General observations :
This paper presents in the same time two main goals which are maybe not sufficiently clearly announced in introduction and maybe not discussed in enough details in the results and discussion:

- **first**, a methodological improvement: the validation of not commonly used transfer function (both fx and tolerant-weighted improvement) presented in Liu et al., 2020 but still not clearly known and commonly tested in the climate reconstruction community. And, more than that, a new version of the fx correction (called fxTWA-PLS2) is presented. In conclusion it appears that this new version of the method is more reliable (especially in the climate extremes) and some supplementary figures show the same conclusion. However, this topic is not sufficiently and clearly presented in the text of the manuscript.
- **second**: the study present a new and more complete, detailed and qualitative climate reconstructions for the Holocene in the Iberian peninsula. Here, the discussion is focused on the west-east climatic gradient and his connection with orbital forcing.

It appears that this last version of the manuscript have really improved the discussion about the Holocene climate in the Iberian peninsula (lots of references added, comparison with other reconstructions, other proxies and other study cases, figures inserted in the main text...). However the methodological improvement of this paper is still not visible in the introduction and in the discussion as well.

Major comments and modifications :
As a specialist of climate reconstruction made on pollen samples, but not in this area of study, I will not focused on the Iberian Holocene climate discussion but mainly on the methodological discussion.

First, the last paragraph of the introduction should highlight the two main goals of the papers (methodological test and Holocene climate improvement such as explain in the general observations).

Then, the methodological part is far richer and clearer than the previous version of the manuscript with a larger and more exhaustive list of existing methods to convert the pollen signal to climate parameters. However, we do think that some point in the methodological choice should be clarified in the manuscript (maybe in introduction and certainly in discussion):

- **1.** How did you selected the studied climate parameters ? Why MTWA, MTCO and alpha instead of MAAT, MAP, GDD0, etc ? This is a important point.
- **2.** About the independence between climate parameters, it is also not really clear. The CCA and VIF show than the climate parameters are independent but in the same time you show Fig. 6 and l. 212-214 than they are closely correlated. This have also to be discussed.
- **3.** Why using 10° resolution climate database instead of already interpolated and discuss climate databases with 1° resolution (such as WorldClim2 or CHELSA data based) ?

All these choices have to be defended in introduction / methods.

About the results and discussion also some modifications are necessary. We think that the text, especially in the discussion is not sufficiently connecting with figures. Figs. 1, 5 and 6 are called only once and all the Figs. 1 to 6 are only called in results and not in discussions.

About the discussion, only the last paragraph of the discussion focus on the improvement made with the fxTWA-PLS2 version of the transfer function. The first sentence of the conclusion is "We have developed an improved version of fxTWA-PLS which further reduces compression bias and provides robust climate reconstructions", however this as not be proved neither discuss in the manuscript. We argue that this topic should be discussed and validated in the first part of the discussion before presenting the climate composite reconstruction and comparing it with other proxies, climate modeling and so on. Especially using the material in Appendix (Table A1 and Figs. A1 and A2).