**Anonymous Referee #1**

This paper presents statistical analyses of a dust record from the EPICA record to describe the temporal variability of the last 800 kyr.

General comments

I do not think that the analyses reported by the paper contain errors, although I did not try to replicate the results. It is difficult to make out the goal of the paper, which appears as a sequence of statistical analyses that the first author seems to have repeated in quite a few recent papers (listed in the manuscript and others). I am surprised that the authors do not cite the paper of Huybers and Curry (Nature, Links between annual, Milankovitch and continuum temperature variability, 2006) that already discussed such statistical analyses, albeit on other datasets.

*Au: That paper is now mentioned several times. However, our paper goes well beyond the Huybers paper both at the level of data quality and quantity as well as the types of statistical characterization that were made. These support a new phase by phase understanding of the scale by scale variability including intermittency of the last 8 glacial cycles.*

So, my feeling is that there is very little new understanding in the manuscript.

*Au: Our paper was indeed largely empirical, providing original characterizations of a unique climate data set. But it is unfair to condemn the paper as providing little understanding: understanding must be based on the highest quality data and on the most systematic, highest quality analysis of that data.*

*Thanks to our analysis new understandings must accommodate the following observations:*

*a) That the commonly used log transformation of dust fluxes does not significantly change the variability – whether it be the spectrum, or the nature of the extremes. Since the log dust flux is not a physically significant variable, we conclude that the transformation is not necessarily helpful beyond a visual aid in plots.*

*b) To our knowledge this is the first time that detailed comparisons between all of the last 8 glacial cycles has been made. We showed in detail that the cycles are statistically quite close to each other while being systematically different according to the phase of the cycle.*

*c) This is the first quantitative characterization of climate stability as a function of cycle and phase. It allows for a clarification idea of Holocene exceptionalism – at least with respect to this high latitude data set.*

Specific comments

The introduction should state clearly the scientific question that will be tackled in the manuscript (not just list scientific problems), and the conclusion should state how the obtained results served to solve the problem (or not). Without such a reorganization, it is very difficult to assess the importance of the paper. At present, the conclusion essentially paraphrases the results, and seems to depend on choices of parameters (like time interval discretization). The authors did something that they are the only ones to understand, to reach a conclusion that is very hard to exploit. I would appreciate that the methodologies used in the manuscript appear under a "Methodology" section. Most of the equations (so, what is done, or not) appear in the "Results" section. This makes the separation of what is new from what is supposed to be known rather tedious.

*Au: We have re-organized the paper as suggested.*

I see no assessment of uncertainty (on data or ice core dating) in the manuscript. Would that mean that the results would be insensitive to the chronology?

*Au: The uncertainty of our results is actually discussed at large. In addition to uncertainty estimates of statistical variables, we performed analyses both in time and in depth, thus providing one analysis that is indeed insensitive to chronology. In addition, we compare two different chronologies: one in time (based on the official dating) and one using nondimensional time; the fraction of a glacial cycle. The manuscript discusses the fact that our results are fairly robust with respect to this quite significant change. In the new iteration, we tried to make this more conspicuous.*

The authors use a dust flux reconstruction (I assume computed from a dust content, divided by time increments).

*Au: In essence, yes. The flux is calculated by multiplying the dust particle concentration with the accumulation rate.*

I am not extremely familiar with such Antarctic records, but a rapid bibliographic search reveals that similar data (dust, chemical species) in Greenland ice cores show that the logarithm of dust (and chemical species) are heavily correlated to isotopic data (Yiou, R., Fuhrer, K., Meeker, L. D., Jouzel, J., Johnsen, S., & Mayewski, P. A. (1997). Paleoclimatic variability inferred from the spectral analysis of Greenland and Antarctic ice core data. Journal of Geophysical Research: Oceans, 102(C12), 26441-26454; Mayewski, P. A., Meeker, L. D., Twickler, M. S., Whitlow, S., Yang, Q., Lyons,W. B., & Prentice, M. (1997). Major features and forcing of high latitude northern hemisphere atmospheric circulation using a 110,000 year long glaciochemical series. Journal of Geophysical Research: Oceans, 102(C12), 26345-26366; Fuhrer, K., Wolff, E. W., & Johnsen, S. J. (1999). Timescales for dust variability in the Greenland Ice Core Project (GRIP) ice core in the last 100,000 years. Journal of Geophysical Research: Atmospheres, 104(D24), 31043-31052.).

*Au: The correlation between dust flux and isotopic series depends greatly on the time scale at which the correlation is determined.* ***A systematic demonstration of this fact will be given in a new paper in preparation; it is beyond the scope of the present paper.***

So, why not consider the logarithm of dust flux?

*Au: We did consider dust flux but this was apparently missed by the referee. To make the point more strongly, we added a new plot of the probability distribution of the logarithms of the flux, further confirming that the transformation only results in minor changes in the statistics. We also added a new spike plot of the log transformed data showing that the qualitative nature of the dust variability is not changed. The spectrum, intermittency and extremes are nearly unaffected.* ***We cannot avoid dealing with the extreme spikiness of the dust flux. Fortunately, unlike standard approaches, our methods are ideally suited for such spiky analyses.***

The authors repeat several times that dust flux is not Gaussian. This is rather trivial, given that it always have positive values.

*Au: Yes, except that we were discussing the* ***increments*** *of the process being non-Gaussian and this is less trivial. In addition we showed that the log transformation does not render the process Gaussian either.*

Why should fitting a Gaussian process to dust be a reasonable null hypothesis to reject? Dust flux is generally modeled by a transport equation, the solutions of which are like multiplicative noise.

*Au: Yes, exactly! Our scaling methodology is based on an understanding of multiplicative turbulent cascades that was developed over the last 35 years (multifractals). The generic result is multiscaling with power law extremes, exactly as found here. We further underscore this point in the new iteration.*

The climate interpretation of the results obtained by the deployment of this arsenal also seems to be a problem. I expect that such an interpretation is natural when considering a variable of the system. The authors use an observable (dust flux) that might be obtained by a complex transformation of a driving variable of the climate system (like temperature, or temperature gradients, or pressure variations). To what extent what is learnt from the analyses does not just reflect something on the complex transformation of an underlying driving variable, rather than the dynamics of the variable itself? Since the authors never discuss the physical meaning of the data they analyze and climate variations (or only in a superficial way), I could doubt that any physical interpretation can be deduced from the analyses.

*Au: We saw that a rather drastic transformation of the flux - a log transformation – does not fundamentally alter its extreme statistical characteristics. This is perhaps not surprising because there are wide scale ranges over which the fluxes are scale invariant and one expects that this reflects an underlying symmetry of the dynamics (no matter what the relation of the latter is to the fluxes). Similarly, the stability/instability conclusions are likely to be robust so that our analyses therefore give a robust indicators of scaling and the limits of the scaling regimes, and this independently of the link between dust fluxes and more conventional climate parameters. But we agree that the* *climatic interpretation was hidden within the technical jargon. We tried to clarify this aspect in the revised version.*

<u>Minor comments</u>
I have too many comments on the manuscript. I will limit them to 10.

p. 1, l. 10: the first sentence of the abstract does not make any sense to me (see the paper of Huybers & Curry, 2006, and many others).

*Au: We removed the sentence.*

p. 1, l. 19: at this point I would need to know what foreground and background processes are.

*Au: We removed this reference.*

p. 2, l. 1-8: all this seems to be a personal opinion.
What is the use of Figure 2?

*Au: It situates the dust variability in the larger scheme of temperature and temperature proxy variability. It shows that it is quantitatively quite similar.*

What does "fluctuation tend to persist because they are unstable" mean? Where is the peak in Fig. 2?

What is $\tau_c$ ? It is never defined.

*Au: $\tau_c$ is the time scale at which the scaling regime switches from the "macroweather" to the "climate" regime. It was defined in line 24, p1. The definition was repeated on line 33 of p2 and on line 8 of p8 and on line 3 of p10 and on line 8 of p12. We repeated the definition many times because of its importance to the paper. We do not understand how it was missed.*

I do not think that Petit et al. computed any $\tau_c$. Their statement on agriculture was a perspective, not a result of the paper itself!

*Au: Exactly our point! They "eyeballed" their series whereas we have an objective quantification. Nevertheless we removed this sentence.*

p. 3, l. 2: The notion of stability/instability was different in the papers of Petit et al. and Berner et al. The paper of Berner et al. discusses Subpolar North Atlantic ocean dynamics instabilities, which are not accessible by Antarctic ice cores.

*Au: Our point was that in both cases, conclusions were drawn with implications about Holocene stability. But we removed that paragraph.*

p. 3, l. 5: Antarctic dust content is necessarily connected to the atmospheric circulation. Dust records in Arctic ice core reflect the atmospheric circulation. Why not compare dust records of both hemispheres? What is the precise question that the authors want to address?

*Au: The comparison between hemispheres is beyond the scope of this paper. We attempt to clarify the nature of the glacial cycles and quantify their variability as functions of the phases of the cycle.*

p. 4, l. 26: What is a statistical symmetry?

*Au: It is an invariance of some aspect of the statistics under some transformation. Here, an invariance of statistical exponents under time dilations and contractions (temporal scale invariance). This is now mentioned in the new iteration.*

p. 8, l. 18: I do not understand this equation. If $s>1$, then any positive power of s gives a number that is larger than one. How can a probability be larger than one?

*Au: There was a typo in the exponent, there should have been a minus sign before the qD. We apologize. This has been fixed.*

p. 9, l. 5: What do the authors mean by "extremes much occur too frequently"? How is this related to the analysis of the paper? The paragraphs between l 3-13 are incomprehensible for someone else than the authors.

*Au: We meant to say that "extremes occur much too frequently for the process to be Gaussian". This conclusion implies that nonstandard theoretical frameworks are required for understanding dust fluxes. We propose multifractal scaling as such a framework.*

p. 9, l.14-onward: In Taleb's book, black swans do not necessarily refer to Lévy flights, but to events whose features cannot be anticipated (like the war in Lebanon, the success of Harry Potter, etc.). Gray swans (Taleb's spelling) are those events for which some sort of anticipation can be provided. Incidentally, Taleb also writes on confirmation bias, which is one of the flaws that I tried to outline when the authors interpret their analyses (e.g. p. 9, l. 30). Therefore, the overall understanding of the cited literature could be improved.

*Au: Yes, we know and tried to make this distinction in the text. Yet, the term "black swan" is often used in place of the rarely used "grey swan" expression, so we used "black swan" even if it is not quite the original usage. We removed this passage to avoid confusion.*