Response to reviewer comments, minor revisions

General:

The manuscript has been improved with respect to the first version, especially concerning a more focused statement of the general setup and potential weaknesses of the model/proxy comparisons and the fact that model results are entirely based on earlier publications of Moreno-Chamarro et al., (2017a, 2017b). Therefore I think in its present form it is conceptually better framed, also taking into account the more general readership of paleoclimatologists.

We'd like to thank the reviewer the comments here and on the previous draft, which have improved the clarity of this paper.

Below are a few comments from the replies that should be addressed in the final version of the manuscript.

Specific:

As outlined, specific concerns were not been addressed because the basic physical and statistical setup was completely based on earlier studies of Moreno-Chamarro et al. (2017a, 2017b), not being intended for additional investigations in the present manuscripts. Therefore most comments e.g. related to the setup of a more objective statistical testing scheme have not been implemented in the update of the manuscript.

That is correct. The study did not involve new modeling but instead used new paleoclimate and historical climatology data to assess the possibility of an eruption trigger for the previously hypothesized SPG slowdown mechanism (Moreno Chamarro et al., 2017a, 2017b).

The remaining comments are adequately addressed in the light of using the results (not the output) of the MPI-ESM-P simulations for (semi-) quantitative comparisons with the proxy data introduced and analysed in the context of links to potential SPG changes.

Introduction: I would like to suggest to include the following paragraph with according references formulated in the reply into the final version into the introduction of the study (e.g. line 89 ff):

Including other last millennium simulations would have blurred the discussion because it would have meant dealing with different models and model sensitivity to external forcings, different volcanic forcings, different background climate states at the time of the eruption, and different internal variability. The only model with a close enough setup is the CESM last millennium ensemble, which includes sensitivity simulations with different external forcing. However, it is not clear whether this model, the CESM-CAM5_CN, shows any sensitivity in the subpolar region to the volcanic forcing (Otto-Bliesner et al., 2016), although a newer model version shows cooling in the North Atlantic during the Little Ice Age associated with a SPG weakening (Zhong

et al., 2018). There is currently limited data for other CMIP6/PMIP4 last millennium simulations available at the ESGF nodes (and not for the MPI-ESM model).

We have included a modified version of this paragraph after line 105 and modified some of the surrounding text to explain the choice of climate model.

Beginning of section 2 and 3: A great improvement is that some weaknesses of the approach are explained in greater detail in the new version of the manuscript and also the focus is properly set to make the reader already aware on the core of the study.

l. 450: This paragraph also helps to address and motivate for outlook and follow-up studies (cf. Forward modeling)

l. 435: I don t understand what the a posteriori line of evidence means – what does this really mean if one could a posteriori say that something is more probable, i.e. having already the knowledge of the outcome ? Maybe the authors should re-formulate, e.g. that chances for a slowdown of the SPG after a volcanic eruption are slightly higher compared to a situation without a volcanic eruption ?

The a posteriori probability referred to our degree of believe in the hypothesized eruption trigger after analyzing our new data. Since this wording was confusing, we have removed the phrase.