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*.

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.

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).

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. 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.

Minor comments:
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.

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

Line 88: Remove hyphens in this sentence.

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?

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.

Line 130: Why is Tierra del Fuego not included?

Lines 145-146: needs revision.

Line 179: Please omit the dash in this sentence.

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.

Line 189: remove the word "for"

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.

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.

Line 220: Please capitalize EPICA.

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.

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 87Sr/86Sr 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 87Sr/86Sr 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.

Line 261: “Persistence” is misspelled.

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.

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 km^2 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.

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.

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

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

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.

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

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.” 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. 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.

Line 384-385: A phrase is repeated

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.

Line 426: remove second use of "dust" in this sentence

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.

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

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.

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.

Line 497: I believe "P.B." should be "P.G.," Paolo Gabrielli.

Figures

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 d18O or dD 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.

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 $^{87}Sr/^{86}Sr$ 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.

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 ($x10^4$) 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.

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

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.

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

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

References cited:

Anderson, R. F., Ali, S., Bradtmiller, L. I., Nielsen, S. H. H., Fleisher, M. Q., Anderson, B. E., & Burckle, L. H. (2009). Wind-driven upwelling in the Southern Ocean and the deglacial rise in atmospheric CO2. *Science, 323*, 1443-1448. doi:10.1126/science.1167441

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Gili, S., Gaiero, D. M., Goldstein, S. L., Chemale Jr, F., Jweda, J., Kaplan, M. R., . . . Koester, E. (2017). Glacial/interglacial changes of Southern Hemisphere wind circulation from the geochemistry of South American dust. *Earth and Planetary Science Letters, 469*, 98-109.

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Moreno, P. I., Francois, J. P., Moy, C. M., & Villa-Martinez, R. (2010). Covariability of the Southern Westerlies and atmospheric CO2 during the Holocene. *Geology, 38*(8), 727-730.

Saunders, K. M., Roberts, S. J., Perren, B., Butz, C., Sime, L., Davies, S., . . . Hodgson, D. A. (2018). Holocene dynamics of the Southern Hemisphere westerly winds and possible links to CO2 outgassing. *Nature Geoscience, 11*(9), 650-655. doi:10.1038/s41561-018-0186-5

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