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Dear Editor T. J. Fudge,

With this cover letter, we are submitting the revised manuscript entitled, "**Imprints of Sea Ice, Wind Patterns, and Atmospheric Systems on Summer Water Isotope Signatures at Hercules Névé, East Antarctica**", for publication in *The Cryosphere*. Based on the comments from the editor and the two reviewers, we have major changes of the manuscript, which are detailed below.

Reply to the comments by the reviewer 2

Reviewer #2: *The paper by Kim et al. investigates the summer (DJF) isotopic characteristics of an ice core from Hercules Névé, East Antarctica, using $\delta^{18}O$, $\delta^{2}H$, and deuterium excess (d-excess) measurements. The authors explore the relationships between these isotopes and large-scale atmospheric and oceanic drivers—including the Amundsen Sea Low (ASL), Zonal Wave-3 (ZW3), sea ice concentration (SIC), sea surface temperature (SST), and ERA5 reanalysis fields (e.g., wind and temperature)—over the period 1979–2015. The study finds that higher temperatures and precipitation during summer are associated with isotopic enrichment, while d-excess shows a positive correlation with SIC and a negative correlation with SST. The authors interpret these relationships in terms of regional moisture source variability and synoptic-scale atmospheric transport.*

This study uses relevant literature and includes appropriate data sets. Some areas of improvements are suggested below. Major revisions are recommended.

Answer: We thank the reviewer for carefully summarizing our manuscript and providing constructive feedback on our analysis of summer (DJF) isotope characteristics from Hercules Névé. In the revised manuscript, we will explicitly clarify and enhance the discussions on how our isotopic data ($\delta^{18}O$, $\delta^{2}H$, d-excess) relate to large-scale atmospheric and oceanic drivers, including the Amundsen Sea Low (ASL), Zonal Wave-3 (ZW3), sea ice concentration (SIC), sea surface temperature (SST), and ERA5 reanalysis fields (temperature, precipitation, wind patterns). Specifically, we will elaborate in greater detail on how the observed relationships, such as the positive correlation between $\delta^{18}O$ and temperature/precipitation, and the significant correlations of d-excess with SIC (positive) and SST (negative), support interpretations regarding regional moisture source variability and synoptic-scale atmospheric transport. To further enhance interpretive clarity, we will explicitly discuss the mechanistic pathways linking atmospheric circulation patterns (ASL, ZW3) to isotopic variability, supported by additional relevant literature. We thank the reviewer for the careful summary of our manuscript and for the constructive suggestions that follow. Below, we provide detailed responses to each of the reviewer's comments.

Moreover, in response to the reviewer's valuable suggestions, we will provide quantitative statistical evidence demonstrating the reliability of ERA5 reanalysis in representing local climatic conditions at Hercules Névé. Specifically, we will include a detailed comparison of ERA5 and AWS temperature data using a York regression analysis, clearly illustrating a strong linear relationship (slope = 1.09, R^2 = 0.57), presented in Supplementary Figure S1C. We will also include wind roses comparing ERA5 wind data with AWS observations (Supplementary Figure S2), further supporting the suitability of ERA5 data for our regional climate analysis. Additionally, we acknowledge the reviewer's concern regarding post-depositional processes. To explicitly address this, we will include a brief discussion based on the $\delta^{18}O$–d-excess relationship diagnostic method outlined by Casado et al. (2018). This analysis indicates a consistently positive slope, suggesting minimal post-depositional isotopic alteration at our site. Collectively, these revisions will significantly improve the manuscript's interpretive clarity and strengthen our isotopic interpretations.

Major revisions

1. This study would benefit from expanding beyond summer to include annual and seasonal composites, especially if the goal is to understand long-range transport.

Answer: We appreciate this thoughtful suggestion. In the revised manuscript, we will explicitly acknowledge that analyzing annual and other seasonal composites could provide broader climatological insights, particularly related to long-range transport processes. However, we will clearly articulate our rationale for focusing specifically on the austral summer months (DJF).

Specifically, we will emphasize two key reasons for our decision. First, DJF represents the period when the Amundsen Sea Low (ASL) reaches its climatologically strongest and most stable state (Turner et al., 2013; Raphael et al., 2016), coinciding with minimum coastal sea ice extent, which maximizes moisture exchange from open ocean surfaces. Consequently, the isotopic response to large-scale atmospheric variability is expected to be most clearly captured during this period. Second, based on ERA5 reanalysis, DJF accounts for approximately 35% of the annual precipitation at Hercules Névé, highlighting its substantial climatological significance for interpreting isotopic variability.

We will explicitly mention these points in the revised manuscript to transparently clarify our research scope and justify our seasonal focus.

2. The study dismisses post-depositional processes without sufficient literature support. This may be true, but better justification is necessary.

Answer: We appreciate the reviewer's concern regarding the potential influence of post-depositional processes. This point is relevant to what the first reviewer suggested in general comments. In the revised manuscript, we will explicitly address this issue by referring to the diagnostic approach proposed by Casado et al. (2021), who investigated the impacts of surface snow metamorphism, primarily driven by sublimation and re-condensation, on Antarctic snow isotopic composition. Specifically, Casado et al. (2021) demonstrated that significant post-depositional alteration typically manifests as a negative correlation between $\delta^{18}O$ and d-excess values.

Applying this diagnostic method to our own raw isotope data, we found that both the entire dataset and the DJF subset exhibit consistently positive correlations between $\delta^{18}O$ and d-excess. This pattern contradicts the signature of post-depositional modifications described by Casado et al. (2021), strongly suggesting that the isotopic signals at Hercules Névé predominantly represent direct precipitation inputs rather than significant metamorphic alterations. Furthermore, we will explicitly note that the relatively high accumulation rates during DJF and associated rapid burial of surface snow layers likely further minimize the exposure to sublimation and isotopic diffusion. Incorporating these findings, we will add a brief summary along with the relevant citation (Casado et al., 2021) to the revised manuscript, clearly demonstrating our careful consideration of potential post-depositional effects.

3. Language describing the 'cause and effect' between large scale patterns with each other, the meteorological fields and the ice core d18O/dxs records are often overstated. Most of these are at best associations, or the influences require better evidence-based justifications.

Answer: We appreciate the reviewer's important concern regarding potentially overstated causal language. In the revised manuscript, we will carefully adjust our language to clearly avoid implying overly strong causal relationships. Specifically, we will replace terms such as "influenced by" or "driven by" with more appropriate and statistically justified terms such as "associated with," "related to," or "coherent with."

Additionally, to provide stronger evidence-based justification for the associations we observe between the isotopic records and large-scale meteorological patterns, we will explicitly present three complementary analyses in our revised manuscript, which are suggested by the first reviewer:

(1) Empirical Orthogonal Function (EOF) analysis of 500 hPa geopotential height fields, clearly identifying spatial circulation modes relevant to our region;

(2) Principal Component Analysis (PCA) of key climatic variables (temperature, accumulation, and ASL indices) in relation to $\delta^{18}O$, providing a robust statistical basis for interpreting the observed correlations;

(3) Back-trajectory analysis (using HYSPLIT) clearly comparing moisture source pathways during years characterized by intensified versus weakened ASL conditions, thereby offering direct physical evidence for the observed isotopic associations.

These analyses will collectively strengthen the statistical and physical rationale behind our interpretations, clearly framing our findings as evidence-based associations rather than direct cause-and-effect relationships.

4. A more rigorous treatment of atmospheric dynamics (e.g., via 500 hPa height fields and moisture source diagnostics), incorporation of modern statistical tools (e.g., York regression, multi-linear feature analysis), and a clearer, more defensible linkage between the isotopic signal and proposed synoptic drivers are recommended.

Answer: We appreciate this reviewer's valuable suggestions regarding enhancing the analytical rigor of atmospheric dynamics and statistical interpretations. This point is relevant to what the first reviewer suggested. In the revised manuscript, we will explicitly incorporate the following methodological enhancements:

(1) Atmospheric dynamics: We will perform an Empirical Orthogonal Function (EOF) analysis using ERA5 500 hPa geopotential height fields, clearly identifying spatial circulation patterns and comparing them with $\delta^{18}O$ variability to better understand the large-scale atmospheric context of our site (Figure S6).

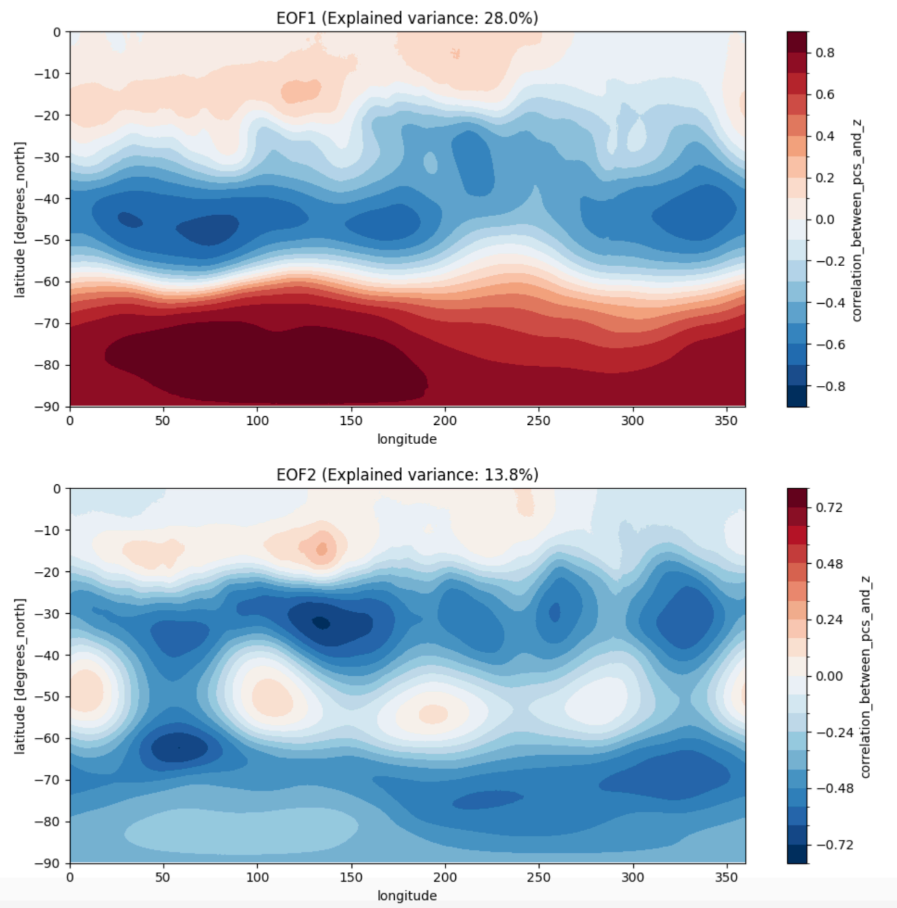

Figure S6. Empirical Orthogonal Function (EOF) analysis of mean sea level pressure (MSLP) during the austral summer (DJF: December–January–February) using monthly data.

The first mode (EOF1, top) explains 28.0% of the total variance and captures large-scale zonal pressure contrasts, consistent with patterns related to the Southern

Annular Mode (SAM). The second mode (EOF2, bottom) explains 13.8% and reflects more regional or asymmetric structures. Colors indicate the spatial correlation between MSLP anomalies and the corresponding principal component (PC) time series.

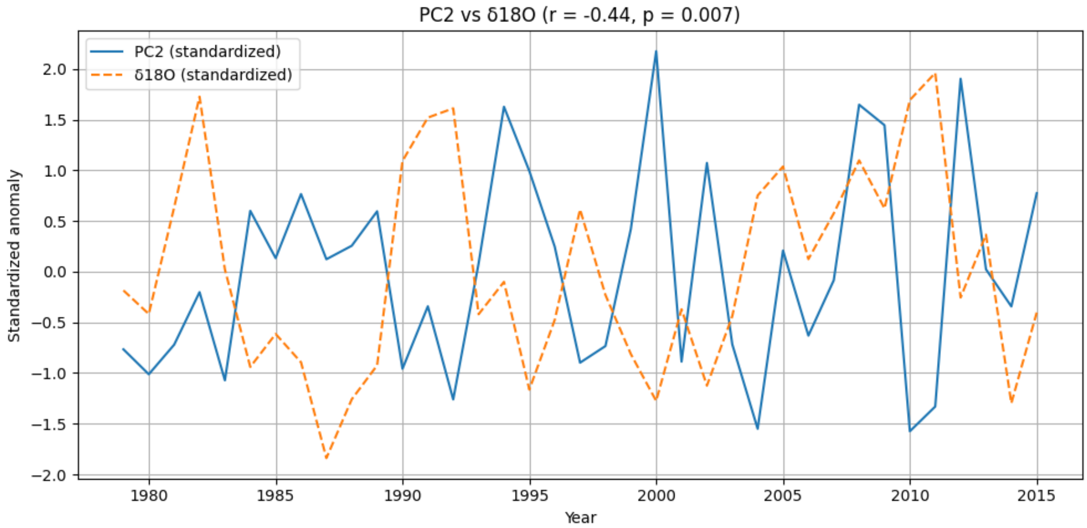

Figure 8. Temporal correlation between the second principal component (PC2) of DJF mean sea level pressure (MSLP) and standardized $\delta^{18}O$ anomalies from the ice core. PC2 corresponds to the second EOF mode shown in Figure Y, which captures subpolar and regional MSLP variability. A significant negative correlation is observed ($r = -0.44$, $p = 0.007$), indicating that $\delta^{18}O$ values are influenced by the large-scale circulation associated with this mode.

(2) Statistical rigor and multi-variable analysis: To robustly assess statistical relationships between $\delta^{18}O$ and multiple climatic drivers, we will explicitly apply Principal Component Analysis (PCA), using $\delta^{18}O$ as the dependent variable and annual mean temperature, accumulation, and ASL indices as explanatory variables. This will enhance the defensibility of our interpretations by clearly quantifying the contributions of different climate factors.

Table 1. PCA loadings of ASL parameters (Actual central pressure, latitude, longitude) from Hosking et al. (2013) datasets. Loadings show variable contributions to each principal component.

Variable	PC1	PC2	PC3
ActCenPres (strength)	0.52	0.69	0.50
Longitude	0.50	–0.73	0.47
Latitude	0.69	0.01	–0.72

(3) Moisture source diagnostics: We will explicitly present a detailed back-trajectory analysis (using HYSPLIT), specifically contrasting moisture source pathways in years characterized by intensified versus weakened ASL conditions. This will clearly provide physical evidence for shifts in moisture transport pathways linked to observed isotopic variability.

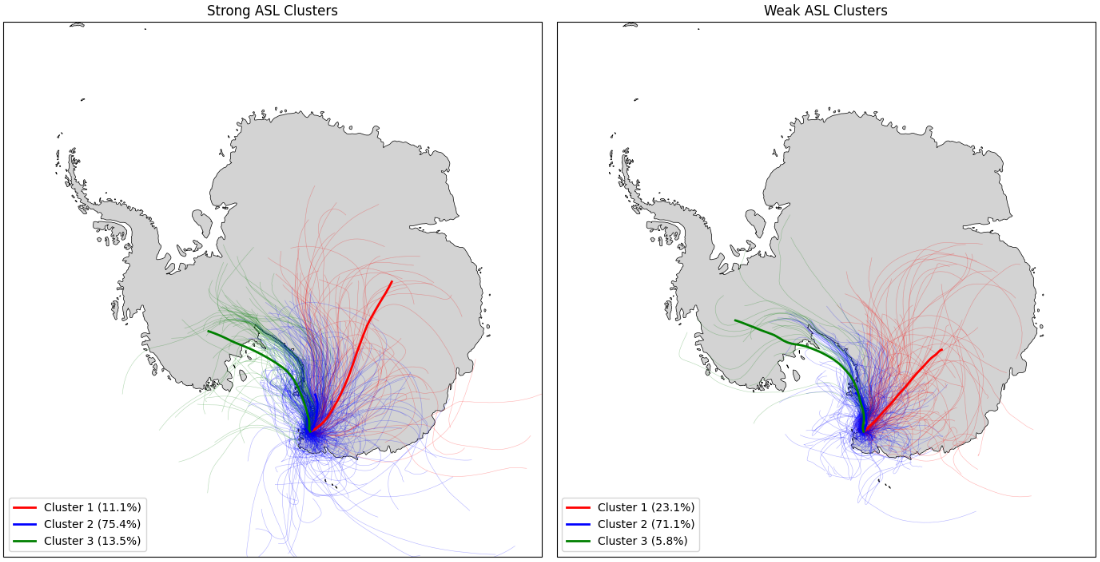

Figure 7. 7-day HYSPLIT back-trajectory clusters arriving at Hercules Névé during (left) the five strongest ASL summers and (right) the five weakest ASL summers (DJF, 1979–2015). Strong ASL cases show dominant air mass transport from inland Antarctica and the Ross Ice Shelf, while weak ASL cases are associated with zonal trajectories originating from the Amundsen Sea and lower-latitude ocean sectors.

(4) Modern statistical methods: To quantitatively evaluate the consistency between ERA5 reanalysis and in-situ AWS meteorological data, we will explicitly include a York regression analysis comparing temperature measurements, taking into account uncertainties in both datasets. This analysis will be clearly presented in the Supplementary Material (Figures S1C and S2), strengthening the reliability of our climatological interpretations.

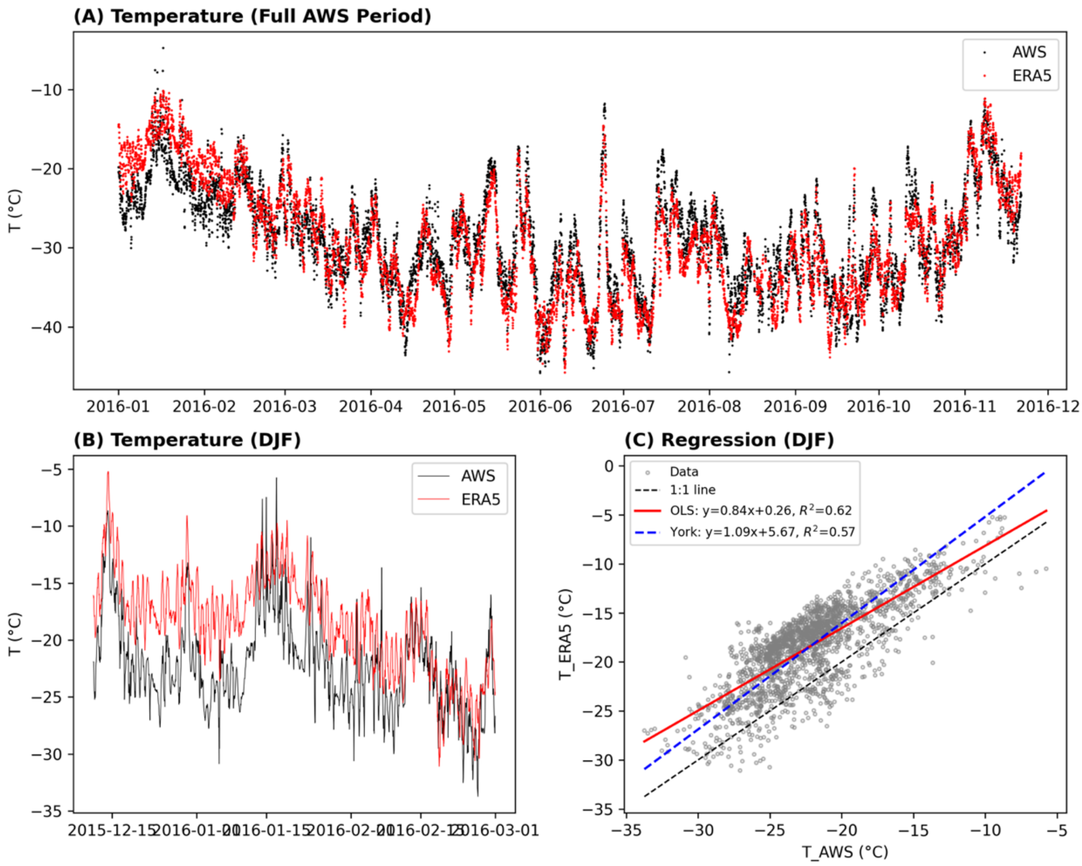

Figure S1. Comparison of AWS and ERA5 2 m air temperature at Hercules Névé. (A) Daily temperature from December 2015 to December 2016. (B) Temperature time series during the DJF period. (C) Regression analysis during DJF, showing the relationship between AWS and ERA5 values using both ordinary least squares (OLS) and York regression. ERA5 generally overestimates summer temperatures compared to AWS.

Collectively, these methodological enhancements will explicitly address the reviewer's recommendations, significantly strengthening our analytical rigor and the interpretative clarity of the isotopic signals and propose synoptic-scale atmospheric drivers.

Minor revisions and expansion on these major recommendations are below.

Line 57 - expanded sea ice does not shift moisture sources to colder regions necessarily, but rather lower latitudes. Expanded sea ice induces more along path depletion.

Answer: Thank you for clarifying this important point. This point is related to what the first reviewer pointed out in general comments. In the revised manuscript (line 57), we will explicitly correct our previous wording and clearly state that expanded sea ice does not necessarily shift moisture sources to colder regions, but rather shifts these sources to lower latitudes, increasing the transport distance. We will further clarify that this longer transport pathway enhances isotopic depletion (along-path depletion). This revision will accurately reflect the physical mechanism involved.

ZW3 - throughout this paper ZW3 is discussed as 'causing' or 'affecting' things. However, it is statistical representation of things such as the ASL, which itself is result of sea level pressure averaging. This language should be scaled back.

Answer: Thank you for highlighting this important issue. In the revised manuscript, we will carefully adjust the language throughout to avoid implying causality when discussing ZW3. Specifically, we will replace terms like “causing” or “affecting” with more accurate statistical descriptors such as “associated with,” “related to,” or “reflecting.” This will appropriately reflect that ZW3 is a statistical representation derived from spatially averaged sea-level pressure fields and ensure our interpretations are more precisely aligned with this understanding.

lines 74-75 - katabatic winds happen all year long, but more in the winter.

The last paragraph starting at line 71 has an unsatisfying defense for only studying DJF. It is possible to assess the impact of summer months. There must be better reasons for looking at only summer months. Also the latter half of February is sometimes consider autumn in parts of the Antarctic. In general, it seems as if this whole study could be expanded to the annual cycle, with seasonal composites. The seasons should be well-defined either by temperature and/or isotopes (d18O).

Answer: We appreciate the reviewer’s detailed comments and suggestions. In the revised manuscript (lines 74–75), we will explicitly correct the description of katabatic winds to clearly indicate that they occur throughout the year but are stronger and more frequent during winter months.

Furthermore, we acknowledge the reviewer’s concern about our justification for focusing exclusively on the DJF period. In response, we will significantly expand our rationale in the revised paragraph starting from line 71. Specifically, we will clearly outline why DJF provides the most favorable conditions for interpreting isotopic variability in the Hercules Névé region. This justification will explicitly include: (1) the seasonal minimum in sea ice extent, enhancing marine moisture availability; (2) higher accumulation rates during DJF, which promote better preservation of isotopic signals by reducing the exposure time to sublimation and metamorphic processes; and (3) the climatologically strongest and most stable state of the Amundsen Sea Low (ASL) during DJF, providing clearer connections between large-scale atmospheric variability and local isotopic signals.

Regarding the seasonal definitions, we recognize that late February is sometimes classified as autumn in certain Antarctic regions. To clarify our analysis, we will explicitly state in the manuscript that we adopt DJF as the austral summer definition consistently based on established climatological practices, while noting this potential ambiguity. Although we agree that expanding our analysis to include the full annual cycle with clearly defined seasonal composites would add valuable context, this broader scope will be explicitly proposed for future studies. We will clearly articulate these points in the revised manuscript to provide a more robust justification for our current seasonal focus.

Overall, the readers need more environmental context for this ice core location beyond AWS temperatures for one year. E.g., multi-year accumulation rates, temperatures, wind speeds and directions from ERA5 provided the context in S1.

Answer: Thank you for this valuable suggestion. We explicitly provided broader environmental context for the Hercules Névé ice core location by incorporating comprehensive analyses based on ERA5 reanalysis data spanning from 1979 to 2015. Specifically, we will include multi-year climatological statistics of surface temperatures, accumulation rates, wind speeds, and prevailing wind directions derived from ERA5, clearly presented in the Supplementary Material (Figures S1–S3). These additions will significantly enhance the environmental context and strengthen the interpretative framework for our isotopic results beyond the limited one-year AWS temperature data.

lines 113-114. The link between high accumulation and drilling season is not clearly established drilling during austral-summer season' does not track with high accumulation rate. The region is one of complexity wrt to when and where accumulation comes from. The high accumulation rate makes this an ideal location to do seasonal and possibly subseasonal isotope studies. Please provide numbers of previous results here on accumulation rate - this will make the 5 cm section number on line 120 more immediately meaningful.

Answer: Thank you for pointing out this issue. In the revised manuscript, we will remove the potentially confusing reference to the austral-summer drilling season from lines 113–114, as it does not clearly establish a relationship with high accumulation rates. Additionally, as suggested, we will explicitly include numerical values for previously reported accumulation rates at Hercules Névé to provide a clearer context. Specifically, we will state that the annual accumulation rate at this site has been estimated as approximately 204.5 ± 54.5 kg m^{-2} y^{-1} (1979–2015 average), clarifying the relevance and resolution provided by the chosen 5 cm sampling intervals mentioned on line 120. This revision will significantly enhance clarity and the immediate interpretability of our sampling strategy.

From the information around line 185 it seems like 5 cm of snow/ice represents about ~1/8 year, or a little more than one month (not accounting for compression properly here). Do I understand the data correctly? It would be good to put this in context for the reader.

Answer: We thank the reviewer for this insightful observation. In the revised manuscript, we will explicitly clarify how our 5 cm sampling resolution corresponds temporally. Specifically, due to the relatively high accumulation rate at Hercules Névé (~204.5 ± 54.5 kg m^{-2} y^{-1}), our annual layer counting indicates that each year typically comprises 5–15 individual data points. This translates to a temporal resolution of approximately one month or slightly finer for each 5 cm interval.

To ensure that this resolution effectively preserves seasonal isotopic signals and is not substantially affected by post-depositional diffusion, we will explicitly present an empirical estimate of the diffusion length. Following Münch et al. (2016), our spectral fitting analysis of the $\delta^{18}O$ data yields a diffusion length of about 6 cm—very close to our sampling interval. We will explicitly discuss this result, clearly indicating that this supports our interpretation that seasonal isotopic signals are sufficiently preserved at our sampling resolution. This additional clarification will be provided in the revised manuscript, supported by the diffusion length estimation and appropriate references.

line 135. This may be relatively 'warm' polar accumulation, but it is best to use the logarithmic definition of dexc.

Answer: We thank the reviewer for this insightful suggestion. In the revised manuscript, we will explicitly acknowledge that using the logarithmic definition of d-excess may provide greater accuracy under certain polar conditions. To carefully evaluate its potential relevance for our data, we compared results obtained from both the conventional and logarithmic definitions of d-excess. Our preliminary analysis clearly indicates that differences between the two definitions are negligible and do not materially alter our interpretations or conclusions. Consequently, we will retain the conventional definition of d-excess in the revised manuscript

Figure S1 could also include local winds (wind rose) from observations and ERA5, and some histograms of the values. S1A is not that meaningful without either a residual subplot or some mean values. The scatterplot of S1B helps here, but is hides information about when and how the differences occur over this year. It also that the authors used an OLS regression for S1B. They should consider using a York regression that will minimize errors in both variables. Otherwise, the slope will be too shallow. This is still a common oversight in observational statistics.

Trappitsch, R., Boehnke, P., Stephan, T., Telus, M., Savina, M. R., Pardo, O., Davis, A. M., Dauphas, N., Pellin, M. J., and Huss, G. R.: New Constraints on the Abundance of 60Fe in the Early Solar System, Astrophys. J., 857, L15, https://doi.org/10.3847/2041-8213/aabba9, 2018.

Answer: We thank the reviewer for these valuable suggestions regarding Supplementary Figure S1. In the revised manuscript, we will clearly state that our temperature comparison between AWS and ERA5 is based on a single year of overlapping observations (2015–2016), and we will explicitly apply a York regression to account for uncertainties in both variables, clearly presenting the updated results in Supplementary Figure S1B. This approach will correct for potential biases inherent in ordinary least squares (OLS) regression.

Additionally, to enhance the environmental context and clearly address the reviewer's suggestions, we will include wind rose plots derived from both AWS and ERA5 data, as well as histograms depicting the distribution of temperature and wind values from both observational and reanalysis datasets. These additions will significantly enhance the informational content and interpretive clarity of Supplementary Figure S1.

line 232 - Why would post-depositional processes be reduced in the summer? Town et al. (2008) show that warmer temperatures would increase post-depositional *processes. Is there a trade-off on higher summertime accumulation rate? This puts more importance on showing the seasonal cycle of the accumulation rate for this site to make your point here. In any case, some reference and better reasoning is necessary to back up this claim here. There are some Antarctic references available in this regard.*

Town, M. S., Waddington, E. D., Walden, V. P., and Warren, S. G.: Temperatures, heating rates and vapour pressures in near-surface snow at the South Pole, J. Glaciol., 54, 487–498, https://doi.org/10.3189/002214308785837075, 2008

Casado, M., Landais, A., Picard, G., Arnaud, L., Dreossi, G., Stenni, B., and Prié, F.: Water Isotopic Signature of Surface Snow Metamorphism in Antarctica, Geophys. Res. Lett., 48, e2021GL093382, https://doi.org/10.1029/2021GL093382, 2021.

Answer: Thank you for this valuable comment. In the revised manuscript, we will fully acknowledge that sublimation and snow metamorphism processes can indeed be enhanced under warmer summer conditions, as clearly demonstrated by previous studies such as Town et al. (2008) and Casado et al. (2021). The references suggested by the reviewer are explicitly cited in the manuscript now.

Moreover, we will clearly address this issue by presenting the slope-based diagnostic approach from Casado et al. (2021), which assesses post-depositional isotopic alterations through $\delta^{18}O$–d-excess relationships. Specifically, our DJF data consistently exhibit positive $\delta^{18}O$–d-excess slopes, contrary to the negative slopes indicative of significant post-depositional modifications described in Casado et al. (2021), suggesting minimal isotopic alteration at our study site.

Additionally, we will explicitly highlight seasonal accumulation patterns derived from ERA5 reanalysis data, clearly demonstrating that DJF accounts for approximately 35% of the total annual precipitation at Hercules Névé. We will emphasize that this relatively high summertime accumulation likely facilitates rapid burial of surface snow, minimizing surface exposure and offsetting the potential increase in sublimation and snow metamorphism due to warmer temperatures.

These clarifications, supported by the reviewer's recommended references, will be explicitly incorporated into the revised manuscript to robustly justify our reasoning regarding minimal summertime post-depositional isotopic effects.

Section 3.2

What is the seasonal cycle of surface pressure in this region according to ERA5?

Answer: Thank you for raising this important question. In the revised manuscript, we will explicitly clarify that our current analysis specifically focuses on the DJF period because of its climatological relevance for isotopic signal preservation and ocean–atmosphere interactions (e.g., peak ASL intensity, minimum sea ice extent, and enhanced precipitation). Consequently, we have not conducted a direct seasonal analysis of surface pressure at our site.

However, to address the reviewer's concern and provide broader atmospheric context, we will explicitly include an Empirical Orthogonal Function (EOF) analysis of ERA5 surface pressure fields across the Antarctic region, directly comparing the resulting spatial circulation patterns with our isotopic data. This additional analysis will offer meaningful insights into how regional and continental-scale pressure variability relates to isotopic variability at Hercules Névé. Furthermore, we will clearly state our intention to incorporate a comprehensive seasonal cycle analysis of local surface pressure in future work, especially as we expand beyond DJF.

line 240-242 - What analysis package is used here in section 3.2 (figures 3/4)? Is this EOF/PCA? How are summer and winter defined in the ice core(s) and indexed to any meteorology time series here (this is a tricky process, especially in the presence of post-dep processes which may or may not be a factor. A broad literature base exists for this problem alone.)

Answer: We thank the reviewer for this insightful question. In the revised manuscript, we will explicitly clarify in Section 2.3.2 that Figures 3 and 4 are based on spatial Pearson correlation analyses conducted between DJF-averaged $\delta^{18}O$ and deuterium excess (d-excess) values from the ice core and ERA5 gridded climate variables (temperature, precipitation, wind components, sea ice concentration (SIC), and sea surface temperature (SST)). These analyses do not involve EOF or PCA methods; however, we will clearly indicate in the methods section that EOF analyses are separately performed on ERA5 pressure fields for additional atmospheric context.

Regarding the seasonal definitions, we will explicitly describe our method in Section 2.3.1. Specifically, each annual layer was dated using seasonal variations in $\delta^{18}O$, assigning the $\delta^{18}O$ maximum to January, and monthly positions within each annual layer were linearly interpolated. DJF values were subsequently extracted and averaged for each year, constructing a consistent seasonal time series from 1979 to 2015. As our study specifically focuses on austral summer (DJF), winter data are not analyzed in Figures 3 or 4.

To carefully address concerns about potential impacts from noise and post-depositional processes, we will explicitly include further discussion in the revised Methods and Results sections. We will present a diffusion-length analysis using spectral fitting following Münch et al. (2016), which yields a diffusion length (~6 cm) comparable to our sampling interval (5 cm). Additionally, we will include the diagnostic approach from Casado et al. (2021) based on the $\delta^{18}O$–d-excess slope, explicitly demonstrating that our dataset shows no significant evidence of post-depositional alterations during DJF. These additions will clarify that the seasonal isotopic signals analyzed in Figures 3 and 4 are minimally impacted by noise or post-depositional effects.

Section 4.1. The dxs results should be presented in the results section first. Their implications go in the discussion.

Answer: Thank you for this helpful suggestion. In the revised manuscript, we will explicitly reorganize the presentation of the deuterium excess (d-excess) results.

Specifically, we will relocate the d-excess findings currently presented in Section 4.1 (Discussion) to a newly created subsection (Section 3.3) within the Results section, clearly separating the empirical findings from their interpretations. Consequently, the Discussion section (Section 4.1) will then focus exclusively on interpreting the implications of the observed relationships between d-excess, sea ice concentration (SIC), and sea surface temperature (SST). This structural change will improve clarity and ensure a logical flow between our results and their subsequent discussion.

dxs can change after deposition, even if d18O does not (Town et al. 2024; https://doi.org/10.5194/tc-18-3653-2024)

Answer: We appreciate the reviewer's follow-up on this important point. In the revised manuscript, we will explicitly address the possibility that d-excess can change independently after deposition, even if $\delta^{18}O$ remains stable, as described by Town et al. (2024). As outlined in our response to the earlier comment on line 232, we will present clear evidence based on the diagnostic approach proposed by Casado et al. (2021), demonstrating that significant post-depositional alterations to d-excess are unlikely in our dataset, particularly during the DJF period. We will explicitly clarify this point and include the relevant references in the revised manuscript.

line 280 - this claim about polynyas-derived (I would rather say polynya-influenced) air masses dominating the regional isotope signature is not supported by evidence. It is a fine hypothesis to pursue, but requires either evidence from this paper or direct references. It seems that some of this discussion about dxs maybe should be in the background? In any case, this work ignores the growing literature base on antarctic atmospheric rivers (of low-latitude origin), which I think is a dominate influence on isotopic content of moisture laden air coming to the Antarctic. Most Antarctic-bound atmospheric rivers do not become so as a result of polynyas - although I do not disagree that polynyas are a strong local source of water vapor to passing air masses.

Answer: Thank you for highlighting this important issue. In the revised manuscript, we will explicitly revise the wording in line 280 to clearly indicate that air masses are "polynya-influenced" rather than "polynya-derived," framing this as a plausible hypothesis rather than a definitive conclusion due to the current level of evidence available. To strengthen our discussion, we will add references to prior studies addressing the potential role of polynyas in influencing isotopic enrichment in coastal Antarctic regions.

Additionally, regarding the role of Antarctic atmospheric rivers (ARs), we will explicitly acknowledge their general importance in moisture transport toward Antarctica. However, we will clearly state, supported by recent literature (Shields et al., 2022; Wille et al., 2022; Hofsteenge et al., 2025), that the Ross Sea sector—including the Hercules Névé site—receives comparatively limited direct moisture input from ARs relative to other Antarctic coastal regions. These clarifications and additional references will be explicitly incorporated to address the reviewer's concerns thoroughly.

In this range of temperatures, Pfahl and Sodemann do not make any strong claim *about the relationship between dxs and SST. (is there some more background research besides the old classics that can be provided here?).*

Answer: We thank the reviewer for highlighting this important point. In the revised manuscript, we will explicitly acknowledge the complexity and potential limitations of applying the findings of Pfahl and Sodemann (2014) regarding the relationship between d-excess and sea surface temperature (SST), especially within our temperature range. To strengthen this discussion, we will clearly adopt more cautious language and incorporate additional references from recent studies (e.g., Kurosaki et al., 2020) that address the complexities and variations in the d-excess–SST relationship across different temperature regimes and atmospheric conditions. These clarifications will explicitly reflect the nuanced nature of the relationship and provide a more robust scientific context for our findings.

In my opinion the text does not provide enough context or assistance in interpreting Figures 3 and 4 with respect to the claims made. Showing the 'significant' correlation spatial patterns across the Antarctic region undercuts any potential significance in valid correlations close to the ice core site in the Ross Sea region.

Answer: We thank the reviewer for this constructive feedback. This point is relevant to what the first reviewer suggested in specific comment (Spatial correlation maps (Fig 3)). In the revised manuscript, we will explicitly adjust Figures 3 and 4 to provide zoomed-in views focusing specifically on the Hercules Névé site and the adjacent Ross Sea region rather than displaying correlations across the entire Antarctic continent. This modification will enhance the spatial relevance and clarity of our correlation results. Additionally, we will expand and clarify the accompanying text in Sections 3.2 and 4.1, explicitly providing more context to aid in the interpretation of these localized correlation patterns and clearly linking them to relevant climatic processes affecting the Hercules Névé ice core site. These revisions will improve both the interpretive clarity and spatial specificity of our analysis.

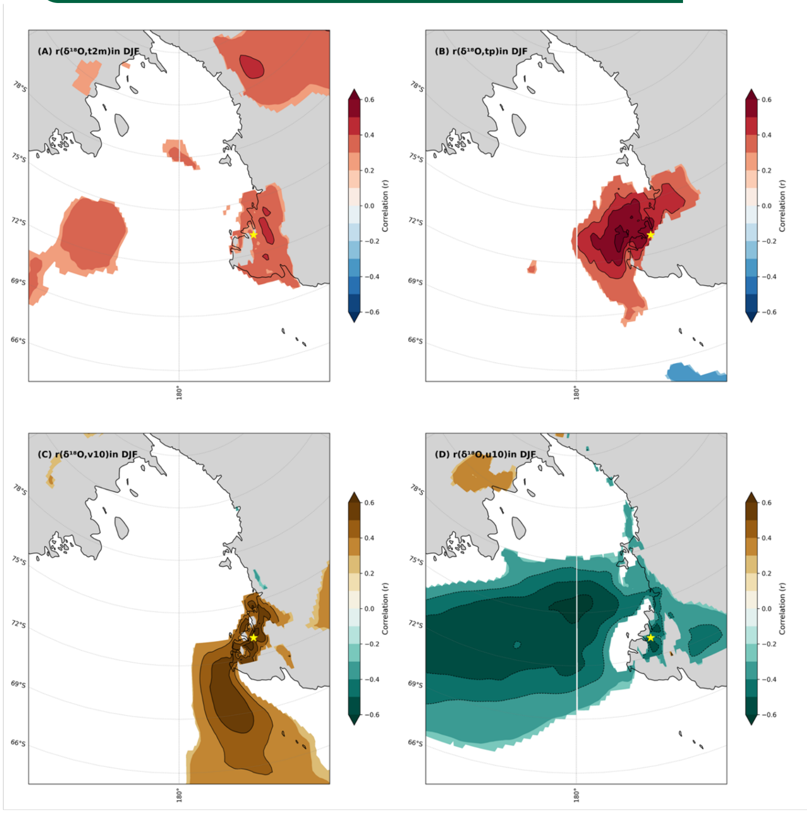

Figure 3: Spatial correlation analysis of (A) the 2 m temperature (T2m), (B) total precipitation (tp), (C) 10 m v-wind (v10), and (D) 10 m u-wind (u10) across Antarctica based on ERA5 data with $\delta^{18}O$ in HN ice core (black symbol) from 1979 to 2015 during DJF. The $p < 0.1$ confidence level is indicated by black contours.

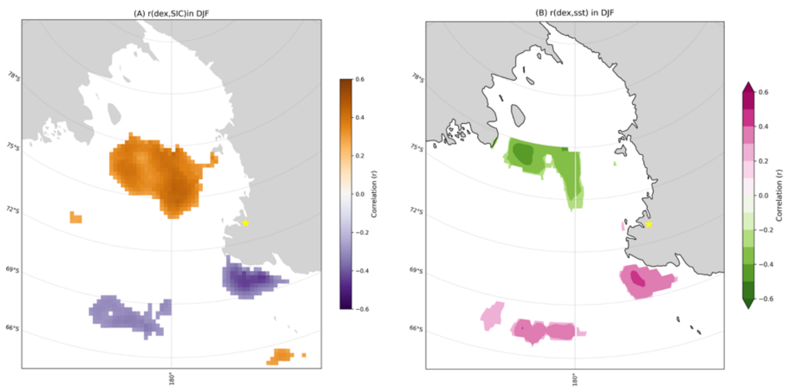

Figure 4: Spatial correlation analysis results between the secondary parameter dexc from the HN ice core (black symbol) and (A) the sea ice concentration (SIC) and (B) sea surface temperature (SST) from ERA5 data during DJF for 1979–2015. The $p < 0.1$ confidence level is indicated by black contours.

Figure 5. This is an intriguing plot. Did the authors also look at 500 mb heights in addition to MSLP? That may provide a better indication of synoptic activity than MSLP. What about a precipitation or moisture-weight temperature feidl for figure 5b? This may result in a field that is more directly related to d18O.

Answer: We thank the reviewer for these thoughtful suggestions. In the revised manuscript, we will explicitly incorporate an Empirical Orthogonal Function (EOF) analysis of 500 hPa geopotential height fields in addition to our original analysis based on mean sea level pressure (MSLP). Preliminary analysis indicates that the second principal component (PC2) derived from the EOF of 500 hPa geopotential heights shows a statistically significant correlation with our $\delta^{18}O$ record. We will clearly present and discuss these additional findings in the revised manuscript.

Regarding the suggestion to use precipitation- or moisture-weighted temperature fields for Figure 5b, we acknowledge that such fields can potentially provide a more direct physical link to $\delta^{18}O$ variations. However, given our seasonal (DJF) averaging and the limited temporal resolution, preliminary tests showed minimal differences between the precipitation-weighted temperature fields and simple seasonal mean fields. Consequently, we will retain the simpler DJF mean temperature fields to maintain clarity and avoid redundancy, but we will explicitly state this rationale in the revised manuscript.

In a similar way, the authors may consider a 'figure 6' that curates similar fields for dxs but uses maybe a combination of SST, RH, and wind speed for the new figure 6b.

Answer: Thank you for this valuable suggestion. In the revised manuscript, rather than adding a new figure specifically for d-excess, we will explicitly clarify and expand the interpretation of the existing Figure 4, which already presents spatial correlations between d-excess and relevant climate variables (e.g., sea surface temperature (SST), sea ice concentration (SIC)). In particular, we will enhance Section 4.1 by explicitly discussing how ocean–atmosphere interactions, incorporating SST, relative humidity (RH), and wind speed, likely contribute to the observed variability in d-excess at Hercules Névé. This expanded interpretation will clearly address the relationships suggested by the reviewer's proposed new figure without introducing redundancy in the manuscript.

Related to the concept of curating feature variables for spatial correlation analysis for Figure 5 (and a possible Figure 6), the authors may consider employing some of the more modern tool boxes for multiple linear regressions in this analysis. They may find (without giving too many variables or variable combinations chances) some efficient success in explaining spatial variance in several fields relevant to d18O (e.g., T, moisture content of air,), dxs (local winds, RH, SST, sea ice concentration), and spatial pressure fields that represent synoptic activity.

Answer: Thank you for this insightful recommendation. In response to your suggestion, we agree that employing multiple linear regression (MLR) or a similar multivariate correlation analysis could enhance our understanding of the spatial relationships influencing $\delta^{18}O$ and d-excess. Although we have not yet conducted such analyses, we plan to explicitly explore multiple correlation analyses incorporating combinations of variables (e.g., temperature, moisture content, SST, RH, wind speed, sea ice concentration, and pressure fields) in the revised manuscript. We will carefully evaluate these results to determine if they provide improved explanatory power for the isotopic variability observed in our dataset, and we will present these findings clearly and transparently.

line 337-338. ZW3 does not interact with the ASL. Part of ZW3 is a spatially broader *climatological representation of the fact that there is a climatological low in the Amundsen Sea region.*

Answer: We thank the reviewer for this important clarification. In the revised manuscript (lines 337–338), we will explicitly correct the description to clearly state that ZW3 does not dynamically interact with the ASL, but rather encompasses the ASL region within its broader spatial climatological pressure pattern. Specifically, we will revise our language to explicitly indicate that ZW3 variability co-occurs spatially and temporally with variations in ASL intensity, without suggesting any direct causal relationship or dynamic interaction. This modification will accurately reflect the spatial climatological nature of ZW3 relative to the ASL.

We appreciate the criticism and suggestions from the reviewer. And we believe that the revised manuscript will be an important asset to the cryosphere and polar community. We are looking forward to its publication. Thank you for handling our manuscript and your patience.

Sincerely,
Jeonghoon Lee