

Q# refer to questions by the reviewers, A# refer to our replies to the corresponding question, **line number** mentioned in our replies refer to the revised version of manuscript.

**Review of Boxho et al, CP
Dec. 30, 2025 by Bess Koffman**

This paper applies a previously developed statistical ‘unmixing’ model to a previously published rare earth element (REE) dataset from the EPICA Dome C ice core in Antarctica to infer changing dust source contributions over time. In general, I think this is a worthwhile exercise with the potential to clarify broad interpretations about changing dust provenance during climate transitions, and their causes. Following major revisions, it should be appropriate for publication in *Climate of the Past*.

We thank the reviewer for this positive overall assessment and for recognizing the potential of our approach to clarify changes in dust provenance across climate transitions. While our study indeed builds on previously published REE measurements from the EDC ice core, we would like to emphasize the fact that our quantitative statistical unmixing framework opens a whole new perspective to this unique REE dataset, offering an unprecedented interpretation of dust source variability through time.

We are grateful to Dr. Bess Koffman for the time and care devoted to this review. Below, we have addressed each comment in detail and describe how we revised the manuscript accordingly to clarify the novelty, scope, and implications of our work.

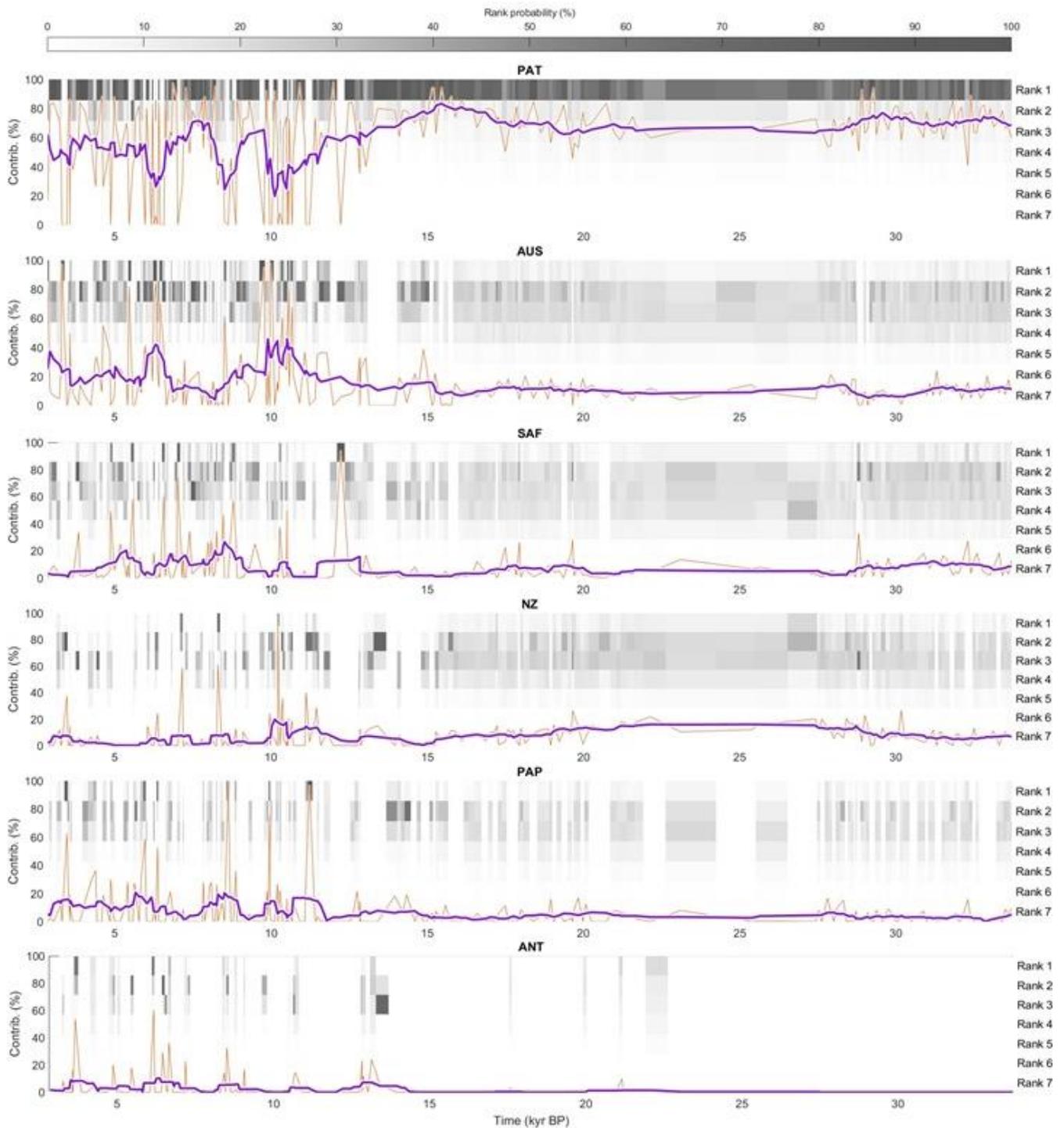
The paper needs substantial work to be publishable. My main critiques are that 1) the estimates of potential source area (PSA) contributions are presented without uncertainties, making them basically meaningless; 2) differences in interpretation between this study and previous ones are not thoroughly described and supported with evidence; 3) the Younger Dryas is included in the interpretation as if it were a climate event in Antarctica; I find this baffling given the geographical focus of the paper; 4) figures have a range of issues (see comments below); 5) the writing needs major revision with respect to grammar, subject/verb agreement, punctuation, and spelling. In addition, the introductory paragraph follows the structure and citation ordering of the Vanderstraeten et al 2023 study from the same group (thanks to the other reviewer for pointing this out). While I wouldn’t call it plagiarism, the similarity is striking. This paragraph should be rewritten and additional citations included.

Q1: Regarding point 1) above: in the paragraph starting on Line 153, analytical uncertainties are discussed, along with their impact on the model outputs, but the uncertainties on the dust provenance estimates from the MC simulations themselves are not given anywhere (unless I missed them somehow). This seems like a simple correction but a very important one. Uncertainties should be incorporated into the text where numerical values are given, as well as into the figures where temporal changes in PSA contributions are indicated (i.e. as colored confidence intervals).

A1: We thank the reviewer for raising this important point. The quantification of the uncertainty of our results captured by the variability between our Monte Carlo (MC) simulations is something we might not have sufficiently reported in the previous version of the manuscript, but the request of the reviewer prompted us to improve this aspect of the study. An important challenge however is to fairly represent these uncertainties. Each Monte Carlo run simulated by our unmixing model yields, a different combination for each time step which do not always include all the sources contributing to the dust sample. This variability results in distributions of the contributions of our PSA across all MC simulations that are generally not gaussian or symmetric and are at times even bimodal. This was already described and illustrated in Fig. S4 of Vanderstraeten *et al.*, (2023) but it implies that the sole reporting of ‘uncertainty range’ only conveys part of the information regarding the robustness of our simulation and, alone, can even be misleading. To circumvent this limitation and provide a complete picture of our results, we believe they should also be completed by a more qualitative assessment related to the ranking of the PSA. Thus, in the revised manuscript, we now explicitly report the statistical dispersion of the Monte Carlo (MC) outputs associated with the dust provenance estimates. The already reported median (P50) values, used as the main estimate of source contributions, are

associated in Table S2 with the 25th (Q1) and 75th (Q3) percentiles of the probability density function (PDF) derived from the 2000 MC simulations for each PSA at every time step. These percentiles characterize the spread of the PDF and thus provide a quantitative characterization of the variability of the simulated PSA contributions.

In addition, to better illustrate the behavior of the MC simulations in Figure 1, we have also introduced a probabilistic visualization of the source ranking. The grayscale shading represents the probability, across the 2000 MC simulations, that a given PSA occupies a particular rank at each time step (see figure below). Darker shades indicate a higher probability that the source consistently occupies that rank, whereas lighter shades indicate greater variability among the simulations. This representation provides a visual measure of the robustness and dispersion of the MC solutions, highlighting periods when a source is consistently dominant versus intervals when multiple sources compete. We believe this approach conveys the variability inherent to the MC framework while preserving the readability of the figure. This probabilistic representation also highlights the stability of the unmixing solution by showing when a source persistently occupies a given rank across the ensemble of MC simulations, thereby providing an intuitive visualization of the uncertainty and robustness of the inferred dust provenance.



Revised figure 1: Evolution of PSA contributions to EDC ice core between 2.852 to 33.699 yr BP. Median values of PDF distribution are in red while bold purple line denotes the mobile average over 9 steps. Grayscale shading indicates the probability (in %) that the source occupies a given rank in the mixing across the 2000 Monte Carlo simulations (from rank 1 to rank 7).

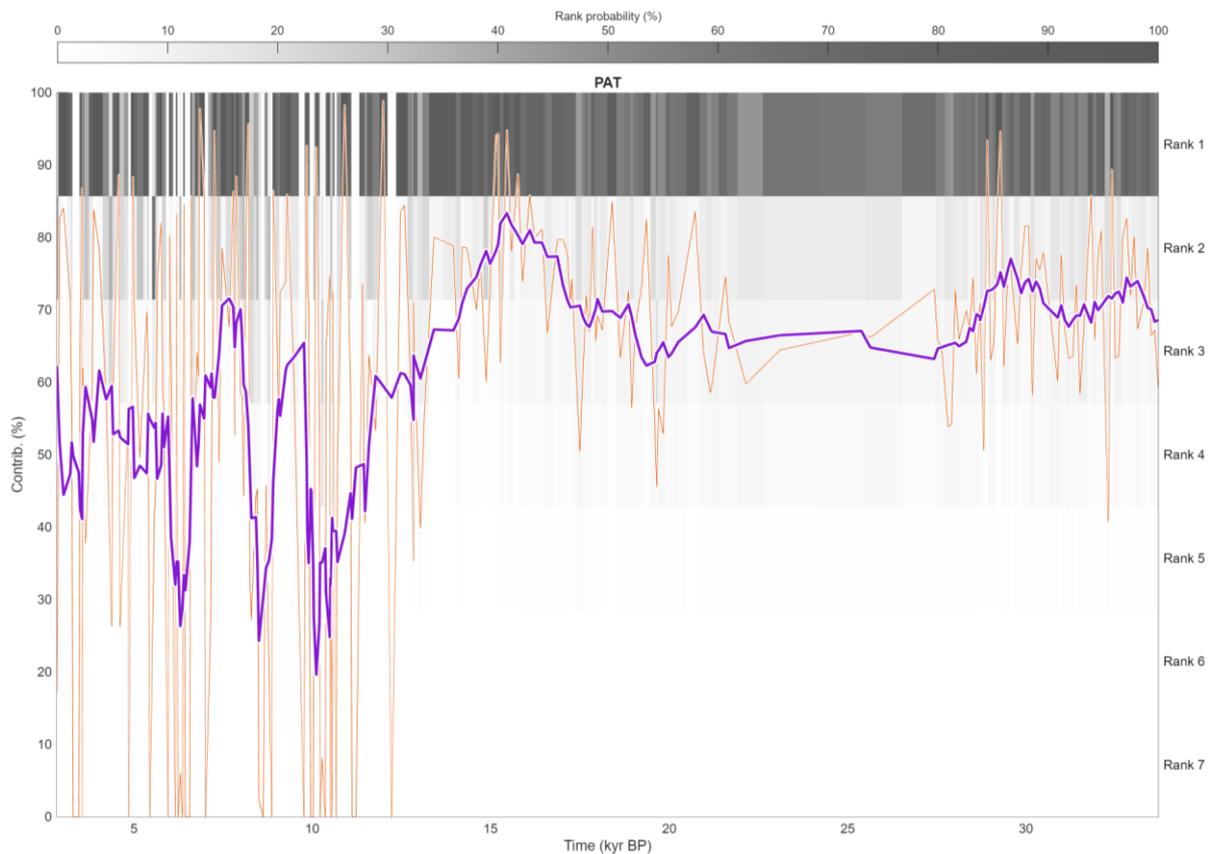


Figure A1: Evolution of PAT contributions to EDC ice core between 2.852 to 33.699 yr BP. Median values of PDF distribution are in red while bold purple line denotes the mobile average over 9 steps. Grayscale shading indicates the probability (in %) that the source occupies a given rank across the 2000 Monte Carlo simulations.

Q2: Regarding point 2) above: there are a number of places where comparisons to prior publications (and their interpretations) are given in a cursory manner, but differences are not fully explored or justified. Considering the volume of published papers on Antarctic dust provenance, this is a place where the present study could really aim to leverage its findings for new insights. One example relates to South American dust sources. The explanation of why the southern Puna and southern Altiplano both contribute dust but not the northern Puna (which is geographically sandwiched between them) needs to be clarified and better justified. This is especially confusing because in Fig. 3a of Gili et al 2017, the REE fields of the northern and southern Puna fully overlap, and their paper includes N Puna as a source. What specific REE ratios support eliminating N Puna as a dust source to EDC? I would also like the authors to reference and/or address the statement in Gili et al 2017 that “REE are less useful for distinguishing sediments from CWA and Puna.” The Gili paper highlighted CWA as a more important source than Patagonia, which I don’t really see addressed in the present study.

A2: We thank the reviewer for this important comment. We acknowledge that our results differ from previous interpretations/claims and we appreciate the opportunity to clarify those differences. In Gili *et al.*, (2017), the Sr-Nd domains of Northern Puna was defined for the first time, and at that time, provided an improved framework for interpreting EDC dust provenance. However, at that same in 2017, isotopic data from Southern western Africa, another important PSA for Antarctic dust, were not available. Subsequent work by Gili *et al.*, (2022) demonstrated that Sr–Nd–Pb isotopes from Namibian coast strongly overlaps with those of Northern Puna – making it difficult to distinguish between those two PSA using isotope alone. Regarding REE systematics, we agree that in Fig 3A of Gili et al., (2017), Southern and Northern Puna domains strongly overlap in Eu^*/Eu vs. La/Yb_N space. Indeed, in that two-dimensional representation REE appear less discriminating than isotopes, as stated

in Gili et al. (2017). Our approach differs in that we do not rely on bivariate projections but instead we use the full normalized REE pattern (13 elements) in a multidimensional unmixing framework. When considering the complete REE patterns, Northern and Southern Puna show systematic differences (i) with substantially higher Sm value in Southern Puna compared to Northern Puna (ii) (ii) the magnitude of the Eu anomaly and (iii) the enrichment in HREE segment. These differences are not readily visible in two-dimensional plots but become resolvable when the entire REE distribution is used simultaneously. On this basis, the model assigns negligible Northern Puna contributions to both EDC and EDML. We acknowledge, however, that additional sampling in Northern Puna and more generally in other PSA would further strengthen our model, and we now state this explicitly in the revised manuscript in the materials and methods section in lines 153-155: “As in most provenance studies, the robustness of the unmixing results ultimately depends on the representativeness of the source dataset, and additional sampling would help better capture the natural geochemical variability of the different source areas.”

We now discuss the Puna-Altiplano Plateau in lines 496-510:

The Puna - Altiplano Plateau emerges as a secondary dust contributor to EDC after 14.5 kyr BP, consistent with its strong sensitivity to aridification and the desiccation of riverbeds, alluvial fans, and paleolake systems. Dust from the South Puna contributes throughout the record, whereas the Southern Altiplano becomes active only after ~11.2 kyr BP (Figure S3), which coincide with the final desiccation of paleolake Coipasa and the establishment of persistent arid conditions (Condom et al., 2004; Blard et al., 2011; Placzek et al., 2006). Although Northern Puna has been suggested in several studies as a major source to EAP (e.g., Gili et al., 2017; Gili et al. 2022; Gaiero et al., 2013), differences in Ce, Sm, Eu and LREE contents allow our algorithm to distinguish between Northern and Southern Puna revealing a clear dominance of South Puna contribution in the EDC. Recently, Pullen et al. (2022) showed that South Puna was an active dust source area during the late Pleistocene and Holocene periods. The combined PAP dust assemblage—comprising a mafic component from the South Puna and a crustal component from the Southern Altiplano—is also consistent with Sr-Nd isotopic compositions of SSA sources and that of the Pampean loess reported by Coppo et al. (2022). Overall, the relatively important contribution of PAP to Est Antarctica during interglacial periods is consistent with previous provenance studies (Gili et al., 2016; 2017; Gaiero et al, 2013; Gili et al., 2022).

Regarding CWA – we addressed that point in our reply to Austin Carter’s review (A8).

Q3: Also, a number of studies have treated Tierra del Fuego as its own source area, but the present study seems to ignore it. Is it simply not included, or is TdF being incorporated into the Patagonia PSA? Some discussion is warranted – and ideally, the paper would provide full treatment of TdF as its own source region. A more in-depth discussion of the differences between this study’s conclusions and those of Gili et al 2017 is needed, with evidence provided to support the interpretations.

A3: We thank the reviewer for raising this point. In our study, Tierra del Fuego (TdF) is included within the Patagonia. Our REE endmember database incorporates six REE patterns from TdF dust and topsoil compiled from Gili et al., (2017), ensuring that this region is represented in the unmixing model. In our provenance reconstructions, the TdF component is consistently detected but contributes only for a very small fraction of the total dust at both EDC and EDML throughout the studied interval. We therefore do not treat it as a separate source region in the discussion.

We added a mention in line 143-144: “This dataset (Table S2) includes samples from Southern South America (SSA), subdivided into Patagonia (PAT; 39 dust sources, DS including mainland Patagonia and Tierra del Fuego)...”

Q4: Regarding point 3) the Younger Dryas is included in the interpretation as if it were a climate event in Antarctica; I find this baffling given the geographical focus of the paper.

A4: We believe the debate about the expression of Younger Dryas in the Southern Hemisphere is out of the scope of the study. We agree that the Younger Dryas is primarily expressed in the Northern Hemisphere as a major cooling event, however it is also evident in the Southern Hemisphere as a distinct climatic interval characterized by relatively mild and generally warmer conditions following the Antarctic Cold Reversal, consistent with the bipolar seesaw framework. In Antarctica, this period is marked by rising $\delta^{18}\text{O}$ values in the EDML and EDC record, indicating continued warming. It is also associated with glacier retreat in southern South America (Kaplan *et al.*, 2010). This transition constitutes, in our view, a climatically meaningful phase that is distinct from the ACR.

Q5: figures have a range of issues (see comments below)

A5: We addressed those point below.

Q6: The writing needs major revision with respect to grammar, subject/verb agreement, punctuation, and spelling.

A6: We revised the entire manuscript to correct for grammar, spelling mistakes.

Q7: In addition, the introductory paragraph follows the structure and citation ordering of the Vanderstraeten et al 2023 study from the same group (thanks to the other reviewer for pointing this out). While I wouldn't call it plagiarism, the similarity is striking. This paragraph should be rewritten and additional citations included.

A7: We have rewritten the introduction to address this point.

Minor comments:

Q8: Line 31: "Linking dust composition to eustatic sea level rise" – This isn't new as other studies have highlighted this potential link before. Also, I don't see any evidence presented in this paper that conclusively links dust provenance changes to any specific process in South America such as those described here. Either these connections need to be strengthened, or this sentence should be cut from the abstract.

A8: We agree that the potential link between Antarctic dust provenance and eustatic sea-level rise has been proposed previously, including in our earlier work Vanderstraeten *et al.*, (2023) but also other such Delmonte *et al.* (2017). In the introduction, we aimed at relaying the findings of our previous article on the dust provenance in EDML ice core, not to make a demonstration, we refer the reviewer to Vanderstraeten et al., (2023) on that matter. More generally, our intention here is not to claim novelty for this point, nor to imply a direct and exclusive causal relationship solely based on the EDC record. Rather, the contribution of this study is that the EDC provenance record independently reproduces the same timing, direction, and nature of the provenance shift previously identified at EDML, despite the different geographic setting, transport pathways, and dust flux regimes of the two sites. In Vanderstraeten *et al.*, (2023), the connections between eustatic sea-level rise, continental shelf submergence, and changes in Patagonian dust emissions were examined in detail, taking advantage of the EDML site's location directly downwind of Patagonia and its greater sensitivity to Atlantic-sector dust transport (see Fig. 8 therein). We believe this is not necessary to replicate such a detailed discussion in the current study. We consider it appropriate to frame our EDC results as being consistent with the influence of eustatic sea-level rise and associated postglacial reorganization of river leading to a reduction and re-routing of sediment in Patagonia. We have revised the abstract wording to emphasize replication and consistency across sites rather than asserting a new causal mechanism.

We modified the ending of the abstract to:

"The consistency of the provenance shift recorded at EDC and EDML ice cores points to a common large-scale control, in line with the influence of eustatic sea-level rise and postglacial reorganization of fluvial sediment routing and availability in southern South America."

B. Delmonte, C.I. Paleari, S. Andò, E. Garzanti, P.S. Andersson, J.R. Petit, X. Crosta, B. Narcisi, C. Baroni, M.C. Salvatore, G. Baccolo, V. Maggi Causes of dust size variability in central East Antarctica (Dome B): atmospheric transport from expanded south american sources during marine isotope stage 2 Quat. Sci. Rev., 168 (2017), pp. 55-68, 10.1016/j.quascirev.2017.05.009

Q9: Line 40: this statement deserves a more complete set of citations.

A9: We added extra reference to support this statement.

Q10: Line 88: Remove hyphens in this sentence.

A10: We rewrote this whole section.

Q11: Line 123: Please provide more information on the digested samples: how many were there? At what temporal resolution? Are they evenly distributed throughout the part of the core used for this analysis?

A11: In Gabrielli et al. (2010), two ice samples were digested for REE analysis: one representative glacial sample and one representative interglacial sample. Each sample was digested in triplicate together with two procedural blanks. These measurements were not intended to provide a temporal reconstruction but were used to determine the correction factor (*cfi*) between partial and total dissolution of dust particles. The triplicate analyses allowed us to quantify the analytical variability of the correction factor, which we propagate in the Monte Carlo simulations used in the unmixing model. We have clarified this point in the revised manuscript.

Added text in lines 138-139: *“we applied cfi derived from parallel measurements of fully digested EDC samples (three replicates of one glacial and interglacial samples) to obtain total REE concentrations allowing fitting with REE patterns from PSA”*

Q12: Line 127: Where are the citations for these data provided? Are the published PSA data all on digested samples? Needs to be fully described here, as differences in leaching/digestion could lead to discrepancies in the resulting REE concentrations.

A12: As mentioned in line 143, Table S1 regroups all information regarding the REE patterns, including the reference from which they were extracted when appropriate. All REE patterns in our database were verified to be measured after full digestion.

Q13: Line 130: Why is Tierra del Fuego not included?

A13: I refer the reviewer to A3. Briefly, Tierra del Fuego is included in our database within Patagonia.

Q14: Lines 145-146: needs revision.

A14: We thank the reviewer for pointing this out. We revised the sentences in question.

Q15: Line 179: Please omit the dash in this sentence.

A15: We removed the dash.

Q16: Line 187: the time period listed as “Late Holocene” is really more “Middle Holocene,” especially considering the cutoff at 2.8 ka. Further, the “Early” and “Middle” portions of the Holocene are commonly distinguished at 8.2 ka. The authors might want to consider this date as the boundary rather than the seemingly arbitrary 7.5 ka.

A16: We agree to change the “Late Holocene” to “Middle Holocene”.

Q17: Line 189: remove the word “for”

A17: We made the correction. Thank you.

Q18: Line 199: What does “mafic-like” mean? Can you just use the word “mafic”? Also, LREE stands for “light rare earth elements” not “low”. Please correct this.

A18: We changed “mafic-like” for “mafic” and “low REE” for “light REE”.

Q19: Paragraph beginning line 201 and throughout: What are the uncertainties on these estimates of PSA contributions? Please give the plus-or-minus values. It’s hard to assess how meaningful these numbers are without quantified uncertainties.

A19: Uncertainties associated with the provenance estimates are derived from the Monte Carlo simulations used in the unmixing model (we refer the reviewer to A1). For each individual time step, we report the median contribution together with the 25th and 75th percentiles, which represent the range associated with the model parameters. These values are provided in Table S2.

In the paragraph mentioned by the reviewer, the values correspond to averages calculated over specific time intervals and therefore represent the mean contribution of each PSA across multiple samples. In this context, the variability reflects natural temporal changes in source contributions rather than the uncertainty of a single model estimate. For this reason, we report only the average contributions for these intervals and refer the reader to Table S2 for the uncertainty ranges associated with the individual time steps.

Q20: Line 220: Please capitalize EPICA.

A20: We thank the reviewer for pointing this out. We rewrote it.

Q21: Lines 202 and 223: The listed PSAs have a different order in these two sentences but it is not obvious that they refer to different time periods. Please double-check what is correct and revise as needed.

A21: We thank the reviewer for pointing this out. We rewrote it.

Q22: Line 258: The authors state there is “close agreement” between the measured and modeled Sr-Nd isotope values, but Figure 2 suggests the agreement during certain intervals is better than others. The modeled “iso” symbols appear to be systematically offset to lower $^{87}\text{Sr}/^{86}\text{Sr}$ from the measured values in the eNd range of ~ -7 to -12 . Then in the LGM interval, the “iso” symbols are tightly clustered in a narrow Sr-Nd range that is also offset to higher $^{87}\text{Sr}/^{86}\text{Sr}$ values compared to the bulk of the actual data. The authors need to address the reasons for these discrepancies between the REE-inferred Sr-Nd isotope data and the measured values.

A22: We refer the reviewer to **A11** (in our reply to Austin Carter’s review) that also raised this point. Briefly, the broader range of modeled Holocene and LGM values reflects the much higher temporal resolution of the REE-based record compared to the sparse Sr–Nd isotope data. The quasi-continuous REE record captures short-term variability and transient source-mixing events that are not resolved by time-averaged isotopic measurements, causing some modeled values to extend beyond the documented isotopic range. Despite this, modeled fields remain reasonably close to observed values, supporting the consistency of the DEEPOT results. Clarifications emphasizing the role of data resolution and availability have been added to the revised manuscript in section 4.1 lines 347-355.

Q23: Line 261: “Persistence” is misspelled.

A23: We thank the reviewer for pointing this out. We rewrote it.

Q24: Line 283: It might be helpful to the reader to provide some context, e.g. by adding “on the basis of combined Sr-Nd-Pb isotope compositions” to the end of this sentence.

A24: We added this precision to the text.

Q25: Line 284-285: Koffman et al 2021 also estimated the expanded outwash plain area available for dust deflation during the LGM with a sea level lowering of 130 m. Their estimate, at ~75,000 km² of exposed continental shelf, is a bit higher than that of Vanderstraeten et al 2023. It might be worth providing both values to give some sense of the range of estimates.

A25: We added this estimation to the text together with ours.

Q26: Line 306: The YD is mentioned here without context. Why is this Northern Hemisphere climate phenomenon relevant to a paper on Antarctic dust provenance? If the paper were focused on interhemispheric climate signals and phasing I could see the logic for highlighting the YD, but otherwise it just seems out of place. I suggest focusing on Southern Hemisphere climate signals such as the ACR in this paper, as this seems more relevant.

A26: We refer the reviewer to our reply A4, above.

Q27: Line 307: The sentence beginning on this line needs grammatical revision.

A27: We thank the reviewer for pointing this out. We rewrote it.

Q28: Line 311: The double angled brackets around “wet scavenging” can be removed.

A28: We thank the reviewer for pointing this out. We change it in the text.

Q29: Line 334: The year for the Speirs article is 2010, not 2001. Further, the direction the winds blow in the McMurdo Dry Valleys, as shown in that article, is generally west-to-east, not east-to-west (see e.g. their figure 4, showing winds from 270 degrees). One must remember that north is “up” toward the coast when looking at maps of Antarctica. Please correct this sentence.

A29: We thank the reviewer for pointing this out. It was a point of Austin Carter’s review, so we completely rewrote this section (now split into 4.3 and 4.4) and removed this reference.

Q30: Lines 339-352: It is not possible to evaluate this paragraph given the lack of labeling on Fig. 5

A30: This part is completely rewriting with the creation of the new Figure 5 (*named Figure 4 in new version*).

Q31: Line 353-362: The explanation of why the southern Puna and southern Altiplano both contribute dust but not the northern Puna (which is geographically sandwiched between them) needs to be clarified and better justified. This is especially confusing because in Fig. 3a of Gili et al 2017, the REE fields of the northern and southern Puna fully overlap, and their paper includes N Puna as a source. What specific REE ratios support eliminating N Puna as a dust source to EDC? I would also like the authors to reference and/or address the statement in Gili et al 2017 that “REE are less useful for distinguishing sediments from CWA and Puna.

A31: We refer the reviewer to our reply A2 above, and A9 of Austin Carter’s review for a detailed discussion on CWA and Puna.

Q32: Also, a number of studies have treated Tierra del Fuego as its own source area, but the present study seems to ignore it. Is it simply not included, or is TdF being incorporated into the Patagonia

PSA? Some discussion is warranted – and ideally, the paper would provide full treatment of TdF as its own source region.

A32: We refer the reviewer to our reply A3 above.

Q33: And finally, a more in-depth discussion of the differences between this study’s conclusions and those of Gili et al 2017 is needed, with evidence provided to support the interpretations.

A33: We refer the reviewer to our reply A2 above.

Q34: Line 384-385: A phrase is repeated

A34: Corrected in the text.

Q35: Line 407: Why are the ACR and YD included together as if they are both climate events in Antarctic ice cores? I suggest removing YD references unless there is clear justification for discussing the YD.

A35: We believe the debate about the expression of Younger Dryas in the Southern Hemisphere is out of the scope of the study. Because we noticed both in EDC in this study and in EDML [Vanderstraeten *et al.*, (2023)] that significant changes in dust sources occurred during this YD period. That is why we maintain this climatic event in our observations, as discussed in A2 and A26 above.

Q36: Line 426: remove second use of “dust” in this sentence

A36: We have deleted this in the new version of the article.

Q37: Line 430: “PSA sources” is redundant. Can simply use “PSAs”. This sentence also should include citations as this is very detailed information about transport pathways.

A37: Corrected in the new version of the article. We added two references to the sentence in question.

Q38: Line 466-470: This is very arm-wavy given the actual evidence presented in this paper. Suggest removing or toning down the language here.

A38: The interpretation proposed in the section in question is not intended to represent a direct demonstration based solely on EDC data, but rather to place our results in the broader framework of previously proposed mechanisms. In particular, Vanderstraeten et al. (2023) suggested that postglacial geomorphological changes in Patagonia—including continental shelf submergence and drainage reorganizations—played an important role in modulating sediment availability and thus high-latitude dust emissions during the LGIT. The results presented here independently support this interpretation. The temporal evolution of PSA contributions reconstructed at EDC mirrors the pattern previously reported for EDML (Section 4.5; Vanderstraeten et al., 2023). The agreement between these two distant Antarctic sites suggests that the mechanisms controlling the decline of high-latitude dust sources during the LGIT operated at large scale over Antarctica. For this reason, we believe it is appropriate to discuss these processes in the manuscript as a plausible interpretation consistent with both our results and previous studies. To address the reviewer’s concern, we have slightly clarified the wording in the revised text to emphasize that these mechanisms represent an interpretation supported by multiple lines of evidence rather than a direct demonstration from the present dataset alone.

Added text in line 644-646: “Collectively, the patterns observed at both EDC and EDML suggest that eustatic sea-level rise and postglacial hydrological reorganization in Patagonia had broad and long-lasting effects on Antarctic dust composition during the LGIT.”

Q39: Line 476: The contributions of Patagonia and New Zealand should be given summarily here. If NZ is considered a major source, then it should not be in parentheses.

A39: Corrected in the text.

Q40: Line 488: More evidence would need to be presented to support this statement. I do not believe the paper as written provides clear evidence of any of these processes or feedbacks beyond loose temporal correlation. I would like to see a more robust treatment of the range of potential mechanisms (including robust citations) driving changes in dust delivery to EAIS in order to support a statement such as this.

A40: We refer the reviewer to our replies A8 and A38.

Q41: Line 497: I believe “P.B.” should be “P.G.,” Paolo Gabrielli.

A41: We agree with the reviewer and modify the text.

Figures

Q42: Fig. 1: Please change the x-axis time units to years or thousands of years. Showing time in units of 10,000 years is atypical and not intuitive. This figure would be greatly improved by showing the dust flux in the ice core in addition to the individual PSA contributions. It would also be very helpful to see a $\delta^{18}\text{O}$ or δD record for climate context, particularly to emphasize temperature variations during the deglaciation and to compare to changes in dust deposition and provenance. I also think the “YD” highlighting should be removed, as it does not seem relevant to the study.

A42: We have modified the x-axis time units as suggested.

We prefer not to add the total dust flux, $\delta^{18}\text{O}$, and δD records to Figure 1 because these datasets are already presented in Figure S3. Adding these three additional panels would substantially increase the complexity of Figure 1 and risk overloading the figure, reducing its readability. In addition, we provide an estimate of the flux contributions from each PSA to EDC in Figure S2.

Regarding the highlighting of the Younger Dryas (YD), we agree that this climatic event is primarily characteristic of the Northern Hemisphere. However, in our records this interval coincides with marked changes in trends across several source regions. For this reason, we consider it useful to retain the YD marker in the figure, as it helps emphasize these abrupt transitions in dust provenance.

Q43: Fig. 2: In general, I question the “blobs” as currently drawn. The Patagonia field appears far too wide given the data published from this region. The Australia field is missing data from the Northwest Territory and South Australia that would deepen the field to much lower eNd values than what is shown here (e.g. De Deckker 2019). The Southern Puna region has a highly improbable field as drawn; it should be more convex around the available data. The extremely high $^{87}\text{Sr}/^{86}\text{Sr}$ value included in the NZ field is likely erroneous. I strongly suggest including only data with well-characterized geologic and geomorphic context, in this case, Koffman et al. 2021. In general, I would like to see the actual data points used to generate these “blobs” and the specific citations included for each. This could be a supplementary figure that supports the main text, for instance. But I also think revision of this figure is warranted.

A43: This figure has been completely rearranged according to the insightful comments made by the two reviewers. In particular, we rearranged the Australian domain and considered the reference suggested by the reviewers. The references used to generate these isotopic ranges are as follows: Grousset *et al.*, 1992; Delmonte *et al.*, 2004; Gingele and De Deckker, 2005; De Deckker *et al.*, 2014; Revel-Rolland *et al.*, 2006; Gaiero, 2007; Gili *et al.*, 2017; Gili *et al.*, 2022; Li *et al.*, 2020; Koffman *et al.*,

2021; Blakowski *et al.*, 2014; Diaz *et al.*, 2020. These are mainly fine fractions, where available, and correlate closely with other studies such as Gili *et al.*, (2022).

Q44: Fig. 5: I understand the motivation for this type of comparison, but if the current study aims to draw broader interpretations about the westerlies, or to use proxy records of the westerlies to help interpret the presented dust provenance record, there needs to be appropriate context and breadth of records included. If a figure of this type is to be included, I would also like to see other records from South America and from Antarctic ice cores for context here, as I think it will really strengthen the interpretations and enhance the impact of this work. For instance, you might want to include the NPI (*Nothofagus* to *Poaceae* Index) from Lago Guanaco (Moreno *et al.* 2010, the Macquarie Island diatom-inferred conductivity record of Saunders *et al.* 2018, the opal upwelling proxy record of Anderson *et al.* 2009, the CO₂ data and dD data from EPICA Dome C, etc.

In addition, it seems to me that dust flux would be more meaningful to compare to Potrok Aike and these other records. Percent contribution, as currently shown, is a factor of the relative inputs of multiple sources and climate and environmental conditions in those regions, so is less meaningful. It would be better, I think, to scale the total dust flux by the percent from Patagonia and use that in this figure instead (e.g. Patagonia dust flux to EDC). Please also correct the x-axis to be in years or ka, and to match the timescale shown in Fig. 1. Here the ($\times 10^4$) is missing so I think the plot is completely uninterpretable as shown. It also needs an x-axis label with units stated. The colored bars are not labeled or described in the caption, but need to be.

A44: We thank the reviewer for the helpful suggestion. We have revised this figure (now Figure 4 in the updated version) by adding several Southern Hemisphere records for comparison. Those includes temperature proxies ($\delta^{18}\text{O}$ in EDC ice core and Winter Sea-Ice Concentration in marine core PS2090 - Bianchi & Gersonde, 2004), wind intensity indicator (Median Destructive Field MDF in Laguna Potrok Aike - Lisé-Pronovost *et al.*, 2015; diatom-inferred conductivity in Macquarie Island - Saunders *et al.*, 2018) and a fire proxy from Lago Pintito (Patagonia) from Moreno *et al.*, (2021).

These additions allow more meaningful comparisons between the Patagonian contribution to EDC dust deposition and regional climatic and environmental changes in Patagonia and the Southern Ocean. These comparisons are discussed in the revised Section 4.1 (see below).

Added text in lines 475-484: *“Following a pronounced decline after ~14.5 kyr BP, Patagonian contributions reach minimum values around 10 kyr BP (Figure 4). Interestingly, those minima coincide with an early Holocene thermal maximum (11-9 kyr BP) which is clearly expressed in the $\delta^{18}\text{O}$ record from EDC and more broadly across Antarctica (Masson *et al.*, 2000) as well as in other Southern Hemisphere records (Kilian *et al.*, 2012; Bianchi & Gersonde, 2004). Those climatic conditions likely reduced the thermal gradient between the mid and high latitudes, resulting in persistently weakened wind intensities. Such conditions are recorded at Macquarie Island (sub Antarctic southwest Pacific 54 ° S - Saunders *et al.*, 2018), in Potrok Aike maar (52 ° S, Patagonia; Lisé-Pronovost *et al.*, 2015) and in the Southern Ocean south and east of New Zealand (Fletcher *et al.*, 2011). These changes may be linked to a poleward shift of Southern Westly Winds (SWW) belt (Quake and Kaplan, 2017; Moreno *et al.*, 2010). At first glance, those conditions appear unfavorable for dust emissions from Patagonia. However, this interval also corresponds to drier conditions and widespread increase in fire activity in eastern Patagonia (Figure 4 - Moreno *et al.*, 2010; Power *et al.*, 2008; Markgraf *et al.*, 2007). Such conditions likely enhanced dust emissions through vegetation loss and soil destabilization making sediments more susceptible to aeolian deflation upon the return*

of stronger SWW (Dukes et al., 2018; Wagner et al., 2021). After ~8 kyr BP, the strengthening of the SWW coincide with elevated PAT contribution between ~8-7 kyr BP. Apart from a marked drop in PAT contributions between 6-6.5 kyr BP, strong SWW conditions have prevailed since ~5 kyr BP in south Patagonia and correspond to on average PAT contribution of ~50%.”

Regarding the possibility of comparing the Laguna Potrok Aike wind record with the total dust flux at EDC, we note that the latter is dominated by the two-order-of-magnitude decline occurring between ~18 and 14.5 kyr BP. This large trend overwhelms shorter-term variability and renders a direct comparison with wind proxies difficult to interpret. For this reason, we retain the percentage contribution from Patagonia as the primary metric shown in Figure 4.

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-Lisé-Pronovost, A., St-Onge, G., Gogorza, C., Haberzettl, T., Jouve, G., Francus, P., Ohlendorf, C., Gebhardt, C., and Zolitschka, B.: Rock-magnetic proxies of wind intensity and dust since 51,200 cal BP from lacustrine sediments of Laguna Potrok Aike, southeastern Patagonia, *Earth and Planetary Science Letters*, 411, 72 – 86, <https://doi.org/10.1016/j.epsl.2014.11.007>, 2015.

-Saunders, K. M., Roberts, S. J., Perren, B., Butz, C., Sime, L., Davies, S., Van Nieuwenhuyze, W., Grosjean, M., and Hodgson, D. A.: Holocene dynamics of the Southern Hemisphere westerly winds and possible links to CO₂ outgassing, *Nature Geosci*, 11, 650 – 655, <https://doi.org/10.1038/s41561-018-0186-5>, 2018.

-Moreno, P. I., Henríquez, W. I., Pesce, O. H., Henríquez, C. A., Fletcher, M. S., Garreaud, R. D., and Villa-Martínez, R. P.: An early Holocene westerly minimum in the southern mid-latitudes, *Quaternary Science Reviews*, 251, 106730, <https://doi.org/10.1016/j.quascirev.2020.106730>, 2021.

Q45: Fig. S2: Please fix the x-axis as in other figures and include appropriate units and labels.

A45: Done.

Q46: Fig. S5: Large y-axis label contains a typo. Actually, this figure is pretty interesting and might be worth adding to the main text once it is revised. I think it provides a nice complement to Fig. 4. I note that the x-axis ages and labeling are distinctly different from the other figures. As mentioned before, figures should all use the same units – either years or kyr.

A46: We transferred this figure to their main text in the new version of the manuscript as Figure 6.

Q47: Table S4 and elsewhere: “Localisation” in English means to make something more localized. It would be better to use the word “Location.”

A47: Done.

Q48: All Tables and Figures: Please use decimals for numbers rather than commas.

A48: We agree with the reviewer and modify the text, Figures and Tables accordingly.