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Interactive comment on "High resolution EPICA ice core dust fluxes: intermittency, extremes and Holocene stability" *by* Shaun Lovejoy and Fabrice Lambert

Anonymous Referee #1

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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 I am surprised that the authors do not cite the paper of Huybers and Curry (Nature, Links between annual, Milankovitch and continuum temperature variability, 2006) that already discussed such

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Discussion paper

statistical analyses, albeit on other datasets. So, my feeling is that there is very little new understanding in the manuscript.

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.

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?

The authors use a dust flux reconstruction (I assume computed from a dust content, divided by time increments). 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 iceâĂŘcore 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 highâĂŘlatitude northern hemisphere atmospheric circulation using a 110,000âĂŘyearâĂŘlong 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.). So, why not consider the logarithm of dust flux?

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

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.

Minor comments

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).

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

p. 2, l. 1-8: all this seems to be a personal opinion.

What is the use of Figure 2? What does “fluctuation tend to persist because they are unstable" mean? Where is the peak in Fig. 2? What is τ_c? It is never defined. I do not think that Petit et al. computed any τ_c. Their statement on agriculture was a perspective, not a result of the paper itself!

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.

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?

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

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?

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.

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.

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