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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 editor

Answer: Thank you for the detailed feedback and for giving us the opportunity to revise our manuscript. We greatly appreciate the reviewers' efforts. Upon confirmation of the editor's satisfaction with the response of the reviewer's comments, we will promptly submit the final version of the manuscript for your consideration.

Reply to the comments by the reviewer 1

Reviewer #1: *Review for "Imprints of Sea Ice, Wind Patterns, and Atmospheric Systems on Summer Water Isotope Signatures at Hercules Névé, East Antarctica"by Kim et al. submitted to The Cryosphere*

This relatively short, well-written study presents stable water isotope data from the top 26 m of the 80 m ice core drilled approximately 80 km from the coast at Hercules Névé, Victoria Land, East Antarctica. The study analyses the stable water isotope record of the top 15m of this ice core, which spans the period from 1979 to 2015, and investigates potential climatic drivers for the observed isotope variability by performing (spatial) correlation analysis between the stable water isotope record and ERA5 reanalysis data. The study further examines the imprint of two atmospheric circulation patterns on the isotope record, specifically the Amundsen Sea Low (ASL) and Zonal Wave-3 (ZW3) and finds significant correlations between the austral summer isotope signal and the indices. The study concludes with the rather generic assessment that climate variables such as temperature, precipitation, wind patterns and sea ice extent influence the water isotope variations in the ice core.

Although the manuscript is mostly clearly written and the presented data is a valuable addition to the growing number of Antarctic ice cores and, as such, suitable for *publication in The Cryosphere, and of interest to The Cryosphere reader community, the findings are rather descriptive, and the authors do not fully utilise the potential of the presented dataset. A couple of interesting research questions are raised in the text, yet they are not investigated in depth and the presented content does not advance beyond spatial correlation analysis and generic statements about the (complex and intertwined) influence of climate variables on the isotope record variability. The manuscript would benefit from a clear research question or hypothesis that is being tested, which would give the study a better structure and increase the reader's interest. I suggest the authors consider restructuring the manuscript around a research question such as whether the Hercules Névé stable water isotope record can be used to learn about sea ice/polynya activity in the past (Section 4.1) or which climatic variables dominate the stable water isotope record variability on different timescales (is this ice core site suitable to study seasonal climate variability (L. 47-51, L.110)?).*

Answer: We sincerely thank the reviewer for the thorough evaluation and valuable insights, particularly the constructive suggestion to clarify our study's research objectives and to explicitly address the potential research avenues raised by our dataset. In response to these suggestions, we have significantly revised our manuscript in several aspects, clearly outlined below.

1. Clarification of the Research Question and Scientific Objective:

We recognize that the initial manuscript lacked an explicit hypothesis or clearly defined research question, reducing its scientific impact. Following the reviewer's guidance, we have restructured the manuscript by explicitly formulating the main research question as follows:

"How do summer δ^{18}O and d-excess records from Hercules Névé ice core reflect variability in interactions between atmosphere and oceans, particularly regarding sea ice extent, sea surface temperature (SST), wind patterns, and large-scale atmospheric circulation modes (ASL and ZW3)?"

To address this explicitly, we have:

(1) Revised the Abstract and Introduction to clearly state the above research question as the study's primary scientific objective.

(2) Enhanced clarity in the manuscript structure by reorganizing Sections 2 (Methods), 3 (Results), and particularly 4 (Discussion) to directly address this central research question.

2. Enhanced Analysis of Climate Drivers and Potential Applications:

We appreciate the reviewer highlighting that our findings were previously presented at a descriptive level without sufficiently exploring their implications or deeper linkages. To address this critique comprehensively, we have performed additional analyses and included new results in the revised manuscript:

(1) Back-trajectory diagnostics have been expanded with a detailed explanation of the back-trajectory analysis performed using the HYSPLIT model. Parameters and assumptions involved in tracing moisture sources are now clearly outlined and explicitly linked to sea ice conditions and polynya activity.

(2) The spatial correlation analysis has been extended and clarified by elaborating on the spatial patterns observed, interpreting them within the context of regional climate dynamics (e.g., the role of the Amundsen Sea Low and Zonal Wave-3 patterns). Figures illustrating these patterns were revised to better communicate the results.

3. Investigation of Sea Ice/Polynya Activity as a Working Hypothesis:

We have explicitly explored and incorporated the polynya activity hypothesis in the Discussion (Section 4.1), clearly indicating how isotopic signals observed in our ice core dataset correlate spatially and temporally with known sea ice variability and polynya occurrences.

To substantiate this approach, we have integrated additional ERA5 reanalysis-based maps (included as supplementary material), demonstrating the spatial link between isotopic variability and areas characterized by frequent polynya activity.

These analyses are presented in detail in the revised manuscript and no longer confined to future work; rather, they form an active part of the current study’s interpretation.

4. Improved Conclusions and Implications:

Our concluding section has been revised to explicitly summarize the key findings in light of the clarified research objectives and hypotheses. We clearly state the implications for Antarctic paleoclimate research, emphasizing the potential of Hercules Névé ice core isotopic records as proxies for regional ocean–atmosphere interactions and climate variability.

General comments:

Please consider including (some) of the following analysis to support your statements and make more nuanced and less generic statements:

Answer: We sincerely thank the reviewer for these detailed and constructive suggestions. We have carefully reviewed each recommendation and have fully incorporated them into our revised manuscript.

- *Trend analysis: Although the time period investigated is short on climate time scales, consider including a trend analysis, potentially also including the full dataset going back to 1949, and compare to potential trends in ERA5 reanalysis.*

Answer: We agree that conducting a long-term trend analysis could add valuable insights. Trend analysis has the potential to identify long-term changes or shifts in climatic conditions influencing isotope variability at Hercules Névé. Additionally, comparing such trends with ERA5 reanalysis has been incorporated to further clarify the robustness and broader regional representativeness of the isotopic records.

However, due to the relatively short period (1979–2015) and high interannual variability in our dataset, robust trend identification remains challenging. In the revised manuscript, we have explicitly acknowledged these limitations and clarified that our main focus is on seasonal variability and spatial correlations. We have also noted that a detailed long-term trend analysis will be pursued when additional longer datasets become available.

- *Principal component analysis: As is stated in L. 316 correlation is not equal to causality, yet only correlation analyses are presented. Consider extending the analysis of identifying drivers of the isotope signal by investigating principal component analysis to be able to make statements about the importance of drivers for specific timescales of climate variability. (Dixon et al., 2012; Noone, 2004)*

Answer: Following the reviewer's suggestion, we performed PCA on key ASL-related atmospheric variables (strength, latitude, longitude) to identify dominant modes of variability. These PCA results have been presented explicitly in Section 4.2 of the revised manuscript.

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

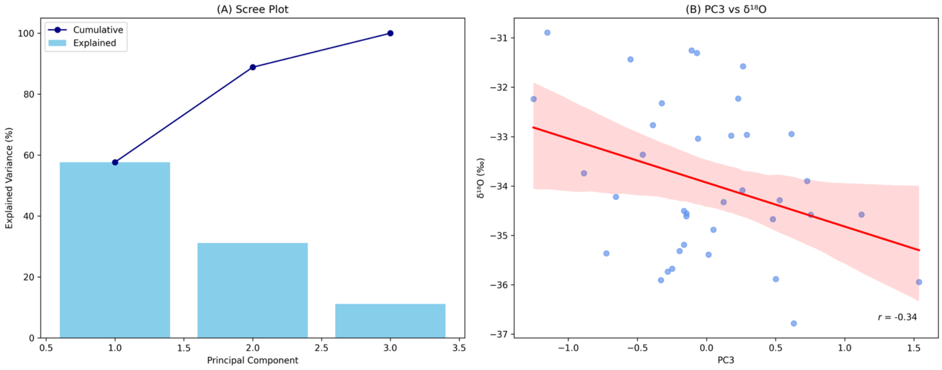


Figure 6. (A) Scree plot showing the variance explained by each principal component derived from ASL monthly variability (central pressure, latitude, and longitude) during DJF (1979–2015). (B) Scatterplot between PC3 and $\delta^{18}O$ values from Hercules Névé (r = –0.34), based on DJF-averaged data, showing the strongest correlation among all PCs.

- *Spectral analysis of the ice core timeseries (how much of the variability is noise and how much is signal): see the extensive literature by Thom Laepple's group on complications of extracting (sub)-annual climate information from a single ice core, specifically in low accumulation sites such as the Antarctic Ice Sheet (Casado et al., 2020; Laepple et al., 2018; Münch et al., 2016; Münch and Laepple, 2018). The*

Hercules Névé site is coastal and as such has a higher annual layer thickness, yet the signal-to-noise ratio is worth investigating before correlation analysis is performed especially if sub-annual data is analysed.

Answer: As recommended by the reviewer, we have performed a spectral analysis using a Lomb–Scargle periodogram to quantitatively evaluate the temporal variability and periodicities embedded within the stable isotope data. Previous studies (Münch et al., 2016; Laepple et al., 2018; Casado et al., 2020; Münch and Laepple, 2018) mainly utilized spectral analysis methods, such as the Fast Fourier Transform (FFT), on evenly sampled ice core data to assess diffusion effects and signal preservation. However, since our dataset has unevenly spaced time intervals obtained through annual layer counting and interpolation, we determined that the Lomb–Scargle method, which effectively evaluates periodicity even for unevenly spaced data, is more appropriate for our analysis. Recently, Hébert et al. (2021) (Laepple's group) successfully applied the Lomb–Scargle periodogram for analyzing paleoclimate data with irregular sampling intervals.

Specifically, we have investigated whether our isotope record exhibits a statistically significant annual cycle, which is crucial for confidently interpreting seasonal climate signals from the Hercules Névé ice core. Preliminary analyses indicate a clear and statistically robust seasonal peak at a period of approximately 1 year, demonstrating a strong annual isotopic signal. In the revised manuscript, we have explicitly presented these detailed spectral analysis results (including significance levels) in Supplementary Figure S3, and thoroughly discuss the implications for the signal-to-noise ratio of the isotopic records, particularly in relation to extracting reliable seasonal climate information.

Hébert, R., Rehfeld, K., and Laepple, T.: Comparing estimation techniques for temporal scaling in palaeoclimate time series, Nonlin. Processes Geophys., 28, 311–328, https://doi.org/10.5194/npg-28-311-2021, 2021.

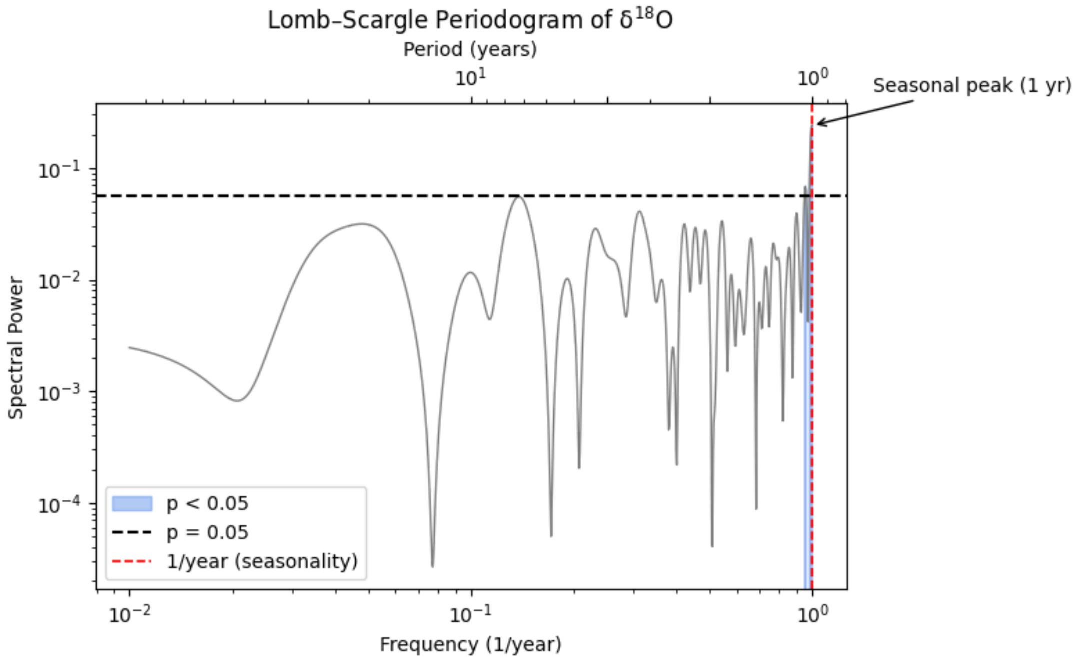


Figure S3. Lomb–Scargle periodogram of the δ^{18}O time series from the Hercules Névé ice core. The analysis reveals a statistically significant spectral peak at 1 cycle per year (red dashed line), corresponding to a seasonal (annual) signal. The shaded blue region denotes frequencies exceeding the 95% confidence level ($p < 0.05$). This result indicates the presence of a preserved seasonal cycle in the δ^{18}O record, justifying the use of DJF-averaged values in subsequent climate correlation analyses.

- *Back trajectory analysis: To support the spatial correlation maps and claims about moisture origin influences, it would be beneficial to show some back trajectory analysis using HYSPLIT or similar. See e.g: (Dixon et al., 2012; Neff and Bertler, 2015; Thomas and Bracegirdle, 2009)*

Answer: Thank you for this valuable suggestion. Following the recommendation from the reviewer, we have performed additional back-trajectory analyses using the HYSPLIT model to better support our interpretations regarding spatial correlations and moisture source regions. Dixon et al. (2012) previously employed HYSPLIT backward trajectories to illustrate the relationship between katabatic winds and moisture transport to coastal East Antarctic sites, while Neff and Bertler (2015) used trajectory analyses to identify varying moisture contributions from the Ross Sea and Southern Ocean during anomalous isotope years. Similarly, Thomas and Bracegirdle (2009) applied trajectory modeling to link variability on the Antarctic Peninsula to moisture sources originating from the Amundsen and Ross Sea regions.

Motivated by these studies, we conducted back-trajectory analyses using HYSPLIT for the five summers characterized by strong ASL conditions and five summers with weak ASL conditions. The resulting differences in moisture transport pathways were clearly observed and have been explicitly illustrated in a new figure (Figure 7). We have presented detailed interpretations and implications of these trajectories in Section 4.1. Additionally, we have provided provide a comprehensive description of the HYSPLIT methodology in the Methods section of the revised manuscript.

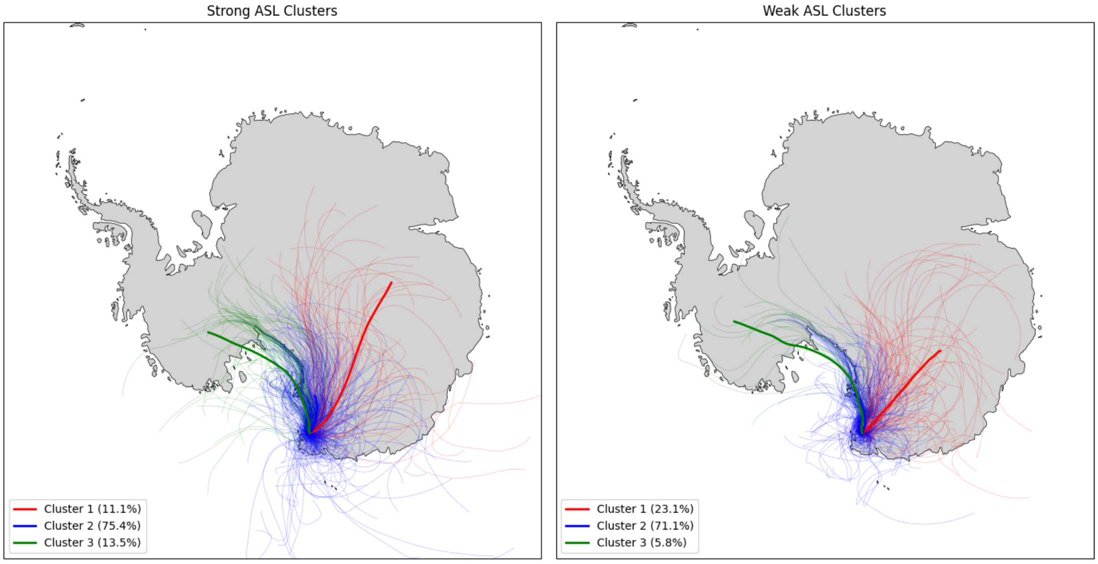


Figure 7. 7-day HYSPLIT back-trajectory clusters arriving at Hercules Névé during (left) the five strongest ASL summers and (right) the five weakest ASL summers (DJF, 1979–2015). Strong ASL cases show dominant air mass transport from inland Antarctica and the Ross Ice Shelf, while weak ASL cases are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

- *Sensitivity analysis of the age model: Do you have access to the impurity dataset of the ice core to support the age model (L. 173)? Relying solely on the water isotope signal itself for layer counting and then correlating the water isotope data with climate variable timeseries bears the potential for a circular argument. Further, it is unclear how the sub-annual age model was developed (see specific comments).*

Answer: We fully acknowledge the reviewer's concern regarding potential circular reasoning in our age model construction. To address this explicitly, we have clearly detailed our approach as follows:

First, annual layers were identified by visually counting seasonal cycles in the $\delta^{18}O$ profile. Each annual cycle was defined by typically 5–15 discrete $\delta^{18}O$ data points, which were subsequently linearly interpolated to 12 monthly values to establish a sub-annual (monthly) chronology. Summer mean values (December–January–February; DJF) were then calculated by averaging the interpolated monthly data for each summer season.

Additionally, to quantitatively assess the reliability of this sub-annual resolution, we have performed an isotopic diffusion calculation that yielded an estimated diffusion length of approximately 6 cm. This diffusion length matches well with our sampling resolution and supports that the chosen monthly interpolation is appropriate, capturing true seasonal signals rather than random noise.

To further evaluate the robustness of our $\delta^{18}O$-based chronology, we independently applied the Herron and Langway (1980) firn densification model using our measured depth–density profile and an assumed mean accumulation rate of 204.5 kg/m^2/yr. This modeling yielded a cumulative age of approximately 37.9 years over the depth range used in this study (~16.2 m), in close agreement with the 37 years determined from $\delta^{18}O$ layer counting. This numerical consistency between two independent methods reinforces the reliability of our age scale.

Moreover, this approach is consistent with previous work conducted within our research group. In Nyamgerel et al. (2020, Journal of Glaciology), a similar firn densification model (Herron and Langway, as well as Morris, 2018) was used to validate age scales derived from δ^{18}O and impurity signals (MSA, nssSO_4^{2-}). While impurity data were not available in our study, we explicitly acknowledge this limitation and applied the same modeling framework to ensure methodological continuity and independent verification of the isotope-derived chronology. We have incorporated this detailed description of layer counting, interpolation, diffusion length estimation, and firn densification modeling into the revised manuscript (Section 2.3.1).

- *Wind analysis: The manuscript investigates the influence of wind variability with regards to katabatic winds, ASL and ZW3 (L. 75 and many more). It would be beneficial to see an analysis of the wind conditions of the Hercules Névé site, both from the local AWS and the reanalysis product to better understand how important and variable the wind is for the site. This is also important to understand the importance of wind on the stratigraphy of the ice core. Please include a wind rose in Fig. 1 and extend the AWS analysis (Section 2.2.2) with findings from the wind data (extend Fig S1).*

Answer: We fully accept the suggestion to incorporate wind rose analyses and comparisons of AWS and ERA5 wind data. Wind roses have been included in the revised Figure 1 and Supplementary Figure S2, and detailed interpretation has been added to Sections 2.2.2 and 3.2 to address wind influences comprehensively.

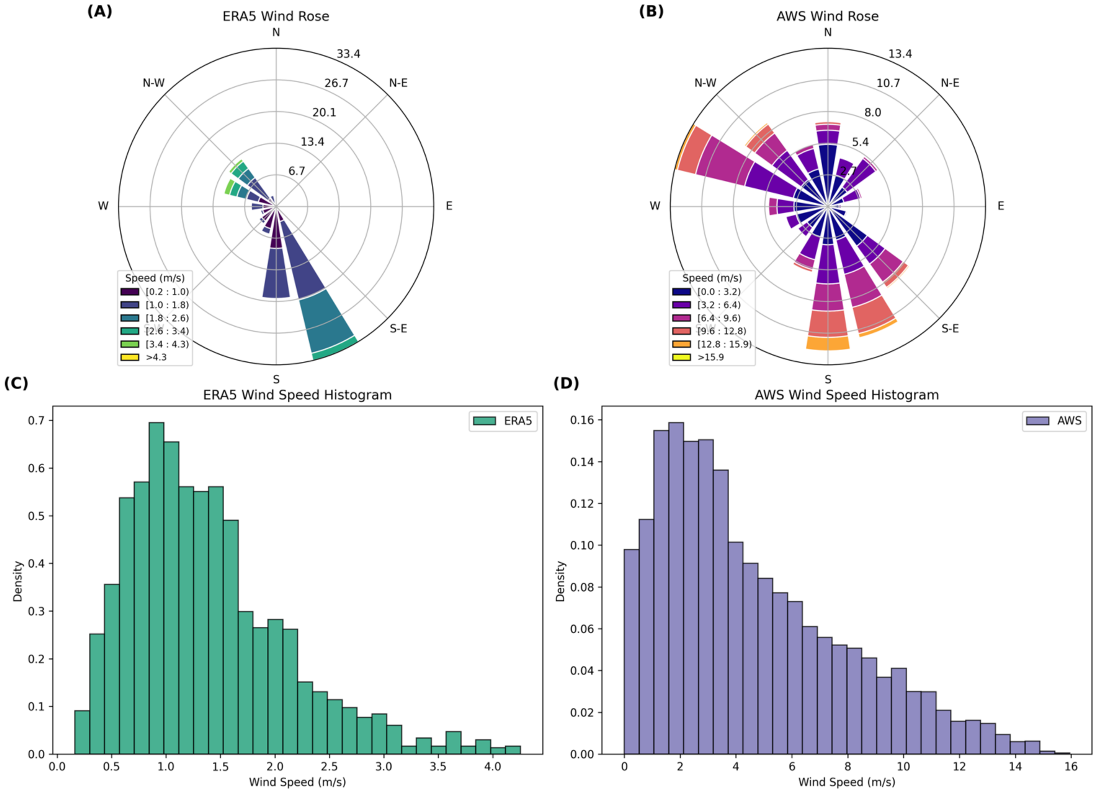


Figure S2. Comparison of AWS and ERA5 wind data at Hercules Névé during DJF. (A–B) Wind rose diagrams for ERA5 and AWS, respectively, showing dominant wind directions. (C–D) Histograms of wind speed distributions. While ERA5 shows a more simplified and weaker wind regime, the AWS captures stronger, more variable local wind behaviour.

- *Section 4.1: The fact that the ice core isotope records (d18O and d-excess) seem to be strongly influenced by polynya activity is very exciting and should be the focus of the study, in my opinion. It would require more supporting literature, sea ice concentration maps, and back-trajectory analysis, but this could serve as a very interesting central research question. In any case, the manuscript would benefit from a map where sea ice extent in summer and winter (and polynya locations) is shown.*

Answer: We fully agree with the reviewer’s suggestion that the potential influence of polynya activity on isotopic variability is highly relevant and could serve as a central research question. Accordingly, we have explicitly framed the relationship between isotope signals (δ^{18}O and d-excess) and regional polynya activity as a key working hypothesis in the revised Section 4.1. To comprehensively address this suggestion, we have included detailed sea ice extent maps for both summer and winter seasons in Supplementary Figure S3. These maps clearly depict the spatial extent of sea ice cover and indicate known polynya locations near the study site. Additionally, we have significantly expanded the discussion by incorporating more supporting literature, and clearly interpret the relationship between sea ice dynamics, polynya activity, and isotopic variability, further supported by our planned back-trajectory analyses.

Specific comments:

Age model (L. 186): - Please specify how the age-depth tie points were chosen. Also *specify how the age was determined between age-depth tie points (interpolation? Weighted?). Since it is identified that accumulation is biased to the austral summer months (L.232, L.104), how can you justify linear interpolation between tie points? And following from this, how robust are your correlation results concerning the chosen tie points? (Would it change if only min, only max or min and max values were used as tie points?) see, e.g. (Gautier et al., 2016; Parrenin et al., 2024; Thomas et al., 2024)*

Answer: Thank you for this detailed comment regarding our age model. The age model was constructed as follows: annual layers were identified by visually inspecting seasonal δ^{18}O cycles, and within each annual cycle, the δ^{18}O maximum was designated as a tie point corresponding to January (peak austral summer). Ages between these annual tie points were assigned by linear interpolation based on relative depth within each annual layer.

However, we realize this detailed methodology was not sufficiently explained in the original manuscript. Therefore, we have explicitly added this detailed explanation of our age model construction, including our assumptions and the linear interpolation method, to Section 2.3.1 in the revised manuscript.

Also, delete L. 185 "To achieve precise age dating, we utilised ERA5 isotope data from the polar regions over the 1979–2015 period" which is unclear. You used ERA5 isotope data?

Answer: Thank you for pointing this out. We acknowledge that the sentence was misleading, as ERA5 does not include water isotope data. The age dating was not based on ERA5 but was solely derived from the seasonal variability in δ^{18}O and d-excess within the ice core. To avoid confusion, we have deleted this sentence and clarify the age determination procedure in the revised text. The revised sentence is as following: "Clear seasonal cycles in δ^{18}O and d-excess enabled manual annual layer counting to construct a high-resolution age–depth model."

Isotope variability and correlation (Section 3.2, 4.1, 4.2): It is often unclear what resolution was the basis for the correlation analyses between reanalysis and isotope record. Was the isotope maximum used as DFJ value, or annual means, or 3-month means (how were the 3 months period defined in this case) please specify in the relevant sections (3.2, 4.1, 4.2).

Answer: Thank you for highlighting the need for clarification regarding the temporal resolution used in the correlation analyses between the isotope record and climate reanalysis data. In the revised manuscript, we have explicitly specified the temporal resolution and methodology in Sections 2.3.1, 3.2, 4.1, and 4.2. Specifically, each isotopic data point was originally assigned a fractional year based on its relative depth position within annually dated layers. Monthly isotope values were subsequently obtained through linear interpolation within each annual layer, and seasonal isotope averages were calculated by averaging the monthly interpolated values for December, January, and February (DJF) of each year. These seasonal averages (DJF means) were used consistently as the basis for all spatial and temporal correlation analyses presented in Sections 3.2, 4.1, and 4.2. We have clearly stated this methodology to avoid any ambiguity in the revised manuscript.

Spatial correlation maps (Fig 3): It is unclear to me why the authors perform a spatial correlation analysis with one ice core and all of the AIS region when you are specifically looking for large-scale meteorological patterns with the indices later. Usually, stacks of several ice cores are used for such analysis to reduce the noise terms and access only the common spatially representative signal (Casado et al., 2023). Without a signal-to-noise ratio analysis first, this spatial correlation analysis seems excessive and not insightful and I would recommend zooming into the region where you expect influential regions. Also, in the discussion section, the authors do not discuss spatial patterns of the correlation results, but rather focus on the strength of the correlation at the site.

Answer: Thank you for this valuable comment regarding the spatial correlation analysis presented in Figure 3. In the revised manuscript, we have modified Figure 3 to focus explicitly on a narrower spatial domain, specifically highlighting the region around Victoria Land and the Ross Sea. By doing so, we have avoided overinterpreting broader pan-Antarctic patterns from a single ice core record and have explicitly targeted the region that is climatologically most relevant to the Hercules Névé site.

Additionally, to address concerns about the representativeness of a single-core record, we have included a spectral analysis using a Lomb–Scargle periodogram (Supplementary Figure). This analysis demonstrates a statistically significant seasonal peak at a period of approximately 1 year, providing quantitative evidence of a robust seasonal signal preserved within our isotopic record.

Moreover, in the revised discussion section (Section 4.1), we have specifically discussed the spatial patterns of correlations observed in the refined spatial domain, rather than solely focusing on correlation strength at the Hercules Névé site. This detailed spatial interpretation provides more meaningful insights into regional-scale climate processes influencing the isotope variability observed in our ice core.

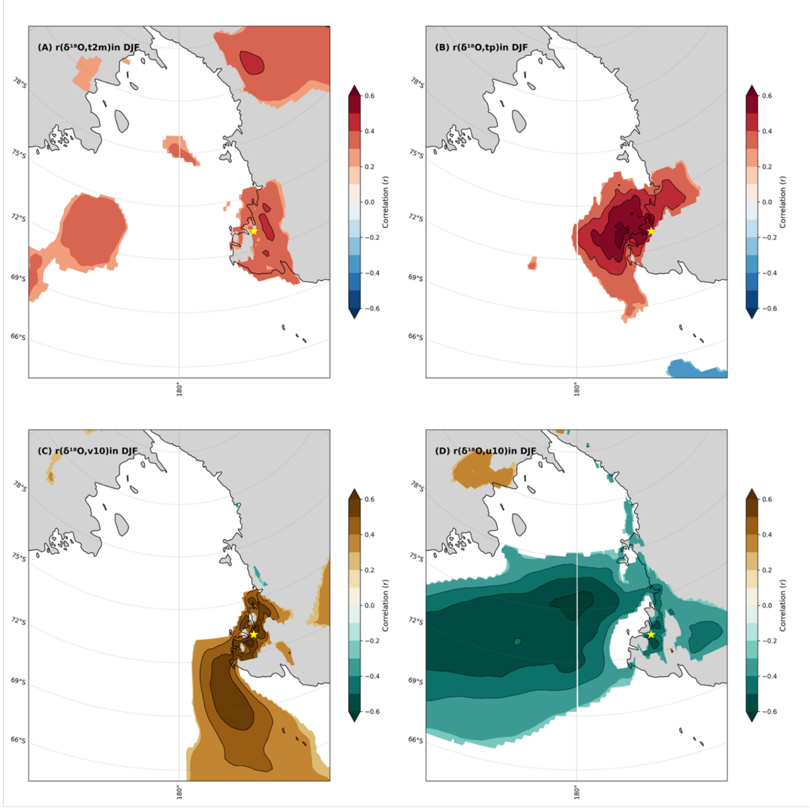


Figure 3: Spatial correlation analysis of (A) the 2 m temperature (T2m), (B) total precipitation (tp), (C) 10 m v-wind (v10), and (D) 10 m u-wind (u10) across Antarctica based on ERA5 data with $\delta^{18}O$ in HN ice core (black symbol) from 1979 to 2015 during DJF. The $p < 0.05$ confidence level is indicated by black contours.

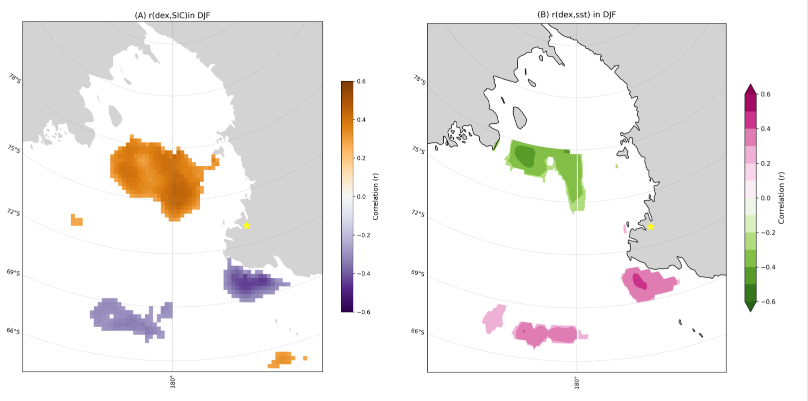
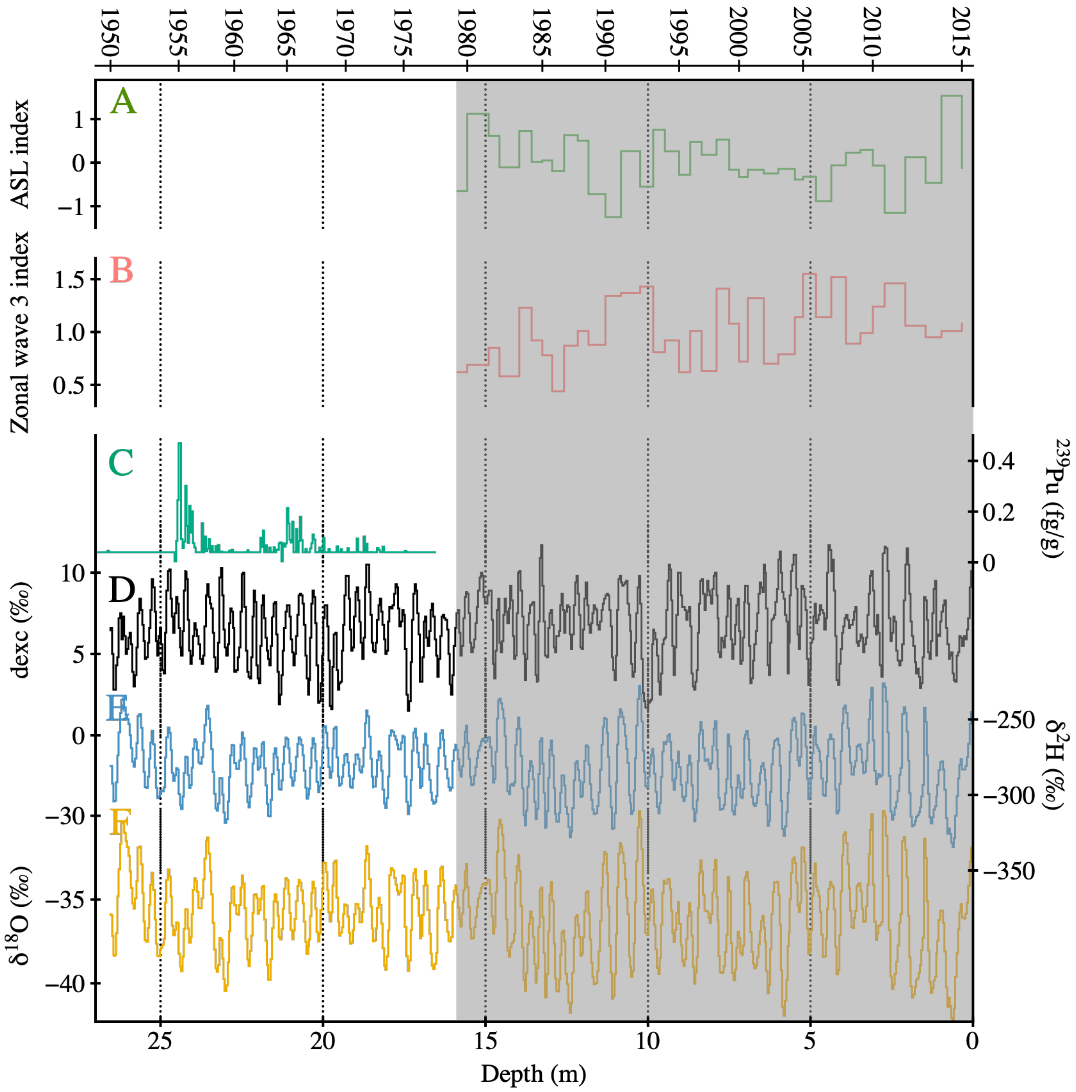


Figure 4: Spatial correlation analysis results between the secondary parameter dexc from the HN ice core (black symbol) and (A) the sea ice concentration (SIC) and (B) sea surface temperature (SST) from ERA5 data during DJF for 1979–2015. The $p < 0.1$ confidence level is indicated by black contours.

Climate indices and isotope record (Section 4.2 and 4.3): please consider plotting a timeline together with the indices that you are investigating, so the reader can better understand the conclusions you draw and elaborate on which timescales these indices influence the isotope records. See for example: (Servettaz et al., 2020a)

Answer: Thank you for this constructive suggestion regarding the inclusion of a timeline figure comparing our isotope record with relevant climate indices (e.g., ASL strength, ZW3). In the revised manuscript, we have included a supplementary figure clearly presenting a timeline of DJF seasonal mean δ^{18}O (or d-excess) alongside the corresponding climate indices used in Sections 4.2 and 4.3. This timeline plot explicitly illustrates temporal co-variability, enabling readers to better visualize and understand the relationships and timescales at which the indices influence isotope variability. Additionally, we have briefly discussed this timeline figure in the main text to further enhance the clarity and interpretation of our correlation and principal component analysis results.



According to Casado et al., (2018) others, it is not only the precipitation input that builds the water isotope record, but post-depositional processes (such as sublimation, stratigraphic noise, metamorphism, diffusion, etc) can also affect the isotope record variability. Sublimation is considered a potential driver that affects the stable water isotopic composition. However, it is influential primarily in summer (Dietrich et al., 2023; Ollivier et al., 2024; Wahl et al., 2022). In lines 231-235, however, the authors make a claim that post-depositional processes are "relatively reduced in summer" without further elaborating on their claim. Please elaborate on whether and how *post-depositional processes might influence the climate signal at Hercules Névé, and on what timescales or for what reasons it might be ok to disregard post-depositional effects in this study.*

Answer: Thank you for raising this important point regarding the potential influence of post-depositional processes on the stable isotope signal at Hercules Névé. We agree that processes such as sublimation, metamorphism, stratigraphic noise, and diffusion can significantly alter the isotopic composition after deposition, especially during summer months, as noted by previous studies (Dietrich et al., 2023; Ollivier et al., 2024; Wahl et al., 2022; Casado et al., 2018).

To explicitly address whether these post-depositional processes substantially affect the isotope record at our site, we applied a diagnostic approach based on the slope of the $\delta^{18}O$–d-excess relationship, following the method outlined in Casado et al. (2021). Typically, a negative $\delta^{18}O$–d-excess slope indicates substantial isotopic alteration due to post-depositional effects such as sublimation, whereas a consistently positive slope suggests minimal modification.

In our preliminary analyses, we have observed that the DJF $\delta^{18}O$–d-excess slope at Hercules Névé is consistently positive, indicating that post-depositional processes likely have limited impact on the isotopic record during summer months at this location. Additionally, the ERA5 reanalysis indicates that the DJF season contributes approximately 35% of the total annual precipitation. This substantial seasonal precipitation input presumably helps preserve the original isotopic signals, even in the presence of minor post-depositional alterations.

In the revised manuscript, we have explicitly presented and discussed these diagnostic slope results ($\delta^{18}O$–d-excess) and clarify this reasoning in detail in lines 231–235. Furthermore, we have stated clearly that while minor surface alterations may occur, the primary climatic signals at the seasonal (DJF) scale are sufficiently robust to justify disregarding substantial post-depositional effects in our specific analysis.

L. 12: The abstract states that the aim of the study was to evaluate the influence of climate variables on the variability of the ice core isotopes, yet the manuscript does not identify drivers for different timescales. Please rephrase or restructure.

Answer: Thank you for highlighting this point. In the revised manuscript, we have rephrased the abstract (L. 12) to clearly indicate that the primary objective of our study is to evaluate the relationship between seasonal (DJF) climate variables and isotopic variability recorded in the Hercules Névé ice core. Specifically, we have clarified that our analysis does not address variability at multiple timescales but explicitly focuses on identifying and quantifying the drivers of isotopic signals at the seasonal scale. This revised wording more accurately reflects the scope and limitations of our study and avoid potential misunderstanding regarding multi-timescale analyses. As the reviewer suggested, we have also added a couple of sentences to explicitly acknowledge this limitation and clearly state that our findings pertain to the DJF season only. This addition helps readers better understand the temporal focus of our study.

L. 45: Based on the annual layer counting, could you include information on annual layer thicknesses and accumulation variability that link with the climate information you analyse as well? This would be supported by the fact that accumulation seems to be much better correlated with isotopes compared to temperature as you state in Section 3.2.

Answer: Thank you for the helpful suggestion. In the revised manuscript (Section 2.3.1), we have explicitly provided information on annual layer thicknesses derived from the manual layer counting performed on the Hercules Névé ice core. Specifically, we have stated that the mean annual snowfall estimated over the study period (1979–2015) is approximately 204.5 ± 54.5 kg m^{-2} y^{-1}, corresponding to annual layers composed of approximately 5–15 samples per year, sufficient for seasonal resolution.

Additionally, we have explicitly mentioned the variability of annual accumulation rates in the Results section (Section 3.1) and clearly discussed how this variability is linked to the isotopic record. Although we do not intend to include a detailed annual layer thickness time series in the main text to maintain clarity and scope, we have briefly addressed its relevance and explicitly acknowledge its potential importance for interpreting isotopic variability. A comprehensive analysis of annual accumulation variability and layer thicknesses will be pursued in subsequent studies.

L. 75: Are katabatic winds only a summer phenomenon? The paragraph focuses on the austral summer months, yet sea ice expansion and katabatic winds are mentioned here which is confusing. Please clarify if these are general statements or summer-specific, and otherwise restructure.

Answer: Thank you for pointing out this ambiguity. In the revised manuscript, we have clarified explicitly in line 75 that katabatic winds are a year-round phenomenon in Antarctica, typically stronger and more frequent during winter months. Additionally, we have restructured this paragraph to clearly differentiate between general climatic conditions (such as year-round katabatic winds and sea ice expansion during colder months) and conditions specifically relevant to the austral summer months (DJF). This revision has removed any confusion arising from the original wording that might imply katabatic winds or sea ice expansion are limited only to the DJF period.

L. 81: Here and elsewhere: It is unclear what part of the isotope record is chosen to represent the "DFJ" austral summer months. Only the maximum? A three-months window? How are the monthly isotope values age-referenced?

Answer: Thank you for highlighting this issue. In the revised manuscript (Section 2.3.1), we have explicitly clarified how monthly isotope values are assigned and how seasonal DJF averages are extracted. Specifically, we have described that annual layers are first dated by visually identifying the clear seasonal $\delta^{18}O$ cycles. The maximum $\delta^{18}O$ value within each annual layer was assigned as January (representing peak summer conditions). Monthly isotope values have then been determined by linearly interpolating between these annual tie points. Finally, December–January–February (DJF) isotope values have been extracted by averaging these interpolated monthly data points, resulting in a single DJF mean value for each year. To avoid ambiguity, we have clearly referenced this detailed methodological explanation at relevant sections (e.g., L. 81), while keeping introductory sections concise.

L. 87: Here and elsewhere the authors refer to "seasonal" climate and isotope variability, yet I think they only want to refer to summer information. Using the word "seasonal" makes the reader expect information on other seasons as well, possibly even the identification of seasonal variability but this is not given in the manuscript. Please add a definition or rephrase.

Answer: Thank you for bringing this ambiguity to our attention. In the revised manuscript, we have explicitly defined and clarified our use of the term "seasonal," clearly stating that all references to seasonal climate and isotopic variability throughout the manuscript specifically indicate the austral summer months (DJF). To avoid potential misunderstanding, we have added a concise statement early in the Introduction (near line 87) explicitly clarifying that our analysis focuses solely on austral summer conditions, rather than multiple seasons or other seasonal periods.

Section 2.2.1: Please specify the CRDS sampling analysis protocol or cite a study that gives details on the protocol that was used.

Answer: Thank you for this helpful comment. In the revised manuscript (Section 2.2.1), we have explicitly described the CRDS sampling and analysis protocol used in this study. Specifically, we have state that each isotopic sample was injected 12–15 times, and the average of the final 5 measurements was used for data analysis. To ensure data quality and analytical stability, international reference standards (VSMOW, SLAP, GISP) had been analyzed approximately every 100 samples, and internal laboratory standards had been measured every 10 samples. Additionally, we have provided a clear citation to a previous publication detailing the analytical protocol used at KOPRI.

L. 150: If the AWS measured for 1 year why are the authors only evaluating the temperature record. Please include at least wind analysis (see comment above) as this is important for the presented discussion.

Answer: Thank you for highlighting this important point. In the revised manuscript, we have included a detailed comparative analysis of wind direction and wind speed measurements obtained from the AWS data and ERA5 reanalysis, in addition to temperature. Specifically, we have presented this wind analysis clearly in Supplementary Material (Figure S2), and explicitly summarized the key findings in the main text (Section 2.2.2 and Section 3.2). This expanded analysis has enhanced the interpretation of isotope–climate relationships by providing additional context regarding local wind conditions at Hercules Névé.

L. 154: The authors claim that ERA5 "effectively" captures temperature and wind yet *this statement is not supported by any statistics. Please add so that the reader can understand this statement.*

Answer: Thank you for pointing out this omission. In the revised manuscript, we have provided quantitative statistical support for our statement that ERA5 effectively captures local temperature and wind conditions at Hercules Névé. Specifically, we have included a York regression comparing temperature from AWS observations and ERA5 data, clearly presented in Supplementary Figure S1C, indicating a strong linear relationship (slope = 1.09, $R^2 = 0.57$). Additionally, we have presented a comparative analysis of wind speed and wind direction between AWS and ERA5 in Supplementary Figure S2, demonstrating overall good agreement in prevailing wind directions. The corresponding sentence (L. 154) has been revised explicitly to include these statistical details.

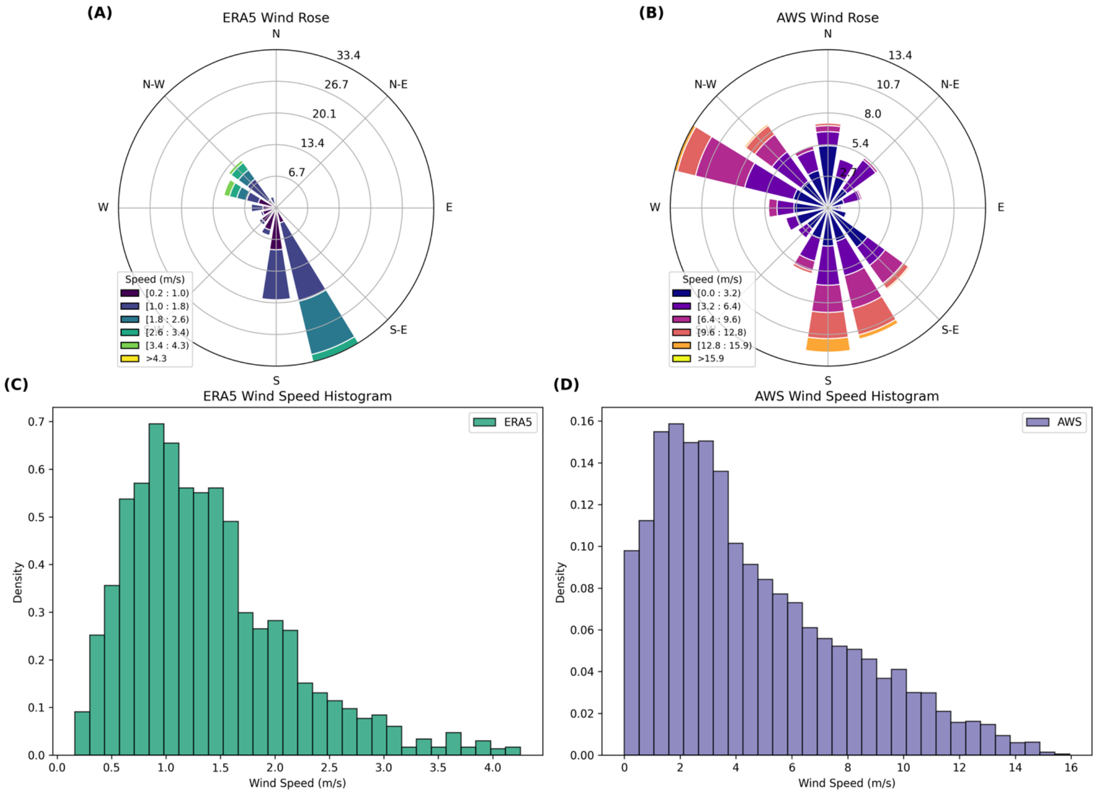


Figure S2. Comparison of AWS and ERA5 wind data at Hercules Névé during DJF. (A–B) Wind rose diagrams for ERA5 and AWS, respectively, showing dominant wind directions. (C–D) Histograms of wind speed distributions. While ERA5 shows a more simplified and weaker wind regime, the AWS captures stronger, more variable local wind behaviour.

L. 153, FigS1: From the analysis (Fig: S1) it actually looks like ERA5 overestimates warm temperatures generally but specifically in the summer months, yet this is the period you are focusing on. How does that influence your analysis using ERA5. Please discuss.

Answer: Thank you for this insightful comment. In the revised manuscript, we have explicitly acknowledged and discussed the observed bias of ERA5 toward overestimating temperatures during austral summer months (DJF), relative to AWS measurements (as shown in Supplementary Figure S1). We have clearly stated that despite this systematic bias in absolute temperature magnitudes, ERA5 remains suitable for our analysis since our main objective is examining the interannual variability and spatial correlation patterns rather than relying on absolute temperature values. In particular, we have discussed explicitly in Sections 2.2.2 and 3.2 how this temperature bias might influence our interpretation and clarify that our primary conclusions, which focus on relative variability rather than absolute temperature, remain appreciate.

"The AWS data was compared with ERA5 reanalysis outputs to evaluate the accuracy of ERA5 in representing local atmospheric conditions at Hercules Névé (Figure S1). The results indicated that ERA5 broadly captured the observed temperature and wind patterns, although a systematic bias toward warmer temperatures during austral summer (DJF) was noted, with ERA5 consistently overestimating warm-season temperatures relative to AWS observations. Despite this bias, ERA5 remains suitable for our analysis as our primary objective is to assess interannual variability and spatial correlation patterns, rather than relying strictly on absolute temperature values."

L. 202 and Section 4.2: It is unclear how you define and test for the ASL strength. Which definition for the ASL are you using? Please give a definition and a brief introduction to the interplay between ASL and Southern Annular Mode (SAM) which is generally known to influence the Antarctic climate. (Servettaz et al., 2020b)

Answer: Thank you for this important comment. In the revised manuscript, we have explicitly defined the Amundsen Sea Low (ASL) strength following the methodology, described by Hosking et al. (2013), in which ASL strength is defined as the central pressure of the climatological low-pressure system over the Amundsen Sea. Monthly ASL variables (central pressure, latitude, longitude) have been obtained from the publicly available British Antarctic Survey dataset, based on ERA5 mean sea-level pressure fields.

Additionally, we have clearly explained how we performed Principal Component Analysis (PCA) on these three ASL parameters for the DJF period (1979–2015) to identify leading modes of variability. Specifically, we have highlighted that PC3—characterized by a southward shift in the ASL position combined with increased central pressure—showed the strongest negative correlation ($r = -0.34$) with the $\delta^{18}O$ record from Hercules Névé. We have provided clear references to these PCA results in Section 4.2, accompanied by supporting figures (Figure 6, Table 1).

Regarding the Southern Annular Mode (SAM), although SAM itself was not explicitly analyzed in our main methodology, we have included a brief introduction and discussion in Section 4.3 about the known interplay between ASL and SAM based on Servettaz et al. (2020b). To quantitatively assess whether SAM influenced our isotope records indirectly, we have also described the results of our preliminary EOF analysis of 500 hPa geopotential height, showing that the SAM-like leading mode (PC1) had only a very weak correlation with $\delta^{18}O$ ($r = 0.09$). Conversely, a ZW3-like structure

(PC2) showed more relevance, indicating that regional circulation features such as ASL and ZW3 likely exert a more direct influence on isotope variability at Hercules Névé compared to SAM. We have explicitly included and clarify this point in the revised Section 4.3.

L. 235: This statement seems misleading. Over 50% of the accumulation falls outside the summer months, which, as a priori assumption, will greatly influence the isotopic composition on an annual resolution. You could elaborate on this by presenting annual vs summer statistics. Please give details and rephrase.

Answer: Thank you for highlighting this important point. In the revised manuscript, we have explicitly corrected and clarified our original statement in line 235, acknowledging that austral summer months (DJF) account for approximately 35 ± 4% of the total annual precipitation (based on ERA5 data for 1979–2015). To better illustrate this seasonal distribution, we have included a brief comparison of annual vs. summer precipitation statistics explicitly in Section 3.1.

Moreover, we have clearly stated that although a significant proportion of precipitation falls outside summer months, the relatively warmer temperatures and higher frequency of snowfall events during DJF may disproportionately enhance the isotopic signal recorded in the ice core. This clarification has avoided previous generalizations and more accurately reflected seasonal representativeness in isotopic composition at Hercules Névé.

L. 248: Why are you expecting reduced post-depositional processes in warmer conditions? Both diffusion and sublimation influences are stronger in warmer conditions. See (Johnsen et al., 2000; Ollivier et al., 2024)

Answer: Thank you for highlighting this issue. In the revised manuscript, we have clearly corrected our previous wording in line 248. Specifically, we have explicitly acknowledged that post-depositional processes such as sublimation and isotopic diffusion generally intensify under warmer conditions, as pointed out by the reviewer (e.g., Johnsen et al., 2000; Ollivier et al., 2024). Our original intent was to suggest that at Hercules Névé, the relatively high snowfall frequency during the austral summer (DJF) might mitigate the overall impact of these processes by accelerating the burial of newly deposited snow layers, thus reducing their exposure time at the surface. This clarification has been explicitly provided in the revised manuscript, accurately describing the intended physical mechanism and avoiding any incorrect implications regarding temperature-dependent post-depositional processes.

L. 251: Calling a correlation of r=0.32 robust is a stretch. Please rephrase.

Answer: Thank you for this comment. In the revised manuscript, we have corrected the wording in line 251 to avoid overstating the strength of the observed correlation ($r = 0.32$). Specifically, we have rephrased the sentence more carefully to indicate that $\delta^{18}O$ is moderately correlated with summer surface temperature, suggesting a direct yet modest relationship, without implying robustness or strong statistical significance.

L. 255: Or atmospheric rivers. Please discuss. (Wille et al., 2025)

Answer: Thank you for highlighting this important aspect. In the revised manuscript (line 255 and related discussion sections), we have briefly discussed the potential role of atmospheric rivers (ARs) in moisture transport to Antarctica, explicitly referencing recent findings (Wille et al., 2025). However, we have also clearly stated that, according to existing studies (e.g., Shields et al., 2022; Wille et al., 2022; Hofsteenge et al., 2025), direct moisture contributions from atmospheric rivers to the Ross Sea and Victoria Land region, including the Hercules Névé area, appear relatively limited compared to coastal West Antarctica. Therefore, while acknowledging the importance of ARs as a general mechanism of Antarctic moisture transport, we have clarified explicitly why AR-related processes were not considered as a primary influence on isotope variability at Hercules Névé in this specific study.

Correlation with temperature: From your analysis, it seems that the parameter to test is the precipitation-weighted temperature correlation, which is expected to perform better than temperature alone, especially in ice cores with seasonally biased accumulation. Please see (Persson et al., 2011) and include in analysis.

Answer: Thank you for this helpful suggestion. Although precipitation-weighted temperature can indeed enhance isotope–temperature relationships in strongly seasonally biased environments (Persson et al., 2011), we have considered the use of unweighted DJF mean temperature appropriate in our study due to the moderate seasonal accumulation bias at Hercules Névé (~34% during DJF) and the absence of extreme snowfall events.

L. 342: Please be more specific in your interpretations. The d18O variability is influenced by ZW3 and ASL on what timescales and what are you basing this on? How does SAM influence the indices you are investigating and could this be a common large atmospheric mechanism (L. 345).

Answer: Thank you for this helpful comment. In the revised manuscript, we have clearly specified that the influence of both ZW3 and ASL on $\delta^{18}O$ variability is evaluated explicitly at the seasonal timescale (DJF) using mean seasonal values from 1979 to 2015. Specifically, we have stated that the ZW3 index (Goyal et al., 2022) was averaged over DJF to correspond exactly with the ice-core derived $\delta^{18}O$ seasonal averages. Similarly, ASL-related parameters (strength, latitude, longitude) have also been averaged seasonally (DJF), and their relationship with $\delta^{18}O$ was analyzed through correlation and principal component analysis (PCA).

Additionally, we have explicitly addressed the broader atmospheric context by introducing and briefly discussing the Southern Annular Mode (SAM). We have clearly stated that an EOF analysis of the 500 hPa geopotential height (ERA5) was performed, and the leading mode, resembling the SAM, showed no significant correlation with $\delta^{18}O$ ($r = 0.09$, $p = 0.58$). Conversely, the second EOF mode, associated primarily with regional-scale variability such as ZW3 and ASL, exhibited a significant correlation ($r = –0.44$, $p = 0.007$). This clearly indicates that $\delta^{18}O$ variability at Hercules Névé is predominantly influenced by regional circulation features (ZW3, ASL) rather than hemispheric-scale modes such as SAM. These clarifications and detailed interpretations have been explicitly presented in the revised Section 4.3.

L. 382: Please delete the last sentence as this manuscript is not actually using stable water isotopes to draw conclusions about the past climate state, but in reverse, climate variables are used to try to explain the isotope record. The conclusions drawn from the Hercules Névé ice core at this point are not clear enough to interpret the stable water isotope record unambiguously back in time.

Answer: Thank you for this clarification. In the revised manuscript, we have deleted the final sentence at line 382 as suggested, to avoid overstating the scope and implications of this study. Specifically, we acknowledge that our current analysis uses climate variables primarily to explain isotope variability rather than directly reconstructing past climate conditions. Removing this sentence clearly reflects the actual scope and limitations of our interpretations.

Technical corrections:

L. 57: This is formulated in a confusing way. The reason for isotopic depletion during expanded sea ice conditions is the increased distillation pathway, not the colder regions (northward expanse would result in more evaporation in warmer regions, except if the authors wanted to highlight increased moisture recycling but this would need to be stated specifically here). Please see (Noone, 2004) and rephrase.

Answer: Thank you for clearly pointing out this confusion. In the revised manuscript, we have explicitly corrected and clarified the mechanistic explanation for isotopic depletion related to expanded sea ice conditions in line 57. Specifically, we have clearly stated that isotopic depletion under expanded sea ice is primarily caused by increased distillation pathways, as demonstrated in Noone (2004), rather than by colder source regions. Additionally, we have explicitly highlighted the complexity and regional variability of sea ice–isotope interactions, referencing recent observational studies such as Song et al. (2023) to emphasize the spatial and seasonal variability involved. This clarification ensures that our description accurately reflects the current understanding of the underlying isotopic processes. The sentence has been revised as following, “Reduced sea ice exposes nearby open ocean areas, enhancing evaporation from local sources and typically leading to enriched δ18O values due to shorter transport distances. Conversely, expanded sea ice displaces evaporation sources farther from the continent, lengthening the distillation pathway and promoting isotopic depletion. However, recent studies indicate that these relationships are not spatially or seasonally uniform, and can be modulated by regional atmospheric circulation, moisture recycling, and synoptic activity (Song et al., 2023).”.

L. 65: Please add information on what timescales the ZW3 index is “critical”

Answer: Thank you for this helpful suggestion. In the revised manuscript, we have explicitly clarified in line 65 that the ZW3 index is particularly relevant (“critical”) on seasonal to interannual timescales. This has more precisely defined the timescale context of ZW3's importance for our study and enhance clarity for the readers.

L. 67: Check citation brackets

Answer: Thank you for pointing out this issue. We have carefully reviewed and corrected the citation brackets at line 67 in the revised manuscript.

L. 75: ice sheet not ice sheets

Answer: Thank you for pointing this out. We have changed the term "ice sheets" to the singular form "ice sheet" at line 75 in the revised manuscript.

L. 105-107: The reason for intensive sublimation during katabatic winds is that these air masses are predominantly very dry and can thus take up a lot of water but their influence on the local climate and temperature is not always one-directional. See e.g. (Davrinche et al., 2024; Vihma et al., 2011). Please rephrase or cite relevant literature.

Answer: Thank you for this valuable comment. In the revised manuscript (lines 105–107), we have explicitly clarified that katabatic winds are characterized not only by low temperatures but also by extremely dry conditions, which significantly enhance surface sublimation and can influence the isotopic composition.

Additionally, we have clearly acknowledged that the impact of katabatic winds on local temperature and energy balance is not always unidirectional, but can vary depending on broader atmospheric conditions. Relevant literature references (e.g., Vihma et al., 2011; Davrinche et al., 2024) have been explicitly included to support this clarification.

The sentence has been revised as following, "This region is characterized by austral summer-dominant precipitation patterns influenced by easterly winds that bring moist air from the ocean onto the continent (Scarchilli et al., 2011). Katabatic winds descending from the East Antarctic Plateau are typically cold and extremely dry. Their dryness enhances sublimation from the snow surface, which can lead to isotopic modification of near-surface layers (Nyamgerel et al., 2024; Vihma et al., 2011). However, their impact on local temperature and the surface energy balance is not always unidirectional and may vary depending on synoptic conditions (Davrinche et al., 2024)."

L. 116: When was the ice core drilled?

Answer: Thank you for pointing out this omission. In the revised manuscript, we have explicitly stated that the ice core was drilled between 11 and 15 December 2015 (line 116).

L. 118: This sounds like not all parts of the ice core were frozen? Please rephrase

Answer: Thank you for highlighting this issue. In the revised manuscript, we have removed the ambiguous wording ("frozen") from line 118 and clearly rephrase the sentence to accurately describe the standard procedures used for ice core handling and storage, avoiding any implication of partial freezing.

L. 130: How long between cutting, melting and subsequent analysis? Add time.

Answer: Thank you for pointing this out. In the revised manuscript (line 130), we have explicitly specify the typical duration between ice core cutting, melting, and subsequent isotopic analysis, clearly detailing the time intervals involved in sample preparation.

L. 138: Are these the manufacturer's numbers for accuracy? Please add a value that is representative for the measurement protocol and the laboratory at which the samples were measured? Usually this is done by referencing a control lab standard. See (Sodemann et al., 2023)

Answer: Thank you for highlighting this issue. In the revised manuscript (line 138), we have clearly specified that the reported measurement precision values (± 0.07 ‰ for δ^{18}O and ± 0.1 ‰ for δ^{2}H) reflect the actual analytical performance of our laboratory at KOPRI. Specifically, we have stated that these precision values were determined based on long-term repeated measurements of internal laboratory standards conducted over multiple years, following our established analytical protocol (as described in Kim et al., 2022). Additionally, we have explicitly mentioned that each sample underwent 12–15 injections, with the final 5 measurements averaged to ensure thermal and instrumental stability, further supporting the accuracy and precision representative of our measurement conditions.

L. 156: Are you using new "data" to calculate the ZW3 index? If I understood correctly you are using the newly developed index by Goyal 2023 et al.. Which data is the basis for the ZW3 index calculations you are using? Please specify.

Answer: Thank you for this helpful comment. In the revised manuscript (line 156), we have explicitly clarified that the monthly ZW3 index used in our analysis is the newly developed index by Goyal et al. (2022), calculated based on ERA5 sea level pressure fields. We have clearly specified this data source and the rationale for selecting this particular version of the ZW3 index in our analysis.

L. 173: Do you have access to "chemical variations" for the age model?

Answer: Thank you for pointing this out. In the revised manuscript (line 173), we have explicitly stated that chemical variations such as ion concentrations or dust records were not available and thus not used for age model development. Instead, we have clarified that the age model was solely based on stable isotope variations and validated using independent reference tie points (e.g., Pu peaks). We have clearly revised the sentence to reflect this limitation.

L. 185: The shaded area is not green in my pdf version.

Answer: Thank you for pointing out this error. In the revised manuscript, we have corrected the description at line 185 by accurately referring to the shaded area as grey, rather than green, to match the actual figure. This correction ensures clarity and consistency for readers.

L. 198: Please include the results of the "multiple linear regression" for the ZW3 index in the results section.

Answer: Thank you for pointing out this mistake. In the revised manuscript (line 198), we have clearly corrected the methods section to explicitly state that we performed only Pearson correlation analyses, rather than multiple linear regression, to assess the relationship between δ^{18}O and the ZW3 index. Accordingly, no multiple regression results have been presented, and we have clearly corrected this error to avoid confusion.

L. 201-203: These lines should go in the introduction.

Answer: Thank you for this suggestion. In the revised manuscript, we have moved the mentioned lines (lines 201–203) from their current position in the Methods section to the Introduction section, as recommended, to improve clarity and the logical flow of the text.

L. 206: Since the averaging periods are not all the same this comparison is not very insightful. I suggest to keep them in the supplementary Table S1 and possibly compare only the overlapping periods in the main text.

Answer: Thank you for this helpful suggestion. In the revised manuscript (line 206), we have simplified the comparison in the main text by clearly focusing only on the overlapping periods for direct comparisons between the Hercules Névé record and nearby cores (e.g., Styx–M, Whitehall Glacier, Talos Dome). The complete comparison of all cores and varying averaging periods have been clearly presented separately in Supplementary Table S1. This adjustment enhances clarity and avoid confusion due to inconsistent averaging periods.

The sentence have been revised as following, "Figure 2 presents the water isotope data for the 1979–2015 period from Hercules Névé. The δ18O values in the Hercules Névé ice core ranged from –42.29 ‰ to –29.67 ‰, with a standard deviation of 1.69 ‰, while those for δ2H ranged from –334.5 ‰ to –226.1 ‰, with a standard deviation of 11.75 ‰. Several other near-coastal ice cores have been studied in the broader Victoria Land region, including Styx–M, Whitehall Glacier, and Talos Dome, each offering different altitudinal and geographical contexts. A summary of isotope statistics and core characteristics for these sites have been provided in Table S1.

L. 215-220: Are lines 219 and 220 a repetition of 215 and 216? If not please rephrase so it's less confusing.

Answer: Thank you for highlighting this repetition. In the revised manuscript, we have removed the redundant lines (lines 219–220) to avoid confusion and improve the overall clarity of the text.

L. 223: include one decimal place in the slope values

Answer: Thank you for the suggestion. In this case, we are directly referring to the Global Meteoric Water Line from Craig (1961), which is conventionally expressed with a slope of 8 and an intercept of 10, without decimal places. For consistency with the original reference, we have retained the conventional format without decimal places in the revised manuscript.

L. 253: delete double "that"

Answer: Thank you for pointing this out. We have removed the duplicated word "that" in line 253 of the revised manuscript.

L. 278: This is formulated in a confusing way. As sea-ice concentration increases, the moisture sources are shifted northward and evaporation from local oceanic sites is limited. Please rephrase

Answer: Thank you for highlighting this confusion. In the revised manuscript (Section 4.1, line 278), we have clearly rephrased this sentence to explicitly state that increased sea-ice concentration shifts moisture sources northward and limits evaporation from local oceanic areas. This revised wording ensures improved clarity and accurately reflect the physical mechanism involved.

Fig. 1: As the bathymetry is not important for this study it might be more useful to show elevation starting from sea level. Please also add wind rose with AWS data to this figure.

Answer: Thank you for this helpful suggestion. In the revised manuscript (Fig. 1), we have modified the map to clearly represent elevation starting from sea level, as bathymetry is not relevant to our study. Additionally, although the AWS wind rose data are currently presented in the Supplementary Material (Figure S2), we have explicitly mentioned this clearly in the figure caption of Fig. 1 for improved cross-referencing and clarity.

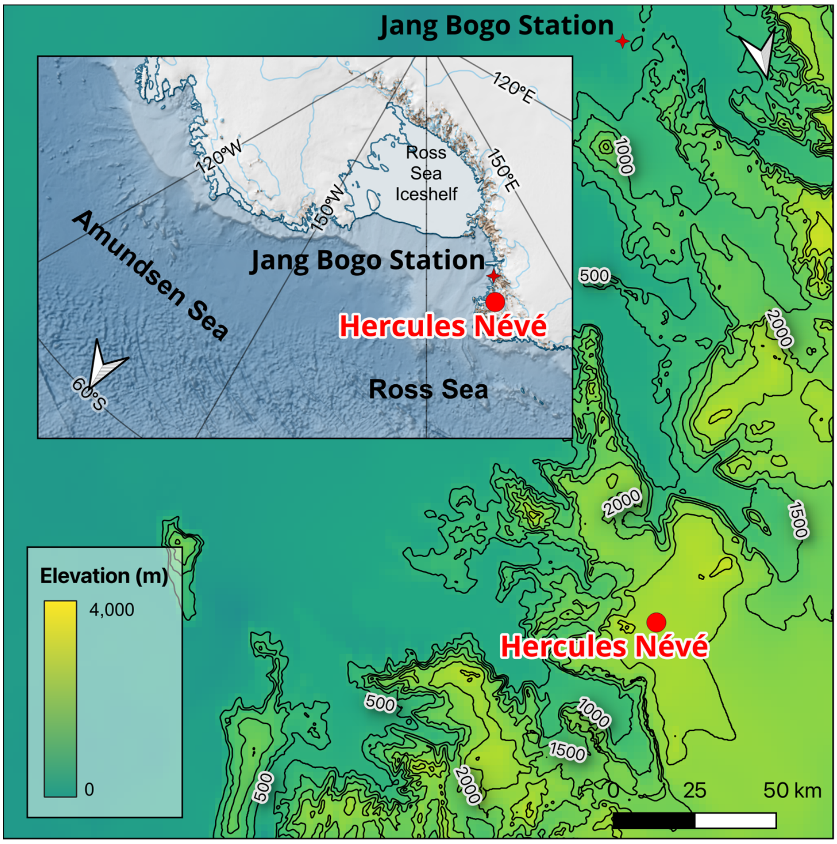


Figure 1 (A) Map of the West and East Antarctica regions showing the Ross Ice Shelf, the Ross Sea, and Hercules Névé. (B) Topographic map of Northern Victoria Land. The red circles indicate the location of the Hercules Névé ice core. The maps were created using Quantarctica 3.2 on QGIS.

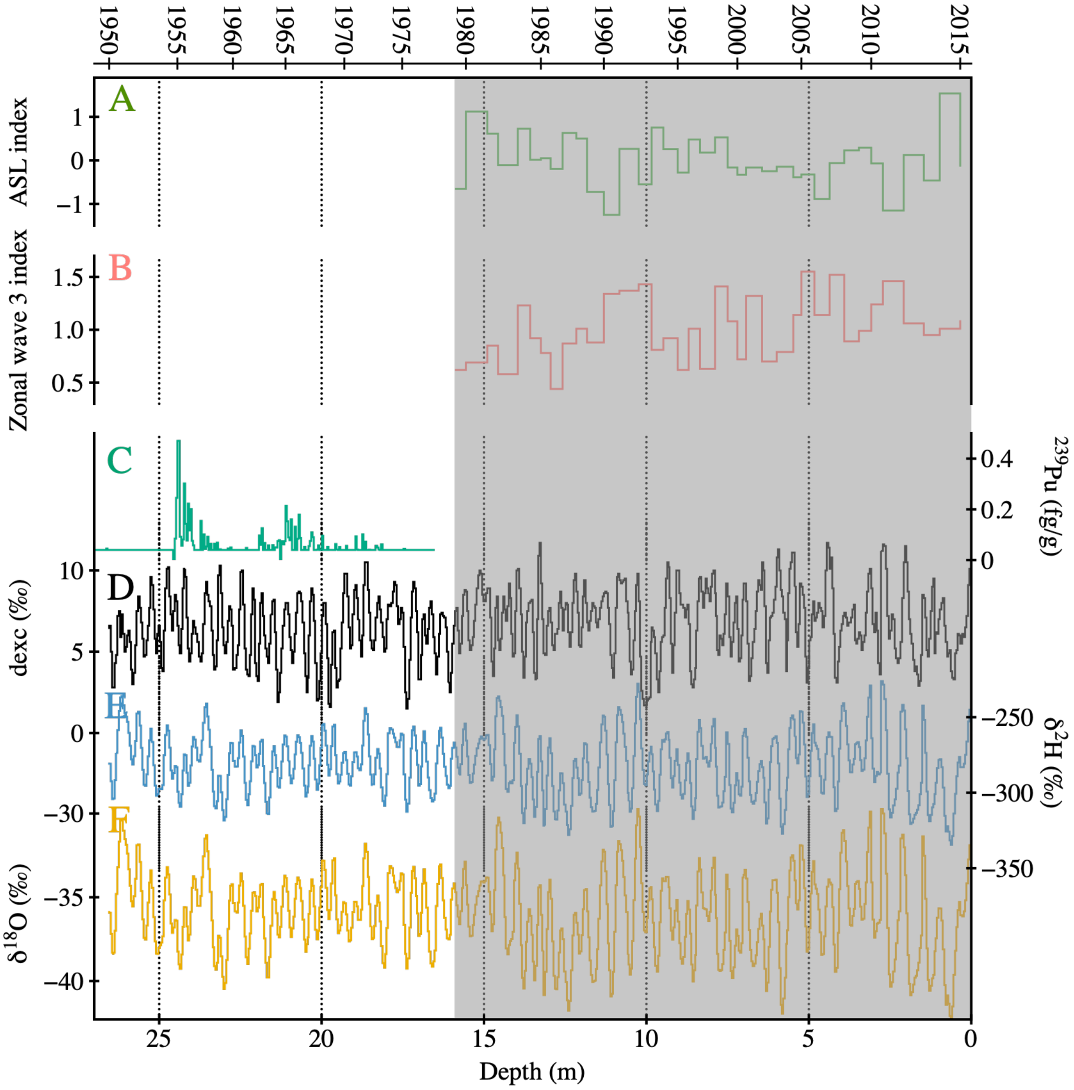


Figure 2: Depth profiles of climate indices and isotopic parameters from the surface to a depth of 26 m from the Hercules Névé ice core. Panel A represents the Amundsen Sea Low (ASL) index derived from principal component analysis (PCA) based on longitude, latitude, and strength of the ASL defined in Hosking et al. (2013), specifically displaying PC3, which showed significant correlation with isotopic data. Panel B shows the Zonal Wave 3 (ZW3) index obtained from Goyal (2022). Panel C depicts the 239Pu concentration profile for the period 1950–1975. Panels D–F illustrate water isotope measurements (dexc, δ²H, and δ¹⁸O, respectively) spanning from 1950 to 2015. Data utilized for detailed analysis in this study (1979–2015) are indicated by the shaded grey region.

Fig. 2: The shaded area is not green in my pdf but grey. Consider adding the indices to this plot or make a new plot including temperature (and precipitation) and indices.

Answer: Thank you for the helpful suggestion. In the revised manuscript, we have corrected the figure caption of Fig. 2 to clearly describe the shaded area as “grey,” rather than green, to match the actual figure accurately.

Regarding the suggestion of adding temperature, precipitation, and climate indices (such as ZW3 and ASL) to Fig. 2, we have recognized the potential value of such an integrated visualization. However, in order to maintain clarity and avoid overcrowding, we have retained the current figure layout focusing specifically on the isotope records. We have acknowledged the value of the proposed composite visualization and clearly state our intention to pursue this approach in future studies with expanded datasets.

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 have explicitly clarified and enhanced 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 have elaborated 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 have explicitly discussed 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 have provided detailed responses to each of the reviewer's comments.

Moreover, in response to the reviewer's valuable suggestions, we have provided quantitative statistical evidence demonstrating the reliability of ERA5 reanalysis in representing local climatic conditions at Hercules Névé. Specifically, we have included 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 have also included 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 have included a brief discussion based on the δ^{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 have significantly improved 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 have explicitly acknowledged that analyzing annual and other seasonal composites could provide broader climatological insights, particularly related to long-range transport processes. However, we have clearly articulated our rationale for focusing specifically on the austral summer months (DJF).

Specifically, we have emphasized 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 have explicitly mentioned 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 have explicitly addressed 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 δ^{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 δ^{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 have explicitly notee 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 have added 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 have carefully adjusted our language to clearly avoid implying overly strong causal relationships. Specifically, we have replaced 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 have explicitly presented 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 have collectively strengthened 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 have explicitly incorporated the following methodological enhancements:

(1) Atmospheric dynamics: We have performed 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).

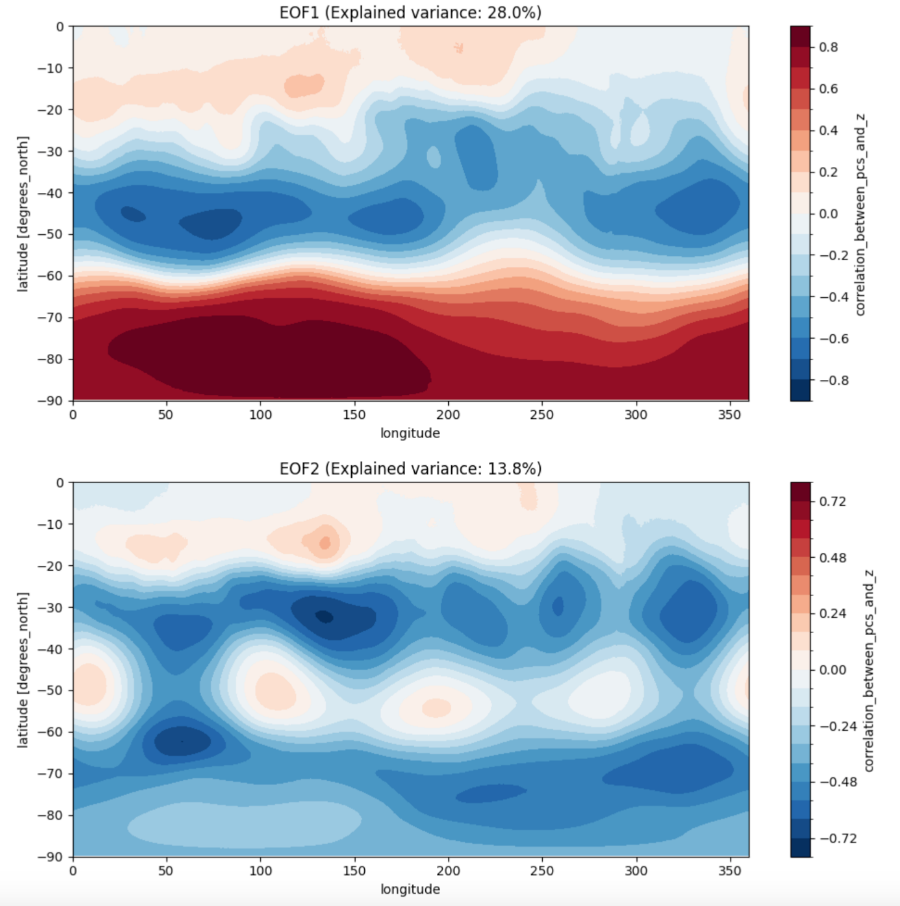
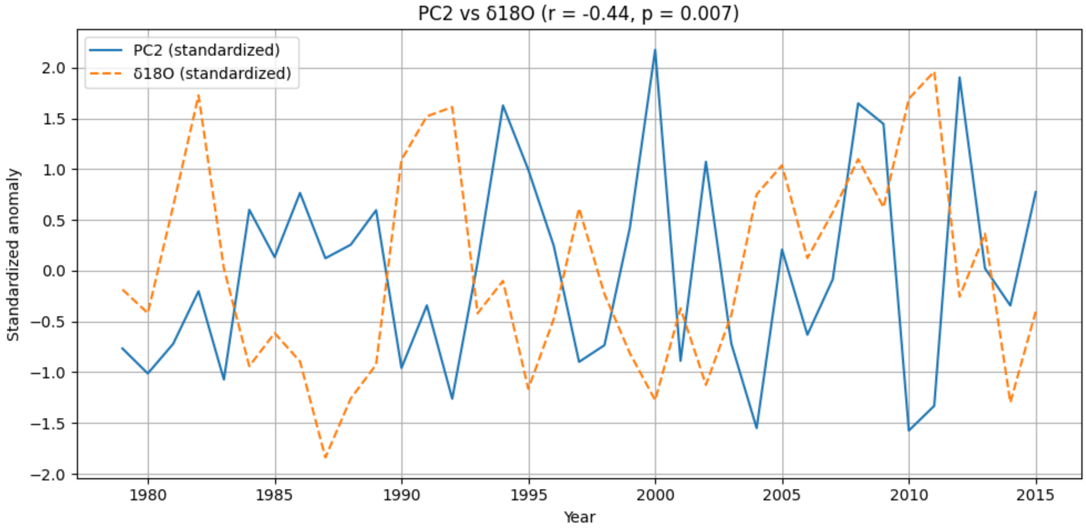


Figure S6. Empirical Orthogonal Function (EOF) analysis of mean sea level pressure (MSLP) during the austral summer (DJF: December–January–February) using monthly data.

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.



(2) Statistical rigor and multi-variable analysis: To robustly assess statistical relationships between $\delta^{18}O$ and multiple climatic drivers, we have explicitly applied Principal Component Analysis (PCA), using $\delta^{18}O$ as the dependent variable and annual mean temperature, accumulation, and ASL indices as explanatory variables. This enhances 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 have explicitly presented a detailed back-trajectory analysis (using HYSPLIT), specifically contrasting moisture source pathways in years characterized by intensified versus weakened ASL conditions. This have clearly provided physical evidence for shifts in moisture transport pathways linked to observed isotopic variability.

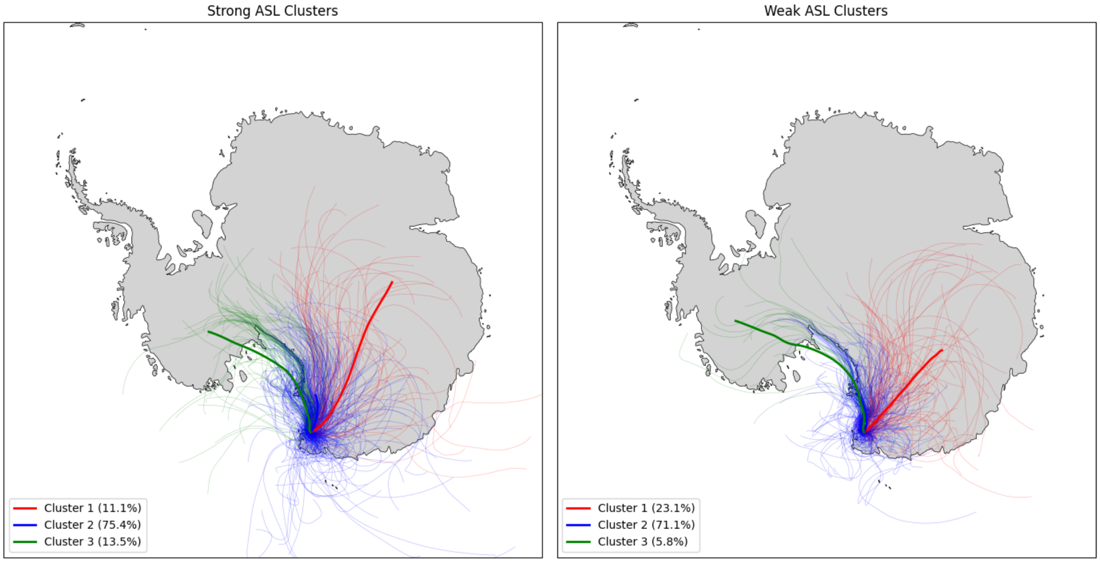


Figure 7. 7-day HYSPLIT back-trajectory clusters arriving at Hercules Névé during (left) the five strongest ASL summers and (right) the five weakest ASL summers (DJF, 1979–2015). Strong ASL cases show dominant air mass transport from inland Antarctica and the Ross Ice Shelf, while weak ASL cases are 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 have explicitly included a York regression analysis comparing temperature measurements, taking into account uncertainties in both datasets. This analysis has been clearly presented in the Supplementary Material (Figures S1C and S2), strengthening the reliability of our climatological interpretations.

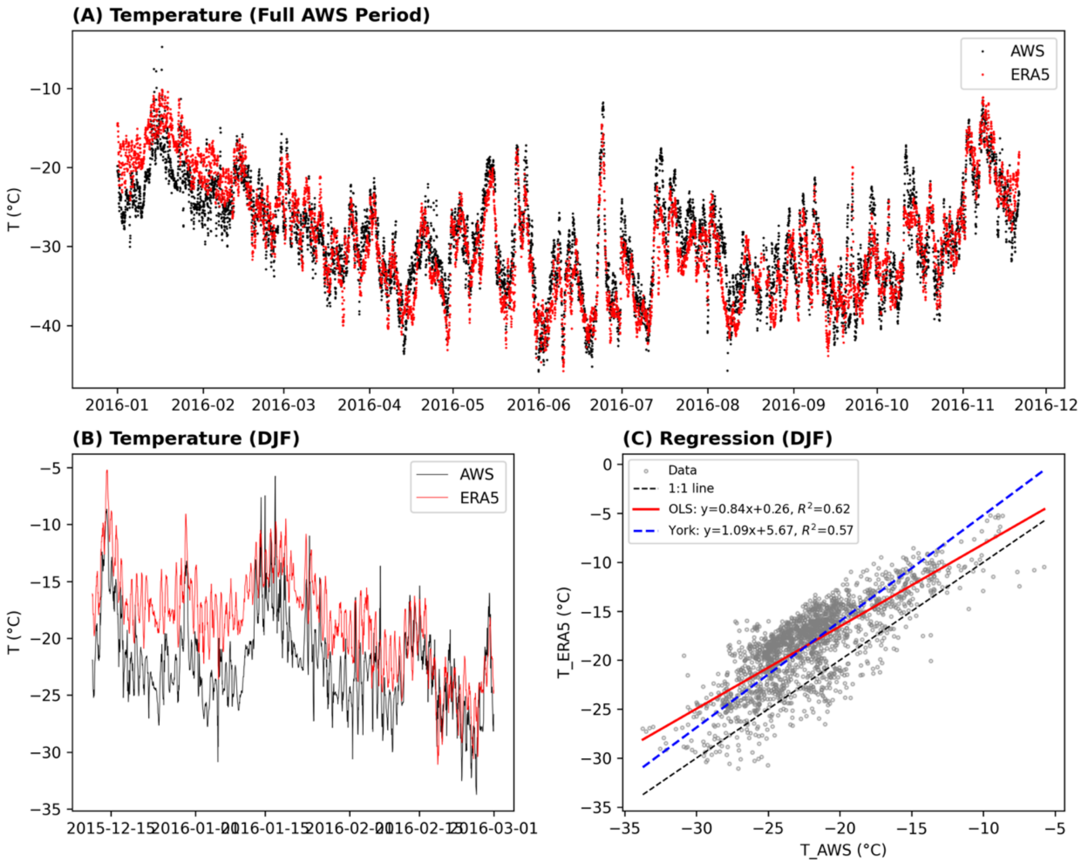


Figure S1. Comparison of AWS and ERA5 2 m air temperature at Hercules Névé. (A) Daily temperature from December 2015 to December 2016. (B) Temperature time series during the DJF period. (C) Regression analysis during DJF, showing the relationship between AWS and ERA5 values using both ordinary least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

Collectively, these methodological enhancements have explicitly addressed 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 have explicitly corrected 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 have further clarified that this longer transport pathway enhances isotopic depletion (along-path depletion). This revision accurately reflects 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 have carefully adjusted the language throughout to avoid implying causality when discussing ZW3. Specifically, we have replaced terms like “causing” or “affecting” with more accurate statistical descriptors such as “associated with,” “related to,” or “reflecting.” This appropriately reflects 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 have explicitly corrected 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 have significantly expanded our rationale in the revised paragraph starting from line 71. Specifically, we have clearly outlined why DJF provides the most favorable conditions for interpreting isotopic variability in the Hercules Névé region. This justification have explicitly included: (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 have explicitly stated 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 have been explicitly proposed for future studies. We have clearly articulated 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 have 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 have included 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 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 have removed 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 have explicitly included numerical values for previously reported accumulation rates at Hercules Névé to provide a clearer context. Specifically, we have stated 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 significantly enhances 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 have explicitly clarified 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 have explicitly presented 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 have explicitly discussed this result, clearly indicating that this supports our interpretation that seasonal isotopic signals are sufficiently preserved at our sampling resolution. This additional clarification have been 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 have explicitly acknowledged 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 have retained 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 have clearly stated that our temperature comparison between AWS and ERA5 is based on a single year of overlapping observations (2015–2016), and we have explicitly applied a York regression to account for uncertainties in both variables, clearly presenting the updated results in Supplementary Figure S1B. This approach has corrected for potential biases inherent in ordinary least squares (OLS) regression.

Additionally, to enhance the environmental context and clearly address the reviewer's suggestions, we have included 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 significantly enhances 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 have fully acknowledged 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 have clearly addressed this issue by presenting the slope-based diagnostic approach from Casado et al. (2021), which assesses post-depositional isotopic alterations through δ^{18}O–d-excess relationships. Specifically, our DJF data consistently exhibit positive δ^{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 have explicitly highlighted 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 have emphasized 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, have been 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 have explicitly clarified 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 have explicitly included 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 has offered meaningful insights into how regional and continental-scale pressure variability relates to isotopic variability at Hercules Névé. Furthermore, we have clearly stated 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 have explicitly clarified 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 have clearly indicated in the methods section that EOF analyses are separately performed on ERA5 pressure fields for additional atmospheric context.

Regarding the seasonal definitions, we have explicitly described 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 have explicitly included further discussion in the revised Methods and Results sections. We have presented 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 have included 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 have clarified 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 have explicitly reorganized the presentation of the deuterium excess (d-excess) results. Specifically, we have relocated 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) have then focused exclusively on interpreting the implications of the observed relationships between d-excess, sea ice concentration (SIC), and sea surface temperature (SST). This structural change has improved 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 have explicitly addressed the possibility that d-excess can change independently after deposition, even if δ^{18}O remains stable, as described by Town et al. (2024). As outlined in our response to the earlier comment on line 232, we have presented 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 have explicitly clarified 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 have explicitly revised 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 have added 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 have explicitly acknowledged their general importance in moisture transport toward Antarctica. However, we have clearly stated, 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 have been 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 have explicitly acknowledged 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 have clearly adopted 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 explicitly reflects 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 have explicitly adjusted 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 enhances the spatial relevance and clarity of our correlation results. Additionally, we have expanded 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 improve both the interpretive clarity and spatial specificity of our analysis.

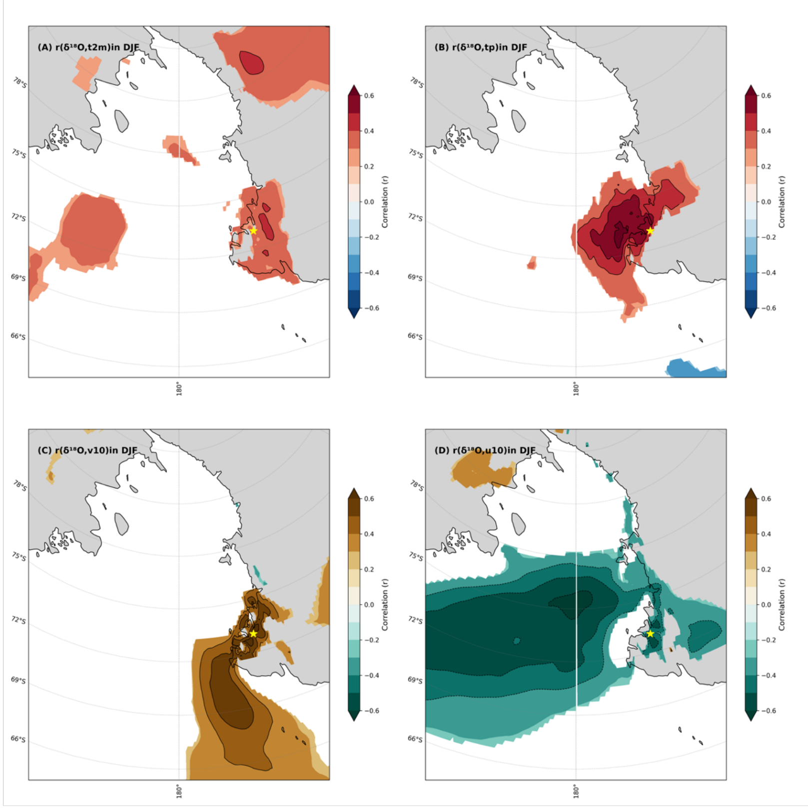


Figure 3: Spatial correlation analysis of (A) the 2 m temperature (T2m), (B) total precipitation (tp), (C) 10 m v-wind (v10), and (D) 10 m u-wind (u10) across Antarctica based on ERA5 data with $\delta^{18}O$ in HN ice core (black symbol) from 1979 to 2015 during DJF. The $p < 0.1$ confidence level is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

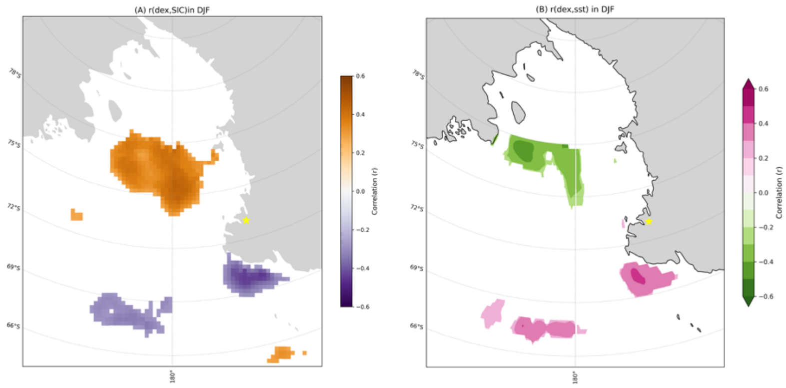


Figure 4: Spatial correlation analysis results between the secondary parameter dexc from the HN ice core (black symbol) and (A) the sea ice concentration (SIC) and (B) sea surface temperature (SST) from ERA5 data during DJF for 1979–2015. The $p < 0.1$ confidence level is indicated by black contours.

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 have explicitly incorporated 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 have clearly presented and discussed 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 have retained the simpler DJF mean temperature fields to maintain clarity and avoid redundancy, but we have explicitly stated 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 have explicitly clarified and expanded 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 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 has clearly addressed 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 subsequent research.

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 have explicitly corrected 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 have revised 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 accurately reflects 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