

Clim. Past Discuss., referee comment RC3  
<https://doi.org/10.5194/cp-2021-69-RC3>, 2021  
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## Comment on cp-2021-69

Anonymous Referee #3

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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-RC3>, 2021

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Thank you very much for giving me the opportunity to review this manuscript and my apologies with the delay since I was on the field for a while. The manuscript presents a record from the Southern Hemisphere high latitudes that is correlated with a record in the Northern Hemisphere to explain the importance of solar forcing as a global climate driver. I unfortunately think that this manuscript cannot be considered for publication in Climate of the past as it presents several conceptual and scientific flaws that I will develop here below.

First of all, the MS is biased towards one climate driver (which is one of the main subjects of the author's group/faculty): solar forcing. There are other external drivers that could explain modifications in the westerly position/intensity and precipitations in the area, but the authors seem not to discuss them. Secondly, the authors ignore a series of papers in the same latitudinal range of the Southern Hemisphere that could serve as solid comparison. Curiously enough, not a lot of those papers talk about solar forcing and invoke other drivers. I encourage the authors to read again those papers (check other records in the Indian Ocean but also Macquarie Island, Marion Island, South Georgia, Falklands, Antarctic Peninsula, etc.) and integrate them in their study. My point being: when working on such an isolated area with the purpose of understanding global drivers, start by looking at the geographically closest records, then move further away. There are records with equivalent depth and age resolution which are closer than DOME C. The same goes for the Northern hemispheric records. Why choosing two specific records when there are plenty of them? Moreover, the selected records (a peatland and a lake) are studies from the same group/co-authors than this paper. If the authors wish to draw such the big picture about such a complex feature as interhemispheric solar forcing, it just seems humble to acknowledge and compare to other records nearby as well as to discuss other drivers. If not, cherry picking one or two records from the similar research group - that just happen to agree with the presented record - to explain one specific short-time event, is of poor scientific practice. I know the case is complex, and the number of papers is high (and that ultimately this will result in selecting papers as journals limit the number of references and one needs to focus to some extent), but here, it seems that there is nothing else done or that there is nothing else to compare with except two papers from the same group and Dome C, which is not the case, and does not help having a global picture.

Besides those practice flaws , I will now focus on other major issues which actually preclude any robust interpretation.

### **Site location and characteristics:**

The authors wish to study wind and precipitation within the westerly core belt. However, their site is clearly located to the east of the island, with mountain ranges to its west. This is far from ideal to study such processes. I therefore question how and why this site was selected, compared to potential sites to the west of the island with more directly influenced by westerly winds and precipitation, and how this location would ultimately affect the relevancy of the record.

To reconstruct wind proxies, one needs of course to have a record that captures atmospheric inputs exclusively, without disturbances due to local erosional (e.g. watershed runoff) or post-depositional processes (dissolution/neof ormation of minerals). As we know, only ice cores, loess or ombrotrophic peatlands may provide such characteristics. While the authors claim that besides peat bogs, closed basins like crater basins (like their site composed of a mire and a pond) can act as ombrotrophic archive, this is erroneous. The authors seem to ignore the processes that apply here. It is not only a question of where the particles come from (long range or watershed erosion), it is also how they are preserved after deposition. Here, the upper part of the record is a mire, in other words a minerotrophic peatland, which is subject to mineral inputs from watershed runoff and post-depositional physico-chemical processes. The authors actually give a clue that post-depositional processes exist as they find terrestrial diatoms in their peat profile, directly witnessing that indeed dissolution and mineral neof ormation exist (see my comment on diatoms further down). Moreover, even from a small crater, there is runoff in a rainy island, and from the site picture one can easily imagine erosion from the almost vertical crater cliff bordering the site. The peat record will also be affected by the fluctuation of the nearby pond water body (that can bring oxygen, dissolved chemical elements and even lacustrine diatoms as it is suggested by the authors). In other words, it will not act as an ombrotrophic peatland or an atmospheric archive. In an ombrotrophic peatland, the water level does not fluctuate and below the very limited oxygenated acrotelm, the remaining layers are under anoxic conditions, guaranteeing the preservation of mineral particles. This is not the case for a mire that is directly connected to a lake where open-air lake water penetrates the surrounding peat deposits. There will therefore be both influence of watershed runoff, and post-depositional processes. Besides the atmospheric/geochemical aspect of it, the authors also wish to reconstruct bog surface wetness (a proxy of precipitation), a common proxy in peat bogs. But here again the wetness could be influence by the lake level. So again, this is problematic. Then the bottom part of the section is a lake record, that, per definition, would not capture atmospheric signals only. In conclusion, to start with, the site is not suitable to attain the scientific objectives of the paper.

### **Coring technique:**

I am quite surprised by the coring technique which consist in pushing a 11cm diameter, 2.5-m long (and then 5-m long?) PVC tube in the peatland, which is against all the common "good practice" techniques used to retrieve undisturbed peat samples. The so-called Russian peat corer technique dates back from 1955 (Belokopytov and Beresnevich, 1955, Torf Prom.) and was refined by Jowsey in the 60's (Jowsey, 1966 New Phytol.). Moreover, several review papers explain why pushing a tube is not recommended as it compresses the peat layers, especially at the top part (and this compression can then vary

with the characteristic of each layer) when entering the tube, and stretches the peat core while retrieving the tube as suction can happen from the bottom part and the outer walls of the tube. Speaking of suction, I would like to know how the authors retrieved a 2.5-m and then a 5-m tube pushed in wet and rather sticky peat? Having cored peatlands quite a bit, I would say this is impossible without a motorized system, but nothing is described here. I would like to know how that was achieved to counterbalance the important suction. Anyhow, the technique of simply pushing tubes to core peatlands has been avoided by peat scientists for several decades as much more reliable, easier and affordable tools are available. My guess would be that the authors use a single tube section to avoid multiple core sections and difficulties related to that, but this has also been solved a long time ago by taking parallel overlapping cores and date each core section as shown in many publications. I assume the diameter of the tube is perhaps to retrieve as much material as possible, but this is rather unjustified as well, as many multiproxy studies using the same proxies as here were achieved on smaller samples/cores. I therefore greatly question this coring technique not only on a scientific point of view but also in terms of impact on such a fragile ecosystem in a remote area. I recommend the authors to read recent reviews on the matter (e.g. Chambers et al., 2012 JQS; Shotyky et al., 2020 Can. J. Soil. Sci., to cite a few).

### **Previously published data:**

I understand that the authors focused on a particular event for this paper, but I think it is worth mentioning that a substantial amount of data appearing in this paper were already published in Van der Putten et al., 2008. For example, except the higher resolution on the specific time window of this paper, the magnetic susceptibility was already published. The macrofossil data were also already published. The new data here are diatoms at low resolution, XRF (which are not really used, see my comment further below) and 15 radiocarbon dates. I consider poor practice to reuse a large portion of an already published dataset for another paper.

### **Mineral flux:**

Calculating a minerogenic flux is not as simple as the authors do from the LOI. First of all because different vegetation types yield different ash content (e.g. Zacccone et al., 2013 QI; Zacccone et al. 2012 Plant and Soil; Leifeld et al., 2011 Plant and Soil, Sapkota 2006, PhD dissertation available online). For example, some plants can concentrate elements like Si, K, P in their root and stem tissue and neo-form minerals. Halophyte plants will concentrate salts. Other plants develop dense parts or wood that will result in a higher density and ash content. Organic matter can mineralize. Moreover, micro-organisms can form mineral tests from dissolved chemical elements (see my comments on diatoms further down), etc. In other words, pure peat deposits (i.e. without any exogenic mineral input) can yield an ash content from close to 0 to 10% or more. So no, unfortunately, LOI cannot be used to reconstruct a minerogenic dust flux. Of course, LOI gives a trend and an idea of where there are mineral enrichments in peatlands (if vegetation has been established to be stable over time, which is not the case here). LOI can help determining mineral peaks, volcanic layers, or biogenic layers that can then be characterized using other tools, but that is pretty much it. Therefore, the only wind proxy presented here, that is derived from LOI, is invalid because 1/ different vegetation yield different ash content, 2/ possible neoformation on root tissue (especially important in silicate rich areas – See Sapkota 2006), 3/ biogenic mineral neoformation (see my comment further below about diatoms). I would suggest the authors to look deeper in their XRF data to see if they could develop an erosion proxy based on some elemental ratios, and distinguish it from the

biogenic part of the mineral content. If they then wish to quantify it, they would need to develop a mineral proxy based on concentrations as it has been abundantly done in ombrotrophic peatlands (e.g. Shotyk et al., 2001).

### **That brings me to the use of the scanning XRF data:**

I wonder why the authors actually put these data in. The only diagram is a PCA on the whole dataset showing the opposition between a mineral component and an organic component and a reported PC1 in figure 2, to show that the XRF coincides in telling that the chemical elements are from minerogenic origin. Of course, they are, and of course this diagram is as it is, opposing organic matter to mineral matter as the record is composed of an organic matrix in which were incorporated mineral inputs. To me, the integration of this dataset is useless as it is. It is used to justify the mineral content, but it is somehow a circular reasoning as per definition XRF measures the inorganic compounds of the core. Moreover, since - as said earlier - the minerogenic flux from LOI is erroneous, the XRF interpretation linked to LOI is unsupported. Again, as a suggestion, I would present for some proxy vs time diagrams of this XRF dataset of parts of it, to see for example the lake/peat transition, to search if this could indeed be considered as ombrotrophic or not (although I doubt it will stand out...), to search for tephras or particular mineral layers that could then be interpreted as local erosional or runoff events (and those events, even if from the watershed, could be linked to particular climatic conditions).

### **Diatoms:**

Diatoms can be found in peatlands, but, of course, to form diatoms, there is a need for a dissolved silica source (here in a volcanic island this is no problem). So terrestrial diatoms are mainly found in minerotrophic peatlands where sufficient amounts of mineral particles are available together with conditions favoring their partial dissolution, rendering dissolved Si available to form diatom frustules. Here, the presence of diatoms in the peat part of the profile strongly suggests a post-depositional mobilization of minerals (ie dissolution of Si minerals) that is a strong evidence for minerotrophy and that tells that part of the mineral content of the peatland is not derived from atmospheric/wind inputs, but is of in situ biogenic origin. So not only it is an evidence that minerotrophic peatland cannot be used to reconstruct wind using mineral fluxes because of post-depositional issues, but it also tells that because a part of the mineral content is of post-depositional, in situ biogenic origin, the interpretation that the authors make of their mineral flux derived from LOI as a wind proxy is once again erroneous. The authors also mention that they find some lacustrine diatoms in the peat section of the record and explain it by "temporary flooding" of the peatland, or wind-blown lake spray. Once again, this is evidence for post-depositional processes in the peat section of the record, and "contamination" of the mineral content by lacustrine diatoms, rendering the mineral flux from LOI useless in terms of wind proxy since some layers are a mixture of in situ deposition, biogenic formation, and inputs from the lake.

### **Resolution:**

The authors claim that they have a low uncertainty (55 years) on the age-depth model during the Homeric phase (2.8-2.5 cal kyrs BP), but at the end, looking at figure 2, the only high-resolution data in terms of depth resolution (i.e. that can effectively pinpoint the 2.8 event with a good resolution) are the invalid minerogenic flux, the magnetic

susceptibility (also a proxy of mineral inputs) and PC1 of the XRF data, useless as it is (see my comments on those proxies earlier). In other words, 3 graphs that tell the same thing, two of which are objectionable. The other proxies are low resolution. The macrofossil data display 40 points/5000 years (= 1point/125yrs). The diatom data display 32points/5000 years (= 1point/156yrs), for a specific event that lasts 300 years at best. So overall, besides the proxy issues I point out before there is no sufficient high-resolution valid data to draw robust conclusions in terms of precipitation and wind changes. The age-depth modeling may be very good with low uncertainties, but then if a proxy is investigated at low depth resolution, the proxy vs age graph remains low resolution.

I have not gone deeper into commenting the interpretation part of the manuscript as I think that there are flaws that render that interpretation speculative: lack of use of existing literature, flaws in the data generation, bias towards one climate driver, relatively low resolution of parts of the dataset. One can also question the novelty of this paper compared to Van der Putten et al. (2008) which already deals with solar forcing in the same core with, in great part, the same data. I am sorry I cannot be more positive but I think this paper needs a considerable amount of modifications before being submitted again for review if the authors manage to make it a novel/original contribution standing out of their previous paper.

### **Other suggestions:**

#### **Introduction:**

Section before last, the authors tell they present a unique terrestrial proxy record for wind and precipitation. This is of course overstated as, as I already wrote earlier, several records exist at around the same latitude, or more largely at high latitudes of the Southern Hemisphere.

#### **Lake core:**

The authors talk about a lake core in the introduction and the methods, but do not really mention it later and do not present diagrams together with the peat core. I wonder why. Could we have a comparison of both with common proxies? Also, I wonder how we could determine if what has been attributed as lake sediment (i.e. before the lake peat transition) is not a former peatland that was subsequently flooded, because the basal ages of the lake core and the peat core are similar. Another plausible process would be that the peatland at the edge of the lake would erode as it accumulates, and deposit in the lake bottom. There is no evidence the authors have investigated these possibilities and indeed the lake transition invoked in the introduction is not really detailed. Moreover, the schematic drawing of figure 1 makes no distinction and is confusing since it looks like it is all peat, even below the lake water body.

#### **Dating:**

I would like to know what where exactly the macrofossils composed of. Remains of above ground plants? Which species, which plant parts? I suggest the authors to add a column on their table about that both for the peat and lake samples.

Why removing outliers? Date inversions are a common feature when dating the Holocene, especially where the calibration curve display wiggles, which is the case here. Removing outliers without providing a relevant explanation (e.g. post-depositional processes in the peatland, contamination during laboratory process) should be avoided at all cost and the authors should accept their results as they are. Moreover, any modern age-depth modeling tool like BACON or CLAM can accommodate outliers. I suggest to include them as these are real results and they may contribute to have a more realistic (perhaps slightly larger) uncertainty on calibrated ages. I also suggest to use SHCAL20 as a calibration curve. Even if there is no difference with the SHCAL13 curve for that precise time window, people wishing to compare their (perhaps longer) records to this one will most likely use the latest calibration curve in the future.

### **Volcanism:**

I do not know the volcanism of that sector but since the island is of volcanic origin, have the authors considered that some of those mineral peaks could be tephras from the island or from further away? Have they looked at the particles under a microscope to see if there were glass shards for example?