In red=Authors reply

In black=Reviewer n.2 comments

POINT-TO-POINT RESPONSE to rev. 2

General comment to Reviewer n.2.

We read with interest his comments, mainly focused on three points that are:

- 1- The structure (organization) of the different paragraphs into the paper.
- 2- The poor statistics in support of some dust flux changes
- 3- The lack of an isotope response to the proposed 300 CE El Nino "analogue" that is definitely marginal with respect to the main topic of the paper

All the basic novelties of this paper have been neither commented or put into discussion. These novelties consist in the behavior of the Ross Sea during a long-lasting period where El Nino conditions dominated, and culminating around 1300 CE, and the progressive eastward expansion of the Ross Sea Polynya during the LIA, with all the atmospheric and environmental changes connected to these events.

Organization of the paper and statistics (points n.1 and 2) can improved and we note that in particular point n.1 has also been suggested by Reviewer n.1, so we will propose a novel structure for the paper.

Statistics can be easily improved. We will introduce boxplots and t-tests as suggested, but we anticipate that nothing will change in our interpretations/conclusions.

Concerning point. n.3, we note a criticism repeated more than once in the course of the review, at least 3 times, We point out that this "analogue" for El Nino is definitely a marginal observation in this work, and in fact we voluntary avoided to discuss it in detail. This because the interpretation of that possible "analogue" would necessitate of further data among which the d-excess record. In fact, 300 CE is still a period of dynamic changes of the Ross Ice Shelf, with the calving line that was finishing or had just finished its last phase of retreat. Under these conditions, it is possible to hypothesize that Eastern Ross Sea surface waters were likely cooler and the response of the stable isotope different. But of course, this assumption would need further support, that is the reason why we did not mention it in the manuscript. We can of course add some lines as a comment but we insist on the fact that this is definitely a marginal aspect compared to the rest of the novelties from this work.

Regarding the rest of the observations raised by the reviewer, among which "useless discussions" (cit.), we note that every paragraph is intended to clarify a specific aspect of the (complex and evolving) paleoenvironmental scenario characterizing in the Eastern Ross Sea during the last 2 kyr. We see no reason to define these discussions "useless" as every sentence is intended to clarify a piece of a scenario that is far from being simple.

Clarified these points, we found the "rejection" verdict inappropriate mainly because all the points raised by the reviewer concern marginal aspects that can be easily adjusted as said before and eventually we appeal to the Editor to contact a third reviewer.

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This is a review for the manuscript from Lagorio and co-authors. The authors present a record covering the last 2000 years for dust, diatoms, snow accumulation and water stable isotopes. Their variability in the RICE ice-core record is linked to past atmospheric circulation and ocean conditions.

I think that the interpretation the authors give is not fully convincing due to:

1. Poor analogue discussions (e.g., 550-1470 CE vs 300 CE) ;

   As above: the 300 CE analogue is only a minor suggestion in our work, of secondary importance compared to all the rest of the paper. El Nino/SOI proxies indicate an interesting period around 300 CE characterized by dominance of El Nino-like conditions, that is showed both in the Ecuadorian sequence and in the SOIpr. Indeed, we noticed an increase in both (1) dust and (2) snow accumulation rate. However, the stable isotope record apparently does not show the same increase as in the later period, so we believe there are some issues to solve (in a dedicated future work) within the stable isotope record, which can be carried out by taking into account both the changes in deuterium excess (not available at the moment) and the dynamics of the calving line at that time. Surface waters in the Eastern Ross sea might have been cooler at that time, and this in relation to the last phase of assessment of the calving line that ended to its modern position around that time. So it is possible to envisage a different behavior of the isotopic signal.

2. Poor statistical evaluation and discussion of the results;

   OK - Improved statistics in the present version of the ms.

3. Contradictive statements. E.g., the authors claim that conditions of reduced sea-ice in the ERS are associated with increased snow accumulation, high dust, and isotopically enriched water vapor (L150-153). However, they also claim that sea-ice is reduced in the ERS around 300 CE, when a isotopically enrichment in deuterium is not visible.

   Same point as above (n.1).

It seems to me that sometimes the interpretation of the results is cherry-picking driven, rather than statistically driven. For example, I was confused on the kind of proxy they used for describing El Nino-like conditions. First SOI was used, then the laminated sediment record from Laguna Pallcacocha. The authors discuss only their results when the two reconstructions agree (e.g., 1000-1500 CE and around 300 CE). However, what about when they do not agree (e.g., 0-300 CE, or 500-700 CE?) Which is the proxy that should be used? And why?

Obviously the two records disagree in part, because they are sensitive to different climate parameters somewhat related to ENSO. The SOIpr index is calculated as the difference between normalized annual rainfalls of tropical western Pacific and the equatorial eastern and mid-Pacific. Proxies include among others, salinity reconstructions from planktonic foraminifera δ18O and Mg/Ca ratios. On the other hand, the color and composition of the sediment layers from the bottom of Laguna Pallcacocha in Ecuador ca be associated with El Niño events: increased rainfall and runoff caused increased input of terrestrial material into the lake, including iron-rich sediments often reddish in color. By analyzing the red intensity of sediment layers, periods of increased/decreased runoff can be detected and consequently El Niño events. Given these considerations, we obviously conclude that there is not a perfect proxy for ENSO but both records can be used as paleo-ENSO reference records. Interestingly, the two periods between 1000-1500 CE (and around 300 CE) are those where they both show strongest El Nino conditions, and for this reason we decided to focus on these rather than on the periods where the two records disagree.

Also, the paper requires an extensive work of revision especially due to its length, useless discussions of topics that are not relevant and confusion in the organization of the different paragraphs (e.g., mixing Methods and Results, introducing sub-chapters in the introduction etc....). For these reasons, my suggestion is to reject the manuscript in its current form.

Organization of the paper and statistics have been deeply improved accordingly. It will be much better structured in the new version.

Regarding the rest of the observations, we don't see where there are "useless discussions" (cit.) in this manuscript but everything is focused towards the delineation of the paleoenvironmental/paleoclimatic context that helps interpreting these data. Every paragraph is intended to clarify a specific aspect of the (complex and evolving) paleoenvironmental scenario characterizing in the Eastern Ross Sea during the last 2 kyr. We understand that sometimes it can be hard to follow but the scenario is indeed very complex and necessitates a thorough explanation.

Please find below some general and specific comments.

General comments:

- I think that an overall revision of the structure of the manuscript structure should be undertaken. For example, section 1.1 is a mix between methods (description of the properties of the ice core) and introduction (what information can be retrieved from the core). Similarly, the results should not be presented together with the Methods, but on a distinct section. In the current form, the Method and Results section is a continuous back and forth to showing results and discussing methods, making the reading hard. I added some specific comments below to guide the authors in rearranging the structure of the manuscript. Also, the introduction alone covers 1/3 of the overall length of the manuscript, this is too much, and it is probably more suited for a thesis rather than for a scientific paper. For this reason, I suggest polishing and reducing the length of the intro.

  OK – better structure now

- I believe that the manuscript can be further improved by adding some more (basic) statistics. For example, the identification of periods with high/low dust and their comparisons it is not supported by any statistical tests.

  OK – more statistics introduced now

- References to climate periods such as the Roman Warm Period or the Medieval Climate Anomaly is completely irrelevant for this manuscript as these periods refer to regional rather than global climate changes. Indeed, the authors discussed the ice-core record based on differences in the dust concentrations, so why referring to periods with no relevance for Antarctic cores? I would remove any reference to these periods as it is absolutely not relevant, and misleading.

Certain terms related to climatic events, such as the 'Medieval Warm Period' are commonly used globally despite their regional variations and lack of a uniform global impact. This is because they refer to significant periods in human history and help readers better contextualize the time frame being discussed. So, we are certainly aware that the name given to these periods refers to some regional changes that are not relevant to Antarctic ice cores, but we note that this is a nomenclature widely used also in the paleo-climatological literature worldwide (e.g. Yan et al., 2011).

- The authors identify as a main period for discussion and data interpretation the one between 550-1470. That's a correct choice. However, they decide not to discuss what happened before 550 in a distinct paragraph as they did for the period 1470-1990. Why? They only briefly discuss a sort of climatic analogue around 300 CE (whose interpretation is poorly convincing).

  Surely the first 550 years of the record can be briefly discussed in the new version of the ms.

- The quality of some figures (e.g., Fig. 5) is extremely poor at a magnification factor of 1.

  Surely the resolution will be improved for all the figures in the final version

- The manuscript will benefit from a scheme, showing the conditions leading to the different sea-ice conditions to happen (e.g., ENSO, dust, water stable isotopes, accumulation and sea-ice condition).

  We avoid the use of schemes unless strictly necessary as in figure 5

SPECIFIC COMMENTS

L26: the authors refer here to the Ross Sea dipole as it is something well known. Please rephrase.

OK

L27: an increase in pack-ice, where?

In the Eastern Ross sea, we will specify.

L28: what do you mean by unprecedented? I would rather be more specific saying "unprecedented over the last 2000 years".

OK

L37-59: I think that most of the text reported here is not relevant for the paper. I would delete this part and add information that can help the reader to have a clear framework of the topic of the paper. The classification among MCA, RWP etc, is in my opinion also wrong as Antarctica has not been influenced by these climatic periods (Neukom et al., 2019). The introduction should be more specific towards the questions the paper aims to address. As it is written is probably good for a thesis or a book chapter, but not for a scientific paper. Neukom, Raphael, et al. "No evidence for globally coherent warm and cold periods over the preindustrial Common Era." *Nature* 571.7766 (2019): 550-554.

As said above we are perfectly aware that RWP or MCA did not influence directly Antarctica but in some way these names help contextualizing the time periods that are discussed. We will rephrase and shorten that paragraph anyway.

L71-83: details regarding the ice-core drilling should be included in the Method section, not in the introduction.

OK

L79: can you provide some numbers? Which is the accumulation rate at RICE? Which is the accumulation rate at other sites?

Yes, although this is widely shown later on in the figures as well as in the text.

L84 – 104: I am not fully convinced that all this long description of trace elements is relevant for the objective of the paper. I suggest to strongly summarize this section to the most relevant aspects that can guide the reader for a clear interpretation of your data. For example, what you write afterwards for diatoms (L105-136) goes more into this direction, albeit it can be still summarized avoiding useless details.

We do not agree with this statement. We believe the behavior of trace elements is very useful as the dust transport at Roosevelt Island is far from being simple and/or obvious, and in this context the evidences brought by trace elements are of key importance to this coastal system.

L107: repetition of "material", choose other terms to improve the flow

OK

L172: be specific when mentioning how many samples were measured for dust (i.e., not saying >400, but the actual number). Similarly, specify what "subset" means and therefore how many samples were analyzed for diatoms counting and identification.

OK

L189: can you specify quantitatively the "pronounced variability", for example by including a relative standard deviation?

OK

L186-L193: this is a mix of results and methods. The results must go in a different section (if you want together with the Discussion, but not with the method).

OK

L192: an average without a standard deviation does not give a lot of information to the reader regarding the dust variability. Please add std. Dev. Or coefficient of variation.

OK

L193: please change "estimation" to "estimate", and provide a standard deviation (or CV) for WAIS. Comparing to average values can provide some information, but says nothing regarding the variability of the signal at the two sampling sites.

Koffman et al (2014) do not report st.dev or CV for WAIS but say that "Dust flux varied between ca. 2 and 25 mg m-2 per year for most of the past 2400 years, remaining generally around 3-5 mg m-2 per year". We will insert this in the ms.

L203: rephrase in "20-80% and 10-50% for FPP% and CPP%, respectively". Rephrase in the same way at L204.

OK

L212: why not plotting the dust in the same Figure?

Unclear – isn't dust is already in the figure?

L215: before you introduced dust fluxes (L192), why here you are back to discuss dust concentration? Please explain in the text the rationale behind this choice.

Dust concentration and fluxes show similar behavior, but given that dust concentration is higher when snow accumulation rate is also at its maximum level, the changes are more pronounced when fluxes are considered. So, we adopted a conservative approach when dealing with concentrations, but we can refer to fluxes, whose changes are even more pronounced.

L215: how did you calculate the beginning and the end of the prolonged dust periods at RICE? You compared the results with changes in the deuterium isotopes that were determined using a minimum threshold parameterisation to achieve a minimised residual error. Did you used the same method? If yes, please be more specific al L214, otherwise please apply the same approach and compare the time series using similar statistical approaches.

As clearly written in the text, it is the "decadally-smoothed profile of dust concentration and flux (fig. 2 d, fig. 5 a) over the last 2 ka" that highlights a long period of prolonged high dust levels starting about 550-600 CE and ending around 1470 CE."

So, given the evidence from the stable isotope record and the variability of the smoothed profile of dust concentration, any specific statistical method was applied further recognize that period, but to confirm our suggestion, the analysis of concentration levels showed that they were above-average for more than 50% of time in that period.

In the new version of the paper the boxplot will convince further the reader.

L219: A box plot showing the distribution of dust concentration (or fluxes) in the three periods (0-500; 500-1500;1500-1900) that the authors identified, would be helpful to immediately identify differences in the three regimes. Also, I think that saying something like: "dust levels remain below/above average for about XX% of the time" does not bring anything. If you want to discuss the statistical differences among the different periods, you should rather introduce some basics statistics (e.g., t-test).

OK – we will introduce boxpolots and t-tests (in the supplementary probably).

L221: "spanned.... spanning", reformulate to enhance flow

OK

L225: I don't understand why you don't show the values for the Holocene? I suggest adding the corresponding plots to the SM. Or at least provide an average value for the Holocene. Written in this way, it does not bring anything to the discussion of the results…

The question about "what happens before the last 2ka" is obvious at this point and we cannot skip this anticipation about the fact that the Holocene never showed diatom influx levels similar to the LIA. But we are not ready to show the Holocene record, because we are still working on it. No possibility at this step.

L225: why don't saying: out of the XX% identified diatoms, >98.5% were…. In this way the reader knows the fraction of identified and unidentified diatoms.

OK

L237-243: this is method, again mixed after presenting results. This part should be included when you discuss how you measured diatoms as a proof of the robustness of your method (e.g., after L185).

Ok as said before the text will be re-arranged.

L286: please use the same names in the text as they are in the figure. MCA in the text is MWP in the Figure. Choose one acronym and stick to this, although I am skeptical in defining these periods for sampling locations that were not affected by them

Same comment as above. Same answer as above.

L288: how much is this negative correlation? Can you provide the value here and associated p-value?

Everything is reported in Bertler et al., 2018. This is not a topic of this work.

If a reader is not convinced about the statistically-significant negative correlation between stable isotopes and with sea-ice extent in the South Pacific, given that the source for this information is duly cited, he can to directly to that work. We start from Bertler's conclusions, and adding useless statistical details from other papers will add only weight to this manuscript.

L299: NA should be Na

OK

L300: can you briefly explain what do you mean in terms of "polynya activity": how is it measured? I would also be consistent with the y-label in Figure 5, where it is reported "polynya efficiency".

We will explain that. This composite index was introduced in Mezgec et al., 2017, duly cited.

L304: dust fluxes are actually decreasing after reaching a peak in 900 CE

L309-L343: I think that while interesting, this part can be significantly shortened and linked to the previous paragraph. If you want to keep the sub-paragraph (not recommended), change 3.2.2 to 3.2.1.

OK

L343: but you also compared ssNa from TALDICE, not just water stable isotopes.

OK we will reword.

L344-L353: If I have correctly understood, high dust values at RICE correspond to low sea-ice conditions in ERS, while high ssNa at TALDICE corresponds to enhanced sea-ice conditions (max pack ice). The authors indicate that this can be associated with ENSO conditions, as inferred by the high SOIpr values peaking in 1300 CE. The authors also observe an analogue condition at around 300 CE, where SOIpr values are peaking. However, besides the peak in the dust flux at RICE, which may correspond to low sea-ice conditions in ERS (however, no increase in the deuterium is observed), ssNa at Taldice it is at its lowest, suggesting low pack ice conditions. Also, the SOIpr values persist at high values even before 300 CE, while dust fluxes at RICE decrease and deuterium is not enriched (as they claim as a needed condition for reduced sea-ice, L150-153). The authors do not discuss these inconsistencies, but rather they put their interpretation in front of the data (i.e., they use another proxy for ENSO that seems to work better).

This is the 3rd time the reviewer raises this point.

The first part of the record is much complicated to understand but as already said, the 300 CE analog highlighted by the two different El Nino proxies used is also reflected in all our data coherently with the longer El Nino period encompassing the medieval times. The only proxy that does not respond in the same manner is the stable isotope of RICE, and the reason why it behaves like that will be investigated deeply in the future. See comments above at the beginning of our rebuttal.

Also, according with the proxy used in Fig. 5d, El Nino like conditions are also around 600 CE, but not significant increase in the dust is observed: why is this not discussed? Without statistics it is hard to give an interpretation of the results.

Indeed, there is disagreement among El Nino proxies (SOIpr and the Ecuardorian record) around 600 CE, so we prefer not to focus on this event.

L378: describing what polynyas are here is a bit too late. Why don't you anticipate this discussion earlier in the text, and explaining what do you mean for polynya activity/efficiency?

OK

L382: is there in literature any figure of diatoms collected far from the source? It would be nice to cite it or provide a simple comparison. However, this is a minor thing.

It is not so simple, depending on the environment, the diatom input can be extremely different from site to site in Antarctica, both in terms of quantities and species.

Tetzner et al., (2021, 2022a, 2022b, see reference list) provided estimates about seasonal diatom influx in different sites from the Peninsula, located at different elevations and distance from the source.

For remote transport to central East Antarctica, a few specimens were detected by different authors (Delmonte et al. 2017 and references therein) for the last glacial period.

With respect to eolian diatoms, the time period investigated in this work was only studied by Kellogg and Kellogg (1996) in their paper entitled "Diatoms in South Pole ice: Implications for aeolian contamination of Sirius Group deposits". At South Pole, the trend in marine diatoms was very similar to ours, with a sharp increase during the LIA, but of course absolute values are much smaller because of the distance of South Pole from the coast. Unfortunately, we could not find raw data from that work to compare with ours, but duly cited that publication.

L479: I question the "unprecedented low dust input". Have you calculated the mean between 0-500 and between 1500-1940 for dust concentration and checked for statistical differences?

The sentence is: …"leading to an immense and unprecedented diatom input at RICE and low dust influx, in tandem with …" . So the adjective "unprecedented" refers only to the diatom input at RICE.

No "unprecedented low dust input" has been mentioned in our text.