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Comment on acp-2022-694

Anonymous Referee #1

Referee comment on "The Holton–Tan mechanism under stratospheric aerosol intervention" by Khalil Karami et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-694-RC1, 2022

The Holton-Tan mechanism under stratospheric aerosol

Intervention

By Karami et al , ACP

Karami et al investigate projected changes in the Holton-Tan effect under two future scenarios as compared to the present day climate. Using a version of the CESM which has a HT effect in its present day climate (albeit one too weak), they argue that the HT effect weakens in the future. This weakening for the RCP8.5 scenario could be due to the weakening of the QBO, however the weakening for the SAI scenario is more likely to do with the overall strengthening of the vortex.

The foundations of a paper that could be published in ACP are clearly present, however the interpretation offered by the authors for many of the results seems to be misleading, and further, some of the key arguments lack any calculation of statistical significance.

However, a careful rewriting of these sections and some additional analysis likely could address these issues, though the revisions are likely to be major.

Major comments:

- The authors argue that the HT effect weakens both in the SAI and RCP8.5 scenarios in both early and mid-winter. The evidence provided does not support this conclusion with regards to the mid-winter, clearly contradicting e.g. line 6 of the abstract.

The slopes in Figure 2 for Jan/Feb are indistinguishable for the three model runs (but please add error bars for the slopes!). Figure 3 and 4 also demonstrate (to my eye) no difference among the three for Jan/Feb. Interestingly, the tropospheric jet shift and presumably surface impact in Figure 3 is actually stronger for RCP8.5 than for PDC! (a finding which agrees with Rao et al 2020). This is actually acknowledged near line 212, however there appears to be a lack of consistency within the paper as to this conclusion. Line 241/242 just adds to the confusion. Line 323/324 adds yet another interpretation of the results which again differs from any previously offered.

To be constructive, please add stippling for statistical significance of the difference between SAI vs PDC and RCP8.5 vs PDC on Figure 5 and 6. Next, revise the text in the aforementioned locations to make the text and figures internally consistent.

2. For the index of refraction analysis, is N (Brunt-Vaisala frequency) allowed to vary in the vertical? The original Matsuno paper explicitly holds N constant, however eq 13 in this paper includes it inside the derivative. If you allow for N to spatially vary, you may as well use the Weinberger et al 2021 definition of the index of refraction which includes more physical processes without being any less physically inconsistent.

3. The stippling on figure 4 seems counter-intuitive, and even possibly incorrect. Is the deep tropical temperature anomaly associated with the QBO really not significant for a composite based on QBO winds? This temperature response should be highly consistent from one event to the other, and should be very robust. The top row looks reasonable, but not the rest.

4. I disagree with the interpretation offered in the first paragraph of section 4 where it is claimed the results agree with the HT mechanism. The F_y feature focused upon is well above the region of easterlies and is instead near 35-40km where westerlies prevail. Rather, this seems to be more consistent with the mechanism of Garfinkel et al 2012 as it occurs where the MMC of the QBO is strong, though it is hard to see the effect of Garfinkel et al 2012 in the index of refraction figures. However, figure 7 and figure 8 do not share the same y axis and labeling, so it is difficult to reach any conclusions as to whether n^2 changes and EPF changes are consistent in a given region. Please revise the axes of figures 7 and 8 to make them consistent, and then provide a more detailed analyses of possible consistencies between the diagnostics.

Weinberger, Israel, Chaim I. Garfinkel, Ian P. White, and Thomas Birner. "The Efficiency of Upward Wave Propagation near the Tropopause: Importance of the Form of the Refractive Index." *Journal of the Atmospheric Sciences* 78, no. 8 (2021): 2605-2617.

Rao, Jian, Chaim I. Garfinkel, and Ian P. White. "Projected strengthening of the extratropical surface impacts of the stratospheric quasiâ□□biennial oscillation." *Geophysical Research Letters* 47, no. 20 (2020): e2020GL089149.

Garfinkel, Chaim I., Tiffany A. Shaw, Dennis L. Hartmann, and Darryn W. Waugh. "Does the Holton–Tan mechanism explain how the quasi-biennial oscillation modulates the Arctic polar vortex?." *Journal of the Atmospheric Sciences* 69, no. 5 (2012): 1713-1733.

Minor comments:

- Line 24 reflecting -> to reflect
- Line 82-85: please rewrite for clarity
- Line 97: the first study (To my knowledge) which demonstrated that SAI affects the QBO was Aquila, V., C. I. Garfinkel, P. A. Newman, L. D. Oman, and D. W. Waugh. "Modifications of the quasiâ□□biennial oscillation by a geoengineering perturbation of the stratospheric aerosol layer." Geophysical Research Letters 41, no. 5 (2014): 1738-1744.