

## ***Interactive comment on “Spiky Fluctuations and Scaling in High-Resolution EPICA Ice Core Dust Fluxes” by Shaun Lovejoy and Fabrice Lambert***

**Anonymous Referee #3**

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Review for "Spiky Fluctuations and Scaling in High-Resolution EPICA Ice Core Dust Fluxes" by Lovejoy and Lambert

In their paper Lovejoy and Lambert present an application of fluctuation analysis to a high-resolution reconstruction of dust fluxes in central Antarctica over the last 800 ka. This type of analysis with a record that is this highly resolved is new and generally merits the publication of the manuscript.

That being said, there are a number some minor and a number of major concerns that need to be addressed before the manuscript can be published in Climate of the Past.

I cannot speak to the correctness of the statistical analysis as I am not an expert on fluctuation analysis. That being said, I can probably speak from the point of view of

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a large fraction of the potential readers of a Climate of the Past paper: I found the description of the method reasonably approachable. Personally, I would have liked to get more intuition for the method, its results and its possible applicability to other paleoclimate records.

Overall, the paper could be improved greatly by adding more discussion of the methods, their results and their interpretation. At present the paper focuses a lot on the listing of the results of the different statistical analysis. This takes away from the potential interest of both the method and the results to the wider paleo-climate community.

One major concern is, that the manuscript is lacking a clear description of the data set that has been used, the way it was generated, and how this affects the analysis presented here. I know that this description is given in the original publication. Nevertheless, I think this is a vital point given that Lambert et al. (2012) state to keep the generation of the dust flux reconstruction in mind when interpreting its variance as it is affected by the assumptions and corrections that were involved.

One other major concern that I have is, that the interpretation of the, I think interesting, results unfortunately seems to be ad-hoc and not very thoroughly argued. From my reading of the previously rejected version of this paper and its reviews this point has only been improved marginally. To strengthen the manuscript and make it more suitable for Climate of the Past, I hope the authors extend the discussion of the results both in comparison with other studies and in terms of their paleo-climatological interpretation.

The discussion and the results sections are completely lacking any information on the uncertainties of the obtained results. The large variability of the results between the different glacial/interglacial cycles indicates to me, that the results might not be very robust. One further observation that I made is that in many Figures the authors omit error or variability indications “for the sake of clarity” which I think is a poor choice. Additionally, the analysis is hinged upon a number of assumptions that are not justified in the text. Specifically, the slopes used for the breakpoint analysis and the range of

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time scales used for the fluctuation analysis. The influence of these choices on the results needs to be shown and clearly discussed.

Specific comments:

Abstract:

P1 L17: The dataset has only a maximum resolution of 5 years. How can fluctuations on the one-year time scale be resolved. Please rephrase.

P1 L24-27: The logic of this sentence is not clear. Please rephrase.

P1 L27f: Why do they suggest this?

Introduction:

P2 L12-15: Please state that you refer to the temperature proxy time series, and also mention that this is a proxy, not a temperature in the strict sense.

P2 L23-27: Please consider explaining why the macro weather to climate transition timescale is important.

Method:

P3 L15: ...the spectrum is the Fourier transform...

P3 L24-26: It is unclear why due to scale invariance, the results from the dust fluxes can be transferred to the temperature proxies if they are affected by different climatic mechanisms.

P3 L26: The analysis in the "future publication" should more thoroughly be discussed here, especially as you present results from it, or alternatively only be mentioned in the outlook. In my experience these "future publications" unfortunately often do not manifest themselves.

P4 L 24: extra comma between "compare" and "the"

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Results:

The data set description should be in the Data + Methods section and more time should be spent describing the dataset used as pointed out above

P25 L27: With the strong emphasis that is put on the number of datapoint and their sampling frequency throughout the manuscript, the numbers should add up. Generally, this information is not strictly necessary for the paper and could be safely removed. Especially in light of the fact that the dust flux reconstruction is a combination of a multitude of measurements and ice flow modelling.

P7 L1: Water isotopes cannot be assigned to one particular atmospheric variable either, even though they are often used to reconstruct Temperature. Please consider rephrasing

P7 L5ff: Please consider mentioning the recent publications by Markle et al (2018) and Schüpbach et al. (2018) that deal with this the relation of en-route washout and aerosol deposition on the ice sheets more quantitatively.

P7 L19: There is no red line in the Figures.

P8 L7f: The unit of the spectral amplitude of the log-transformed fluxes is wrong.

P8 L10f: Consider moving the comparison of the spectral densities with the results of the fluctuation analysis to after their introduction or to the discussion section.

P8 L13ff: This list of scaling exponents is completely irrelevant here and should be removed for clarity.

P8 L14f: It is unclear why this supports the use of dust as a proxy for atmospheric variability. Please clarify.

P8 L 25-30: The results presented here are very hard to follow. Please consider reformulating.

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P9 L1: Whether the Haar fluctuations of the dust flux have simple interpretations is not shown in the Figure but is rather a matter of taste and should be left to the reader to decide. Please reformulate or remove this statement.

P9 L6f: After spending a lot of time describing and interpreting the spectral analysis in the previous section the description here is rather short. As the fluctuation analysis is the main focus of this study the authors should spend more time describing their results and leading the uninitiated reader through them.

P9 L4: "positive definite" seems to be the wrong phase here, consider replacing with "always positive" or similar.

P9 L18: Why is it important that the value is similar to those obtained from other ice cores. Please move all comparisons to other studies to the discussion clarify the interpretation and relevance.

P9 L20: This statement is somewhat superfluous: If the dust fluxes are not log-normally distributed due to the occurrence of large spikes, their logarithm will not be normally distributed.

P9 L24: Please move the interpretation and the relevance for tipping point analysis to the discussion and consider expanding on this point if you think it is an important application.

P9 L30f: Strictly, the statement that the scaling spectrum is the underlying behavior is an assumption and has not been shown in this study. Even though this a reasonable assumption, consider rephrasing.

P10 L4f: Please clearly state that any of the stacking approaches assume that all the glacial cycles and their sub phases are realizations of the same underlying process.

P10 L20ff: The start of this paragraph makes the reader expect spectra averaged over the different cycles. Please consider rephrasing and more extensively introducing the Figure.

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P10 L29ff: The Figure indicates a slope of 0.35, not 0.25 as mentioned in the text. More importantly it is entirely unclear why the authors choose to set the slopes before and after the transition as constants and then only fit the transition time scale. The effect of the chosen values on the presented results needs to be clearly discussed and the uncertainty of the results are missing completely from the text. I strongly urge the authors to do a proper breakpoint and error analysis.

P11 L5-9: As the other method is arguably only slightly more objective than the breakpoint inference by hand, please consider removing this entire section.

P11 L20f: I suggest that the authors remove a couple of lines from the figure for clarity instead of the uncertainties or find a different way of visualizing the results.

P11 L23: Please state why this exact time range was chosen.

P12 L10: There is nothing black in Figure 12, please correct.

P12 L18-21: Consider moving this sentence to the beginning of the paragraph to make it easier for the reader to follow the results.

Discussion:

P13 L7-9: You do not perform any significance analysis, so please reformulate this statement. Also please discuss why the lack of power around the obliquity cycle is surprising.

P13 L28-32: Is there any supporting evidence for the glacier variability?

P14 L1: If A reflects the amplitude of the variance a change indicates only a change in the variability, not in their overall abundance, please reformulate.

P14 L14-17: Please discuss this statement in the context of recent proxy and model studies that indicate fast Southern Hemisphere circulation changes during DO events. (Markle et al. 2016, Buizert et al. 2018, Pedro et al. 2018).

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P14 L24: Only ice cores have the intrinsic property that they become less resolved with increasing depth and thus age. Sediment cores are not affected by this. Please reformulate.

P14 L30: The paper does not show that the data neither over-samples nor smooths. Please either add this to the paper or remove this statement.

P15 L24: The reduction to the characterization of the different phase is a decision that the authors take, not an intrinsic property or the result of some analysis. Please reformulate this statement.

P15 L25: Missing dot between “A” and “We”

P15 L 26f: How is the variability of the dust flux at Dome C connected to the “activity” of the Patagonian ice sheet. Please extend and clarify this argument.

Figures:

6b: The units for the axis are wrong.

11: I suggest that the authors remove a couple of lines from the figure for clarity instead of the uncertainties or find a different way of visualizing the results.

Cited literature:

Buizert, C. et al. (2018), Abrupt ice-age shifts in southern westerly winds and Antarctic climate forced from the north, *Nature*, doi:10.1038/s41586-018-0727-5.

Lambert, F. et al. (2012), Centennial mineral dust variability in high-resolution ice core data from Dome C, Antarctica, *Climate of the Past*, doi:10.5194/cp-8-609-2012.

Markle, B. R. et al. (2016), Global atmospheric teleconnections during Dansgaard-Oeschger events, *Nature Geoscience*, doi:10.1038/ngeo2848.

Markle, B. R. et al. (2018), Concomitant variability in high-latitude aerosols, water isotopes and the hydrologic cycle, *Nature Geoscience*, doi:10.1038/s41561-018-0210-

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Pedro, J. B. et al. (2018), Beyond the bipolar seesaw: Toward a process understanding of interhemispheric coupling, *Quaternary Science Reviews*, doi:10.1016/j.quascirev.2018.05.005.

Schüpbach, S. et al. (2018), Greenland records of aerosol source and atmospheric lifetime changes from the Eemian to the Holocene, *Nature Communications*, doi:10.1038/s41467-018-03924-3.

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Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2018-171>, 2019.

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