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Comment on acp-2021-337

Anonymous Referee #2

Referee comment on "A weather regime characterisation of winter biomass aerosol transport from southern Africa" by Marco Gaetani et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-337-RC2>, 2021

Review of "A weather regime characterisation of winter biomass aerosol transport from southern Africa", submitted to ACP, by M. Gaetani et al.

This paper presents a statistical analysis of various climate fields to relate the tropospheric air quality in the Southern African continent, atmospheric circulation and SST.

The general idea is interesting and the paper is well structured (although I appreciate when figures appear in the text, where they are cited, rather than at the end of the manuscript, which makes reading rather tedious on a computer).

My major comment is on the application of the statistical methodology. The authors seem to use ~15 years of geopotential data from the CAMS reanalysis. The rationale is that AOD data are only available on that period. But the authors use SST data that cover more than one century (and use only a small subset). I think it would be more appropriate to apply the k-means algorithm on a longer period of time (e.g. with ERA-I, ERA5, or NCEP reanalyses) to compute weather regimes in a statistically robust way, and then classify CAMS data onto such weather regimes. This would reduce the uncertainty on the computation of WRs.

My second methodological suggestion is to use a cross-validation approach to E2C and C2E, by “learning” the associations between WR and AOD on a decade, and “testing/validating” this association on the remaining 5 years. This would give credence to the alleged predicting power of the statistical approach.

Minor comments:

The first paragraph of the introduction states that aerosols modify the radiative properties of the atmosphere. Fine. “As a consequence, they can influence on the atmospheric synoptic and large-scale dynamics” seems strange, as the radiative properties of aerosols are rather local, which contradicts large-scale atmospheric motion, where radiation is not so crucial. Please explain.

The end of the introduction lacks a paragraph that states the scientific question that the manuscript is dealing with. At present, the introduction states rather general questions, then states what the authors intend to do. How this endeavor corresponds to the many general questions seems to be left to the imagination of the reader.

When the authors compute the correlation between SST and WR frequency (Figure 9), they could do this on a much longer period, as WR can be determined from longer reanalyses. This would provide a more robust assessment of interannual relations.

Could the authors compare their results with computations of particle trajectories, for well chosen events?

The paper does not present any discussion of comparisons with already existing results. I am not an expert on the subject, but I would have expected that the results reported by the authors could be placed in a context of existing literature.