Comments by Aymeric Servettaz.

We want to thank Aymeric for his thoughtful and constructive comments that have improved the manuscript. Our responses below. We wrote our responses in the form of proposed changes to the text that we would make in a potential revised manuscript.

WDC 86 Krxs is sometimes noted WD 86Krxs

Thanks for catching. We now use WDC throughout, and have replaced three instances of "WD".

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.

Good point. We added "Antarctic" $^{86}Kr_{xs}$ and "southern hemisphere".

Page 5 Line 138 (and 142): in the equations 86Krxs is written as the difference to a thermally corrected δ40Arcorr (or δ15Ncorr). While this mirrors the deviation from the gravitational fractionation, it covers the fact that both δ40Ar and δ15 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 86Krxs.

We have added the following to the text (below Eq 3):

Note that both definitions rely on having measurements of three isotope ratios (δ^{86}Kr, δ^{40}Ar and δ^{15}N), as the thermal correction requires δ^{40}Ar and δ^{15}N be known

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)

Agreed. We added:

This unit (per meg ‰$^{-1}$) is mathematically identical to ‰, but we use it to emphasize the normalization in the denominator.

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

We have removed this reference to Fig. 6

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 86Krxs 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

Yes, adsorption could perhaps explain the observation in case there is no isotopic fractionation associated with this process. We added:

As before, adsorption of Kr onto firn grain surfaces may contribute to the observed discrepancy, and laboratory tests of this process are called for. Further, the impacts of gas loss are greater on elemental ratios than on the isotopic ratios which may contribute also.

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 86Kr/82Kr, 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)?

This is an interesting question, and in theory it would be possible. The isotopic definition is preferred for multiple reasons:

First of all, measuring isotopic ratios is more precise than measuring elemental ratios. For isotope ratios, all masses are monitored simultaneously in the IRMS on different cups at a single magnet setting. For elemental ratios we have to rely on so-called “peak jumping”, where the IRMS magnet is switched. This means that isotope ratios can be measured to a greater precision that elemental ratios can.

Second, argon suffers from gas loss in ice core samples, which impacts the Kr/Ar ratio more than the $^{40}Ar/^{36}Ar$ ratio. This complicates a definition of disequilibrium based on elemental ratios. Besides the artifactual gas loss from samples in storage, there is indeed the natural size fractionation during close-off that the reviewer refers to that one would also have to account for.

Last, the definition of disequilibrium requires a comparison between two gravitational ratios. A disequilibrium proxy based on elemental ratios would for example compare the Kr/Ar ratio to the Ar/N2 ratio. There would be a smaller contrast in diffusion rates, which makes the proxy less sensitive.

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?

This is a very good question that goes to the heart of the current difficulty in interpreting this proxy. We fully agree with the reviewer sentiment that we need to better understand where within the firn column the Kr-excess signal is acquired. If the pores that facilitate the barometric pumping flow represent a small fraction of the entire firn cross-section, then indeed a single barometric pumping event could conceivably introduce a large amount of unfractionated air deep into the firn, and impact the atmospheric composition close to pore close-off. But more likely, the barometric pumping displaces air in the deep firn over much shorter distances, providing a longer integration time of the barometric signal. In response to this comment, we modified the text. We now state that the seasonal variation in storminess as an explanation for the observations “seems improbable to us at present”. We indeed cannot rule out a scenario as sketched by the reviewer.

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

This was mentioned in the caption under panel 5A, but we now specify that it holds for all panels. The use of a superscript “o“ is out of laziness, as it is faster to type in MS word. We fixed this instance pointed out by the reviewer, but cannot rule out other instances in the text. We trust this will be fixed by the copy editor during typesetting.

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.

It is unclear to us what the reviewer is suggesting we do or change here in response to this comment.

The SAM is commonly defined in terms of sea level pressure and not in terms of the actual atmospheric dynamics. Sea level pressure integrates over dynamics in the entire atmospheric column. Our point is merely that the modern-day interannual variance in the SHW (the "SAM") is dominated by internal variability, whereas changes to the SHW on orbital timescale are driven by the energy distribution at the surface. Due to geostrophy any shift in the SHW, regardless of its origin or dynamics, will map onto the SAM index as conventionally defined. The SAM index by itself is simply not a good tool to discuss the *dynamics* of the SHW, particularly on longer timescales. Internal month-to-month SAM variability is expected to occur even when the mean position of the SHW were to be shifted due to a change in orbital configuration. It is also confusing that in common usage the SAM *index* and the internal mode of variability that leads to variations in the SAM index are both referred to simply as "the SAM".

In response to the reviewer comment we have rewritten the paragraph to make our point more clearly. It may have been confusing the start the paragraph noting that the SAM is an analogue for past shifts in the SHW – which it probably is not. We have changed this in the revised text. We have made further edits for clarity:

"The SAM index reflects the meridional position of the SHW and eddy-driven jet. During positive SAM phases the SHW are displaced poleward, and during negative phases equatorward. Present-day month-to-month changes in SAM index represent a mode of internal variability, with anomalies persisting for only weeks to months – the timescale is longest in late spring and early summer reflecting a stronger planetary wave–mean flow interaction (Simpson et al., 2011; Thompson and Wallace, 2000). By contrast, shifts in the ITCZ and SH jet structure on millennial and orbital timescales have a much longer lifetime and different dynamics, being driven from the tropics via hemispherically asymmetric changes in Hadley cell and STJ strength. Therefore, present-day SAM internal variability is not expected to be a good analogue for past changes in SHW position. We find that the present-day SAM month-to-month internal variability mainly impacts synoptic variability over the Southern Ocean and does not have a statistically significant WDC (Table 2). Such variability is likely to have occurred during other climatic regimes also, possibly just centered around a mean SHW position that is displaced meridionally relative to today. At first glance it may appear contradictory to state, as we do, that synoptic activity at WDC is not sensitive to the SAM while also suggesting that during the last deglaciation synoptic activity at WDC is linked to changes in the position of the SH eddy-driven jet and westerlies. Based on the considerations above, both claims may be true without contradiction."

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

Fixed!

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

Fixed!

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

Thanks, we added this into the caption.

Page 39 Line 1238: it is noted that "For campaigns 4 and 5 the sample was not split, and no δ15N data are available". It is unclear if the thermal correction for δ40Ar was calculated in these campaigns, as Appendix A2 mentions the need for 15Nxs in this correction.

Good point. We added:

"Thermal corrections in the WDC $^{86}Kr_{xs}$ records are based on firn model simulations."