Point-by-point Response to Reviewers

Reviewer 1 (Michel Crucifix)

The present manuscript has undergone a first review round in Climate of the Past. Previous reviews had not formulated strong objections about the statistical treatment of the data, but were worried about poor presentation and lack of focus. The manuscript was rejected and resubmitted, which unfortunately resulted in depriving the current reviewers from the benefits of a point-by-point rebuttal to the first review. Upon inspection, most figures are the same, key equations are unchanged, but more room is given to the interpretation of the results, and the abstract is more informative and, to my opinion, better written.

Given these improvements, and given the fact that there is no major objection to the interest of the statistical approach (see however specific comments below), the study should be published. This said, the authors may want to seize the opportunity of this review to clarify or perhaps improve a few zones of discomfort that, I think, has somewhat hindered the reception of their recent works.

First, something needs to be done about graphics. There are many problems. To cite but a few: vertical axes with different tick marks have been superimposed (Figures 4 and 5), labels show aberrant disproportions caused by vertical stretching (many figures), numbers alignment is sometimes inconsistent (horizontal axis on Figure 6), horizontal ticks seem to have been added by hand, with shadows (Figure 5), or inconsistently aligned (Figure 2), and colour legends are missing (Figure 9).

Author Response: Thank you for the constructive comments. We have improved the figures according to your suggestions.

Second, it would be useful to have some estimates of the variance of estimators. Assuming a stationary signal, and given the amount of data at hand, which variance do we expect for the estimate of τ_c, or H, or even C_1 ? Figure 10 seems indeed to make it clear that there is a significant difference between τ_c of phase 1 and 8; trends on Fig. 12 are much less obvious and having clearer ideas as to whether variations can be attributed to statistical sampling, and to non-stationarity (or at least, whether they are a sign that null-hypothesis of stationarity should be rejected), would be helpful.

Author Response: It is actually not trivial to talk about the "variance" of the exponents, as the exponents themselves are only really meaningful in a stochastic framework: there is not a deterministic parametric model that underlies the distribution. As explained in a new paragraph (in the discussion section), to go further would require the elaboration of a more precise stochastic model that could predict the expected variation of exponents from one realization to another. We have rephrased the text to talk about dispersion instead. We also expanded the discussion of Figure 12 as you suggest.

I leave it to the editor whether addressing these two comments in full is a hard request, especially the second one. This brings me to the point by point comments:

- *p. 2: "two extremes" : They are not "extremes". Perhaps write again: between daily and orbital time scales...*

Author Response: We changed "extremes" to "scales"

• *p. 2 l. 30: "temperature record" → "deuterium record" (especially for readers of Climate of the Past, they know that deuterium concentration and temperature are not the same)*

Author Response: Corrected

• *p. 2 l. 34: "The analysis of the dust record" → "The dust record"*

Author Response: Changed to "the dust data used here"

• *p. 3, l. 10: "can themselves be power laws" : be more specific about the conditions that generate power laws*

Author Response: Scale invariance of dynamics in time and in space lead to space-time statistics that are characterized by power laws. In this sentence we are referring to something rather different, to power laws in the tails of probability distributions. However, it turns out that several mechanisms have been proposed that link the two so that we could largely expect power law probability tails to emerge from space-time scaling dynamical processes.

• *p. 3, l. 18: "In particular, etc." this is not a sentence.*

Author Response: Corrected

• *p. 5, l. 8: "Since K(1)=0" add "by definition".*

Author Response: Changed to "Since K(1) = 0 is a basic property,…"

• *p. 5, l. 8: "While the mean to RMS ratio is intuitive" : the sentence is unclear. In what sense is C1 a 'mean to RMS' ?*

Author Response: We have clarified the sentence: "While the mean to RMS ratio is an intuitive statistic, it does not give a direct estimate of C_1:…"

• *p. 5 Eq(9) : use standard notation limΔq→0.*

Author Response: We have modified the equation and text accordingly.

• *p. 6, l. 13: full sentence needed after a semicolon ";"*

Author Response: Corrected

• *p. 8, l. 13: "The plot graphically conterposes two views of variability". I suspect you mean Figure 4, but yet what is meant by this sentence is still not clear to me.*

Author Response: Changed to "The variability shown in Figure 4 can be interpreted broadly or in detail."

• *p. 8, l. 20: 100 ka is not a Milankovitch frequency (Milankovitch was unaware of 100-ka cycles, he focused on 40-ka cycles).*

Author Response: Changed to "orbital" throughout the text.

• *p. 8, overall, I found the lines 17-30 difficult to read. Consider whether it is possible to express the same message more concisely.*

Author Response: We tightened up the paragraph, it is shorter, hopefully it is clearer.

– p. 9, l. 5 "possibly (but not obviously) scaling". In order to be possible but not obvious, a strict definition of what scaling means is needed.

Author Response: We eliminate the "(but not obviously)". In this context, "scaling" means a power law behavior. Due to the shortness of the range and the poor statistics at theses long time scales, the situation is not clear.

- p. 10, l. 22: we compare, without 's'

Author Response: Corrected

- p. 11, l. 13: "p value of 0.12". See the above comment. We do not really know whether the distribution is normal. Do we know anything about a theoretical distribution, or perhaps one that could be simulated ?

Author Response: The problem is that we don't have a detailed stochastic model. Without one, we don't know how much variability is "normal" or "typical". Using the standard deviation is agnostic in the sense that it is simply a standard nonparametric characterization of the spread. The only place where we go one step further is to assign a p value = 0.12. We have added a comment on this.

- p. 13, l. 9: "low internal feedback" → "low response"

Author Response: Corrected