## Final Author Response Van der Putten et al., cp-2021-69

We take the opportunity offered to us to respond to the review reports of our paper.

Although our paper is rejected for further publication we have to address some of the comments by Referee 1 and 3 that we consider unjustified, as they undermine our scientific integrity without any basis. We believe that a significant number of comments are based on misunderstandings.

Before going into detail we want to summarise our main points:

- Referee 1 reproaches us of "*poor scientific practice*" as we are "*focusing on a single mechanism from the start without evaluating other possibilities*". We agree that in this paper we test the hypothesis of a Sun-climate link, based on chronologically well constrained data together with Reanalysis and Model data. How can testing a hypothesis be considered poor scientific practice? However, we agree that it could be stated more clearly that testing the Sun-climate link is the focus of this paper and that other possible forcing factors could be better discussed.

- Referee 1 and 3 accuse us of "*republishing old data*" and again, "*poor scientific practice*". Indeed, we have revisited a core from an exceptional site at 46°S in the southern Indian Ocean, for which part of the data were published in Van der Putten et al. (2008). However, at that time we could only speculate about solar forcing as our age-depth model was far from accurate enough. For our current paper we obtained a **high resolution chronology**, absolutely necessary for testing a Sun-climate link, together with **additional proxy-data** around the change of interest and supported by **Reanalysis and Model data**, to test our **hypothesis of a solar forced change in the Westerly wind belt**. Interestingly, in the reports of Referees 1 and 3 the words "Reanalysis" and "Model data" are not even mentioned.

- Referee 3 also states that our manuscript "*presents several conceptual and scientific flaws*" which in our opinion are based on misunderstandings by the referee on our proxy-data interpretation as we elucidate below. It seems that Referee 3 thinks that we present the results of a (atmospheric) dust record from a (ombrotrophic) peat bog which is not the case at all for our study as explained below.

Last but not least, we want to highlight that Referee 1 and 2 conclude that our data as well as our interpretations are sound. However, we do agree with Referee 2 that we have to "*provide additional details on our proxy data that will reinforce our interpretations*". This would probably have avoided certain comments from Referee 3.

## Response to anonymous Referee 2

We acknowledge R2's comments and suggestions. We will change the title to better reflect the content of the paper and we will elaborate on our proxy-methods in more detail, as this is necessary for a wider readership.

R2 shows concerns with the Reanalysis results of our study: "*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 dymamics 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."*

We agree with R2 that using the 11-year solar cycle as an analogue to Grand Solar Minima is imperfect and born out of the constraints of the available observations. However, while the oceanic response may be different on longer timescales, the atmospheric (top-down) response does not need to be, since already 11 years is longer than the memory in the atmosphere. More importantly, the lack of a good analogue in the observational record is precisely why we consulted the single-forcing model run. This allows us to overcome the limitations of a short observational period, and demonstrate that the atmospheric response we isolate from the Reanalysis is qualitatively similar to what we obtain for a centennial solar minimum in a state-of-the art climate model that includes the relevant physics. Hence, we believe these two independent results from Reanalysis and Model support each other.

We also agree with R2 that the response to solar forcing is intermingled with all forced and unforced components of the climate system. We want to point out, that our approach to i) exclude years of known abrupt forcing (i.e., volcanoes) and ii) pool the data into composites (or populations) according to the phase of the solar cycle and compare those using classical statistics is an established method (e.g., Ineson *et al.*, 2011; Thieblemont *et al.*, 2015; Woollings *et al.*, 2010). Importantly, this method accounts for the valid concerns of R2: Climate variability, internal or external, not forced by solar variability will contribute to increasing the scatter within each population and thus, lower the significance of a given difference between the populations (Sol. Min vs Sol. Max).

## References


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