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Climate of the Past Discussions

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

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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 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

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and its possible applicability to other paleoclimate records. Author Response: Many thanks for the positive comment. Although the fluctuation analysis presented here has been previously used on some mostly modern climate records (temperatures, various climate indices, precipitation, CO2 concentrations etc.), none of these are as classically "paleo" or intermittent/spiky as the EDC dust fluxes. In the following publication we will make a comparison between various ice core dust and temperature records and in that one we will definitely talk about wider-scale application and potential geographical disparities. Here, it is more of a proof-of-concept paper. 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. Author Response: We agree. However, as stated above, this is the first application of this method to a dataset of this kind and we want to focus here on the statistical and mathematical correctness and robustness of the method. In the subsequent paper we intend to talk more broadly about application for the broad 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. Author Response: We added a new paragraph (2.4) in the methods section that describes the dataset and it's features in relation with our analysis: "The dust flux data used in this study is based on a linear combination of insoluble particles, calcium, and non-sea-salt calcium concentrations (Lambert et al., 2012). Because missing data gaps in the three original datasets were linearly interpolated prior to the PCA, high frequency variability can sometimes be underestimated in short sections that feature a gap in one of the three original datasets. This occurs in about 25% of all dust flux data points, although half of those are concentrated in

the first 760 m of the core (0-43 kaBP), when an older less reliable dust measuring device was used. Below 760m these occurrences are evenly distributed and do not affect our analysis. Due to the sometimes slightly underestimated variability, the analysis shown here is a conservative estimate." 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 extent 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 time scales used for the fluctuation analysis. The influence of these choices on the results needs to be shown and clearly discussed. Author Response: We have added a new paragraph in the conclusion section about uncertainty estimates: "The results presented in this paper are largely empirical characterizations of a relatively less known source of climate data: dust fluxes. Dust flux statistics defy standard models: they require new analysis techniques and better physical models for their explanation. These reasons explain why our results may appear to be rough and approximate. Readers may nevertheless wonder why we did not provide standard uncertainty estimates. But meaningful uncertainties can only be made with respect to a theory and we have become used to theories that are deterministic, whose uncertainty is parametric, and that arises from measurement error. The present case is quite different: our basic theoretical framework is rather a stochastic one, it implicitly involves a stochastic "earth process" that produces an infinite number of statistically

identical planet earths of which we only have access to a single ensemble member. From this single realization, we neglected measurement errors and estimated various exponents that characterized the statistical variability over wide ranges of time scale, realizing that the exponents themselves are statistically variable from one realization to the next. In place of an uncertainty analysis, we therefore quantified the spread of the exponents (which themselves quantify variability). In the absence of a precise stochastic model we cannot do much better."

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. Author Response: We changed this sentence to "The temporal resolution ranges from annual at the top of the core to 25 years at the bottom ,..." P1 L24-27: The logic of this sentence is not clear. Please rephrase. Author Response: Changed to "In other words, our results suggest that glacial maxima, interglacials, and glacial inceptions were characterized by relatively stable atmospheric conditions, but punctuated by frequent and severe droughts, whereas the mid-glacial climate was inherently more unstable." P1 L27f: Why do they suggest this? Author Response: We're putting in plain text what is stated in technical words in the previous sentence. 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. Author Response: Changed to "Fig. 1 shows this visually for the EPICA Dome C Antarctic ice core temperature proxy (5787 measurements in all);" P2 L23-27: Please consider explaining why the macro weather to climate transition timescale is important. Author Response: We added this: "The macroweather-climate transition scale marks a change of regime where the dominant high frequency processes associated with weather processes and reproduced by GCMs in control runs gives way to a new regime where the variability is dominated by either the responses to external forcings or to new, slow internal sources of variability." Method: P3 L15: ...the spectrum is the Fourier transform... Author Response: corrected 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. Author Response: Scale invariance is a symmetry under time dilations. In a dynamical regime in which two different components such as temperature and dust are strongly coupled, each may have different scaling properties, but both should respect the scale symmetry including the transition scale at which the symmetry breaks down. It is only these aspects that can be “transferred”. We have changed the text to reflect these ideas. 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. Author Response: The key analyses – the systematic comparison of temperature and dust as a function of time scale - have already been performed but this paper is already long enough. We prefer to discuss them in a separate paper. P4 L 24: extra comma between “compare” and “the” Author Response: corrected 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 Author Response: We moved the data description to a new subchapter 2.4 and describe the data in detail as suggested. The new chapter 2.4 now reads “The dust flux data used in this study is based on a linear combination of insoluble particles, calcium, and non-sea-salt calcium concentrations (Lambert et al., 2012). Because no-data gaps in the three original datasets were linearly interpolated prior to the PCA, high frequency variability can sometimes be underestimated in short sections that feature a gap in one of the three original datasets. However, the amount of points that feature Unlike water isotopes that diffuse and lose their temporal resolution in the bottom section of an ice core at high pressures and densities, the relatively large dust particles diffuse much less and have been used to estimate the dust flux over every centimetre of the 3.2 km long EPICA core (298,203 measurements, (Lambert et al., 2012)). The temporal resolution of this series varies from 0.81 years to 11.1 yrs (the averages over the most recent and the most ancient 100 kyrs respectively). The worst temporal resolution of 25 years per centimeter occurs around 3050 m depth, with the result that at that resolution, there are virtually no missing data points in the whole

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record (Fig. 1)." The last paragraph in this text was moved from sub-chapter 3.1. P6 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. Author Response: The rest are data gaps due to cleaning of the raw data (contamination, missing ice, etc.). We changed the text to "298,203 valid measurements" 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 Author Response: We changed that phrase to "Polar dust flux measurements cannot be assigned to one particular atmospheric variable, like temperature for the water isotopes." 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 quantitively. Author Response: Yes, maybe a distinction between short and large timescales is in order here. We changed these sentences to "At any given moment, the amount of dust deposited in East Antarctica will depend on the size and vegetation cover of the source region (mostly Patagonia for East Antarctic dust (Delmonte et al., 2008)), on the amount of dust available in the source region (can depend on the presence of glaciers), on the strength of the prevailing winds between South America and Antarctica, and the strength of the hydrological cycle (more precipitation will wash out more dust from the atmosphere (Lambert et al., 2008)). Over large scales it is thought that temperature-driven moisture condensation may be the major process driving low-frequency variability (Markle et al., 2018), although that may not globally be the case (Schüpbach et al., 2018)." P7 L19: There is no red line in the Figures. Author Response: Yes, we removed the word "red". P8 L7f: The unit of the spectral amplitude of the log-transformed fluxes is wrong. Author Response: Thank you, corrected. 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. Author Re-

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sponse: Comparison moved to the discussion of Figure 5. P8 L13ff: This list of scaling exponents is completely irrelevant here and should be removed for clarity. Author Response: Sentence removed P8 L16f: It is unclear why this supports the use of dust as a proxy for atmospheric variability. Please clarify. Author Response: We argued that temperature and dust variability are of the same statistical type yet with significant differences. The former makes it likely that the dust signal is a real climate signal while the latter shows that it has different information. We eliminated the original sentence and replaced it with one similar to the above. P8 L 25-30: The results presented here are very hard to follow. Please consider reformulating. Author Response: We have modified the paragraph. 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. Author Response: Changed to "Haar fluctuations allow some direct interpretations. . ." 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 spent more time describing their results and leading the uninitiated reader through them. Author Response: Yes, this is a good point, thanks. We replaced this first paragraph by three paragraphs giving a fuller explanation of the figure.

P9 L14: "positive definite" seems to be the wrong phase here, consider replacing with "always positive" or similar. Author Response: Corrected. 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. Author Response: We have removed that section in the revised manuscript. 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. Author Response: Yes. But due to the prevalence of log-transforming dust data in climate science, we would like to stress that statement. 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. Author Response: We have moved this sentence to the discussion. Discussion of this point would be a (actually several) paper in itself and is not central to this paper. But we feel that it is an important point to make for the tipping point community, many of which will read this paper. 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. Author Response: Yes, we only show that the data are consistent with this hypothesis. No more can be done. We added a sentence to this effect. 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. Author Response: We added ". . ., assuming that the major underlying processes were constant over the last 800,000 years" to that phrase. P10 L21ff: The start of this paragraph makes the reader expect spectra averaged over the different cycles. Please consider rephrasing and more extensively introducing the Figure. Author Response: We have modified this paragraph as suggested. 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. Author Response: The value 0.25 was a typographic error, the value used was actually 0.35 as indicated in fig. 9 (not 10 as indicated in the text). The hypothesis here was that there were two regimes, each characterized by a different exponent each of which was estimated from the ensemble statistics. Given the hypothesis, the analysis only needed to estimate the scale at which the low frequency process exceeded the high frequency one. Therefore, we found the break point that minimized the RMS deviation from the bi-power law behaviour. We have added material explaining this better. 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. Author Response: We are dealing with a system with strongly nonclassical statistical characteristics: scaling and strongly non-Gaussian fluctuations. We do not have a precise stochastic model of the dynamics, the aim of this study is to yield a first, initial empirical characterization that could in the future provide the basis of a more precise model that would allow us to justify the estimation of uncertainty limits. 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. Author Response: Perhaps the problem is that the dashed lines were mistaken for uncertainty limits of the solid lines? We have added a phrase in the text to clarify this. There was also a reference two lines earlier to fig. 10 that should have been fig. 9. This correction may also help understand fig. 11. P11 L23: Please state why this exact time range was chosen. Author Response: As indicated in the text, the idea here was to estimate exponents from fixed ranges rather than ranges that varied depending on somewhat uncertain estimates of ïĄt'c. The range of time scales was chosen so that in most phases, most of the range of time scales was in the climate regime (H>0), hence the lower limit of ïĄĎt>500 year. The upper limit of ïĄĎt < 3000 years was chosen because at longer scales, the statistics were less reliable: for phases 12500 years longer, there are only 4 disjoint intervals available and for larger ïĄĎt there are fewer. The range choice was a compromise that aimed at quantifying systematic phase to phase changes (the solid lines connecting the points) as well as the cycle to cycle dispersion of the exponents at each phase (the error bars). P12 L10: There is nothing black in Figure 12, please correct. Author Response: Corrected with additional information. P12 L18-21: Consider moving this sentence to the beginning of the paragraph to make it easier for the reader to follow the results. Author Response: Moved as suggested 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. Author Response: We replaced the precise word "significantly" by the vaguer term "barely" which is adequate for our purpose here. It is true that the absence of a 41kyr cycle is no longer surprising, we

have modified the sentence accordingly. P13 L28-32: Is there any supporting evidence for the glacier variability? Author Response: We added the reference Sugden et al., 2009 and Garcia et al., 2018 on South American glacial 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. Author Response: Since dust is positive definite, a higher variability amplitude will produce a higher average. We added more explanation to this sentence: "Since the Argentinean continental shelf was still submerged at that moment and the outwash plains not yet fully extended, the higher dust emissions may have been due to a transformation in vegetation cover about 30 kyr after glacial inception, possibly accompanied by changes in glacial and periglacial processes in the Andes." 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). Author Response: We have expanded this paragraph, which now reads "The recuperation of vegetation cover would be more gradual, though, resulting in a saw-tooth shape of the dust spike that we do not observe in the data. Similarly, it has been suggested that rapid climate change in the Northern Hemisphere (e.g. Dansgaard/Oeschger events) would have synchronously changed the Southern Hemisphere atmospheric circulation and wind belts (Buizert et al., 2018; Markle et al., 2017). This could again have quickly changed the source or transport conditions, but would again have resulted in a saw-tooth shaped peak, either by steady regrowth of vegetation in the dust source areas, or as climate conditions in the north Atlantic gradually return to stadial (Pedro et al., 2018)." 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. Author Response: In this sentence we only address the low (compared to the dust record used here) resolution of existing paleotemperature timeseries, not the change in resolution. We changed "temperature reconstructions" to "Pleistocene temperature reconstructions" to clarify. P14 L30: The paper does not show that the data neither over-samples nor smoothes.

Please either add this to the paper or remove this statement. Author Response: We deleted that last sentence and changed the previous one to "..., we therefore took advantage of the unique EPICA Dome C dust flux dataset with 1 cm resolution measuring 320,000 cm, whose worst time resolution over the whole core is 25 years." 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. Author Response: We changed this sentence to "We addressed the task of statistically characterizing the cycles by primarily characterizing the phases' variability exponents H, C1, qD and amplitude A." P15 L25: Missing dot between "A" and "We" Author Response: Corrected P15 L 26f: How is the variability of the dust flux at Dome C connected to the "activity" of the Patagonian ice sheet. Please extent and clarify this argument. Author Response: We removed the mention to the Patagonian ice sheet and changed this sentence to "However, the low amplitude of dust variability during glacial inceptions indicates that vegetation cover and dust production processes did not significantly change until ∼30 kyr after glacial inception." Figures: 6b: The units for the axis are wrong. Author Response: Yes, thanks, we also fixed the far-right expression for the number distribution. We have now indicated the units in the caption. 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. Author Response: We responded to this suggestion above.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-171/cp-2018-171-AC3-supplement.pdf

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