

## Responses to the reviewers' comments.

**Note:** We repeat the reviewers' statements in grey, while our responses are typeset in black.

Changes made in the manuscript are highlighted in red.

### Reviewer 1

Following my previous comments, the authors revised the article, improved the description of the motivation for the study, and revised the discussion of the findings in the current version of the article. I am appreciated that the authors further prove the robustness of the statistical connection between the MJO and MA temperatures by showing the results in different cases, such as whether bandpass filtering is performed or whether SSW is considered. At the same time, the authors have revised the mechanisms by which the MJO may affect the middle and upper atmospheric temperatures and have discussed and speculated in more detail the possible roles of the MJO, SSW, QBO, and other atmospheric phenomena in Section 4. I only have some minor suggestions:

**Response:** We would like to thank reviewer 1 for taking the time to go through the manuscript again and review the changes we made. We appreciate that the reviewer has acknowledged the improvements of the manuscript achieved with the previous revision.

1 The MA response mentioned in the abstract can reach 10 K, but such a temperature response is mainly for the mid and high latitudes of the winter hemisphere, while the effect on the summer hemisphere via the IHC is much less significant, which needs to be clarified.

**Response:** This information was removed while shortening the abstract in the last iteration, but **we have now again added a respective sentence.**

2 As the authors mentioned, the MJO affects MA probably via the action of planetary waves. A simple analysis of planetary wave activity based on the observed temperature would be helpful, even if this paper does not intend to do a detailed analysis of the mechanism. The analysis of planetary wave activity can be very helpful in understanding the regions and processes occurring in the IHC.

**Response:** Also after careful consideration, we are not really sure, how a satisfying *simple* planetary wave analysis could exactly be conducted: It should not add much content to the paper, but at the same time it should be robust enough to remove the reviewer's doubts which are apparently raised by the present analysis. However, the reviewer classifies the points raised here as "minor" himself and there has no strict requirement for the present manuscript been formulated by the reviewer. Therefore, we treat this point as suggestion for our future work, which must then not be restricted to a *simple* planetary wave analysis. We would like to point out again, that we generally agree that the presented statistical connection should be further investigated also w.r.t to the underlying physics.

3 I also agree with another reviewer that we should not be limited to the composite analysis approach. As suggested by another reviewer, wave coherence should help understand the MJO-related MA IHC activity.

**Response:** As written in the last iteration, we also agree that further analysis approaches are of interest in the future, but still think that they would not fit very well into the present manuscript, which is still

required to be shortened with this revision. As before, we treat this point as suggestion for our future work, since the reviewer classifies himself the points raised here as “minor” and as there has no strict requirement for the present manuscript been formulated by the reviewer. By the way, many points raised by the reviewers in both iterations will serve as guidelines for our future work and we would like to thank both reviewers once more for the detailed comments.

## Reviewer 2

This paper studies the response of the middle atmosphere (MA, i.e. 261 hPa – 0.00046 hPa) to the MJO using daily MLS temperature for the period of 2004 to 2021. The MJO signature is studied using composite analysis for all the eight MJO phases. The same analysis is repeated for easterly and westerly QBO phases and for boreal and austral winters / summers. The paper the paper has benefited by considering of previous reviewers’ comments. The authors have done a relatively good job this round as it becomes easier to follow the motivation, the methods and the results. But I find the discussion section is unnecessarily long and hard to follow. I recommend the authors to consider the comments below before the paper is accepted for publication.

**Response:** We would like to thank also reviewer 2 for taking the time again to go through the manuscript and for the very detailed comments w.r.t to content and language, which we appreciate. It is obvious that the reviewer favors a different, probably much shorter, presentation style. While we see also the advantages of papers, which are optimized for information density, we also find that those are partly harder to read and often harder to reproduce. We chose the longer style intentionally for this manuscript, which possibly connects two different areas of atmospheric research, but will still try to further optimize it in the sense of the reviewer’s comments by including as many suggestions as appear compatible with the original style. We will specify this below in the individual answers.

Main comments:

1) The results shown in figure 6 are most interesting and possibly most robust as well. Although the magnitudes of the MA temperature anomalies are smaller than those in boreal winter, the statistical significance of those MJO signals in Austral winter is evidently higher, thus more believable in terms of the so-called “interhemispheric coupling”. The magnitude of the signal is also agreeable with existing literature, e.g. Karlsson et al. (2009a).

**Response:** We thank the reviewer for sharing this view on the results. We agree that the interpretation of the austral winter results appears to be clearer due to the weaker (maybe correlated and uncorrelated) variability during this season and area. We are not sure if this comment is meant to indicate a requirement for a change in the manuscript. And since the specific interest in individual parts of the results is probably different from reader to reader, we decided to make no (unrequested?) change in the manuscript. Still, we appreciate sharing this thought and we keep this view in mind for future discussions.

2) The MJO signal in MA temperature during boreal winter peaks at the north pole (Figs. 2-5) but during Austral winter, the largest temperature anomalies are found near the polar vortex edge, i.e. at 60S, e.g. Figs. 6-8. This characteristic difference should be mentioned in the text.

**Response:** This is actually already mentioned in the text (lines 437 to 439 in the reviewed manuscript), although it might have become shorter during the first revision. This feature is actually consistent with the IHC literature and therefore part of our reasoning that the description of IHC actually applies to our observations (lines 431 to 437). **Still, we have restructured these lines** (also in response to the specific comments), so that this feature is easier to notice.

3) The entire paper can be written much more concisely than its current form. There are a lot of repetition and detailed comparison with previous studies. In particular, Section 4, i.e. the discussion section is currently 7-8 pages. If the detailed comparison is removed, the same message can be delivered in two pages at most. Furthermore, some of the phrases, e.g. “more or less”, “roughly”, “probably”, or “appears” should not be used so frequently. Sentences that are unsupported by the results or only remotely related should be removed to improve the readership.

**Response:** Some aspects of the criticism, particularly w.r.t to section 4, seem to be to some extent contradictory to the judgement of reviewer 1 in his introductory text, where he writes “At the same time, the authors have revised the mechanisms by which the MJO may affect the middle and upper atmospheric temperatures and have discussed and speculated in more detail the possible roles of the MJO, SSW, QBO, and other atmospheric phenomena in Section 4.” Hence, the perception of the section seems to be not totally consistent. **Still, we can understand many aspects of the criticism of reviewer 2 and have gone through the whole manuscript again to bring it closer to the form recommended by reviewer 2. We will specify this also below in the specific comments.** Still, we can imagine that reviewer 2 might have had more drastic changes in mind, but we hope that the new form is now agreeable when considering that the overall style might be seen differently by different people.

4) The authors need to be very careful with their expression in terms of the statistical linkage between the MJO and the MA temperatures. Try to avoid using words such as influence, affect, or response. “Signal” is more appropriate.

**Response:** **We have used the word “signal” more often in this revision.** However, the other words mentioned by the reviewer do still appear in the manuscript, at least for reasons of variability in the wording. Therefore, **we have inserted a sentence at the end of the “Approach”-section (Sect. 2.2), which clarifies the meaning of all of these words.**

#### Specific comments

Lines 5-9, the word “influenced” is too strong as there is no mechanisms revealed by this study. I suggest changing to “We show that the MA temperature anomalies are significantly related to the MJO and its temporal development. The MJO signal in the zonalmean MA temperature is marked by a particular spatial pattern in the MA, which we link to the .... The signal with the largest magnitude is found in the polar MA during boreal winter in the order of  $\pm 10$  K when the QBO at 50 hPa is in its easterly phase”.

**Response:** **changed.**

Line 11, remove “found”.

**Response:** **changed.**

Lines 13-14, remove “Because of the wide coverage ... included dynamical features” as it does not add anything to the content.

**Response:** We prefer to leave these sentences in the abstract. We got some feedback from the modeling community that these aspects are actually of interest. Therefore, we think the abstract should contain a respective hint so that the paper can easier be found by researchers, who are interested in atmospheric couplings, but not specifically in IHC etc.

Line 45, “inner-tropospheric connections” -> “internal variability of the troposphere”.

**Response:** We wanted actually to refer to the inner-tropospheric connections mentioned in the paragraph before and not generally to “internal variability of the troposphere”. **We have rephrased our version of the sentence to “than the inner-tropospheric teleconnections mentioned before” to make our intention clearer.**

Line 82, remove “key parameter”.

**Response:** **changed.**

Line 89, “consider instead” -> “instead consider”

**Response:** **changed.**

Line 125, remove “Nevertheless, we might check our boreal summer results with a special BSISO index in future.”

**Response:** **changed.**

Lines 133-135, I wonder how the days are defined for the composite analysis. Are the temperature anomalies are estimated using 90-day or 10-day forward windows? I would recommend using a 91-day or 11-day windows if the temperature anomalies are estimated based on centred averages.

**Response:** We use indeed centered averages and we agree that an uneven window length would be a more symmetric choice. We used the even window lengths since they are more frequently used in the literature and therefore more directly comparable. Having additionally in mind that also window length changes greater than 1 day do not change the results much (as, e.g., stated in the public responses to the initial reviews), we think that a change from 90 to 91 days and from 10 to 11 days would be mainly aesthetically motivated, so that the effort to recompute all the results appears not to be justified.

**We have added the word “centered” in the respective sentence.**

Lines 141-142, “We take generally only days into account, during which the MJO strength was greater than 1” -> We only consider those days when the strength of the MJO is greater than 1.

**Response:** **changed.**

Line 143, citations to other MJO related studies are required here. Also remove “apparent on all considered days”.

**Response:** Citations inserted; Inserted some details on the common range of used MJO strengths; removed the phrase.

Lines 149-151, Again, it is not clear to me how the days in the MA temperature anomalies are defined, e.g. centred or forward from the day of a given MJO phase?

**Response:** We do not use temporal windows here. Instead, we have the MJO index and the temperature anomaly on a daily grid. Hence, for the epoch averaging, we have a 1-to-1 matching between the state of the MJO and the temperatures at this stage of the analysis.

Line 189, “if a we” -> “if we”

**Response:** changed.

Line 200, “present manuscript” -> “this study”

**Response:** changed.

Lines 201-202, “On the other hand, by considering previous publications, which overlap in particular aspects, it becomes expectable that a physical mechanism actually exists (i.e. that we do not describe a purely statistical artifact) as we will outline in Sect. 4.” This does not add anything to the manuscript; consider removing for better flow of the paper.

**Response:** removed.

Lines 204-214, these sentences can be either removed or placed in discussion section. They stop the flow of the paper.

**Response:** We have removed the lines 204-207. We have left the other lines in the manuscript, but moved them to the end of the results section, since we think that they still contain useful information for some of the readers. Readers that are more familiar with the topic can skip this very short subsection based on the subsection title.

Line 217, citations required after “during that season” to support the statement.

**Response:** We did not want to make a scientific statement here with supporting references, but only motivate the order of the subsections. We have rewritten the sentence, so that it sounds less scientifically motivated and added a reference to the discussion section, where all the references are found.

Line 230, “Put in other words, areas ...” can be dropped.

**Response:** removed.

Line 232, “whereas these four zones” -> “Whereas these two dipole patterns”.

**Response:** changed.

Line 234, “the first four zones” -> “the quadrupole pattern lowerdown”

**Response:** changed.

Line 235: “we will call it temporarily” -> “we denote these MA anomalies temporarily to”

**Response:** changed.

Line 236: “in the following” -> “in sect. 4.1”. And remove the sentence after.

**Response:** This change would be incorrect, since we use the term in many more sections and not only in Sect. 4.1. We have left this phrase as it is.

Line 254: “the temperature signals” -> “the MJO signal in the MA zonal-mean temperatures”

**Response:** changed.

Lines 259-260: “It is obvious the temperature anomalies are even stronger than ...” -> “It is evident that the temperature anomalies in the boreal winter are of larger magnitude than those shown in Fig. 2.”

**Response:** changed, but corrected to in the boreal winter with QBO easterly conditions.

Line 264: “Important is” -> “The importance is”

**Response:** changed.

Line 274: “the negative polar winter anomaly” -> “the cold anomalies in the boreal polar winter”

**Response:** changed.

Lines 290-295, I am completely lost from “However, ...” onwards. Consider rephrasing.

**Response:** We have shortened and completely rephrased the passage. Basically, the content of the comment below by reviewer 2 (w.r.t the lines 363-369) was explained in these lines of the manuscript. But we agree that this was difficult to understand and we have used some words of the reviewer’s comment below to make it clearer.

Lines 296-305: Condense and move these sentences to Discussion / conclusion section, which would help the follow of the main results.

**Response:** done.

Line 310: I suggest further condensing the discussion in this section or merge it with section 3.3. The five-zone response is only recognizable during MJO phases 5 and 7 (with opposite signed signals to phase 5). There is no clear evidence in terms of signal descent.

**Response:** We have considerably shortened this section and merged it with the above section.

Line 356: remove “(whereas it was ...)” to get the main message across better.

**Response:** removed.

Lines 360-363: the gradual descent of the temperature could start in MJO phase 2 in the Austral winter. The signal intensifies during phases 3-4 while they move downwards. Another cold anomaly zone then appears at 40-50S, 0.02-0.07hPa during MJO phase 5 and then gradually descends to the lower levels.

**Response:** Yes, one could also start with phase 2. Of course, one could in principle start with any MJO phase, if it were a really cyclic transition. In any case, **we have added a sentence to give the reader the hint that starting with MJO phase 2 lets one see the transition particularly easy.**

Lines 363-369: the abrupt pattern reverse shown in Figure 4 between MJO phase 4 and 5 might be just an artefact, e.g. the signal in the MA during phase 4 is rather weak and statistically insignificant.

**Response:** That is true and was already written higher above in the manuscript (lines 290-295), although obviously hard to understand. **We have rephrased both paragraphs for more clarity in this respect.**

Lines 386-389: “in both directions” -> “in terms of two aspects”. Then: 1) to provide a broader picture that integrate and interrelate current and previous studies; 2) to put forward some possible physical explanation in terms of the responsible mechanisms.

**Response:** Changed, although not totally identical with the reviewer’s suggestion.

Line 395: be specific about “first publications”.

**Response:** done.

Lines 394: It would be very helpful to properly state the mechanism of the IHC if it is identified to be the most important mechanism that is responsible for the MJO signal in the MA temperature. Changes in planetary wave drag is not enough to explain IHC. Gravity wave drag must be involved.

**Response:** We have actually removed a more detailed description of the IHC mechanism in the first revision to meet the criticism by the reviewers that the manuscript reads too much like a review paper. Additionally, since the literature for the IHC mechanism consists of only a few established papers, it is simple for the readers to find the information themselves based on the references (**which we have more explicitly specified with the above point**), so that we are now ourselves in favor of not repeating the mechanism here. **Still, we have mentioned that gravity waves play a role.**

Lines 419-439: need to be much condensed to bring out the key message. It is currently too detailed to grasp the main idea that the authors want to deliver.

**Response:** **We have shortened and restructured this part.** We note that this part already contains the information that reviewer 2 requests in his main comment 2. We have put this information now into the beginning of these lines.

Lines 485-486 and line 491: No evidence provided for the MJO influences on the PW activity in this study. These statements are pure speculation and should either be removed or rewrite as hypothesis.

**Response:** We note that we have started this subsection quite cautiously in order to make the hypothetical character of the criticized statements clear (The title of subsection contains the word

“potential” and the introductory sentence starting in line 483 directly before the criticized lines reads “Although not proven by our statistical approach, the MJO appears to act as a source of the initial PW disturbances”). **Nevertheless, we have rephrased the criticized lines to avoid the impression that an active role of the MJO in controlling PWs was proven in our analysis.**

Lines 497-505: my quick examination of Fig. 2 signal suggests that the temperature responses in the boreal winter stratosphere is almost entirely agreeable to the finding of Wang et al. (2018a). Note that stratospheric response may involve one month lag in relation to tropospheric wave forcing. I do agree that this work adds additional information in terms of mesospheric responses, which should be emphasised. The apparent downward descent of the high latitude anomalies is just dynamical response of initial wave forcing. Thus, there is no need to make such a lengthening discussion.

**Response:** We have also stated that a general consistency with Wang et al. (2018a) is apparent and only discussed possible gaps in the explanations of Wang et al. (2018a) in the following lines. Overall, we have therefore difficulties in applying the criticism exactly to our manuscript. **Still, we have shortened the paragraph a bit and weakened the emphasis on the discrepancies.**

Lines 562-564: Remove “This can be seen ...” to improve the readership.

**Response:** removed.

Lines 565-579: section 4.5.2. I do not think that this section is needed or adds any new information. The QBO influence on the stratospheric polar vortex is on the seasonal time-scale while the MJO influence is sub-seasonal. The mainly reason that the boreal winter signal of the MJO becomes stronger during eQBO is the MJO becomes more active, and deeper during eQBO. Thus, there is a stronger poleward propagation of wave activity during MJO phase 4, which induces more planetary waves entering the winter stratosphere, as it has been reported by Wang et al. (2018a).

**Response:** In other parts of the paper, reviewer 2 was missing physical evidence for statements, which we made based on statistical results. Here, we have discussed two physical possibilities (subsections 4.5.1 and 4.5.2, respectively), which we can statistically not distinguish (as stated in Sect. 4.5.3), but which could in principle both contribute to the statistically found QBO influence on the MJO-MA signal. We understand the criticism of reviewer 2 such that option 1 (subsection 4.5.1) should physically be the relevant one, so that the mentioning of option 2 can be omitted. We also think that option 1 is important, which is the reason why we have presented option 1 as actually the first option. However, we feel that excluding option 2 is not justified without further evidence for its irrelevance and must therefore be mentioned as possible mechanism as long as there is no particular reason against it. This becomes even more important as both mechanisms could actually contribute at the same time and the individual contribution strengths should be quantified in future. Only if such a quantification reveals that option 2 has indeed a neglectable contribution, this option could be safely dropped.

Line 615: “For the sake of brevity ...” This statement should be in methodology or introduction section. Not in conclusion. If it has already stated, remove it.

**Response:** removed.

Line 635: no evidence provided for this, i.e. the MJO can lead to the initial planetary wave drag disturbances of the IHC mechanism. As far as I understand, part of the IHC mechanism involves

changes in gravity wave drag not just planetary waves. I would recommend remove this paragraph entirely as it has been discussed in Section 4 in detail.

**Response:** We have left the paragraph in the summary; however, we have removed any reference to an alteration of PW drag. Hence, we now only claim that we have presented statistical results that the MJO can trigger IHC, but without referring to the physical mechanism, which we have indeed not proven. With that the paragraph has also become shorter. In addition, we have called the last section “Summary and conclusion”, so that a very condensed repetition of the discussion in Sect. 4 is intended. We just want to briefly mention that, of course, also gravity wave drag is important for the understanding of IHC. However, in the manuscript, we only refer to the initial disturbance, which starts the chain of dynamical effects und this initial disturbance is related to planetary waves.

Lines 655-659: This is not true. During winter, the stratospheric polar vortex descends with time climatologically. Once it is disturbed by a large-enough wave forcing, the associated warm temperature anomalies would also descend with time. On the timescale of the MJO, the descent of stratospheric anomalies in the polar region is thus fully expected as long as the initial wave drag is sufficiently large. The downward descent of polar vortex anomalies is one well-known pathway whereby the stratosphere influences the troposphere.

**Response:** We think that the general rejection of these lines by the reviewer 2 is based on a misunderstanding: Of course, we agree that a descending pattern is a well-known feature of the winter stratosphere. We wanted to put emphasis on the point that the descent has not been described in the context of IHC or the MJO influence and still think that some open questions are connected to this. In any case, we have rewritten the lines and weakened that statement to avoid such misunderstandings.