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 1

We thank Referee 1 (R1) for the comments and suggestions on our manuscript. Below we elaborate on the main points raised by R1.

R1 states that "*our data clearly show a shift from drier to wetter conditions at 2.8 cal kyr BP*" and that "*the interpretation of the data in terms of shift in the southern westerlies is justified*". Moreover, R1 states that "*the external forcing seems to be justified*" and "*the synchronicity between this record and other records from the NH is a valid argument*".

We appreciate that R1 acknowledged the quality of our data/record, as well as the interpretation of the data as an intensification of the SHW over the Crozet archipelago, shortly after 2.8 cal kyr BP.

However, R1 (& R3, see below), accuses us of *"poor scientific practice"* as we are *"focusing on a single mechanism from the start without evaluating other possibilities"*.

In this paper we test the hypothesis of a Sun-Climate link, a hypothesis that we cannot reject, based on our chronologically well constrained data combined with Reanalysis and Model data for testing the hypothesised forcing mechanism. How can testing a hypothesis be considered poor scientific practice? We do however agree that it should be more clearly stated in the paper that we are testing the hypothesis of a synchronous equatorward shift in both Hemispheres at the onset of the Homeric minimum.

R1 also reproaches us of "*republishing old data*".

We indeed revisited a core from an exceptional site at 46°S in de south Indian Ocean for which the data are published partially in 2008. However, back then we only could speculate about solar forcing as our age-depth model was far from accurate enough. For our current paper we did a significant amount of additional analyses as we will outline below:

- a high-resolution wiggle-matched ^{14}C chronology, **absolutely necessary** for testing a Sun-climate link
- a probabilistic model, together with the probability density estimates on magnetic susceptibility data to precisely pinpoint the onset of the change in our data set
- elaborate additional proxy-data around the change of interest
- a comparison with likewise well-dated records from the Northern Hemisphere
- Reanalysis AND Model data to test the hypothesis of a synchronous solar forced change in the westerly wind belts in both hemispheres

So, even if we (partially) reuse published data we believe that there is a wealth of new data to explore further which was only a suggestion in Van der Putten *et al.*, 2008. Interestingly, in the report of R1 the words "high resolution dating", "Reanalysis" and "Model data" are not even mentioned.

We do agree with R1 to be more clear on certain aspects of our paper:

- emphasize what is new in the paper
- add the necessary information and a figure of the lake record
- elaborate the discussion on alternative forcings
- refer to other SH climate records. However, chronology is an issue here as most SH records lack the necessary high-resolution age-depth model.

In response to the following comment of R1 (R3) "*If the mechanism is really global, as the authors argue here, many other mid-latitude SH records should show a shift at 2.8 cal kyr BP*" we would like to argue that this does not have to be the case for several reasons: (i) not all proxies/archives register past climate change in the same way, (ii) as we know, the SHW do not have to act as a hemisphere-wide zonal system, especially when proxy-data from different ocean basins are compared (see fig. 3a, c and e for instance) and (iii) we see no alternative external forcing mechanism that can explain the observed synchronous shifts in both hemispheres (global?) on which we focus in our paper, supporting the suggestion of R1 for "*an alternative explanation is that the SWW shift is long-lasting (multi-centennial/millennial)*". Also, in our manuscript we did refer to long-lasting orbital forced changes (lines 295-305): *Both long-term orbital insolation forcing as short-term solar irradiance variability influence Holocene climate and thus our record. The transition seen in both peat records could be the result of a long term (orbital) trend, pushing the system to cross a threshold resulting in a*

regime shift. It seems unlikely that a NH and SH peat record, both supported by a high resolution chronology, show a shift, within dating errors, at the same time without being externally forced. In order to obtain such a synchronous signal, a relatively short forcing is required. Hence, orbital forcing might provide the necessary insolation background climate conditions, but the trigger for the shifting to increased westerly influence on the island is likely connected to the onset of one of the largest and longest Holocene solar minimum. The Homeric minimum forms the onset of a period with series of grand solar minima, following a period of a rather quite Sun between about 5000 and 2800 cal years BP (Stienhilber et al., 2009; Wanner et al., 2015).
We could have added that, around the period of interest (2800-2700 cal yr BP) there is no evidence from ice cores for exceptional volcanic activity (Cole-Dai *et al.*, 2021; Kobashi *et al.*, 2017; Wanner *et al.*, 2015).

Last, but not least, R1 (R3) also writes "*The interpretation is therefore biased towards solar activity, which uncoincidentally seems to be one of the main research topics of the research group at VU Amsterdam. This is the main issue with this manuscript.*". The subject of the paper is to test the hypothesis of a Sun-climate link and the record presented provides an excellent opportunity to do so. We do not understand how it is relevant that some of the authors worked on the topic before. Also, there is no solar forcing research group at VU Amsterdam. However, we could, as mentioned before, deepen the discussion on alternative forcing mechanisms (but see lines 295-305 in the manuscript).

References

Cole-Dai, J., Ferris, D. G., Kennedy, J. A., Sigl, M., McConnell, J. R., Fudge, T. J., Geng, L., Maselli, O. J., Taylor, K. C., and Souney, J. M.: Comprehensive Record of Volcanic Eruptions in the Holocene (11,000 years) From the WAIS Divide, Antarctica Ice Core, Journal of Geophysical Research: Atmospheres, 126, e2020JD032855, 2021.

Kobashi, T., Menviel, L., Jeltsch-Thömmes, A., Vinther, B. M., Box, J. E., Muscheler, R., Nakaegawa, T., Pfister, P. L., Döring, M., Leuenberger, M., Wanner, H., and Ohmura, A.: Volcanic influence on centennial to millennial Holocene Greenland temperature change, Scientific Reports, 7, 1441, 2017.

Steinhilber, F., Abreu, J. A., Beer, J., Brunner, I., Christl, M., Fischer, H., Heikkilä, U., Kubik, P. W., Mann, M., 530 McCracken, K. G., Miller, H., Miyahara, H., Oerter, H., and Wilhelms, F.: 9,400 years of cosmic radiation and solar activity from ice cores and tree rings, Proceedings of the National Academy of Sciences, 109, 5967-5971, 2012.

Van der Putten, N., Hébrard, J.-P., Verbruggen, C., Van de Vijver, B., Disnar, J.-R., Spassov, S., Keravis, D., de Beaulieu, J.-L., De Dapper, M., Hus, J., Thouveny, N., and Frenot, Y.: An integrated palaeoenvironmental investigation of a 6200 year old peat sequence from Île de la Possession, Îles Crozet, sub-Antarctica., Palaeogeography, Palaeoclimatology, Palaeoecology, 270, 179-195, 2008.

Wanner, H., Mercolli, L., Grosjean, M., and Ritz, S. P.: Holocene climate variability and change; a data-based review, 610 Journal of the Geological Society, 172, 254-263, 2015.