Boxho et al. "Quantifying Southern Hemisphere dust sources during the Last Glacial-Interglacial Transition using rare earth elements in the EPICA Dome C ice core"

Reviewed by Austin Carter

This paper presents a high-resolution record of source contributions to embedded dust within the EPICA Dome C (EDC) ice core, specifically focused across the time interval of 33.7-2.9 kyr BP. The authors apply a statistical model previously developed by Vanderstraeten et al., 2023 to a large dataset of discrete rare earth elemental (REE) concentrations of ice core dust from EDC as reported in Gabrielli et al., 2010. The previously published dataset contains nearly 300 sections of ice from EDC which have been acid leached using 1% HNO3 and individually characterized for REE concentrations. Subsequently, the model in Vanderstraeten et al., 2023 includes a database of REE concentrations for a variety of samples from potential sources within the Southern Hemisphere. After making the EDC leached REE data comparable to total REE concentration using element-specific enrichment factors, the authors then modeled various combinations of sources that would result in the most closely matched data for each ice core dust sample. In this way, source contributions are effectively "unmixed" from the dust.

The authors show that before 14.5 kyr BP high-latitude PSAs (Patagonia + New Zealand) dominate the EDC ice core dust record and are stable. After 14.5 kyr BP, there is more heterogeneity in PSA contributions with periods of greater low-latitude PSA contributions (Australia, South Africa, and Puna-Altiplano Plateau). These changes are attributed to both hydrological changes in Patagonia and its corresponding shelf exposure as well as regional source dynamics at low-latitude sources. Candidly, I found the latter to be a bit hard to follow in its current organization. Lastly, when compared with EDML, the EDC record's PSA contributions over time are broadly similar with subtle differences owing to their geographic positioning. Overall, I enjoyed reading and learning more about this statistical application, and I find it very clever and interesting; however, I have some comments on clarity and the inherent limitations in the methods that I believe merit additional discussion in the paper after which I believe it would have my full support. I hope the following suggestions can help improve the manuscript:

Specific comments

L17: The authors frequently refer to their record as "continuous." However, it is my understanding that the EDC REE dataset from Gabrielli et al., 2010 used for this study is not continuous. Instead, these measurements were on discrete sections of ice. If this is correct, then I do not think it's valid to describe the paper's results as continuous, and this should be edited for clarity.

L34-52: The opening paragraph of the introduction parallels that of the opening paragraph of Vanderstraeten et al., 2023 very closely with regard to sentence structure and reference order etc. While I recognize the parallel logic between the papers, it would be nice to differentiate this manuscript's introductory paragraph a bit further.

L69: It would be useful to elaborate or define what is meant by "REE patterns" here.

L121-124: The first limitation when applying the DEPOT statistical model lies in the ability to compare acid-leached REE concentration data with other REE concentration data from PSAs. There is a possibility for preferential leaching of LREE (La, Ce, Pr, Nd) during a 1% HNO3 leach as shown in Gabrielli et al., 2010. Hence the need to apply enrichment factors to translate data from acid-leached to its potential total concentration form, which I think is handled well in the manuscript. However, the readers are left to assume that all of the PSA data are in total REE concentration form. Can you confirm this? Or are enrichment factors also applied to these data in their respective studies? I think some added clarity and its importance for direct comparisons would be valuable and would strengthen the paper's methods.

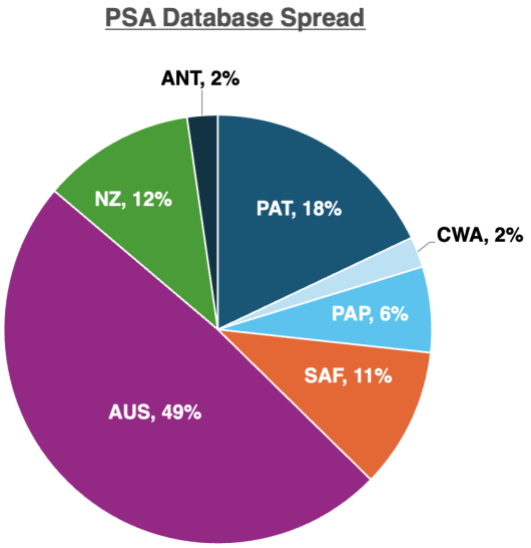
L129: While I recognize the term Dust Sources (DS) was previously adopted in Vanderstraeten et al., 2023, I do think it can be slightly misleading. From my understanding, these are not unique sources, rather these are individual samples of dust, loess, sediment, and/or soil that make up a Potential Source Area (PSA). The text would benefit from some clarity on that and additional descriptions here. The confusion is made clear when we take Patagonia, for example. The authors describe 39 Dust Sources that make up the Patagonia PSA. To me this implies, 39 unique regions, but I only count 19 markers on Fig. S2 for Patagonia. This is likely because multiple samples come from similar sites, which therefore would not necessarily be a different dust source.

L127-133: The second and most important limitation in this approach is related to the quantity of data in the PSA database. Because the goal is to compare all samples in the PSA database individually to the ice core samples, PSAs that have a greater quantity of REE concentrations might have a bias. Perhaps this is less of an issue after taking the median of each PSA-specific PDF, but it is hard to know without seeing an example. I made a pie chart showing the distribution of the PSAs below. Australia makes up nearly half of the database, which I'm concerned that the % contributions might be biased heavily as a result. Additionally, based on Pb isotopic data for both EDC ice core dust and PSAs, Gili et al., 2016 argues against significant glacial and interglacial Australian contributions to the EDC dust record, yet this paper's finding suggests otherwise. If there is no bias in the DEPOT model, then the findings showing large influences from Australia are interesting and worth discussing further in that regard. While I agree with L366-355, importantly this study's findings with respect to Australia are not fully consistent with those in Gili et al., 2016.

Similarly, I believe the Antarctic PSA might be discounted too easily and quickly despite it being argued as a plausible contributor during the Holocene at EDC in Gabrielli et al. 2010 from which the REE dataset originates. Additionally, Delmonte et al., 2013 highlights how the measured EDC ice core dust for the Holocene are geochemically similar in Sr-Nd isotope composition to Antarctic PSAs. However, the PSA database contains only 5 samples from the McMurdo Dry Valleys to represent the Antarctic continent (though only 4 are reported in Table S2). Because there are so few available samples, it seems inherently less likely that there will be enough trends to compare to and, thus, less likely they will make up a significant portion of the EDC ice core dust in the statistical model. Part of this issue may also be a result of limited measured Antarctic PSAs for REE concentrations within the literature. Some added caution here would be valuable and perhaps the inclusion of dust size might be useful as well.

Along the same lines, Coppo et al., 2022 argues that the activation of S-CWA is a plausible dominant PSA to the East Antarctic Plateau during the LGM; however, this paper's

findings suggest it is not a dominant contributor during any time across the LGIT. This is an interesting difference and may also be related to its limited quantity (n=5) in the PSA database. All of these above points, especially when in contrast to papers that are heavily cited in the text, are worth noting/discussing further.



L258-260: I agree with the principle of modeling Sr-Nd isotope compositions and comparing it to established data as a fidelity check for the DEPOT algorithm and thank the authors for exploring this; however, I have some concerns in the way this is currently presented in the manuscript.

- First, the measured Sr-Nd isotope compositions of samples are described as coming from dust in ice cores as reported in previous studies; however, the Iso Early Holocene category appears to consist predominantly of dust from modern snow pits on Berkner Island (not from ice cores; Bory et al., 2010). Furthermore, in the text Early Holocene is classified as 11.7-7.5 kyr BP, but these Berkner Island data do not represent that time interval. The dust described in Bory et al., 2010 span a two-year period from 2002-2003. Therefore, I do not think these are relevant and should be removed.
- Second, after removing the Berkner Island data, I would not necessarily agree that there is close agreement between the modeled EDC Sr-Nd isotope compositions and the literature measured Sr-Nd isotope compositions of dust from Dome C and Taylor Glacier. For East Antarctic sites during the Holocene, ε_{Nd} values of ice core dust are generally below 0 but not lower than -10 and for $^{87}Sr/^{86}Sr$ range approximately between 0.707 and 0.715. While the modeled Holocene data do capture a wider range than the modeled LGM data (as expected), the modeled Holocene data appears to extend beyond what would be expected from measured ice core dust, which merits some explanation. Similarly, the modeled LGM measurements span a wider range than would be expected for MIS 2 samples, which based on the literature should be tightly clustered around ε_{Nd} of -1 or -2 and $^{87}Sr/^{86}Sr$ of ~709. Furthermore, when looking at the PSA % contributions during the LGM, all PSAs appear relatively stable and with a consistent mix of sources, I

would expect a much tighter modeled cluster as a result. Why might be the modeled data result in these wider ranges?

- Lastly, I think part of the difficulty in deciphering Figure 2 is that there are multiple goals rooted in several comparisons (i.e., comparing time interval changes, modeled vs. measured data, and PSAs). Perhaps this could be split into multiple figures or panels. One could focus on directly comparing modeled vs. measured data and one on just comparing modeled data to PSAs.

Section 4.3: This section is packed with useful information but does struggle a bit in its structure. The opening paragraph provides three processes to explain the variability across the Holocene. As a reader, I expected the subsequent paragraphs to be focused on a discussion of these processes in greater detail, but that was not necessarily the case. The next paragraph (L315-334) feels very results heavy and compares the record broadly to other papers. Then, the following paragraphs are subdivided into PSA-specific discussions, which are very detailed but also hyper-focused. I think this section would be greatly improved by a slight restructuring and including a broader discussion of the glacial-eustatic and hydrological feedbacks that connect the regional source dynamics.

L467: I don't think the data necessarily "demonstrate" unequivocally that these feedback mechanisms influenced dust deposition rather they "support" that scenario or "suggest" it, which may be better phrasing.

L480-482: The "hydrological rearrangement processes in Patagonia and the rapid submersion of its shelf" would explain the decline in high-latitude PSA contributions but probably not the rise in dust from low-latitude sources? I think the current phrasing implies the latter is providing confirmation when it may not actually be related.

Figure modifications

- Figure 1
    - It would be best to change the units on the x-axis to kyr to be consistent with descriptions in the text and the same for the caption
- Figure 2
    - The marker colors appear identical to the colors used for the different domains. Because of this, at first glance, it seems like the markers would represent PSA data rather than ice core dust. I would suggest exploring additional color schemes or styles to make this a bit clearer.
    - It is not obvious from looking at the figure legend which samples are measured data vs. modeled data. To add clarity, one suggestion would be to include legend titles for each column. For example, one might say "EDC Modeled (this study)" and "Measured (previous work)." It also should be made clear in the figure caption which sites the measured data come from. Notably, there are only 3 samples shown which are measured from the Dome C area; the majority of which come from the coastal and low-elevation Taylor Glacier and snow pits on Berkner Island.

    - If you are specifically looking for ice core dust only, below are some paper suggestions in which you may find additional data relevant to the study's time interval:
        - Aarons et al., 2016: Taylor Dome (MIS 2-1)
        - Delmonte et al., 2010a and 2010b: Talos Dome (MIS 3)
        - Delmonte et al., 2010b: Talos Dome (MIS 2-1)
        - Delmonte et al., 2007: Dome C and Vostok (MIS 1)
        - Delmonte et al., 2004b: Dome B and Komsomolskaia (MIS 2)
        - Delmonte et al., 2017: Dome B and Old Dome C (MIS 2)
    - Technical comments
        - The compilation is listed as in "Table S1" but this should be "Table S4."
        - Edit: "Note that the SAF and AUS domains are more extended than shown in the figure."
        - $^{87}Sr/^{86}Sr$ ratios are shown with commas instead of periods, but in the text are referred to using only periods. This should be changed to be consistent with the text.
- Figure 5
    - The x-label is missing.
    - Panel B could benefit from some additional discussion in the text just to elaborate further on why its inclusion is relevant.
- Figure S3
    - Missing units on the y-axis
    - Would be helpful to make the x-axis kyr to be consistent with the text

Technical corrections

Epsilon notation should be briefly defined somewhere in the text and should be displayed consistently. Of note, it appears three different ways throughout the text and figures: ε_{Nd}, εNd, and ε(Nd).

The age of events and ranges should be shown in a consistent format both in units and in chronology. For example, L109-110 provides a range spanning young to old and in yr BP but previously this has been described spanning old to young and in kyr BP.

The use of hyphens, en dashes, and em dashes are used interchangeably often and in the same paragraphs. For example, L293 and L301 as well as L185 and L188. I would suggest combing through and editing this.

Sometimes percentages are written X% and sometimes as X % with an additional space. I would suggest removing this extra space from all percentages.

<u>Typing errors</u>

- L62: is → are
- L77: reveal → revealed
- L101: camples → samples
- L146: step. The → step, the
- L147: la-lu → La-Lu

- L193: averages → average
- L199: low → light
- L220: Epica → EPICA
- L236: describes → described
- L246: e.i. → i.e.
- L249: represents → represented
- L261: persistance → persistence
- L374: kry → kyr
- L458: planform → platform?
- L501: supplentary → supplementary
- Table S1 caption: th → the

Author Contributions: I'm assuming P.B. is supposed to be P.G.? Also, I think this section is missing N.M. and A.B. contributions.

Throughout the references, there are some issues with what appears to be accented characters. For example, see Markgraf et al., 2007.