

Clim. Past Discuss., referee comment RC1
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Comment on cp-2021-69

Anonymous Referee #1

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-RC1>, 2021

I appreciate the opportunity to comment on manuscript CP-2021-69 by Van der Putten et al. This study investigates late Holocene changes in the hydroclimate of the SH mid-latitudes through the multi-proxy analysis of a peat sequence (and to a lesser extent one adjacent lake sediment core) from a small volcanic island in the Indian Ocean. The authors focus on a short time-window around 2.8 cal kyr BP, which corresponds to the Homeric solar minimum. They attribute a shift to wetter conditions that occurred shortly after 2.8 cal kyr BP to an equatorward displacement of the southern westerly wind belt, and they argue that it represents a response to solar forcing, coeval with similar changes that occurred in the Northern Hemisphere. They conclude that the Homeric solar minimum is responsible for wind belt shifts in both hemispheres around 2.8 cal kyr BP.

The manuscript is concise and well written but, in my opinion, the current version suffers from three major issues:

- Data interpretation is biased. The data clearly shows a shift from drier to wetter conditions at 2.8 cal kyr BP. This shift was already identified in the same core(s) by the same authors more than 10 years ago (Van der Putten et al, *Paleo3*, 2008). The interpretation of the data in terms of shift in the southern westerlies is justified. However, here, the authors attribute this SWW shift to a change in solar activity, without evaluating any other possible mechanism. The fact that the timing matches the Homeric minimum and that solar activity is known to modify pressure gradients is simply not sufficient to link the observed moisture change to solar activity. It may well be the case but focusing on a single mechanism from the start without evaluating other possibilities is poor scientific practice. Why is this shift not visible in other records from the Southern Hemisphere if the forcing is global? 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.
- What data is new vs previously published is unclear. This manuscript is based on cores published in Van der Putten et al *Paleo3* (2008). Compared to that earlier publication, the authors have improved the core chronology around 2.8 cal kyr BP and apparently

measured additional variables (eg XRF core scanning), but whether the other data (eg macrofossils, MS) has already been published or was remeasured at higher resolution for this manuscript is unclear. The authors need to clearly identify the previously published vs new datasets.

- Some of the results are not properly illustrated, making part of the manuscript difficult to follow. For example, the shift in lake sedimentation described at lines 187-189 is not shown anywhere. Some of these results may already be presented in Van der Putten et al 2008 but the current manuscript should be a standalone publication.

In summary, the authors present data that confirm the presence of the 2.8 cal kyr BP shift in the SWW that was identified by Van der Putten et al 2008. They argue that this shift is a response of the wind belt to the Homeric solar minimum but this interpretation is biased. Having a single record in the SH that shows a SWW shift coinciding with the 2.8 cal kyr BP solar minimum does not necessarily mean that solar forcing is responsible for the apparent interhemispheric synchronicity (cf title). Therefore, I cannot recommend this manuscript for publication in *Climate of the Past* at this stage. This manuscript could be reconsidered for publication if (a) the authors can demonstrate that the dataset is significantly new and (b) solar activity is proposed as one of the possible explanations, leaving the door open to other mechanisms. The absence of this event in other records from the SH should also be discussed. The relative lack of novelty (compared to Van der Putten et al 2008) may become an issue then.

**Minor comments (line number):

1-2: The title reflects the biased interpretation. The authors should be more honest and focus on the 2.8 cal kyr BP shift in SH hydroclimate. Observing a change in one record from the SH doesn't make the signal interhemispheric.

4: Affiliation 2 should appear before 3

36: add "possibly" before attributed and modify the rest of the manuscript accordingly.

58: I do not specifically follow the literature on solar activity but evidence for a solar influence on climate doesn't really seem to have increased in the last decade. This is not particularly supported by the references listed here either. Delete "increasing".

76: add comma after perspective

Figure 1: Why is panel (d) not labeled? This part of the figure is entirely from Van der Putten et al 2008. Please indicate it here. Also, add "MRP" (MRL is on the figure), and delete "onset organic lake deposits 2753 cal yr BP" (there is no reason to add lithological information for MRL and not MRP, and an age without error bar is not appropriate). Also, what does the second vertical dashed line represent? Another lake sediment core? Why was this core not used? What is core selection based on?

102: Clearly state when these cores were collected, which results have already been published, and do not cite S4 before S2 and S3

106: Replace "additional coring" with "In addition, Morne Rouge Lake (MRL) was cored ...". When I first read the manuscript, I did not immediately realize that a 2nd site had been cored since the data is not shown anywhere (see comments to Fig 1 above too).

108: "One of the lake sequences". How many are there? How/why was this one selected?

122-124: This is not illustrated anywhere and therefore difficult to follow

126-129: The reference to Van der Putten et al 2008 applies to the data (previously published) and not just to the method. Please be clear about it.

133: latter part – do you mean 185-344 cm? If yes, please repeat it here to avoid confusion.

137-139: Why not using centered log ratios instead? See Weltje et al. DPER 2015.

145-155: What is new and what has already been published in Van der Putten et al 2008? Did you increase the resolution around the 2.8 kyr interval? This should be crystal clear.

187-189: Where can readers see this transition?

199: What about erosion from the watershed? The peatbog doesn't seem ombrotrophic and it seems to be surrounded by steep slopes (Fig 1d). No inlet doesn't mean no terrestrial runoff during intense precipitation, especially since sediments did reach the lake. Does the grain-size of these particles support and aeolian origin? This comment also applies to line 240.

Figure 2: (a,b): blue dots linked by green lines are confusing. I first thought that the dots and lines represented different datasets. Please simplify. (b): Delete "log-transformed" and add unit. It seems that you simply plotted the MS data (in 10^{-5} SI?) using a log scale. (c) the mix of colors is confusing. "Morne Rouge" (written vertically): is this from the lake or peat record?

230: I agree with "minerogenic" but what is the evidence for "wind-driven"?

256: Morne Rouge lake or peat record?

262-265: This observation is honest and correct but it was already written as such in Van der Putten et al 2008.

266-304: What about other records from the Southern Hemisphere? 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. There are certainly enough records from Patagonia, New Zealand, Tasmania, and other sub-Antarctic islands. Why is the shift not visible in the Amsterdam Island record for example (Li et al QSR 2020)?

287: An alternative explanation is that the SWW shift is long-lasting (multi-centennial/millennial). This would be in better agreement with other Holocene records from the SH and would not require a relatively complex threshold explanation. Does this threshold explanation also apply to the lake record?

296-304: The external forcing seems to be justified but the authors should also evaluate/discuss mechanisms that do not involve solar activity. This interpretation is biased. Although the synchronicity between this record and other records from the NH is a valid argument, other mechanisms not involving solar activity are possible and should not be neglected here. If a solar minimum was the reason, why is a particularly marked shift at 2.8 kyr not observed in most other SH records?

335-339: Their record does not show any decrease in precipitation contemporary to the solar forcing minimum at 2.8 cal kyr BP.

336: add "than" after dated? Or add comma.

341-342: the "wetting" (is that a word?) of the mid-latitudes is just not supported by data from the literature.

Fig 3 and S1: Why was 10m selected for zonal wind speed? A pressure (altitude) of 850 or 700 mb is generally used to represent moisture transported by the SWW.