***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

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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 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.

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:

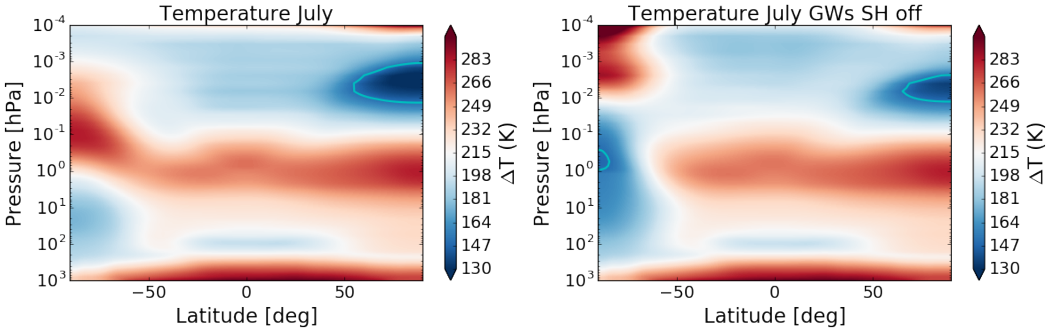
Major comments: Line 75-82: The purpose of this paragraph is not clear.

The text is now rewritten in order to make clear which purpose these paragraph serves.

l.64-73:*"These anomalies are responses to different wave forcing in the winter hemisphere. To understand how these anomalies come about we have to understand the interhemispheric coupling mechanism. The mechanism, as discussed here, is for the case of a stronger winter residual circulation, but works the same for a weakening of this circulation (Karlsson et al., 2009). A stronger planetary wave forcing in the winter stratosphere yields a stronger stratospheric Brewer-Dobson circulation (BDC). This anomalously strong flow yields an anomalously cold stratospheric tropical region and a warm stratospheric winter pole, due to the downward control principle (Haynes et al. 1991)."*

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?

If the gravity waves break at a higher altitude, the summer mesopause will be colder. This is a real cooling: lower temperatures are reached in the mesopause, as can be seen in the figure below.



Temperature July for the control case (left) and the case, for which there are no GWs in the SH (right). The blue contour indicates the region where the temperature is below 150 K.

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.

This part was put in the introduction because the debated status of IHC mechanism is an additional motivation for this study. However, we understand the objections the reviewer has against this section, indeed this is not further studied in this paper and has now been removed.

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.

The introduction has now been reordered and there is a new section (l.161-190) explaining what will be done in this study, we hope it is now clearer for the readers what is going to be discussed.

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

The temperature increase in the NLC region, which I have now defined to be between 61°N - 90°N and 0.01 - 0.002 hPa, is approximately 16 degrees.

In a radiation only atmosphere the temperature in the NH NLC region is about 210-220 K. Without GWs in the winter hemisphere, there is still a mesopause region, as can be seen in temperature fields for July as shown as response to an earlier comment.

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.

A plot with the changes in the GWD is added to figure 1. Figure 3 is now removed. A section discussing the differences has now been added. However, the point of this study is not so much to explain in detail how the differences in responses between KMCM and WACCM come about, but rather to reconfirm that in the absence of winter gravity waves, there is a warming of the summer mesopause region and to strengthen the evidence for the interhemispheric coupling mechanism, with the equatorial mesosphere region as crucial region of importance.

l. 275-290. "*When we compare our results with the results in Karlsson and Becker (2016, their figure 3), we observe there are some quantitative discrepancies in the structure of the responses. For example, Karlsson and Becker (2016) found that removing the winter GWs resulted in a warming of the mesosphere globally, although the response was strongest in the polar mesopause region. They attributed that the warming over the equatorial and winter mesosphere to the effect that GWs have on tides: when GWs are absent, the tidal response is enhanced. The same behavior is not found in WACCM - in fact, the equatorial upper mesosphere is anomalously cooler when the GWs are removed. These differences could perhaps be explained by for example the different gravity wave parameterization of non-orographic GWs, the different dynamical cores between the models and the presence of interactive chemistry in the middle atmosphere in WACCM. However, the qualitative response of the temperature and zonal wind change due to turning of the GWs in the SH corresponds well with the results from the KMCM as well as with our hypothesis.*"

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

We understand the objections the reviewer has to this section. We agree that this disrupting the main point of the paper. This part has now been removed.

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 qualitatively agreement but differences in magnitude and also in structure. These differences should be mentioned and discussed.

A comparison with the results of K+B16 has now been added. However, as stated before the point of this study is not so much to explain in detail how the differences in responses between KMCM and WACCM come about.

l. 312-324. *"Comparing the results show in Figure 2 (upper left) to Figure 8e in Karlsson and Becker (2016), it can be seen that the correlation coefficients are of similar magnitudes, but the spatial responses differ in altitude and in* *latitudinal extent: whereas the correlation signal is significant in the CMAM30 July high latitude summer mesopause, the WACCM July response reaches only the lowermost latitudes (about 50°N in latitude).*

If the GWs are removed in the winter hemisphere, the temperature in the summer mesopause region anti-correlates with the temperature in the winter stratosphere. Also, the temperature in the equatorial mesosphere does no longer correlate and co-vary significantly with the temperature in the winter hemisphere, in agreement with the results of Karlsson and Becker (2016)."

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.

The GW drag has now been inserted in figure 5. A comparison with the results of K+B16 has now been added.

l. 353- 361. *"Comparison between the responses found using WACCM with those found with KMCM (Karlsson and Becker, 2016, their Fig. 3), shows that the temperature change is larger and extends all the way to the summer pole in KMCM, while this is not the case in WACCM. Moreover, the change in temperature in this region is not statically significant in WACCM. The differences in temperature and zonal wind responses are larger in January than in July when comparing the results of WACCM with that of KMCM. Nevertheless, the qualitative structure of the temperature and zonal wind change due to turning of the winter GWs corresponds convincingly well."*

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.

It is right that there are more planetary waves in the NH winter. This means that there is a stronger Brewer-Dobson circulation in NH winter – thus a weaker zonal flow. This allows for the upward propagation of more GWs with an eastward phase speed, which reduces the westward GW drag. This results in a reduction in the strength of the winter-side mesospheric residual circulation, which causes an anomalous warming of the equatorial mesosphere as compared to the case where there would be less planetary waves in the winter hemisphere. This explains why the equatorial mesosphere is substantially colder in July than in January.

A warmer equatorial mesosphere leads to a positive temperature anomaly in the summer mesopause. Since the NH winter stratosphere zonal flow oscillates between being weak and strong, the equatorial mesosphere is modified continuously: it varies between being cooled and warm, so – if thinking about it in a more 'climatological sense' – the effect of IHC is not going to be as strong as for the SH winter, when the eqatorial region is constantly cooled by the strong residual flow. Taking away the GWs in the NH winter will have a smaller effect on the SH summer mesopause than taking away the GWs in the SH winter on the NH summer mesopause, as there is already less GW drag in the NH winter as compared to the SH winter.

Hence, the interhemispheric coupling mechanism gives a plausible explanation to why the July summer mesosphere region is considerably colder than the one in January. This is now clarified on lines 345-352.

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

This section has been rewritten:
l.393-407. *"In Fig. 1, it is seen that if there are GWs in the SH winter hemisphere the temperature in the winter stratosphere is positively correlated with the temperature in the NH summer polar mesosphere. This means that for a stronger Brewer-Dobson circulation (BDC) and the resulting anomalously warm (cold) temperatures in the stratosphere at 40°- 60°S, there will be also an anomalously warm (cold) temperature in the summer polar mesosphere.*

A strong or weak BDC results in a temperature change in the equatorial mesosphere, which changes the meridional temperature gradient in the summer mesosphere. As a result of the change in strength of the BDC, there is a change in the meridional temperature gradient as well, however, this gradient will have an opposite sign, as can be seen from Fig 1."

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.

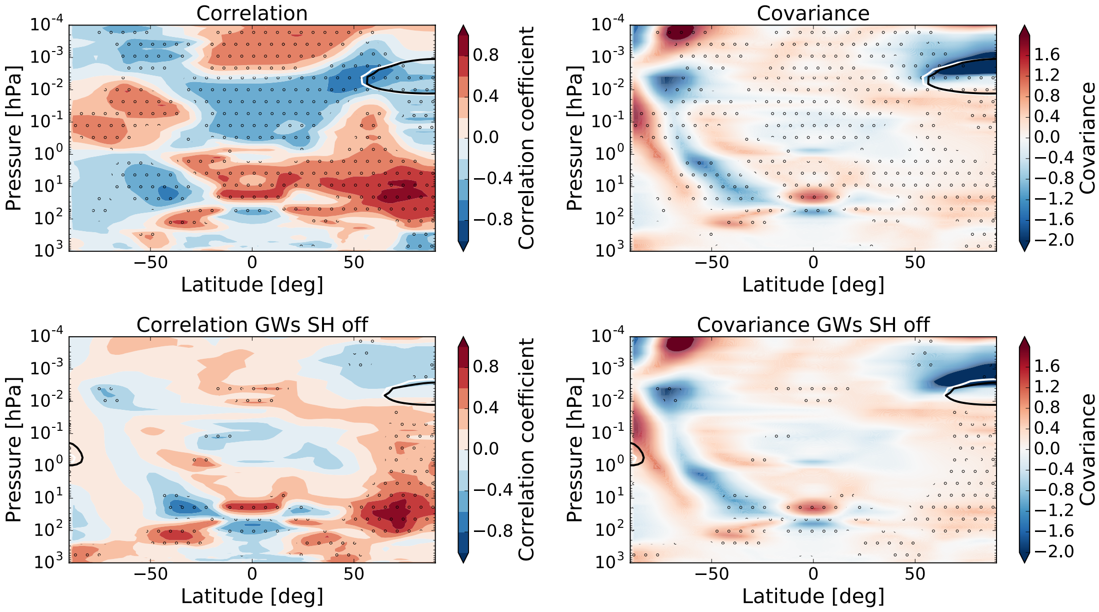
Section 3.1 has been rewritten. The introduction explains the purpose now hopefully more clear:

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."*

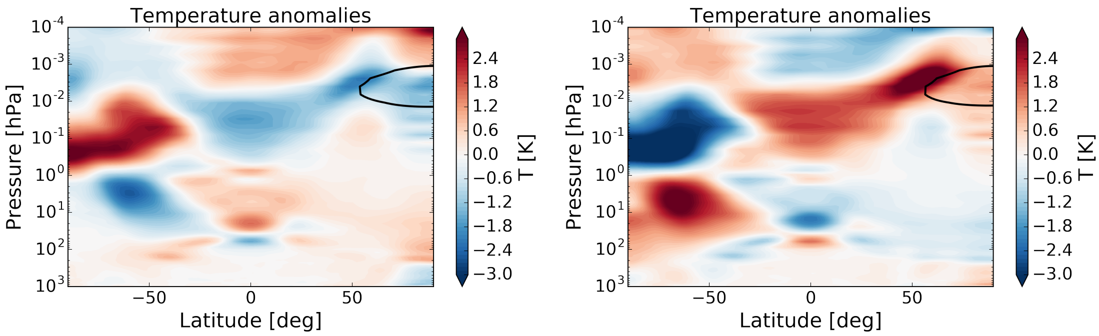
I would like to see the results when you correlate the summer stratosphere with the rest of the atmosphere similar to your figure 4 and 6. Furthermore a discussion of this topic is missing and should be included.

Below we include figures of correlations and composites studies that start out in the summer stratosphere. As can be seen, if there is variability in the summer stratosphere, this will indeed influence the summer mesopause. E.g. if we had a large variability in the year-to-year ozone heating, this would probably influence the summer mesopause via GW filtering. It is however not so easy to sort out what drives variability in the summer stratosphere. From the correlation plot, the IHC pattern jumps out even though the correlation point is set in the summer stratosphere (which by the way varies very little from one year to another, as confirmed by the composite studies below (anomalous T-fields).

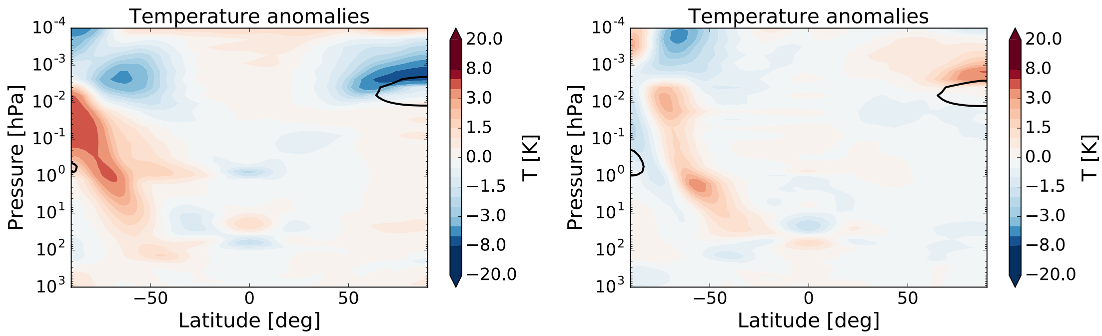
Hence, we argue that the variability (globally) is driven by PWs in the winter hemisphere: via the BDC the summer stratospheric temperatures are slightly modified and via the winter mesospheric flow, the summer mesopause temperatures are affected. Our point is to show that the temperature response to the variability in the summer mesopause really goes via the equatorial mesosphere, and not via the summer stratosphere. We can verify this by removing the GWs in the winter and show that the mesospheric response of the variability in the summer stratosphere has the opposite sign (see figure 2).



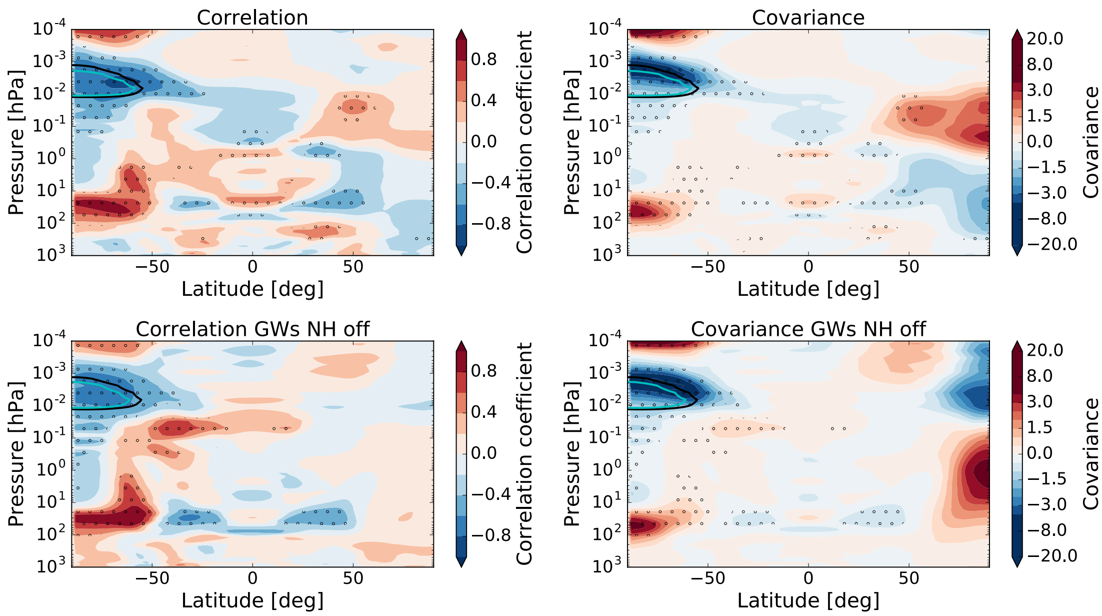
Correlations and covariance with the summer stratosphere (52°N-90°N, 1-100 hPa) in July.



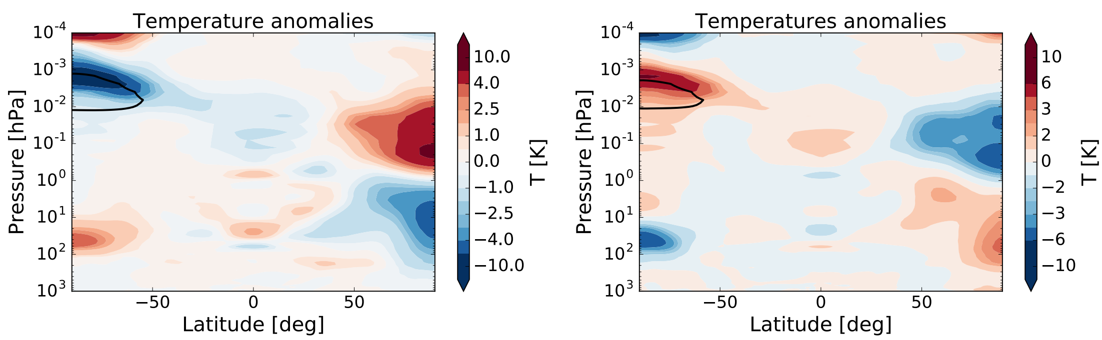
The temperature anomaly field for July taking the summer stratosphere (52°N-90°N, 1-100 hPa) as a proxy for the control case.



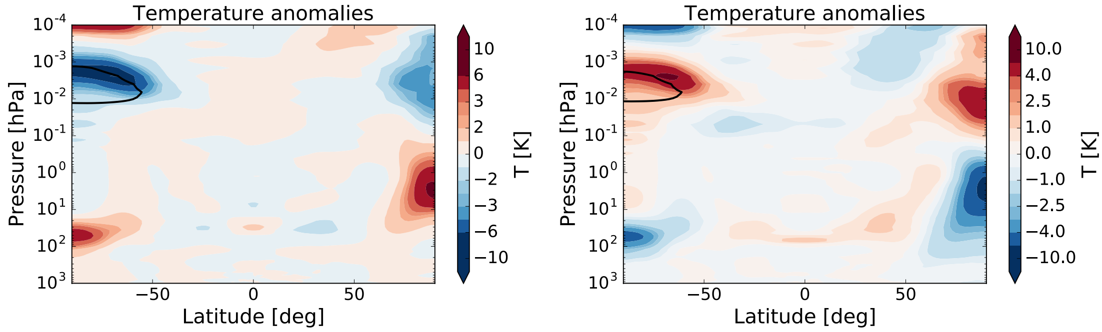
The temperature anomaly field for July taking the summer stratosphere (52°N-90°N, 1-100 hPa) as a proxy for the GWs in the SH off.



Correlations and covariance with the summer stratosphere (52°S-90°S, 1-100 hPa) in January.



The temperature anomaly field for January taking the summer stratosphere (52°S-90°S, 1-100 hPa) as a proxy for the control case.



The temperature anomaly field for July taking the summer stratosphere (52°N-90°N, 1-100 hPa) as a proxy for the GWs in the SH off.

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.

It may be true that it is possible to derive the information from Fig. 9 and 12 from Fig. 1 and 2, but it is not that easy to see. The profiles show what the point we want to make in this section. Comparing Fig. 9 and 12 also shows that that even though the signal is weaker in the SH, the general pattern of in the regions of interest are very similar.

Minor comments:

Line 34: ...(e.g., Fritts and Alexander, 2003)

l.35. *(e.g., Fritts and Alexander, 2003).*

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

I don't really understand what the reviewer means here. I stated that the IHC pattern manifests itself as a quadruple structure in the temperature fields in the winter hemisphere. In the sentence before this part I explain the temperature anomalies in the stratosphere. Then I have temperature anomalies in the mesosphere as well, otherwise it is not clear that there is a quadrupole structure. I reformulated this part, I hope it is clearer now.

l.55-62. "Its pattern consists of a quadruple structure in the winter hemisphere with a warming (cooling) of the polar stratosphere and an associated cooling (warming) in the equatorial stratosphere. In the mesosphere, these anomalies are reversed: there is a cooling (warming) in the polar mesosphere, and an associated warming (cooling) in the equatorial region. The mesospheric warming (cooling) in the tropical region extends to the summer mesopause (see e.g. Körnich and Becker, 2010)."

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.

The idea was to give first an introduction to the mechanism and give a quick qualitative discussion and then give a detailed discussion. But I agree it might be clearer if I change the structure. The text has been reordered.

Line 121: ..., with an anomalous cooling ...

This has been changed.

l. 111-115. *"In the case of an equatorial mesospheric cooling, the response is the opposite: the relative difference between the zonal flow and the phase speeds of the gravity waves increase to that they break at a slightly higher altitude, with an anomalous cooling of the summer mesopause as a result."*

Line 144: please insert: ... lower breaking GWs in the summer hemisphere and a warmer...

This has been inserted.

l. 129-133. *"Karlsson and Becker (2016) hypothesized that if the GW-driven winter residual circulation would not be present, the equatorial mesosphere would be warmer, which would lead to lower breaking levels of GWs in the summer hemisphere and a warmer summer mesosphere region."*

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.

l. 161-167, the text has been changed.

l.143-150. *"If – as hypothesized by Karlsson and Becker (2016) – the fundamental effect of the IHC is a cooling of the summer mesopauses, it would mean that the mechanism plays a more important role affecting the temperatures in the summer mesopause in the NH compared to that in the SH, since the weaker planetary wave activity in the SH results in an increased gravity wave drag and a strengthening of mesospheric poleward flow in the winter mesosphere. The equatorial mesosphere is adiabatically cooled more efficiently than when the winter mesospheric circulation is weak."*

For the part 167-171: I don't understand the objection the reviewer has against this formulation?

Karlsson and Becker (2016) hypothesized that in the absence of the equator-to-pole flow in the SH winter, the summer mesopause in the NH would be considerably warmer. Moreover, removing the mesospheric residual *circulation in the NH winter would not have as high impact on the SH summer mesopause.*

Line 256: Please add: ...parameterized GWs in the winter hemisphere.

This has been added.

l. 229-231. *"In the perturbation runs, the equator-to-pole flow is removed by turning off the parameterized gravity waves in the winter hemisphere."*

Line 268-270: Please insert a reference.

The reference has been added.

l. 247. *"This is because GWs in the winter hemisphere drive downwelling, adiabatically heating these regions (e.g. Karlsson et al., 2009)."*