Response to reviewer comments for "Warm conveyor belt activity over the Pacific: Modulation by the Madden-Julian Oscillation and impact on tropical-extratropical teleconnections"

Re-submission of revised manuscript (egusphere-2023-783)

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Reviewer 1

The authors investigate the sensitivity of the warm conveyor belts activity in the North Pacific to the different Madden-Julian Oscillation phases and El Niño Southern Oscillation, how the tropics-extratropics teleconnection associated withe MJO is modulated by the WCB activity, and if an idealised general circulation model can reproduce the teleconnection and its modulations. Overall, the manuscript is interesting and well written, the methods used appropriate, and the topic fits the scopes of the journal. My main remark concerns the idealised simulations, which would greatly benefit from a better description. I would recommend publication after minor revisions associated with my comments listed here after, following the order of the manuscript.

Dear Reviewer,
We are very grateful for your positive and constructive feedback. We indeed agree that the description of the idealised simulations was lacking some detailed description. In the revised version of the manuscript, we provide a more detailed description and modified the manuscript as suggested in the minor comments. Please find our responses to your comments in blue. For your convenience, we also uploaded a track-changes version of the manuscript.
Kind regards,
Julian Quinting on behalf of all authors

Line 42: Could the following more recent references also be appropriate here? Jiang et al. (2016): The relationship between the Madden–Julian Oscillation and the North Atlantic Oscillation https://doi.org/10.1002/qj.2917
Fromang and Rivière (2020): The Effect of the Madden–Julian Oscillation on the North Atlantic Oscillation Using Idealized Numerical Experiments https://doi.org/10.1175/JAS-D-19-0178.1
Many thanks for your suggestion. These two references are very appropriate and we included them in the manuscript.

Lines 60-61: "Warm conveyor belts (WCBs) are rapidly, mostly poleward ascending airstreams in the storm track regions..." As written in the next sentence, WCBs originate in the warm sector

of the cyclones. Therefore, I suggest to replace "in the storm track regions" with something like "within mid-latitudes cyclones" or "within mid-latitudes cyclonic systems".

We absolutely agree and replaced it with "within extratropical cyclonic systems".

Lines 106-107: Could the authors provide an explanation about the "matching" between WCBs (or the trajectories) and the cyclones mask? What is performed at this stage is no clear. Does it select all ascending trajectories starting within the cyclone mask? Is there also a warm sector mask? Is it assumed that all ascending trajectories overlapping a cyclone mask will start in the warm sector?

Indeed, our explanation was not entirely clear. The matching means in this case that at any time of the 48-h period a trajectory needs to be collocated with a cyclone mask. Accordingly, it is assumed that all of these trajectories will start in the warm sector. We modified the text accordingly. It now reads "After calculating the forward trajectories from all seeding points, WCBs are defined as all trajectories that ascend by at least 600 hPa in 48 h and are collocated with an extratropical cyclone mask, taken as the outermost closed contour around local sea-level pressure minima (Wernli and Schwierz 2006), at least once during the 48-h period."

Section 2.4: It is not clear how the MJO days/events are treated. Are the authors detecting events with the first day of the event being day 1 of pentad 0 (or lag 0)? Are the authors using all days with RMM index greater than 1 as first days of pentads (or lag0)? Please clarify.

We, the author-team, had exactly the same discussion during the preparation of the manuscript. So, many thanks again for bringing up this point and for highlighting that our explanation is still not clear. Your understanding is almost correct. The first day of an event is day 0 and we consider all days with RMM index amplitude greater than 1 as the first day of pentads. We revised the manuscript and strongly hope that text is clearer now: "Pentad 0 indicates the average of days 0–4 following each day in the considered period with an RMM index amplitude greater than 1, pentad 1 corresponds to days 5–9, and so on."

In addition, no indication is given on the number of events or days considered in each composite, especially Fig. 1. Please provide these numbers.

We included these numbers in the figures of the revised version of the manuscript.

Section 2.8: I find that the description of the idealised simulations could be improved:

- It seems that the imposed heating in the tropics or extratropics covers all longitudes and not only the specific range of longitudes associated with the MJO phases. Is it true? If yes, the authors should explain why they did that and how this is a "realistic heating".

  The detailed information concerning the heating was indeed missing. We now explain that the tropical heating covers all longitudes. We also mention that "by applying heating at all longitudes, we intend to represent the heating anomalies as realistically as possible rather than focusing only on the region of the active MJO". Further, we would like to emphasize that we also conducted experiments with heating in the North Pacific region only. For these additional experiments, the results are very similar.

- The latitudinal range of the heating anomalies should be provided.

  We appreciate your comment. The tropical heating is imposed at all latitudes between 20° S to 20° N and the extratropical heating is applied poleward of 20° N/S. We provide this information in Section 2.8 of the revised manuscript.

- Line 195: In several places, I found the use of "diabatic heating" confusing as the authors use a dry atmospheric model. Here, I would replace "The diabatic heating is derived from

ERA-Interim precipitation anomalies" with "The imposed heating mimics the diabatic heating derived from ERA-Interim precipitation anomalies".

Thank you very much for this important remark. We changed the wording as suggested and also carefully checked the manuscript for other occurrences.

- Line 201: same comment as above for "anomalous diabatic heating imposed". The authors impose a heating, not a diabatic heating.

  We absolutely agree and removed the word "diabatic".

- How long are the simulations? 10 days to get two pentads?

  The simulations are run for 15 days (three pentads) though only the first two pentads are investigated in this study. We provide this information in the revised manuscript: "After that, we run the model ensembles out to 15 days with the anomalous heating added only during the first five days of each model run and switched off afterwards."

Line 223: "into upper-tropospheric ridges": The ridge does not seem well pronounced especially for phase 3 and there is no positive anomaly of Z300 (contrary to what is seen for phase 7 in Fig. 1l). Please check your sentence.
Our wording was indeed not very accurate as we used the terms "ridge" and "positive Z300 anomaly" synonymously. We modified the text and now write explicitly that the outflow is directed into upper-tropospheric anticyclonic anomalies. This description corresponds better to the figure where positive outflow frequency anomalies (Fig. 1b,e) are collocated with positive geopotential height anomalies (Fig. 1c,f).

Lines 223-224: It is not clear what the authors mean with this sentence, especially as there is no notion of velocity in Fig. 1, and how to relate what it says to Fig. 1.
We agree that it would need a temporal sequence of figures to actually show the rapid eastward advection of outflow with the upper-tropospheric jet. Accordingly, we decided to remove this sentence from the manuscript.

Line 225: Figs. 1g-k − > Fig. 1g,h,j,k
Done.

Line 263: Here, I would refer to Fig. 2 instead of Fig. 1 as the authors are now using the CNN-based data.
Thanks for spotting. We changed the text accordingly.

Line 275: I would add that in addition to being weaker, the anomalies are also less persistent for phases 2 and 3 especially.
We very much agree and changed the text accordingly.

Longitude/latitude values (lines 321, 333, 336): these locations are a bit difficult to identify on the figures as there are no lon/lat labels on the figures. Please consider adding some or add in the captions what the grid lines refer to.
We very much agree and now include latitude/longitude labels in Fig. 6.

Line 330: "enhance the ascent of WCBs" − > "enhance the ascent frequency of WCBs".
We changed the manuscript as suggested.

Line 354: "anomalous ridge" I do not see a ridge. Do the authors mean a positive anomaly?

Our description was indeed not very precise. Here, we are referring to the positive anomaly. We modified the manuscript accordingly and now refer to the "anomalous anticyclonic anomaly".

Line 361: "of the Azores high," Please add a reference to Fig. 7a,c and Fig. 7d for the high WCB activity.
We added a reference as suggested.

Line 362: Add reference to Fig. 7b.
We added a reference as suggested.

Line 399: "diabatic" In line with a previous comment above, I find the use of "diabatic heating" here confusing.
We very much agree and removed "diabatic" here.

Line 400: "The diabatic heating". It is not clear here to which data the authors refer to. I suspect they write about the reanalyses (and not the idealised simulations). If so, change the start of the sentence to "In ERA-Interim, the diabatic heating ....". Otherwise, clarify.
Here, we are indeed referring to the reanalyses. We changed the manuscript as suggested.

Lines 407-408: It is not clear to me what the authors mean here. Do they add heating in all areas with anomalous precipitation in ERA-Interim or at all longitudes regardless of the precipitations anomalies? In the first case, what threshold in precipitation anomalies is used (>0)? Please clarify here and in the methods section.
The heating is applied in all areas with positive precipitation anomalies. Accordingly, the threshold is set to 0. We added this information here and in the methods section: "By applying heating in all areas with positive precipitation anomalies, we intend to represent the heating anomalies as realistically as possible rather than focusing only on the region of the active MJO". The precipitation anomalies are now shown in the supplementary Figures S5–S8. Still, we would like to emphasize that we also conducted experiments where the heating was limited to the midlatitude North Pacific region. The results of these additional experiments are very similar.

Lines 428-429 and 436-438: the authors attribute the differences between ERA-Interim and the idealised simulations to the different state of the atmosphere at the initial time between the simulation and the reanalysis. Could not the different Z300 anomalies be also linked to the atmospheric internal variability, ERA-Interim being just one possible realisation? Have you found an ensemble member giving the same result as ERA-Interim (here Fig. 7f but applies to all composites shown in Fig. 7)?
We absolutely agree that the different Z300 anomalies may be linked to atmospheric internal variability and ERA-Interim being just one possible realisation. Indeed, Zheng and Chang (2019) showed that individual members may have their own MJO extratropical response pattern with the strongest signal in slightly different geographic locations. Then, the ensemble mean signal will be weaker than individual members and weaker than ERA-Interim. In order to answer directly to your comment, we have investigated individual responses to the same heating following MJO phase 6 events with high WCB activity. Though the ensemble mean response to phase 6 heating with high WCB activity is a ridge off the California coast (Fig. 9e), there are individual members with the response being a very strong ridge near Alaska, looking much closer to (or even stronger than) the response found in ERA-Interim (Fig. 7f). These results support our claim that a potential reason for the difference between our model results and those based on ERA-Interim could be related to differences in the background flow, but also — as you suggested — due to internal variability. We have included these additional results in Fig. 1 of this document and write in the revised paper: "Nevertheless, for individual members of the
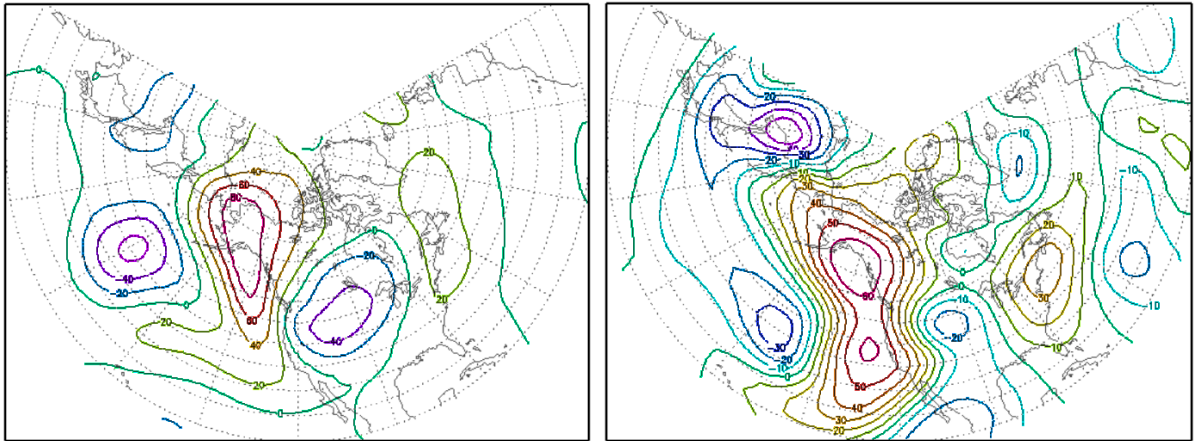
Figure 1: Two individual members of the 100 member ensemble showing a strong positive 300-hPa geopotential height anomalies near Alaska during pentad 2 of the simulations.

ensemble experiments positive geopotential height anomalies are observed over western North America with similar magnitude to that seen in ERA-Interim (not shown)".

Line 494: "diabatic heating": again the model is forced by an anomalous heating somewhere and not by diabatic heating because diabatic heating does not exist in a dry model, does it? Please rephrase.
We changed the wording accordingly. Thank you.

Figures 3 and 4: I suggest to merge Figs. 3 and 4 to make the comparison between them easier. If they are not displayed on the same page, one cannot look at them at the same time and the comparison is difficult.
Many thanks for this suggestion. Though we initially planned to merge Figs. 3 and 4, the second reviewer also suggested to include the difference between La Niña and El Niño in Fig. 4. By doing so, the two figures unfortunately do not fit on a single page so that we would prefer to keep them separately. We very much hope for your understanding.

References

Zheng, C., & Chang, E. K. M. (2019). The role of MJO propagation, lifetime, and intensity on modulating the temporal evolution of the MJO extratropical response. Journal of Geophysical Research: Atmospheres, 124, 5352–5378. https://doi.org/10.1029/2019JD030258

Reviewer 2

This study investigates the role of warm conveyor belts in modulating the teleconnection patterns associated with the MJO. A key result is that the link between the MJO and the NAO is substantially moderated by the frequency of occurrence of WCBs. Overall I find the manuscript well-concieved, clearly written, and scientifically sound. I recommend publication with minor revisions. Comments below.

Dear Sugata Narsey,
We are very grateful for your positive and constructive feedback. As pointed out also by the first reviewer, we agree that the analysis of the idealised simulations was lacking some detailed information. In the revised version of the manuscript, we incorporate your detailed feedback and modify the manuscript as suggested in the minor comments. Also, we decided to add supplementary figures in order to respond adequately to your comments. Please find our responses to your comments in blue. For your convenience, we also uploaded a track-changes version of the manuscript.
Kind regards,
Julian Quinting on behalf of all authors

Main comments

- The focus on limited MJO phases was useful but why not group them (phase 2–3, phase 6–7) for simplicity of presentation? It seems unnecesary to focus on each phase individually.
  Thank you for this question. Indeed, in our initial analysis we grouped phases 2–3 and phases 6–7. However, we soon realised that the variability of downstream teleconnection patterns varies greatly between individual phases when stratified according to the North Pacific WCB activity. For example, when considering periods with high WCB activity over the western North Pacific it is only Phase 3 which is followed by a significant increase in the frequency of NAO+ (first column Fig. 8c). Such increase does not occur after phase 2 with high WCB activity. Likewise, Scandinavian Blocking occurs preferentially after phase 2 with high activity but not after phase 3 (third column Fig. 8c). Accordingly, we would prefer to consider the phases individually and to not change the figures.

- The use of idealised experiments is really interesting, and should be insightful but I struggled to understand the patterns described. The results of fig 7 and fig 9 were quite distinct. A little more text elaborating the differences may be helpful.
  This comment is in line with some comments of Reviewer 1 who suggested that the different Z300 anomalies in ERA-Interim and the experiments may be linked to atmospheric internal variability and ERA-Interim being just one possible realisation. Indeed, Zheng and Chang (2019) showed that individual members may have their own MJO extratropical response pattern with the strongest signal in slightly different geographic locations. Then, the ensemble mean signal will be weaker than individual members and weaker than ERA-Interim. To confirm this hypothesis, we investigated individual responses to the same heating following MJO phase 6 events with high WCB activity. Though the ensemble mean response to phase 6 heating with high WCB activity is a ridge off the California coast (Fig. 9e), there are individual members with the response being a very strong ridge near Alaska, looking much closer to (or even stronger than) the response found in ERA-Interim (Fig. 7f). These results support our claim that a potential reason for the difference between our model results and those based on ERA-Interim could be related to differences in the background flow, but also due to internal variability. We have included these additional results in Fig. 1 of this document and write in the revised paper: "Nevertheless, for individual members of the ensemble experiments positive geopotential height anomalies are

observed over western North America with similar magnitude to that seen in ERA-Interim (not shown)".

- While it seems clear that there is a forward lag relationship between MJO and large-scale extratropical states modulated by WCB activity you only look forwards, which implicitly suggests a causal relationship. Are there no backward lag relationships?
  We very much appreciate this comment. Indeed, most earlier studies as well as our study consider forward lag relationships. This is due to the intention of using the state of the MJO as a predictor for downstream midlatitude flow evolution. However, backward lag relationships exist, as is for example shown in Figs. 3 and 4. To provide the full picture including backward lag relationships, we now added supplementary information which included lagged composites of geopotential height as well as lagged composites of precipitation indicating the heating anomalies used later on in the experiments with the primitive equation model. We generally try to be careful with statements on causal relationships when interpreting the lagged composites. The experiments with the primitive equation model are one suitable approach to make statements about causality. Thus, we are actually confident to talk about a causal relationship after having evaluated the experiments with the primitive equation model.

- The substantial intra-MJO phase variability of WCB activity is not explained. Is this unavoidable weather noise, or is it also predicted by large-scale states of the atmosphere? e.g. the modes you considered.
  Section 3.3 of the manuscript aims to answer the question concerning the intra-MJO phase variability of WCB activity. Through composite analysis we find that poleward moisture transport over the western Pacific due to the subtropical anticyclonic Rossby gyres is stronger during events with high WCB activity. We hypothesize that once the moist air masses start to ascend along sloping isentropic surfaces, moist diabatic processes further enhance their ascent. Still, the question remains whether this variability is predictable by slower modes. Though we do not have a definitive answer to this, the results suggest that the variability would in principle be predictable by the MJO if the MJO indices accounted for the structural differences with regard to the Rossby gyres.

Specific comments

Line 8: It's not clear to me what aspect of La Nina enhances poleward moisture fluxes. Is it dynamic or thermodynamic? The Rossby gyres associated with MJO should exist regardless?
Concerning the second question, we very much agree that the Rossby gyres associated with the MJO exist during La Nina as well as during El Nino. However, we found in line with previous studies (e.g., Toride and Hakim 2021) that moisture fluxes, AR activity and WCB activity are stronger during La Nina over the Pacific. The mechanisms for this behaviour have not been investigated yet (at least to our knowledge) and an in-depth analysis would go beyond the scope of our study. We slightly re-phrased this sentence in the abstract so less emphasis is put on the state of ENSO.

L149: detrended ONI?
The ONI data are detrended in a sense that anomalies are calculated relative to a 30-year base period for successive 5-year periods in the historical record. This is the procedure of the Climate Prediction Center (`https://origin.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ONI_change.shtml`). We now specify in the text that we use detrended data.

Section 2.7: I don't think you have mentioned in the text anywhere the sample sizes. Can you put that detail here? especially when subsetting beyond MJO phase.

Thank you very much for this suggestion which is in line with a comment by the first reviewer. Thus, we now provide the sample sizes in the corresponding figures (Figs. 1, 2, 6, 7, 8).

L202: Can you show somewhere the pattern of anomalous heating applied? Since you derived these using composites of MJO phase, wouldn't these heating anomalies derived from ERA-Interim also implicitly contain a signature of the resulting WCB and other teleconnection associated convection?

Thank you. We decided to resubmit the manuscript with supplementary information which includes figures of the precipitation anomalies from which the heating was derived (Figs. S5–S8). These clearly show the heating associated with the MJO and with WCBs over the Pacific. Concerning the second question, this is exactly what we would like to achieve. Instead of only applying heating in the tropics, the novel aspect is to also account for midlatitude heating. In the revised version of Section 2.8, we now provide a more detailed description of the experiments which should avoid misunderstandings.

L211: can you remind us how many events?

As noted above, this information was indeed missing. We provide the sample size in the corresponding figures.

L247: But the signal over north Atlantic is quite a bit stronger in the CNN approach. Why? Is there an obvious reason why the col 3 in Fig 1 is not shown for CNN method in Fig 2?

Understanding the reasons for the stronger anomalies in the North Atlantic region with the CNN approach would require careful experiments with the CNN. Since the modulation over the Atlantic is not the main focus of this study, we here only hypothesize what the reasons might be. One reason for differences between the trajectory and CNN approach could be that the CNN models do not explicitly match WCB candidates with extratropical cyclones which the trajectory approach does. Accordingly, the CNN models may identify also some WCB outflow objects that do not overlap with a cyclone mask and are therefore filtered out in the trajectory-based WCB data set. We briefly comment on this in Section 3.1 of the revised manuscript and give potential reasons for the discrepancies between the trajectory- and CNN-based approach: "Only after MJO phases 2 and 6 do the outflow anomalies show a greater amplitude. We attribute this to the fact that the CNN models do not explicitly match WCB candidates with extratropical cyclone masks (Quinting et al. 2022) which the trajectory approach does (Madonna et al. 2014). Accordingly, the CNN models may identify also some WCB outflow objects that do not overlap with a cyclone mask and are therefore filtered out in the trajectory-based WCB data set". The reasons for not showing col 3 was that this would have been the same as col 3 in Fig. 1. Still, we decided to now include column 3 in Fig. 2 of the revised manuscript.

L257: is it an illusion, or do the WCB freq anomalies appear to propagate against the MJO phases? Diagonal structure in Fig 3 and 4. And do the outflow freq show the same structure?

Figs. 3 and 4 are not including any longitudinal information so that a propagation signal can not be inferred based on these figures. The figure is rather showing that a lead-lag relationship exists between MJO phases and WCB activity. For example, Fig. 3a indicates that WCB acitvity over the West Pacific is significantly enhanced –10 to –5 days prior to MJO phase 5. With a typical duration of 5 days per MJO phase, such a lag corresponds to phase 3.

L287: not sure you've justified the use of the word "linearly" here... are you basing this on qualtitative comparisons of fig 3 and 4, or quantitative comparison e.g. is 4c minus 4a very similar 4d minus 4b?

Thanks very much for this question. Our description was indeed not very precise. Our main intention is to say that La Niña leads to an overall increase of WCB activity which then strengthens

positive anomalies and weakens negative anomalies. This overall higher WCB activity during La Niña can now be seen in Figs. 4e, f which are characterized mostly by negative anomalies especially after lag 0. We include this information in Section 3.2: "Accordingly, the modulation of WCB activity by the MJO is primarily linearly influenced by ENSO as can be seen by mostly negative values in Fig. 4e and f)".

L306: what fraction of WCBs are ARs? or are WCBs a subset of ARs?
Sodemann et al. 2020 provide an estimate of the simultaneous occcurrence of WCBs and ARs. They find that during DJF on average 6.71% (5.84%) of the Northern Hemisphere is covered by ARs (WCBs). The area that is covered simultaneously by both WCBs and ARs amounts to only 0.76%, i.e., it is 13% (100%.*(0.76/5.84)) of all WCBs that occur at the same location and time as atmospheric rivers. We explain this relation between WCBs and ARs in a footnote in the revised manuscript.

Section 3.5: Are WCBs quanitatively and qualitatively comparable in the idealised GCM and the real world?
As explained in Section 2.7, the idealised GCM is a dry-dynamical model which does simulate cyclones and anticyclones. WCBs, however, are characterised by an average latent heat release of 20 K in 48 hours (Madonna et al. 2014). Accordingly, WCBs would not be represented as a weather system in the idealised GCM which might be regarded as a disadvantage. However, representing WCBs qualitatively and quantitatively in the idealised GCM is also not the goal of our analysis. Instead, we aim to show that the average heating anomalies which typically occur in the core WCB ascent regions importantly modulate MJO teleconnection patterns. Since diabatic heating associated with the WCBs are not represented in the model, heating anomalies are added as forcing in the model to mimic the diabatic heating associated with WCBs.

L400: Perhaps worth reminding us at this point what the main approximations are in this idealised GCM?
Thank you for this suggestion. In the revised manuscript, we now mention a major approximation which is the way the heating anomalies are generated. The temporal averaging of the precipitation anomalies (and thus also of the heating) over one pentad, results in a longer temporal scale and a smaller amplitude of heating rates compared to that associated with actual WCBs.

L404: "not shown" I think you base statements on this point in the conclusions. If its so important then why not show it?
Thank your for this comment which motivated us to include additional material in the supplement. We now show the precipitation anomalies with high and low WCB activity in supplementary material (Figs. S5–S8).

L453: Are they really separable like this? If you take identified ARs and WCBs do WCBs start where ARs end? Or are they just focusing on different parts of the same feature (poleward advection) of extratropical cyclones?
ARs and WCBs are not the same phenomena and should be distinguished. WCBs often occur at the end of ARs, which leads to a depletion of the horizontal moisture transport in the ARs. The reason is that WCBs are defined as rapidly ascending airstreams. This ascent is accompanied by a strong moisture reduction because of rain-out and, therefore, most parts of WCBs do not fulfill the AR criteria. Correspondingly, the AR criterion does not include any ascent information, so that ARs do not fulfill the WCB criteria of 600 hPa ascent in 48 hours. Sodemann et al. 2020 provide a detailed discussion of this question. We hope that the footnote in Section 3.3 is helping the reader in this regard.

L488: Here and elsewhere in the conclusion section, can you repeat the questions for the reader? The repetition may seem redundant to you as its your content, but for readers it is useful and clarifies the utility of the results described.
Thank you very much for this suggestion. We now repeat the questions in the conclusion section.

L492: In this paragraph can you elaborate on the causal connection between latent heat release and ridging/upper level divergence?
Indeed, this explanation was missing int the description. We now include a brief explanation by stating "The reason for this amplification is the net transport of lower-tropospheric low potential vorticity air into the upper-troposphere as well as irrotational winds which impinge on the upper-tropospheric potential vorticity gradient and thus constitute a Rossby wave source."

L495: Am I confused here - I don't think you showed this for the idealised GCM experiments?
Thank you very much for this valuable comment. Indeed, we missed to show the precipitation anomalies from which the heating rates are derived. In the revised version, we now added supplementary information which show the ERA-Interim based precipitation anomalies.

L500: I don't really understand this point. Differences between "experiments forced with tropical heating only" and what? Please be more explicit here, and perhaps consider breaking it into two or more sentences? I'm looking for a clear description of the evidence of the causal links between latent heating in WCBs and MJO teleconnection pattern.
We very much agree that our explanation was unclear. Thus, we revised this section of the text which now reads "To corroborate the causal relationship between differences in WCB activity and the different teleconnection patterns and to exclude that differences in tropical diabatic heating have a decisive influence, we conduct two more experiments, but only with heating in the tropics. The differences between these two experiments are small in terms of the teleconnection patterns. This confirms that the different teleconnection patterns found in reanalysis data after periods of high and low WCB activity are indeed due to heating of the WCBs and not due to differences in tropical heating."

L502: In this paragraph I see the big picture conclusions - the amount of WCB activity in association with MJO phase is a good predictor of ensuing climate regime. Is the link causal, or are they correlated for other reasons? And what can be done to predict WCB activity (and then by proxy, the climate regime)?
Regarding the first question, our answer is: both. The experiments with the idealized GCM show a clear causal relationship. On the other hand, weather systems like WCBs are embedded in the large-scale flow and do not evolve independently of it. Thus, both are correlated as well. Regarding the second questions, our conclusion is that verification of subseasonal forecasts should focus not only on the dominant modes of subseasonal variability such as the MJO, but also on how the modulation of the activity of midlatitude weather systems conditioned on these modes is represented. Accordingly, we included this aspect in the final paragraph of the manuscript.

Fig 4: could you show EN minus LN? and label the rows and columns (ENP, WNP, LN, EN)
We appreciate this comment. We show the difference EN - LN in the revised manuscript and label the rows and columns accordingly.

Fig 7: can you add high minus low column, as you have in fig 9?
Thanks for this suggestion. We include the high minus low column in Fig. 7.

Fig 9: why not show the 300hPa gz mean state in a,b,d,e as you do in Fig 7?

We appreciate your suggestion. In the revised manuscript, we now show the 300 hPa geopotential height mean state.

References

Sodemann, H., Wernli, H., Knippertz, P., Cordeira, J. M., Dominguez, F., Guan, B., Hu, H., Ralph, F. M., and Stohl, A.: Structure, Process, and Mechanism, pp. 15–43, Springer International Publishing, Cham, `https://doi.org/10.1007/978-3-030-28906-5_2`, 2020

Toride, K., & Hakim, G. J. (2021). Influence of low-frequency PNA variability on MJO teleconnections to North American atmospheric river activity. Geophysical Research Letters, 48, e2021GL094078. `https://doi.org/10.1029/2021GL094078`