Anonymous Referee #2

GENERAL COMMENTS

1. The authors apply interesting and novel concepts to a high-resolution record, so there should be a considerable potential for new insights.

Au: Thank you for your appreciation.

2. However, the structure of the paper is too chaotic (almost intermittent) with data, methods, results and discussion randomly mixed. This renders the result barely readable and obscures potential interesting results. The introduction and discussion is currently myopic.

Au: We agree and have restructured the paper and rewritten various sections.

3. There are inconsistencies in the results, with the Holocene transition time Tau_C identified at 4, 3-5 and 7.9 kyrs at different points in the paper. Without a substantial restructuring this paper is not acceptable.

Au: The problem is that there were 8 phases and 8 cycles yielding 64 different τ_c *values. Each was estimated in three different ways (two different chronologies and two different estimation techniques). Various averages over phases and cycles were also given. We have rewritten some sections to make it more consistent.*

DETAILED COMMENTS

p1l12/13 please indicate that you are using the definitions you provided in earlier work. These are not common concepts in (palaeo)climatology.

Au: Done.

p1l20 what are the hypotheses underlying these two analyses with fixed/variable cycle durations?

Au: These are described in the text. The segment definition is based on the spectrum with peak near 100kyrs, the cycle definition is based on the glacial maxima.

p1l25 **_c**=4kyrs

Au: τ_c *is not exactly 4kyrs which is why we use the ≈ sign.*

p1l30–p2l4 A sharp peak in a spectrum due to a periodic component in the signal would be blurred and broadened by temporal uncertainty – which is, conceivably, larger in the earlier parts of the Pleistocene, the "41kyr world", for which data is based off marine records. The ratio of age uncertainty to period length is less favourable then, and many records are orbitally tuned (although possibly not using the analyzed signal).

Au: The Huybers [Huybers, 2007] *reconstruction avoids orbital tuning yet shows a strong spectral peak at (41kyrs)$^{-1}$, but only in the period before 800kyrs. We have removed this section as it was not absolutely relevant to the scope of the paper.*

p2l7 A brief definition of macroweather vs. climate regimes would be helpful here.

Au: Done.

p2l16020 Mitchell's drawn spectrum was conceptual, and we know that it isn't accurate from earlier work (Huybers & Curry, 2006; Laepple & Huybers 2013).

Au: We agree. The point is precisely that because the inaccuracy was quantitatively astronomical, that Mitchell's conceptual framework is untenable. We have removed the historic context to simplify the paper.

p2l34/p3l1 This lacks recent literature. Interglacial vs. Glacial period climate scaling and variability have been repeatedly compared in the literature. Whereas Ditlevsen et al. (1996) and Shao & Ditlevsen (2016) investigated the scaling properties for the different climate periods and found strong differences, Rehfeld et al (2018) suggested that on millennial scale Glacial vs Holocene variability is approximately 4:1.

Au: Thank you. We have added a paragraph with these references.

p3l4-9 This implies that you are analyzing dust as a temperature proxy, but the two signals scale differently. Dust concentrations are non-negative and non-Gaussian by definition.

Au: The dust and temperature have scale dependent links; we will investigate this in detail in a future publication, but it is not true that dust is a temperature proxy. However, scaling regimes and the transition scale between a stable and unstable regimes are fairly fundamental characteristics and should be observable in either record.

p4l1 How were these spectral analyses performed? Why are there no confidence intervals?

Au: The spectra were analyzed using FFT with standard Hanning windows with the smooth (red and blue) curves obtained by averaging over logarithmically spaced bins as indicated in the text. For the Green curve, we have followed the usual practice in turbulence which is to display the full spectrum. In this case, the uncertainty is directly judged by the spread around the mean.

It would help if fluctuation analysis and spectral analyses were performed and displayed for the same datasets, given that most readers would be familiar with the latter.

Au: The fluctuation analyses of the same data were indeed shown in fig. 7 (Figure 5 in the new version).

p4l14/15 Presumably these estimates (like most others in this paper) have some uncertainty. Please state them!

Au: As usual, the uncertainty depends on your basic model. Here, for the fraction in the background, various different models were compared so that the reader can judge. Later, the difficulty is that the only way to satisfactorily judge the parameter uncertainties is to have a well defined stochastic model of the process. That being said, standard regression uncertainties could be used as uncertainty proxies for many of the exponents.

Unfortunately the real source of uncertainty is not the traditional statistical variation about a regression fit (e.g. the slope of a straight line on a log-log plot), it is rather the difficulty of objectively choosing the scale range over which the regression exponents are estimated. For example, in one of the methods for estimating τ_c*, an objective method for determining the transition between two scaling regimes was used. But this only works if the high and low frequency exponents are fixed beforehand; a kind of "bootstrap". In the end, we estimated the uncertainty by comparing three different methods (fig. 10) and by comparing estimates from different cycles (fig. 12). All told a huge effort was made to quantify uncertainty.*

p5l22 : Definitions belong to the methods, not the results. It would benefit the paper – and justify it – if methods, results and discussion were separated. Then the authors could devote a couple of paragraphs to the actual discussion of the processes and dynamics suggested by their results - such as the progression of deserts during Glacials that could be one of the reasons for the larger variance mid-cycle - which are lacking.

Au: We agree. We have re-organized the paper to separate better methods, results, discussion.

p6l26 To bear in mind: Mitchell draws a spectrum for temperature (conceptually), but data-based estimates have to rely on proxies for temperature, which potentially nonlinearly transforms the original processes.

Au: Yes, But Mitchell's spectrum still implied that successive million year temperature averages differed by microKelvins. Whereas the usual paleo calibrations imply differences of about 1 Kelvin. In our opinion, the latter is more plausible.

p6l4 Haar fluctuations and intermittency should be introduced in a methods section.

Au: Done.

p7l6/7 Please add a statistical test, considering age uncertainty, uncertainty in the transfer function and measurement noise. Otherwise robustness of the results cannot be judged.

Au: Again, standard statistical tests require a stochastic model of the process. We are attempting a kind of a bootstrap whereby a statistical characterization of the data is given and uncertainties judged by a series of measures including cycle to cycle comparisons. The aim is indeed to develop a stochastic model that could reproduce the dust series as a single realization of stochastic process with well defined parameters. In that case, the uncertainties are measured with respect to the model. We are not quite there yet and so have to judge the uncertainty by a series of less precise alternatives, which give us a good idea of the uncertainty range, though.

p9 Dust concentrations cannot be Gaussian, as they are counted variables and by definition positive definite.

Au: *Actually the claim was that the* ***process*** *might be considered Gaussian - meaning that the increments (denoted here by* ΔF*) - might be Gaussian or approximately Gaussian variables. Although it is indeed a terrible model for dust, it cannot be trivially rejected simply on the basis of the observation that the signal itself is positive definite over a range.*

p10l18 Holocene _**c**=7.9kyrs.

Au: Thanks.

p12l22 Holocene _**c**=3-5kyrs.

Au: Thanks.

p13l1-3 Maybe this early analysis can be progressed to an actual robust analysis of this dataset. Dataset Is this the dataset used? Please provide links to the versions and/or where the data is available. https://doi.pangaea.de/10.1594/PANGAEA.779311

Au: Yes, we also hope that this is the beginning of new approaches to analyzing dust fluxes.

Figure 2 presumably the hourly temperatures from Landers Wyoming and the daily temperatures at 75 degrees North were not measured by the authors, could you give the original references, please?

Au: The Landers series was from data from the US climatalogical data network and the

75°N data were from the Twentieth Century Reanalysis. Full references and discussion can be found in: Lovejoy, S., and D. Schertzer (2013), The Weather and Climate: Emergent Laws and Multifractal Cascades, 496 pp., Cambridge University Press, Cambridge.

Figure 3b The axes here are unreadable. Rather than show the obvious (Gaussian assumption makes no sense for dust fluxes), why not consider nonparametric confidence levels, or show at least the log of the dust flux).

Au: We have redone the figure with higher contrast axes/fonts.
In fig. 3c (added) we show the same plots after the log transformation. The spikes are less pronounced but are still very far from being Gaussian. We also performed log-log transformations. These resulted in extremes much further from Gaussian at the small scales, but closer to Gaussian at the largest scales. Finally, we added a new probability distribution for the log transformed data (Fig 6b) that showed that the extremes were still power law with low exponents q_D: questioning the general habit of log transformation of dust fluxes.

As for nonparametric confidence levels, the spike plots already give confidence intervals for the Gaussian, allowing it to be rejected with high levels of confidence. Adding the corresponding plot for the log transformed data would similarly reject the log-Gaussian hypothesis. Our distributions indicate that the power law tail hypothesis would not be rejected, but quantifying this is nontrivial because the theory doesn't indicate the probability level at which the tail is expected to be a power law.

Figure 4 The decrease in power towards the lowest frequencies (>400,000, beta=-2) may well be an artifact: By construction, periods longer than the time-series length divided by two cannot be interpreted, and rules of thumb/good practice is to stick to 1/3rd of the record length. For this 800,000 year-record this would mean that the spectrum could be considered estimable up to timescales of _1/266,000 years. It would further be good practice to subtract at least a linear trend (Chatfield 2016).

Au: Actually even if we only keep the $\omega = 3$ and higher frequencies as the referee suggests, our smooth line is still a pretty good fit to the spectrum. More information can be gleaned about the lower frequencies from fig. 7 that shows that the RMS fluctuations fall off quickly, bounded by $\xi(2)/2 = -1$ (i.e. the true $\xi(2)<-2$). Since 1+ $\xi(2)$, this shows that $\beta<-1$. This analysis is more robust at low frequencies than the spectrum and at least demonstrates that the low frequencies do decay at the lower frequencies.
Figure 10 How can the Holocene, being 11,700 years long, have a transition time scale_τ_c of 7,900 years?

Au: We used a data segment of 12,500 years and then calculated two fluctuations for each of the logarithmically spaced scales longer than 6250 years. One of these fluctuations starts at the beginning of the record, the other ends at 12500 years. These two estimates do overlap and thus are noisy. However there are several different scales that are considered and this adds some extra information.

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