

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## major revisions required

Anonymous Referee #2

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Referee comment on "Evolution of the intensity and duration of the Southern Hemisphere stratospheric polar vortex edge for the period 1979–2020" by Audrey Lecouffe et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-676-RC2>, 2021

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This paper reports on the evolution of four parameters describing Antarctic polar vortex properties since 1979 based upon ERA-Interim reanalysis data. Two parameters describe the strength/stability (mean PV gradient at the vortex edge) and the area (expressed by the equivalent latitude of the vortex edge) of the Antarctic vortex. The other two parameters are the start and breakup dates of the Antarctic vortices. In the first part, the impact of dynamical processes (via proxy data representing solar activity, QBO, and ENSO) on the Antarctic vortex properties is evaluated in the form of the so-called seasonal evolution of composites separated by years of maximum and minimum solar activity. The second part focuses more on the trends of all four parameters over the 42 years.

The study is highly relevant, as changes in vortex properties are expected with changes in the ozone-depleting substances (ODS) and climate change. As ODS has been declining since about 2000, the ozone hole (and polar vortex) will likely weaken. This relevance needs to be more discussed in detail in the introduction to motivate this study. As reviewer #1 already noted, recent literature on the connection of solar activity, ENSO, and QBO with the polar vortex are not summarized in the paper. It is, therefore, not clear which results are new and which one only confirms earlier findings. The introduction needs to be expanded to summarize the current state of science on this topic.

In the annotated manuscript, I provided detailed comments/edits, and I will only briefly summarize the more important issues not mentioned above. The paper is not particularly well written and, at some places, not sufficiently clear (see annotations), so some more edits from the co-author team could have been helpful. In summary, the paper needs major revisions.

General: I find the terminology of the vortex properties somewhat awkward in particular if they are used repeatedly (vortex edge intensity, vortex edge position). My suggestion is to use terms such as vortex strength and size/area, what these parameters stand for.

Section 2.1: Why is the MIMOSA model needed to determine PV fields. It can be determined directly from the ECMWF data. Does MIMOSA provide higher time and spatial sampling?

Section 2.2: For the definitions of years with QBOe and QBOw as well as SCmin and SCmax, the upper and lower third of the distribution, is used, but why is this not done for ENSO (no intermediate years). Why is the one-third limit for each solar cycle determined separately?

p6,l2: "other studies", please cite them here (see also introduction)

Conclusion: Are the trends in the breakup dates after 2000 (the tendency for earlier breakup dates) a new result? Should it not be then highlighted in the abstract? Also, "trends are not halted" just by adding one year (2020) which has been extreme.

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2021-676/acp-2021-676-RC2-supplement.pdf>