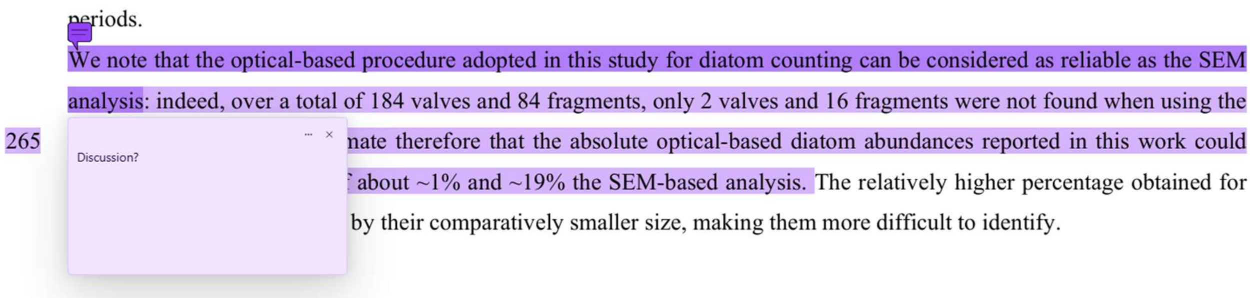
POINT – TO – POINT RESPONSE TO REVIEWER N.1

We thank the reviewer for his very useful comments, that helped us to improve the quality of our work.

We accepted <u>all suggestions</u> including re-phrasing where needed, or shortening long sentences, etc.

Some minor specific clarifications that were not included in the main text are reported here below.



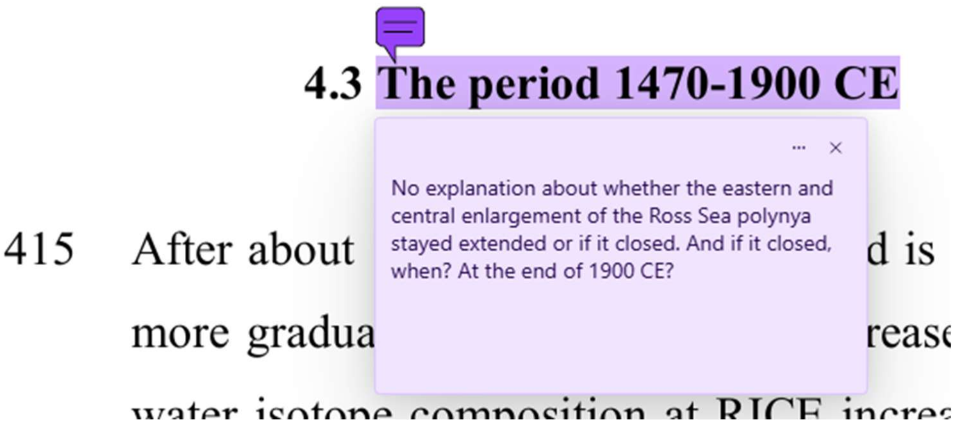
periods.

We note that the optical-based procedure adopted in this study for diatom counting can be considered as reliable as the SEM analysis: indeed, over a total of 184 valves and 84 fragments, only 2 valves and 16 fragments were not found when using the … nate therefore that the absolute optical-based diatom abundances reported in this work could … f about ~1% and ~19% the SEM-based analysis. The relatively higher percentage obtained for … by their comparatively smaller size, making them more difficult to identify.

Discussion?

We have left this consideration in the Results, but reworded the whole paragraph as follows:

Regarding diatom analysis, a comparison between optical and electron microscopy revealed that out of a total of 184 valves and 84 fragments, only 2 valves and 16 fragments were missed using the optical method (SF6). Therefore, we estimate that the optical-based diatom abundances reported in this study may be underestimated by approximately 1% for valves and 19% for fragments compared to the SEM-based analysis. The higher percentage for fragments may be due to their smaller size, which makes them more challenging to identify. Overall, we conclude that the optical-based method used for diatom counting in this study is as reliable as the SEM analysis.



4.3 The period 1470-1900 CE

No explanation about whether the eastern and central enlargement of the Ross Sea polynya stayed extended or if it closed. And if it closed, when? At the end of 1900 CE?

After about … d is

more gradua … rease

Good question. Dust and diatom data extend to the end of 1800, so we don’t have data for the 20th Century. At the end of 1800/beginning 1900 while the Polynya index from Mezgec et al. (2017) suggests a polynya that is still open and well-established, as also confirmed by the chemical record of marine elements from Brightley (2017) and by indirect evidences from elephant seals, our diatom record does show an important diatom influx until about 1850-1860, while data from the most recent samples (1860-1900) show a more moderated diatom influx with respect to the earlier period (1500-1850). Because of the poor resolution of our data we do not comment this on the paper, but we believe that at the beginning of the 20th century, it is very likely that the polynya remained open in its central-western part and probably also in the eastern part, although to a lesser extent compared to the period of the Little Ice Age (LIA).

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time of the LIA. The increased efficiency of the RS polynya over the last ~500-600 years is also testified by important ecological variations in Victoria Land (figure 6); indeed, during this period, Hall et al. (2006, 2023) observe the almost complete disappearance of elephant seal (*Mirounga leonina*) colonies in Victoria Land, related to the increased persistence of coastal sea ice. A key factor for the reproductive success of elephant seals is the proximity of open water to the nursery sites. Thus, the complete disappearance of elephant seal colonies in Victoria Land during the LIA is interpreted be related by a significant increase of coastal sea ice. This hypothesis is also corroborated by independent marine data as those from Edisto

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Inappropriate extension of results to cover seal colonies. Do we even know where seal colonies were during the LIA? There are not enough of your own statistical results to support that sea-ice extent can then be linked to seal populations.

About the Reviewer's comment on Elephant Seals, we underline that we are aware that Hall et al. (2006, 2023) did not directly link their findings to the Ross Sea polynya, but they did discuss the impact of changes in landfast sea ice and ecological changes in Victoria Land, and this specifically regarding elephant seals. They noted the near-disappearance of elephant seal colonies in Victoria Land, which they attributed to the increased persistence of landfast sea ice. We reworded the paragraph as follows, introducing also that we "speculate" and that more detailed field studies in VL are needed to confirm this hypothesis. Indeed this link with field data was also introduced in the former work of Mezgec et al., 2017, where some co-authors of this paper were actively involved.

" *The increased efficiency of the Ross Sea polynya over the last ~500-600 years is also reflected in significant ecological changes in Victoria Land (fig. 6). Hall et al. (2006, 2023) observed the near-complete disappearance of elephant seal (Mirounga leonina) colonies in Victoria Land, which they linked to the increased persistence of coastal sea ice. We note that a key factor for the reproductive success of elephant seals is the proximity of open water to their nursery sites. Thus, the disappearance of elephant seal colonies in Victoria Land during the LIA can be interpreted as being related to a significant increase in coastal sea ice. This hypothesis is also supported by independent marine data, such as those from Edisto Inlet in the north-western Ross Sea (Tesi et al., 2020), where persistent summer fast ice has been observed over the last 700 years. Given that the formation, persistence, and variability of Antarctic polynyas are known to be influenced by landfast sea ice (Fraser et al., 2019; Mezgec et al., 2017), we hypothesize that our data (Fig. 6) support the connection between the permanent abandonment of the elephant seal population along the Victoria Land coast during the LIA and the expanded extent and occurrence of the Ross Sea polynya. This relationship was previously proposed by Mezgec et al. (2017) for the Holocene period."*

POINT – TO – POINT RESPONSE TO REVIEWER N.2 (in red=author's response)

This review addresses the revised manuscript by Lagorio and co-authors, based on their responses to my previous review and the updated manuscript. I acknowledge that the manuscript has improved compared to its initial version. However, while the authors recognize the complexity of the climate dynamics they describe, they still tend to oversimplify some interpretations without adequately acknowledging the limitations of their approach. In conclusion, I have three main concerns (along with additional comments) that need to be addressed before the manuscript can be accepted for publication.

We thank the reviewer and answer the following points:

1. My first concern relates to the authors' response regarding the discrepancies observed among the different ENSO proxies used. I fully understand that proxies have inherent limitations, and I agree with the authors that they only discuss periods where all three proxies present a consistent picture. However, this methodological choice is not explicitly discussed in the main text. I strongly suggest that the authors briefly acknowledge the limitations of the proxies used and clarify that they focus their discussion on periods where all three proxies align in terms of the link between the Ross Sea Dipole and ENSO.

OK this has been done in this new version; we introduced this paragraph (L. 394 to 418) where we clarified that the two periods selected (1000-1400 CE and around 300 CE) correspond to the two longest periods showing sustainted multidecadal El Nino conditions.

On longer (climatological) timescales, these considerations suggest a possible relationship between dust input to RICE, local snow accumulation, stable water isotopes, sea ice in the WRS and ENSO. Since ENSO is a complex climate pattern that involves interactions between the ocean and the atmosphere, relying on a single proxy for paleo-ENSO may oversimplify the underlying dynamics, which cannot be fully captured by a single proxy alone. For this reason, in this study we use two widely-recognized and well-established paleo-records to investigate ENSO behavior over the past 2 kyr. These are the SOI-precipitation (SOIpr) index from the tropical Pacific (Yan et al., 2011, fig. 5d) and the red color intensity record from Laguna Pallcacocha in southern Ecuador (Moy et al., 2002, fig. 5e). The SOIpr index is calculated as the difference between normalized annual rainfall data from the tropical western Pacific and the equatorial eastern and central Pacific (Yan et al., 2011), with negative values of the index indicating El Niño-dominated conditions. For the past two millennia, precipitation records from Indonesia and the Galápagos Islands were selected for the calculation of the index. Specifically, historic rainfall data for Indonesia were derived from salinity reconstructions based on planktonic foraminifera oxygen isotopes and Mg/Ca ratios, while rainfall history in the Galápagos was reconstructed using lake level data from El Junco (Yan et al., 2011). Indeed the grain size of sediments in El Junco lake (fig 5d) is highly sensitive to precipitation changes associated with the Pacific Walker Circulation and El Niño events. The second ENSO proxy we refer to in this study is based on the colour and composition of sediment layers at the bottom of Laguna Pallcacocha (Ecuador). This is linked to intense El Niño events, since increased convective precipitation driven by anomalously high sea surface temperatures in the Pacific leads to higher stream discharge and increased terrestrial material input into the lake. This detrital input includes iron-rich minerals, typically reddish in colour. As a result, periods of increased or decreased runoff can be detected by analysing the red intensity record of sediment layers, from which a time series of moderate-to-strong El Niño events has been constructed (Moy et al., 2002). Because these two series capture the regional impacts of ENSO events and are based on different proxies, they exhibit both similarities and differences; however, some time intervals where both paleo-ENSO records *exhibit dominant El Niño-like conditions over the last 2 kyr can be identified. Taking into account periods when SOIpr index is negative and at the same time the red colour intensity record is above the 75th percentile, as example, some intervals dominated by El Niño-like conditions can be identified. The longest of these is the ~400 years long period from 1000 CE to about 1400 CE, followed by a ca. 80 years long period from ~255 to 335 CE (fig. 5, grey bars).*

2. My second concern pertains to certain statements that should be revised or tempered, particularly regarding the links between ENSO and the Ross Sea Dipole. While I find the discussion for the period 1000–1450 CE convincing, the limitations of this connection in other parts of the record, such as during the El Niño analogue at 300 CE, should be more explicitly addressed. I anticipate the authors in saying that this is the fourth time I raise this issue. However, my concerns from previous review remain insufficiently addressed. Given that the study presents a 2000-year record—not just a 1500-year one—it is essential to discuss the entire dataset and properly address any inconsistencies in the proposed interpretation. Simply stating that "the first part of the record is much more complicated to understand" is not a valid reason to overlook it or interpret it in a superficial manner without supporting references (see specific comment below). This also contradicts the statement in Line 368: "to accurately interpret our 2kyr record." The authors have not, in fact, accurately interpreted the entire 2000-year record, but rather only three-quarters of it.

Specifically regarding my concern: reading the revised paragraph it seems that the authors still underline that at 300 CE a similar El Nino event was in place, and consequently a similar Ross Sea dipole to that of 1000–1450 CE was still in place. However, δD is not the only proxy that does not respond as expected. As also highlighted in my previous review (but ignored), ssNa from TALDICE shows a minimum during this period, indicating that ENSO did not seem to induce a Ross Sea Dipole: both the eastern and western Ross Sea regions exhibit minimum sea ice extent. This suggests that the ENSO-Ross Sea Dipole relationship may not be as straightforward as proposed or that the proxies used for this comparison have limitations. For example, the ssNa proxy is typically averaged over several years to capture long-term sea ice changes (see Crosta et al., 2022) and may not be reliable for short-term events such as the one observed at 300 CE. While it performs well for sustained El Niño periods (e.g., 1000–1450 CE), its applicability for shorter events may be uncertain. Acknowledging this, or considering alternative explanations, is crucial—especially in light of the manuscript’s conclusion: "El Niño-dominating conditions promoted the establishment of the Ross Sea Dipole" (L539). Well, the available data do not fully support such a definitive claim.

In the new version of the paper and specifically in paragraph 4.2 and sub-paragraph 4.2.1., we clarified all these points.

About the “lack” of clear response from the Deuterium record around 300 CE, see L. 420-427.

About ssNa from TALDICE, we did not reply in the previous revision of the ms because we did not interpret correctly your observation. Indeed, the point is that we do NOT relate directly this proxy to El Nino. To clarify: the sea-salt sodium (ssNa) record from Talos Dome is related to the extent of newly-formed pack ice in the Western part of the Ross Sea. Pack ice extent in that part of the RS is NOT directly related to El Nino, since literature studies from present-day demonstrate that there is not a statistically-significant correlation on meteorological timescale (Li et al., 2021). Conversely, a sea ice antiphase (dipole) between WRS and ERS seem to emerge from modern meteorological data in correspondence to blocking anticyclones off the ERS coast (see Emanuelson's studies cited in the paper). But blocking anticyclones and El Nino are not one-to-one related of course.

Also, the pack ice in the WRS, and hence the ssNa from TALDICE, is NOT related to the Ross Sea polynya formation: indeed, Mezgec et al (2017) used diatoms from the coastal sea ice zone and ssNa from Taylor Dome (and not Talos Dome) as base records for the construction of the RS polynya index, because ssNa from TY is related to landfast sea ice, and not to pack ice.

So, our observation shows that around 1200-1300 CE the sea ice dipole inside the Ross Sea reaches its maximum expression cannot be related directly to ENSO. We only observe that in this time window, the sea ice in the ERS and Amundsen Sea was likely minimum while, conversely, pack-ice extent in the WRS was at its maximum extent, and the Ross Sea experienced a strong sea ice dipole between its eastern and western parts, in parallel with the temperature dipole of Bertler et al., 2018. Surely El Nino leads to a temperature dipole inside the RS, but any evidence exists for the ENSO relationship with a sea ice dipole involving pack ice in the WRS.

So, in the new version of the paper we deeply modified paragraph 4.2 in order to make clear that TALDICE ssNa maximum around 1300 CE occurred at the same time of ERS sea ice minimum, and therefore that is a moment over the last 2 kyr when we observe the maximum expression of a sea ice dipole in the RS. We note also that this feature is related to northerly winds to RICE, and likely to blocking conditions in the ERS. But in this paragraph we do not mention any correlation with ENSO.

In sub-paragraph 4.2.1 that is the one dedicated to ENSO, we do not relate ssNa from TALDICE to ENSO and to avoid confusion we clarify that (L.483-484): *"No significant relationship between pack ice in the western part of the Ross Sea and ENSO emerges from modern data, while in the Amundsen and ERS the sea ice decreases by 10-20% during El Niño events and increases in the Eastern Amundsen and Bellingshausen Sea as well as in the Weddell Sea. This pattern is known as the Antarctic sea ice dipole, representing the leading mode of ENSO-related Antarctic Sea ice variability (Li et al., 2021)."*

We believe the TALDICE-ssNa issue is much clearer in this new version of the manuscript.

3. Finally, in their reply, the authors stated that they would shorten the initial paragraph on the different climate periods, but they did not. Unfortunately, this is not the only instance where they claimed they would make changes but failed to do so. While it is entirely acceptable to disagree with a referee's comments and provide well-reasoned responses, I find it somewhat disrespectful to deliberately disregard certain suggestions (although answering "OK"), especially when they were made solely to enhance the quality of the manuscript.

We decided to keep the initial paragraph as it was originally but forgot to add this in the second version of the point-to-point response.

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OTHER COMMENTS (line numbers refer to the manuscript version without track changes)

L26: Consider providing a brief definition of the Ross Sea Dipole here, as its introduction feels somewhat abrupt. You might condense the explanation from L171–173 to ensure clarity. I raised this point in my previous review as well.

In the new version of the ms, we decided to give less emphasis to the situation around 1300 CE, when proxies suggest the maximum expression of a dipole (sea ice dipole, in this case) inside the Ross Sea. So, we reworded the abstract accordingly.

L66: Since the manuscript presents three questions, a “(3)” should be added after “LIA?” for consistency.
sure

L128: "Over the last 2 ka" should be written without "BP," as "over the last" already implies a time span relative to the present.

done

L218: Since the mean is a specific value, please provide the actual number (e.g., 16.x) or a range.

done

L218: If the dust record over the Holocene is not shown in this study, the phrase “this study” should be reconsidered. As currently written, it suggests that the temporal dust record is presented, while only an average value is reported. If the authors do not want to disclose the value, they should either refer to other similar sites, or remove this reference to the Holocene as it is a sentence that cannot be verified (the data are indeed not shown).

Part of sentence removed from text.

L220: The average snow accumulation rate should be given with the same number of significant digits as in L76 for consistency.

OK, we kept official values 25±2 cm water equivalent per year.

L235: Does "Supplementary Information" here refer specifically to Figure SF3? Clarifying this would be helpful.

Yes, we reworded SF3

L237: Perhaps my previous comment was unclear. I understand that the period 550–1450 was identified based on the decadally smoothed profiles of dust concentration and flux. However, my question is: How were these periods determined? Was an abrupt change-point detection method applied to the smoothed dust profiles, or was this classification made qualitatively? You mention that “a detailed comparison between dust and stable isotopes is limited by the different sampling resolution of the two records.” However, it would be interesting to see if applying the same approach used by Bertler et al. yields similar results in your dataset. If the authors choose not to use such a method, they should explicitly clarify that the step changes in the dust profiles were identified qualitatively. The validity of this classification is nonetheless supported by the boxplot in the Supplementary Material.

Ok, we clarified that the step changes in the dust profiles were identified qualitatively.

L246: Shouldn't the periods be three?

Yes sure, this was a typo

L256: In their response to my previous review, the authors stated that they are not yet prepared to present the Holocene diatom record, as they are still working on it. Since these data remain unpublished and cannot be verified, the reference to "even over the Holocene" should be removed.

OK, removed this reference to Holocene data that are not shown.

L263–L267: Since the comparison between SEM and optical microscopy is a methodological outcome, it belongs in the Methods section. Please move this paragraph after L211.

We reworded the paragraph as follows, and let it in the Results as suggested by one of the other reviewers:

Regarding diatom analysis, a comparison between optical and electron microscopy revealed that out of a total of 184 valves and 84 fragments, only 2 valves and 16 fragments were missed using the optical method (SF6). Therefore, we estimate that the optical-based diatom abundances reported in this study may be underestimated by approximately 1% for valves and 19% for fragments compared to the SEM-based analysis. The higher percentage for fragments may be due to their smaller size, which makes them more challenging to identify. Overall, we conclude that the optical-based method used for diatom counting in this study is as reliable as the SEM analysis.

L265: Add "6" after "SF" for clarity.

Done (see above)

L345: Why did the authors not calculate the average ssNa values for the two periods (800–1000 CE vs. 1000–1300 CE) and compare them? Simply stating that "it increased" without providing numerical values is open to interpretation, particularly when the change is not so immediate. The authors should calculate the mean for the period before the increase (e.g., 500–1000 CE) and compare it with the mean from 1000–1300 CE, then test the differences for statistical significance. A box plot similar to what was done with the dust concentration would be appreciated. The authors have the data, so this can be easily achieved, and this will help in giving a stronger statistical support to the discussion.

The issue related to ssNa has been clarified in this new version of the ms.

This proxy for newly formed pack ice in the WRS is neither related to ENSO nor to the polynya but to blocking anticyclones that influence atmospheric circulation patterns, leading to colder conditions and enhanced sea ice formation in the western Ross Sea. We consider this proxy therefore of secondary importance to this study and cite it only to say that around 1300 CE we observe a sea ice dipole in the RS. Not necessarily related to ENSO (but interestingly occurring inside a long period of sustained El Nino conditions).

L410: The discussion regarding the lack of response in the RICE stable isotope record is appreciated. However, the following statements require references: “surface waters of the Eastern Ross Sea were much cooler” and “300 CE marks a period of dynamic changes in the Ross Ice Shelf, with the calving line either terminating or having just completed its last phase of retreat.”

Done, Yokoyama et al., 2016.

Yokoyama, Y., Anderson, J. B., Yamane, M., Simkins, L. M., Miyairi, Y., Yamazaki, T., Koizumi, M., Suga, H., Kusahara, K., Prothro, L., Hasumi, H., Southon, J. R., and Ohkouchi, N.: Widespread collapse of the Ross Ice Shelf during the late Holocene, P. Natl. Acad. Sci. USA, 113, 2354–2359, https://doi.org/10.1073/pnas.1516908113, 2016.

L423–L426: The authors cite Figure 6 in Brightley (2017), but this figure does not exist in the version available at this link: https://openaccess.wgtn.ac.nz/. The last available figure is 5.7. Is there another version? If so, please provide the correct reference. Alternatively, are the authors referring to Figure 4.1?

Indeed, we refer to our figure 6 with data from Brightley. So, we reworded:

*fig. 6, **data** from Brightley, 2017.*

Additionally, the phrase “significant increase of marine compounds” should be clarified. First the authors should know that Na+, Ca2+, K+, and Mg2+ are ions, not compounds!! I suggest the use of "marine tracers", or "marine species". Second, the comparison needs to be more specific: significant increase relative to which period? Based on Figure 4.1, the comparison appears to be with 1200–1400 CE, but this should be explicitly stated.

Yes, “marine species” replaces “compounds”, so the sentence has been reworded as follows:

Diatom peaks occur concurrently with stable water isotope enrichments and decreased snow accumulation. This means they correspond to a period of intense influence of local low-elevation marine air masses originating from the marine boundary layer. This is also suggested by the significant increase of marine species (Na+, Ca2+, K+, Mg2+, SO42-) in the RICE ice core (fig. 6, data from Brightley, 2017) in correspondence to diatom peaks, with respect to the period 1200-1400 CE.

FIGURES & FORMATTING
Figure 1: A compass rose would be helpful

OK done, some other minor details have been ameliorated in figure 1.

Figure 2: Panel (c) already includes FPP, dust concentration, and Loess, correct? If so, panel (d) seems redundant and could either be omitted or clearly separated. Please also specify the color coding in the

caption. To enhance readability, the Loess 20-year running mean should be shown in a distinct color (e.g., red). Additionally, the arrow in panel (e) should be better aligned and centered with the panel it refers to.

In the previous version of the paper panel (c) included only FPP and loess of FPP. Panel (d) included dust concentration and loess of dust concentration. Color coding was not specified. Panel (e) did not display arrows. In the new version of the paper, panel (c) includes both FPP, in grey and dust concentration in dark red, each with its loess (black for FPP, red for concentration). Panel (d) corresponds to former panel (e). Now, the color coding is specified.

Figure 5: Since the unit for concentration is provided for dust, it should also be reported for ssNa, RICE snow accumulation rate, and Fe concentrations for consistency.

ssNa is in ppb, snow accumulation in m water equivalent per year, Fe in cps. These units of measure were reported in the figure.

Figure 6: The units should be consistent with those in other figures—either use ppb or µg/kg, but maintain uniformity throughout the manuscript.

Changed in ppb in fig. 6

GENERAL FORMATTING & DATA AVAILABILITY

Double spaces: There are multiple instances of double spaces throughout the text. Please review and correct.

Abbreviations: Both ka and kyr appear in the manuscript. Please choose one and apply it consistently.
Kyr instead of ka is used now throughout the text.

Data availability: In accordance with Open Access principles (FAIR) and the journal's data policy, I recommend uploading the data from Tables S1 and S2 to an open-access repository.

OK, we will do that (it can be done right after acceptance of the ms).

SUPPLEMENTARY MATERIAL

Figure SF1: The caption is unclear. Does this figure present dust fluxes for particles smaller than 5 µm? If so, please specify this in the caption. Also, clarify the meaning of the elements in the figure: "Mean dust fluxes (bars), Late Holocene dust fluxes (circles), and elevation of each drilling site (crosses)."

Reworded as follows: Mean dust fluxes (bars) calculated for different East Antarctic sites. Data are referred to particles smaller than 5 micron in diameter and are calculated over the entire Holocene (from Delmonte et al., 2020 and references therein). The altitude of each drilling site is also reported (crosses). The Late Holocene dust fluxes for RICE (this work) and WAIS (Koffman et al., 2014) are indicated by ellipses.

Figure SF3: Please present the p-values using scientific notation and calculate it also for 0-500. If they want, the authors can also add a small diagonal 3x3 matrix reporting the p-values for the three periods.

Scientific notation for p-values has been introduced.

But we do not understand the comment “calculate it also for 0-500”. Does it mean that we have to calculate the relationship between concentration and size for a part of the record? Why?