

***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 #1**

This manuscript revisits the mesospheric Interhemispheric Coupling (IHC) contribution to control temperature in the summer mesopause, using the comprehensive climate model CESM/WACCM. The main result is that this model is able to reproduce the mechanism as shown by Karlsson and Becker (2016 J Clim, KB16) with the KMCM model. The manuscript is well written and structured, but the new scientific insights it offers are not clear. Regarding this, I have one general concern, and some specific comments, that the authors could address before meriting publication:

First of all, we would like to thank the reviewer for their constructive criticism, and time spent to analyze our manuscript. We are grateful for the valuable suggestions provided. Responses to each of the comments are listed below:

1) What is the motivation for trying to reproduce KB16 results with WACCM? Are there processes included in WACCM and not in KMCM that justify the study? It is relevant that Figs. 1 to 6 are basically the same figures as those in KB16, but with WACCM instead of KMCM. The authors could offer a detailed comparison between the two models, because those figures present some differences that are not highlighted in the text. For example, it would seem that the correlation is very weak in the NH summer polar mesopause in WACCM (Fig. 4 top left), but quite significant in KMCM (Fig. 8A in KB16).

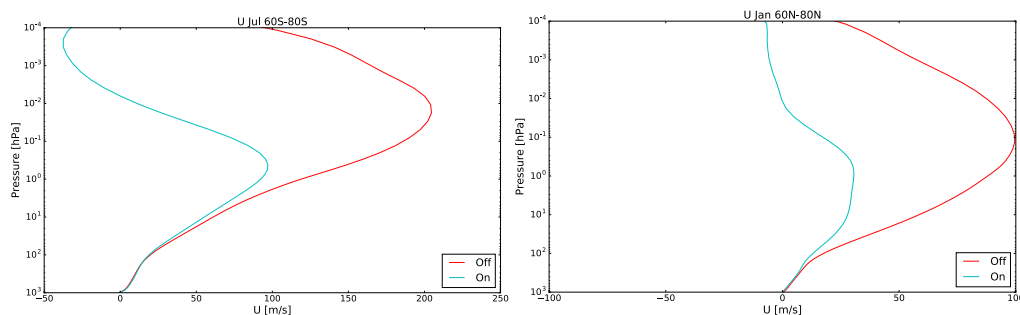
WACCM is in some aspects a more comprehensive model than KMCM. E.g. a major difference is that WACCM contains interactive chemistry in the middle atmosphere, while KMCM does not. WACCM also uses a different parameterization for non-orographic GWs than KMCM. KMCM uses a simplified dynamical core and convection scheme as compared to WACCM. Moreover, the WACCM model is well-established within the community: this study confirms the results of the less known - yet advanced and high-performing - KMCM. Confirming that the responses are the same in a variety of models simply serves to strengthen the validity and robustness of our findings. We emphasize this on lines 161 – 169.

Please, note that Figure 8a in KB16 is from 8 years of MLS data (2005 – 2012) and not from the KMCM model. Figure 8e is showing the correlation from a 30-year run of the CMAM30 (which is as comprehensive as the WACCM) and as can be seen, the correlation coefficients have decreased considerably although they are significant and the structure is robust. If comparing Figure 8e to previous Figure 4a (now Figure 2 upper left), the correlation coefficients are similar. However, the responses differ in altitude and in latitudinal extent. We now point these differences out in the text: lines 312-324.

### Specific comments:

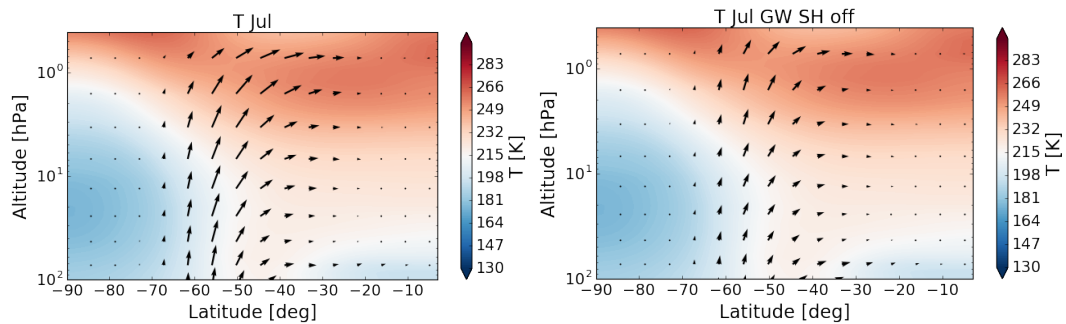
2) It would be interesting to include a discussion of the effects of turning off the GWD on the Brewer-Dobson circulation (BDC) itself. In the experiments where the GWD in the winter hemisphere is turned off, does the amplitude of the planetary waves change? In other words, say the GWD represents 80% of the total wave forcing in the winter mesosphere; is  $w^*$  80% weaker in the experiments versus control? (i.e. does the EP flux divergence increase in the experiments, trying to compensate the missing GWD?)

This is for sure an intriguing question. We speculate that as the winter GWs are removed, the eastward zonal flow will not be reversing into westward flow in the mesosphere. Hence, the PWs could potentially propagate further up in the stratosphere before reaching their critical levels (?). In such scenario, the PW drag on the zonal flow would be distributed over a larger altitude range, thus (since the drag is not so concentrated in a specific height region) the PW would have a less dramatic impact on the zonal wind. The zonal flow (attached below), particularly in the NH winter, is somehow confirming that. We also note that in Figure 1, there is a significant warming signal in the equatorial stratosphere indicating a weaker BD-circulation (which would agree with less PW drag/GW drag). Moreover, when we composite into high (and low) PW activity in the winter stratosphere, the warming (cooling) anomaly from the enhanced (reduced) BD-circulation extends into the mesosphere (see e.g. figure 2, left, bottom row, where we would otherwise have a cooling (warming) as a response of the GW drag (see figure 2, left, top row). We won't go into further details about what happens to the PWs in the winter stratosphere/mesosphere this study.

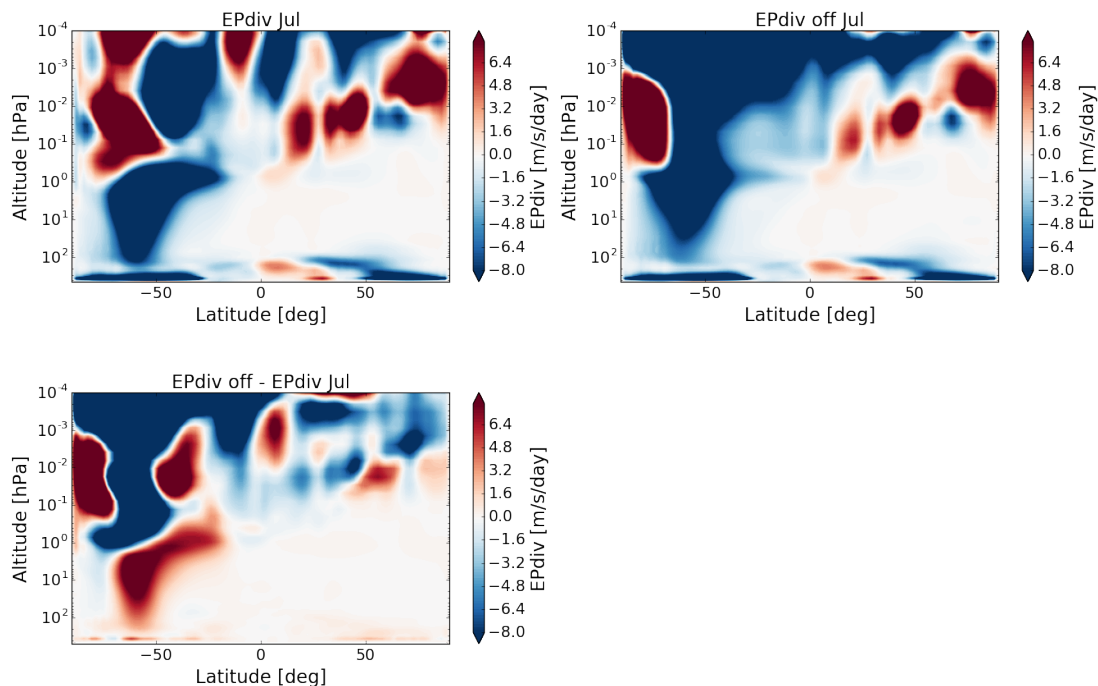


Zonal wind profiles July for the latitude band 60°S-80°S (left) and January for the latitude band 60°N-80°N (right).

For your information, I do show the Eliassen-Palm flux and Eliassen-Palm flux divergence. The EP flux divergence does indeed increase in the winter stratosphere, if there are no GWs in the winter hemisphere, suggesting that the amplitude of the PWs changes. We don't investigate this further for this study.



Eliassen-Palm flux for July for the control case (left) and the case where there are no GWs in the NH (right).



Eliassen-Palm flux divergence July for the control case (above, left) and the case where there are no GWs in the NH (above, right) and difference between them (below).

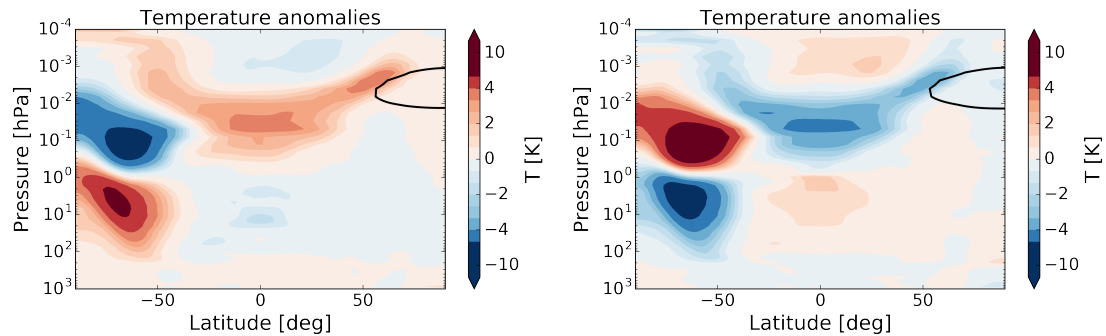
3) Lines 302 and elsewhere. For the correlation, why is the SH temperature averaged over 40-60S, and not over polar latitudes (as the authors do in the NH)?

This is because in the SH, the PW forcing is weak so that the residual flow does not reach the highest latitudes (see Kuroda and Kodera, 2001; their figure 4). This is now clarified on lines 300-304.

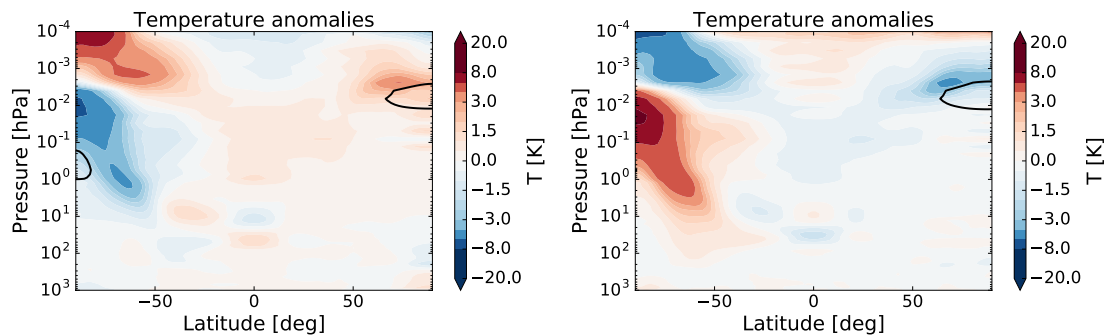
I. 302-306. *“The latitude and altitude ranges chosen for July is the region where the SH winter stratosphere variability is best captured (see Karlsson and Becker, 2016; their figure 9). This is related to the relatively weak PW forcing in the SH – the BDC is not reaching all the way to the polar region (Kuroda and Kodera, 2001).”*

4) Figure 4 (and 6). If the point of these figures is to highlight the importance of the equatorial mesospheric temperatures on controlling the summer mesopause T, why not correlating the equatorial T (instead of extratropical T) with T elsewhere?

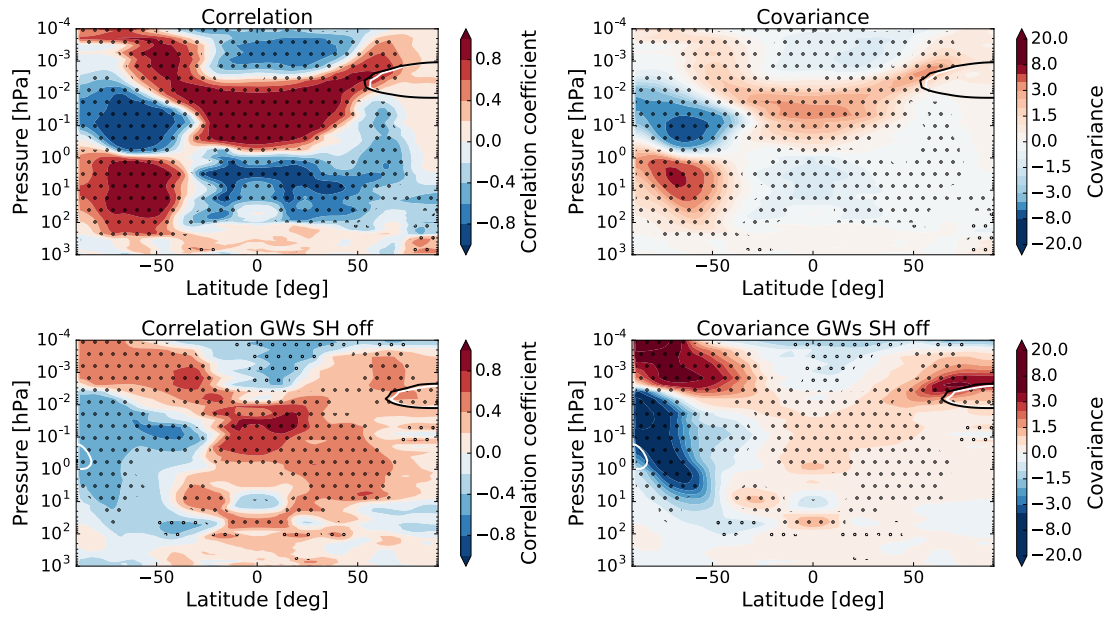
This can also be done and as shown below first for July then January, the results look similar as shown below. The idea was to start from the strong/weak BDC and then explain the mechanism behind the temperature change in the equatorial mesosphere and the effect on the summer mesosphere.



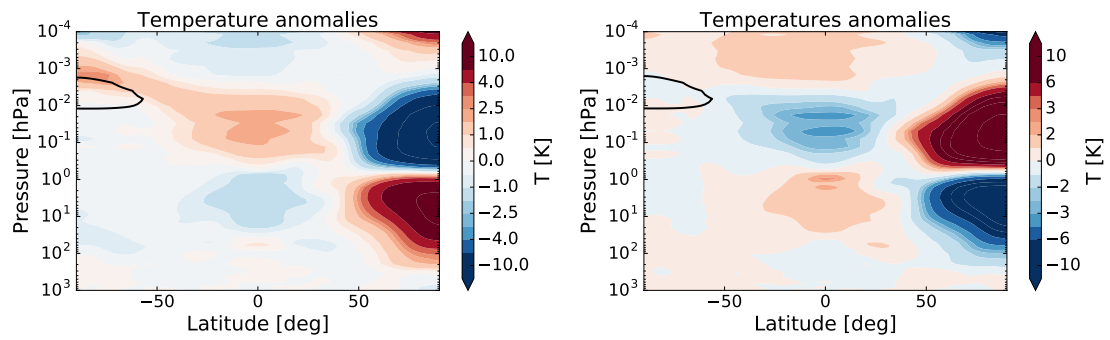
The temperature anomaly field for July taking the equatorial mesosphere as a proxy (20°S – 20°N, 0.13-0.01 hPa) for the GWs on.



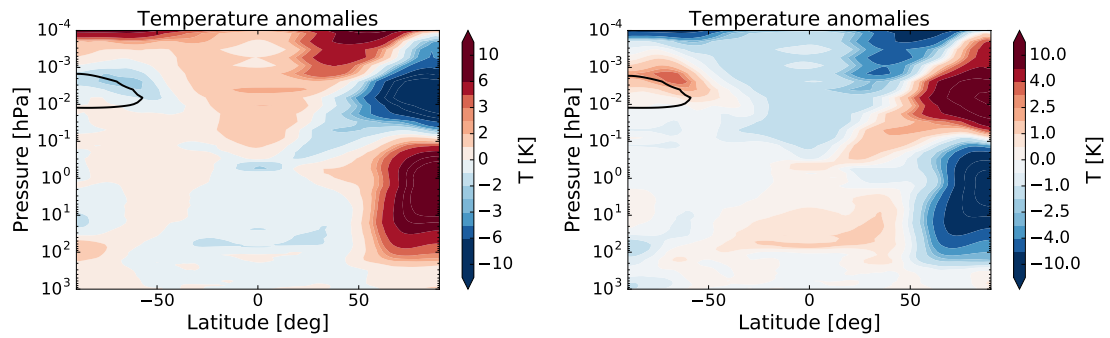
The temperature anomaly field for July taking the equatorial mesosphere as a proxy (20°S-20°N, 0.13-0.01 hPa) for the GWs in the SH off.



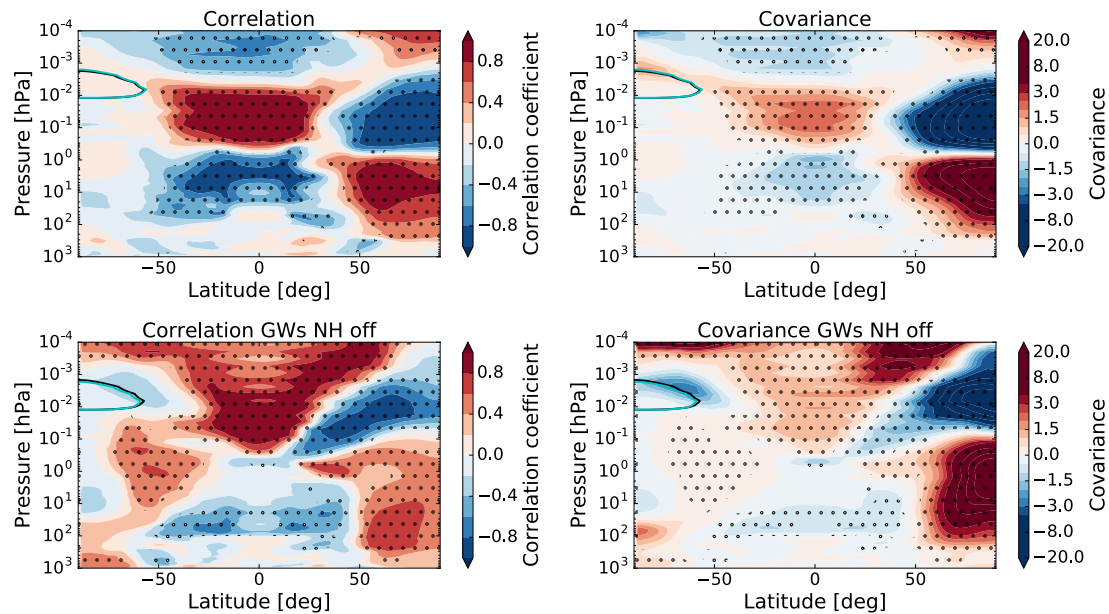
Correlations and covariance with the equatorial mesosphere (20°S-20°N, 0.13-0.01 hPa) in July.



The temperature anomaly field for January taking the equatorial mesosphere as a proxy (20°S – 20°N, 0.13-0.01 hPa) for the GWs on.



The temperature anomaly field for January taking the equatorial mesosphere as a proxy (20°S-20°N, 0.13-0.01 hPa) for the GWs in the SH off.



Correlations and covariance with the equatorial mesosphere (20°S-20°N, 0.13-0.01 hPa) in January.

5) Lines 327-328. What is the NLC region? Is it the region bounded by the contour? If so, it is hard to see any response in temperature there.

No that is true, there is no clear increase in temperature in this region. There is a small positive correlation in this region as can be seen in Fig. 4. However, this change is not statistically significant, this is something we can understand as explained in the introduction.

6) Lines 358. It seems not quite conventional to use T in the extratropics as a proxy for the strength of the BDC, when the model provides with all the variables needed to calculate it. Please comment on this choice.

The EP-flux divergence is not given as an output in WACCM. Since it is evident from Karlsson et al. 2007 and 2009 that the winter stratospheric temperature is an excellent proxy for the PW activity, we decided to use what was available. However, we ended up calculating the EP-flux divergence anyway (see above), but only for the 30-year mean. We hope that the reviewer is satisfied with our motivation for using the temperatures for compositing between high and low PW activity instead of the EP-flux because it was quite time consuming to calculate the EP-flux divergence and we need to remake that computation for all the 30 years. To assure that the results are very similar, we can carry out the EP-flux divergence calculations for each and every year, but only if the reviewer find it necessary.

7) I wonder how necessary are Figures 7, 8, 10 and 11; they seem to provide the same piece of information as Figures 4 and 6. I believe similar conclusions can be reached with the latter. Also, in lines 350-356 the authors decide to focus the discussion on the NH summer in July because of the stronger influence of the SH winter on the NH summer than vice versa. However, several paragraphs are devoted to this weaker connection between



the NH winter on the SH summer. I recommend suppressing 412- 450 (and the corresponding figures) for the sake of concision.

We agree that the information from the mentioned figures can be derived from Fig. 1 and the new Fig. 2. The section about the influence of the summer stratosphere has now been made shorter and more to the point.

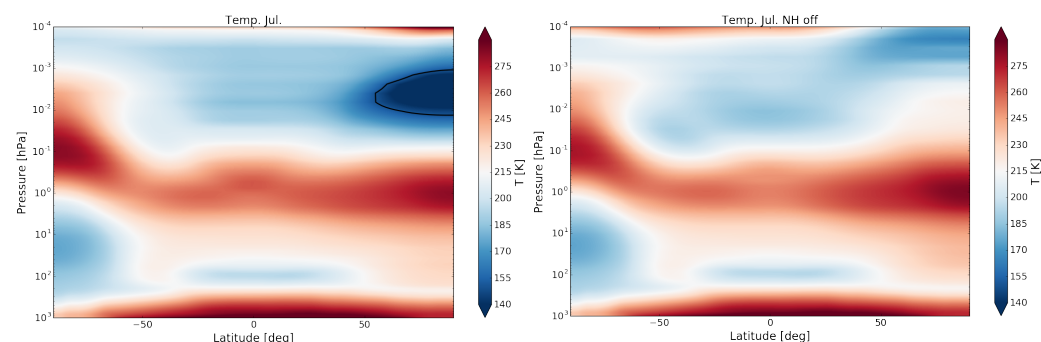
8) Section 3.1. I have some trouble trying to understand the objective of this section. Why is it interesting to discuss the role of the summer stratosphere on the summer mesospheric T in situations that are far from being realistic?

The section on the summer stratosphere has been rewritten. We hope the introduction to this section now gives a clearer picture on what is done.

l.386-391. *“The BDC is modifying in the summer stratospheric meridional temperature gradient. Hence, filtering effects taking place below the mesosphere may seem like an additional - or alternative – mechanism to the response observed in the summer mesopause. In this section, we will discuss why this cannot be the case. We focus again mostly on the NH summer polar mesosphere region.”*

Perhaps more interesting would be to perform an additional experiment in which the summer GWD is turned off. This way you can compare the importance of the summer BDC versus the IHC on the mesospheric T, and would definitely add new information from that given in KB16.

This simulations have been done already, as they come automatically when one runs the whole year without the GWs in the SH or NH. The problem with looking at these data is that without the GWs in the summer hemisphere, there is no summer mesopause region at all. The summer GWs are crucial for making the summer mesopause cold: the winter flow is only modulating where the summer GWs break. For further information, see the study by Kornich and Becker, 2011: they show that the IHC signal is not communicated to the summer mesosphere when the summer GWs are absent.



Temperature July (left) and temperature July when the GWs in the NH off (right)

Technical comments:

Figures: It is hard to see the dots that signal the statistical significance, and it is also quite difficult to assign a color to a value (in the colored figures). Perhaps adding black contours helps.

I agree that it was hard to see, the figures are now quite small and adding more contours makes the figures a bit chaotic. Instead what is done now, is shading the areas in which the confidence level of 95% is not reached.

- Line 283: At the same "time"?

Yes, this was what was meant, this section has now been removed though.