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Interactive comment on "The Earth's climate system recurrent & multi-scale lagged responses: empirical law, evidence, consequent solar explanation of recent CO_2 increases & preliminary analysis" *by* J. Sánchez-Sesma

Anonymous Referee #1

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This paper presents a statistical analysis of several environmental and solar proxies. The objective is to outline time lags between terrestrial environmental proxies and solar variability. The paper suffers from a large number of flaws, including in the presentation that prevent me from recommending its publication.

1. Major comments

The presentation is not adequate for a scientific paper. The author cites in extenso various excerpts from publications, as if they were biblical quotations. Most of the citations seem picked out of their original context and are probably not relevant to the precise analyses carried in the paper. The overall writing yields too much confirmation bias to be adequate for a scientific paper: the author expresses (in a rather confusing way) many general hypotheses, the tests that are made are not very convincing (I will treat this after), and he cites the conjectures formulated by others (and in other contexts) to “confirm” his own hypotheses. Handwaving can be sometimes useful if alternative explanations are formulated. Otherwise, it is rather misleading at best.

The introduction mixes time scales (from millions of years to centennial variability) and tries to convey the impression that of something is valid on a multi-millennial time scale, then it has a form of validity on longer and shorter time scales. The analyses are presented in one-paragraph subsections that just state that a figure shows the results or supports a discussion. The discussion brings other results and further analyses, which are barely enlightened by the appendix (the so-called power laws).

The author claims that his temperature forecast for the 21st century is lower than those of (I guess) CMIP5 simulations. None of the figures/results support such a claim. The results could never support it because the regression is based on millennial averages, and the ongoing climate change is much shorter time scales.

I think that good scientific papers should only marginally deviate from the following structure: (i) statement of the scientific problem with quantitative elements, (ii) presentation of hypotheses, data and methods, (iii) description of results, (iv) discussion, (v) conclusions. Arguments that are not supported by the results of the paper are red herrings. The present manuscript is hence far from the ideal that is recommended by most journals (including ESD).

The data are not presented in a proper way and are used outside of their context. The carbonate ion record is not a proxy for atmospheric CO2. I wonder if the author read the paper of Yu et al. (2014), who focused their discussion on glacial to interglacial changes, which is hardly the subject of the manuscript. There are less than 10 data points between 11 kyr and 0 kyr BP (the common period of all proxy records of the paper). The detrending of the Congo River data does things that would make most paleoclimatologists wince, because what Weijer et al. (2007) discuss is precisely the trend, the residual being noise from the reconstruction method. The author first states that the Congo temperature is independent of the ocean circulation (p. 6, l. 5), then makes the hypothesis that they are dependent (p. 6, l. 25). Please be consistent. The Congo river data comes from a marine sediment core (Weijer et al. 2007), and the authors of that paper are very clear on the uncertainties of the continental temperature reconstruction. Why not use other high-resolution continuous records of the Holocene?

The simple fact that all the proxies have different time resolutions and variable time samplings precludes the type of analysis that the author wants to make. The author states that he avoids any analysis of uncertainty (e.g. due to dating uncertainties). But he cannot ignore that there are large dating uncertainties in continental proxies and ice core records. Such uncertainties might dominate in the estimation of the parameters of the three models. A "preliminary" analysis is no excuse for not investigating uncertainties, given the strong conclusions that he draws from the analyses. And I cannot encourage the editors of a journal whose impact factor exceeds 4 to publish a paper that does not carry analyses of uncertainty, when the whole topic is about statistical models.

The author has the strange habit of not defining acronyms the first time they are used (e.g. GOC, AMOC, SA?). This is particularly frustrating in the three model equations. What is SC? I understand that P is a proxy variable. Is it one of the datasets shown in Figure 1? What is the magnitude of the error? The formulation of the three models indicates a monotonic trend (the beta term). None of the figures yield such a trend. The value of the parameters is never discussed. The third model is some sort of lagged auto-regressive process. If alpha>=1, beta!=0, gamma!=0, then this model always diverges. This is not very useful.

The results section is based on a further methodological section, with an appendix that brings essential information. Appendixes are there to enlighten some technical issues,

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Discussion paper

not to carry essential results that are discussed throughout the paper.

What is called a power law has nothing to do with power laws in probabilities or in physics. Eq. (A1) does not make sense. The right hand side no longer depends on t, which is the function argument. What is the relation between this obscure appendix and the three models, which are the gist of the paper?

There are many other problems that could also suffice to reject the paper. Let me conclude this section by a last one. The author makes a “forecast” of atmospheric CO2 for the next century, based on information of the last millennia. It does not seem to have occurred to the author that there are several ways of tracing the origin of the atmospheric CO2. One of the ways is to determine the ratio of stable isotopes in the CO2. It has been known for many years that such measurements point to a fossil fuel origin, not natural variability (or forced by natural causes, like solar activity). There is no chance that atmospheric CO2 decreases that fast after 2100. This simple observation (that can be obtained by reading papers or reviews, such as the various IPCC reports) simply invalidates the model presented by the author to draw conclusions about the future. Basic physics dismisses the whole statistical analysis.

2. Less crucial comments

The bibliographic search of the author seems rather incomplete. He claims that no one looked at the effect of interstellar dust on climate. There were quite a few papers in the 1990’s on the subject. They are now barely cited because they did not survive the shock of new observations. The effect of cosmic rays on the stratosphere has been discussed for decades (this is the origin of the cosmogenic isotopes that can be measured at the surface of the Earth).

The author should do a more careful search on the effects of volcanoes on climate, which depend on the time scale (from months to millennia). Since he discusses time scales overs millions of year, he should be aware that volcanoes also expel greenhouse gases like CO2, which eventually warm the troposphere.

The suggestion that a ∼9500 year cycle is a harmonic of an excentricity cycle and that it has any link with decadal signals is rather ludicrous. There are no systems (ideal or real, linear or nonlinear) that have such high order measurable harmonics. This whole discussion sounds like numerology or a confirmation bias (I recommend that the author reads the book of D. Kahneman, “Thinking, Fast and Slow”, Farrar, Straus and Giroux, New York, 2011).

A true confirmation of the model for prediction would use observed temperature data for the recent period. The author would realize that his model does not predict anything very useful for the 21st century.

The latest IPCC report (in 2013) no longer uses A1 or A2 scenarios, but Representative Concentration Pathway (RCP) scenarios.

The time axes of the figures should have the same format, in order to facilitate the reading and appreciation of results.

The author makes strong statements about what the scientific community should do (i.e. study the climate as an “open system”, etc.). I would have appreciated that he makes a more careful bibliographic search. Most of what he emphatically advocates has already been published.

I will stop my review here. I think that each paragraph of the manuscript is debatable, but I afraid that I do not have enough time to dwell on the details.

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