

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-69

Anonymous Referee #2

Referee comment on "Synchronous Northern and Southern Hemisphere response of the westerly wind belt to solar forcing" by Nathalie Van der Putten et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-69-RC2>, 2021

I carefully read this manuscript by Van der Putten and colleagues entitled: "Synchronous Northern and southern Hemisphere response of the westerly wind belt to solar forcing". I feel that this is a much needed manuscript. It presents sound paleoecological and paleoclimatological interpretations of a peat core located on a well-positioned island that offers a plentiful of possibilities for the reconstruction solar-driven anomalies in the vigor and direction of westerlies in the southern hemisphere. I am not a specialist of peat chronologies, but I believe that my comments may be shared by others who are not peat specialists and that wish to better appreciate the results found in this manuscript. I therefore suggest to accept this manuscript providing that minor revisions are made.

First, the title does not indicate that this a manuscript about paleoecology, paleoclimatology or any other paleoscience. The title is misleading and could well refer to a "modelling-only" manuscript about pure, dynamical interactions that exist between solar forcing variability and the behaviour of westerlies in both hemispheres. The title is thus too vague, and probably too general to really describe what the manuscript is really about. Please provide a way to clarify either the timescale, the methods, or the type of data directly in the title and clearly specify that this is a manuscript is based on both model and data in the field paleoclimatology or paleoecology.

Second, The introduction is mute about the necessity to base such a research on multiple proxies. The authors spend quite a lot of time defining the necessity to precise the chronological framework to investigate, from a paleo-perspective (L76), sun-climate relationships. However, when we keep reading the manuscript, we discover that the

authors use a vast array of proxies, each with its own sensitivity, that include XRF analysis, magnetic susceptibility, geochemical tracers, in addition to more standard paleoecological (macrofossil and diatom) analysis. In my opinion, the authors should insist more, in the introduction, to precise the need to consider so many different proxies. What is the originality with such a multi-proxy approach?, what is the innovation compared to previous proxy-based studies?, is there an assumption / hypothesis that makes these proxies more sensitive to wind and solar-driven dynamics than those used in previous studies or this just standard analysis; in my opinion it is not the case? Without such additions, the use (and strengths) of multiple proxies falls under the radar of the reader, and the strengths originating from the multi-proxy approach is significantly devaluated.

Section 3.7. Modelling ... of what?. Section 3.7 is too short and not clear about the objectives pursued by the modelling. It is necessary to precise the objectives and hypothesis pursued by the modelling experiments, along with the limitations of such an approach.

L200. Change from low to high MS values is interpreted as an increased influx of minerogenic material due to stronger winds. How can the authors be so confident about such an explanation? Wouldn't a period of drought and / or reduction in the density of vegetation cover in the vicinity of the site also result in an enhanced minerogenic input? Please provide additional details that will reinforce your interpretations.

Section 4.3. Comparison between 11-yr solar cycles in the 'modern' period and the Homeric Minimum falls a bit short of argumentation and description. First, how do these two timescales compare in terms of W/m² reduction attributable to solar forcing remains unexplored. I fear that comparing 11 year cycles with much longer change in irradiance is a bit like comparing apples and oranges, both in terms of ocean-atmosphere response, and also in terms lag, magnitude and persistence of response in the hemispheric climate as a whole (that includes retroactions with sea ice, thermohaline circulation coupling with pressure fields and wind dynamics etc.). Also, it is not clear to me how the authors can "isolate" the spatial patterns of SST variability in the ERA-20C reanalysis, as solar forcing, if any, is intermingled with many other sources of internal and external variability. Attribution to solar forcing here is risky, and not well supported by analysis. Please improve section 4.3 to document how the attribution (to solar forcing) is made in the ERA-20C which is, contrarily to the 1200 year run, not uniquely forced by TSI.