

# ***Interactive comment on* “The role of the winter residual circulation in the summer mesopause regions in WACCM” by Maartje Sanne Kuilman and Bodil Karlsson**

## **Anonymous Referee #2**

Received and published: 22 September 2017

Review of “The role of the winter residual circulation in the summer mesopause regions in WACCM” by Maartje Kuilman and Bodil Karlsson (acp-2017-647).

The scientific question behind this paper is to what extent WACCM reflects the results of a KMCM study regarding the interhemispheric coupling mechanism published by Karlsson and Becker 2016 (hereafter: K+B16). The main focus lies on the interhemispheric coupling mechanism describing the impact of the winter stratosphere on the summer mesopause region. The authors are able to reproduce and reconfirm the results of K+B16 qualitatively to a large extent. However there are also differences in structure and magnitude of the effect that are not mentioned and discussed. In general

[Printer-friendly version](#)

[Discussion paper](#)



the paper has a very detailed introduction giving a good overview of the current status. The presentation of the results can be shortened since some figures include almost the same information. The idea of this study is solid and worth to publish. However a discussion and a valuation of how the WACCM results are comparable to that from KMCM, as promised in the abstract, are mostly missing Thus I recommend a publication after a major revision only.

Major comments: Line 75-82: The purpose of this paragraph is not clear.

Line 121: In this context is the anomalous cooling of the summer mesopause a real cooling or a shift in altitude of the summer mesopause?

Line 124-137: I think this paragraph is more suitable for the discussion part. However you argue that the QTDW is an additional mechanism without showing it nor discussing it later in the paper. Please remove this sentence and put this fundamental discussion in the discussion part later in the paper.

The introduction includes all that is needed and more but needs a new grouping in order to a better preparation of the reader for the results.

Line 265-267: What is the magnitude of the temperature increase and how is its relation to a radiation-only driven atmosphere?

The information one can get from figure 3 can also be get from figure one expect for the GW drag. I would suggest to add a plot of the difference in GW drag as a function of latitude and altitude in figure 1 and remove figure 3. This would also improve the understanding of the IHC mechanism for the reader. A valuation and discussion on how the WACCM results correspond to the KMCM results is missing not only for figure 1 and 3 but in general. A comparison of your figure 1 and figure 3 in K+B16 shows differences in magnitude and structure even though they qualitatively correspond to each other.

Figure 2 shows the difference in water vapor and ice mass resulting from the GWs.

[Printer-friendly version](#)[Discussion paper](#)

The effect of the IHC on the NLC concurrency is interesting but the results are neither discussed nor brought in relation to other studies. Additionally I think that a discussion on this topic disrupts the central idea of the paper at this position. I would suggest to either remove the ice mass topic from the paper or to put it at the end so that the central idea of the paper is not interrupted.

Figure 4 shows the covariance of the control run and the run without GW in the SH for July. A critical comparison of these results with those of K+B16 (their figure 6) shows again a qualitative agreement but differences in magnitude and also in structure. These differences should be mentioned and discussed.

Similar to figure 1, please insert the difference in GW drag in figure 5. Again a discussion and comparison of your results with those of K+B16 is missing. This is particularly important in the case of January since there are much larger differences between the results of WACCM and KMCM as it is the case for July. The same applies to figure 6.

In line 333-334 you hypothesized that the IHC less affects the SH summer. However, the magnitude of the IHC effect in the SH summer is weaker since it is more disturbed in the NH winter by planetary waves.

Line 361: Please describe shortly how a weak and strong BDC is defined here.

In section 3.1 the introductory text gives the impression that the effect of the summer stratosphere on the summer mesosphere is studied in the following. However, the descriptions of the figures 7 and 8 for July and figures 10 and 11 for January mostly replicate the results regarding the IHC shown in figure 4 and 6 and do not give a further insight into the effect of the summer stratosphere on the summer mesosphere. Additionally, the information taken from figures 7, 8, 10 and 11 can be obtained from figure 4 and 6 and therefore are redundant. I would like to see the results when you correlate the summer stratosphere with the rest of the atmosphere similar to your figure 4 and 6. Furthermore a discussion of this topic is missing and should be included.

[Printer-friendly version](#)[Discussion paper](#)

The information from figure 9 and 12 can be obtained from figure 1 and 5 respectively and therefore are also redundant. However, a light discussion on the effect of the summer stratosphere on the summer mesosphere can be found in line 405-411 and 446-449 but none of the suggestions are shown or proven and are not compared to other studies.

Minor comments:

- o Line 34: ... (e.g., Fritts and Alexander, 2003)
- o Line 59: ... reversed with a cooling (warming) on top of the stratospheric warming (cooling) in the polar mesosphere -> your explanation is more clear without this
- o Line 51-62: You start the description of the IHC mechanism here and interrupt it for 40 lines. Especially for people without in depth knowledge of the IHC mechanism it is hard to follow you. It is better to describe the IHC mechanism in one go.
- o Line 121: ..., with an anomalous cooling ...
- o Line 144: please insert: ... lower breaking GWs in the summer hemisphere and a warmer ...
- o Line 161-171: The magnitude of the IHC effect is weaker in the SH summer mesopause than in the NH summer mesopause and not the impact.
- o Line 256: Please add: ... parameterized GWs in the winter hemisphere.
- o Line 268-270: Please insert a reference.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-647>, 2017.

[Printer-friendly version](#)[Discussion paper](#)