Climate of the Past Discussions

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Comment on cp-2021-88

Anonymous Referee #1

Referee comment on "Regional validation of the use of diatoms in ice cores from the Antarctic Peninsula as a Southern Hemisphere westerly wind proxy" by Dieter R. Tetzner et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-88-RC1, 2021

This paper proposes the use of open marine diatoms in firn/ice cores from the Antarctic Peninsula as a proxy for wind strength. Basically, it represents the follow-up of a companion paper (Tetzner et al., 2021a) recently published in TC.

Although extremely intriguing and definitely novel, the discussion and conclusions of this study have to be revised and clarified according to the following considerations, before acceptance for publication in Climate of the Past:

- Whilst the SO is the principal source of marine diatom to this region, which is the argument allowing to rule out completely contributions from exposed sediments? Marine diatoms can be windblown to coastal sites and can be remobilized easily by winds. This does not imply that the amount of diatoms is necessarily related to the amount of dust since different dust sources can have a very different abundance of microfossils.
- Why the abundance of freshwater or brackish-water diatoms is not taken into account? It seems completely neglected but it can be very useful. Are sponge spicules or other microfossils also present in the samples?

- Is it possible to show the two diatom abundance records (marine and nonmarine/brackish)?
- Also, which species of diatoms are you looking at? Imagine that not all readers switch between the two papers in order to understand what you are effectively counting.
- Are the marine species identified in the cores comparable to marine species which are found in typical Sirius formation?
- In the introductions, lines 66-67, you cite as references for the sentence "Once in the atmosphere, they can be transported by winds over long distances" the papers from Gayley, 1989 and McKay et al., 2008 [I have no access to the 3rd publication cited]. Both papers consider diatoms as associated mainly with deflation of dry sediments. So,

please provide references clearly indicating sea spray as the primary source for long-range transported marine diatoms.

- In your statistical analysis, The diatom abundance ($n*a^{-1}$) is a sort of annual depositional flux of diatoms (number of specimens per year) that already takes into account the snow accumulation rate, since it is calculated over an entire year. For the chemical parameters, conversely, you use average concentrations per year (I see "ppb" in your figure!??), not fluxes..(?). So if concentrations are used instead of depositional fluxes, how can you get free from the snow accumulation rate for chemicals? All data must be transformed into fluxes otherwise the comparison of chemical/dust records among different sites has no sense.

- When looking at figure 2, one can observe that JUR and SKBL nicely show similar diatom abundance variability that is quite obvious given the location of the two sites and their common sensitivity to open ocean species. The SHIC core instead shows a different pattern of variability since it is sensitive to sea ice taxa. These are conclusions from the companion paper Tetzner 2021a. So, I think it is not well clear in this work what is novel and what is part of the conclusions drawn in the companion paper.
- The attribution of JUR and SKBL marine diatoms to the POOZ suggested by Tetzner (TC, 2021a) is interesting, but given the very coarse size of such marine diatoms, a mechanism for strong uplift and transport inland is required. So, is it possible that diatom abundance reflects not only wind strength *sensu stricto* but low-pressure systems generated in that POOZ area or passing through that area and then directed towards the Peninsula? Indeed wind strength around LP systems is generally higher, so it is just a different interpretation of this correlation.
- The novel proxy for wind strength is interesting but different from proxies like Calcium and dust. It is not correct to say (330-333) that particles and calcium reflect wind strength as they have been always associated with the cumulative effect of different factors that are: the primary production at the source(s), the humidity/precipitation en route during atmospheric transport, the snow accumulation rate in Antarctica, ... Conversely, a proxy that is much more directly related to transport (including wind strength) is dust grain size. So it is not correct to say that these are traditional proxies for wind strength. Different proxies are related to different dynamics. Please change these considerations accordingly.
- In general, it must be clarified to the reader that given the position of the sites, dust and Calcium are probably dominated by the effect of the local dust sources from marginal ice-free areas, that are not the same sources of marine diatoms but can provide diatoms through eolian reworking.

Since dust deposited at JUR likely comes from proximal sources (and to a lesser extent from remote areas) I cannot find sense in the correlation between dust at JUR and wind strength 10m altitude around 40-45°S. Also, dust from remote continents must travel at high elevations in order to reach Antarctica. So, again I am not sure that all correlations that are shown in figure 3 make sense and are worth to be considered.

Minor comments:

Line 78 - Are these really ice cores or firn cores?

Line 115: if microparticles are measured with an Abakus sensor, it is possible to get an idea of the degree of sorting of the dust, that is useful to constrain sources and transport distance?

Line 120: Can small diatom fragments (that you discard from your counts) provide an idea of the degree of diatom reworking?

Line 121: “Diatom abundance” means marine-only diatoms or really "all diatom valves"?

Paragraph 3.1.2: The correlation between diatom abundance per year and wind strength is interesting and is probably one of the key new messages of this work. However, figure 3 is too rich and the attention of the reader is not immediately captured by that. I also wonder if many of these correlations make sense. I suggest splitting this figure in order to focus on the most interesting part of it while moving the remaining part to the supplementary information.

For example, both JUR and SKBL show a correlation between diatom flux and wind strength, while the correlations related to Calcium and dust that are found at one site are very different from the other, and in any case, they are difficult to understand. Is there a possible bias related to the use of average concentrations instead of depositional fluxes? Actually, in line 227 you mention that “No clear or consistent pattern was identified when comparing chemical proxies from different ice core sites” – and this is quite strange when JUR and SKBL are considered.