can be seen as “an effective diffusive column height” (**Line 385**). Although the gravitational equilibrium is indeed reach after several years as the gases go through the entire diffusive column height (DCH), I would suppose deviation from this equilibrium may be achieved within much shorter periods, because it relies on kinetic mixing. Then we need to better understand where the Kr-excess signal is acquired. If the entire firn air column is actively pumped out and pushed back in due to the passage of depression system, my guess would be that the kinetic motion would affect the gases depending on the diffusivity in the column (or the inverse of porosity). Could it imprint a new Kr-excess signal directly into the deep firn layers, even if the gases have been effectively isolated from the atmosphere for a longer period and have an age distribution of several years? Or is the entirety of Kr-excess signal acquired at the top of diffusive column through exchanges with the open atmosphere?

Page 14 Line 477: "The green line denotes the latitude of maximum Φ, corresponding roughly to the latitude with the highest storm track density (57.8°S on average)." Should be in the figure 5 caption. Also, in the text "°S" is written with a superscript "o" letter in lieu of a degree sign.

Page 19 Lines 654-657: The authors write "the present-day SAM does not have a statistically significant impact on synoptic variability at WDC (Table 2). Perhaps the SAM is not a good analogue for these past changes in circulation after all, in particular when considering the impact of SHW shifts on Antarctic storminess" to question the fact that "present-day SAM is sometimes suggested as an analogue for past shifts in the meridional position of the SHW and eddy-driven jet" (**line 650**).

I think this justification is not logical. Here, the authors show in their Fig. 5B that the correlation between storminess and SAM is limited to the oceanic regions, and is only weakly correlated on the coastal regions of the Antarctic continent (except a high correlation in the marine-dominated Antarctic Peninsula). This is supported by other studies showing that positive SAM is associated with more frequent cyclones (Grieger et al., 2018), and their location is shifted south but limited to the oceanic regions around Antarctica, with limited impact inland (Pezza et al., 2008). I do agree with the later statement that "synoptic activity at WDC is not sensitive to the SAM" (**line 664**) and this may be true for other sites inland Antarctica.

This does not impede the relation between SAM and westerlies, because the of SAM signature on pressure variability may be restricted to a narrow band of latitudes where SAM-related changes on storm activity is located (north of ~70°S). Modelling and reanalysis studies show that there are clear connections between the SAM phase and the surface SHW strength and position (Marshall and Thompson, 2016), or between SAM and the polar and subtropical jets (Fogt and Marshall, 2020). Confusion may arise from the fact that southward shifts of SHW as reported from Fig. 4A influence the Φ value at WDC, which clearly shows "the impact of SHW shifts on Antarctic storminess" (**line 657**). However, this pattern of wind changes is zonally asymmetric, and resembles more changes associated with the PSA1 as shown in Fig. 5C, with a geopotential high anomaly in the Pacific. Pressure variability (Φ) at WDC may therefore be driven by changes in PSA1. In my understanding this is a complex situation where changes in westerlies related to SAM variability do not influence the storminess at WDC, but other changes in westerlies (mainly PSA1?) may change the storminess at WDC.

I would like to add that even though pressure variability at WDC is not influenced by SAM, some other parameters such as source water for precipitations (as recorded in deuterium excess) are influenced by SAM and may reflect more zonally symmetric changes (Markle et al., 2017; Buizert et al., 2018). Direct comparison of the two proxies in a future study may prove interesting, and here in this study interpretations of Kr-86 excess from WDC should rely more on the geographical extent of the regression shown in Fig. 4A.

Page 26 Line 862: "per meg ‰" is missing an exponent (‰$^{-1}$)

Page 26 Line 867: missing a space in "300m"

Page 38 Line 1222: The contour lines lack a description to discriminate between positive geopotential height anomalies (continuous lines) and negative anomalies (dashed lines).

Page 39 Line 1238: it is noted that "For campaigns 4 and 5 the sample was not split, and no $\delta^{15}N$ data are available". It is unclear if the thermal correction for $\delta^{40}Ar$ was calculated in these campaigns, as Appendix A2 mentions the need for $^{15}N_{xs}$ in this correction.

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