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Response to the Reviewers' comments on

Summertime circumglobal Rossby waves in climate models: Small biases in upper-level circulation create substantial biases in surface imprint

By Fei Luo, Frank Selten, Kathrin Wehrli, Kai Kornhuber, Philippe Le Sager, Wilhelm May, Thomas Reerink, Sonia I. Seneviratne, Hideo Shiogama, Daisuke Tokuda, Hyungjun Kim, and Dim Coumou

We thank both reviewers for their thoughtful and constructive comments. We find most of the suggestions are valid and will help us to improve the manuscript. We address each comment point by point below. The reviewers' comments are given in **black** and our responses in **blue**.

Reviewer #1

General comments

In this manuscript, the authors assess the representation of mainly wavenumber 5 and 7 patterns in three different climate models (EC-Earth, CESM and MIROC), and search for the reason of model biases with respect to the ERA5 reanalysis data. They find that the models represent these wave patterns reasonably well, however, small biases in the upper level circulation lead to large biases in surface variables, like temperature, precipitation and mean sea level pressure.

In my previous comment, I asked the authors to take care of a few deficits of the paper and to improve the scientific quality by
1) increasing the accuracy of the wording and presentation of the results,
2) improving the objectiveness related to the used methods and data set, and
3) the work should be put in a broader scientific context.

I appreciate the effort of the authors in answering and addressing every single comment. I think that, after the first round of revision, the paper improved a lot. The authors extended the scientific context in which they embed their results, and the objectiveness of the work increased by adding a critical discussion of potential drawbacks of the applied methods. **The accuracy of the wording improved as well, however, there is still substantial need for further modifications. Especially, many recently added parts are unclear and contain several grammatical and technical mistakes. In my opinion, although the nature of mistakes is mostly minor, they are almost inacceptable for a revised manuscript. Thus, I ask the authors again to revise the text thoroughly, otherwise I cannot recommend the acceptance of this paper by WCD.**

We thank the reviewer for the acknowledgment of the improvements we made to the manuscript during the last revision round. The suggestions from the reviewer are much appreciated. We agree with the reviewer that the text should be modified thoroughly to improve the readability of the manuscript and to eliminate the grammatical and technical mistakes in the paper. We invested substantial time and energy in improving these aspects.

<u>Specific comments</u>

L224-227:

"... *the v250 field is shown in absolute values, together with surface variables in anomalies with respect to climatology mean"*
You mention this a few lines above (L217-218).

Thank you for spotting this, we have rephrased the rest of the paragraph as follows to improve the readability and clarity of the message.

"Figures 3 and 4 show the upper-level meridional wind (v at 250 hPa, absolute field), and anomalies of near-surface temperature (t2m), precipitation (prcp), and sea level pressure (mslp) during high-amplitude events in ERA5 (panel a). The same variables are shown for the free-running atmosphere and soil moisture (AISI) experiments using the three climate models (panels b-d). Extended analysis for other wavenumbers revealed that the evaluated models are capable of reproducing the high-amplitude Rossby waves 4 to 8 and their associated surface anomalies reasonably well (Fig. B3 - B5). The results imply that the model is able to reproduce summertime surface anomalies associated to different wavenumber events. Using anomalies for the upper-level meridional winds, in contrast to absolute v250, gives consistent results (compare Fig. 3 and 4 and Fig. B10 and B11, respectively)."

"*The same analysis is carried out ... during wave 5 and 7 events (see Appendix Fig. B10 and B11)*"
Which analysis exactly? Too unclear, please reformulate.

We have rewritten this part to make it clearer as follows:

"Figures 3 and 4 show the upper-level meridional wind (v at 250 hPa, absolute field), and anomalies of near-surface temperature (t2m), precipitation (prcp), and sea level pressure (mslp) during high-amplitude events in ERA5 (panel a). The same variables are shown for the free-running atmosphere and soil moisture (AISI) experiments using the three climate models (panels b-d). Extended analysis for other wavenumbers revealed that the evaluated models are capable of reproducing the high-amplitude Rossby waves 4 to 8 and their associated surface anomalies reasonably well (Fig. B3 - B5). The results imply that the model is able to reproduce summertime surface anomalies associated to different wavenumber events. Using anomalies for the upper-level meridional winds, in contrast to absolute v250, gives consistent results (compare Fig. 3 and 4 and Fig. B10 and B11, respectively)."

"*By comparing Fig.3 and Fig.4 to Fig. B10 and Fig. B11, the observation can be obtained that the spatial patterns of v250 do not differ much*"
Instead of writing "comparing Fig. X and Fig. Y" please write what you are comparing exactly in terms of the figure contents. You could write, for example, that you compare the anomalies corresponding to wave 5 and 7 events with the ones obtained for the other wave events. Written in this form, it becomes easier for the reader to understand your message. Please rephrase "*the observation can be obtained*".

We have rewritten this sentence to make it clearer, please see the quoted sentences :

"Figures 3 and 4 show the upper-level meridional wind (v at 250 hPa, absolute field), and anomalies of near-surface temperature (t2m), precipitation (prcp), and sea level pressure (mslp) during high-

amplitude events in ERA5 (panel a). The same variables are shown for the free-running atmosphere and soil moisture (AISI) experiments using the three climate models (panels b-d). Extended analysis for other wavenumbers revealed that the evaluated models are capable of reproducing the high-amplitude Rossby waves 4 to 8 and their associated surface anomalies reasonably well (Fig. B3 - B5). The results imply that the model is able to reproduce summertime surface anomalies associated to different wavenumber events. Using anomalies for the upper-level meridional winds, in contrast to absolute v250, gives consistent results (compare Fig. 3 and 4 and Fig. B10 and B11, respectively)."

Overall, please rephrase this recently added part of the paragraph: don't write the same several times, formulate the message in a clear way and be sure that the new text fits well to the rest of the paragraph.

Thank you for the suggestion and we have modified this part to make it clearer, please see the quoted sentences in the first response in the "Specific comments" section.

L229-232:

"*Furthermore, Fig. 3 and Fig. 4 with significant tests at confident level of 95%, as well as False Discovery Rate (FDR) method (Benjamini & Hochberg, 1995) were applied in Fig. B12 and Fig. B13.*" You actually write here that you apply Fig. 3 and 4 in Fig. B12 and B13, which makes no sense. Please reformulate.

Thank you, we have deleted the original sentence and wrote the following instead:

"Furthermore, Fig. B12 and B13 show that composite anomalies for v250, t2m, and mslp are significant for both waves 5 and 7, accounting for False Discovery Rate (FDR; Benjamini & Hochberg, 1995). "

"Significance test" instead of "*significant test*" and "confidence level" instead of "*confident level*".

Thank you for spotting this, we have changed the wording.

"*Areas with highlighted fuchsia color are the locations passed the significant tests*"
Locations do not pass the significance test, instead the differences over those areas are significant. Additionally, the sentence grammatically incorrect. Please rewrite.

Thank you, we have deleted this part and made it clearer in the caption of the Fig. B12 and B13.

Please rephrase the whole paragraph. You could incorporate it in the rest of the text because there is no new scientific message here, instead it points out that some already presented results are significant.

Thank you for the suggestion, we have rephrased and merged this paragraph with the one directly above it.

Fig. B12 and B13: please rewrite captions ("significant applied at..." ??).

Thank you, we have rewritten the captions as follows:

"

Figure B12: Same as Fig. 3 but with significance test at 95% confidence level applied. Values significantly exceeding the 95% confidence level from composites between amplified and non-amplified periods are hatched.

Figure B13: Same as Fig. 4 but with significance test at 95% confidence level applied. Values significantly exceeding the 95% confidence level from composites between amplified and non-amplified periods are hatched.

"

Remark regarding emergent constraints: The option of emergent constraints based on observational data should be considered with caution, see for example Sanderson et al., 2021, Earth Syst. Dynam.

Thank you for the comment. We have removed this line referring to emergent constraint.

Technical corrections

L156: Instead of "the weeks are" "the numbers of weeks are".

Thank you, changed as suggested.

L206: "*can be found at Fig. B2*". Please rephrase.

Thank you, we rephrased "can be found at Fig. B2" to "are provided in Fig. B2".

L299: "*Still in For t2m*"?

Thank you, this part should be "Still in EC-Earth, for t2m".

L322-323 "the" is missing before "soil moisture part" and "overall bias".

Thank you, changed as suggested.

L329: "*wave-7*" appears twice.

Thank you for spotting this, we deleted the second "wave-7 events".

L358-359: “*has little effect on the representation of circumglobal waves nor their surface imprint, or, at least not on the anomaly these events produce.*”
“has little effect on the representation of circumglobal waves **and** their surface imprint, or at least **on** the anomaly these events produce.”

Thank you for the suggestion, changed as suggested.

L367: “*for during*”?

We changed “applied for during” to “included in”.

L372: “*Since its on hemispheric scale...*” Incorrect formulation, please rephrase. “Its” is a possessive pronoun.

Thank you, we rephrased “Since its on hemispheric scale…” to “Since the analysis is done on hemispheric scale…”.

L373: “*Depending on the stationarity of the dataset, the results can also differ substantially with FFT method.*” What do you mean?

Thank you, we have added new parts of writing to make it clearer (see section 4.2):

“As with any choice of circulation metrics, our approach based on Fourier analyses of the zonally oriented wave component has its limitations. This approach implies that if a particular wavelength is pronounced in only one part of the hemisphere, this can result in a high-amplitude FFT signal. Thus, high-amplitude waves (as defined by our metrics) do not necessarily have to be circumglobal. They can either result from a circumglobal wave pattern or a pronounced regional wave pattern. Still, the fact that we find pronounced and significant wave-patterns in our composite analyses reveals that those reflect preferred wave positions. In particular, wave 5 and wave 7 are subject to this phase-locking behavior. Whenever those quasi-stationary waves grow in amplitude, they tend to do so in the same longitudinal phase position thereby causing temperature and precipitation anomalies in the same geographical regions. This has been reported before for observational data, highlighting the risks this creates for multiple breadbasket failures (Kornhuber et al, 2020). The prime motivation of our study is to see how well climate models reproduce these waves, and to that end our FFT-based metric is useful.”

L382-383: Please move “*patterns*” to the end of the sentence.

Thank you, changed as suggested.

L392: "flow" instead of "*slow*". I pointed this out in the previous review already.

References: I'm sure it's "Röthlisberger, M." instead of "*Röthlisbergera, M.*". This is also something I mentioned in the previous review as well.

Apologies for the typos, they were corrected during last revision, but due to some unknown version changes in the Word document, these changes are not recorded. Now they are corrected again.

Fig. B2 caption: "Same as" instead of "*Same with*".
Figure captions: "*seal level pressure*" is written instead of "sea level pressure".

Thank you, changed as suggested.

Response letter to Reviewer #2 continues in the next page.

Reviewer #2

General comments

The current paper investigates the representation of strong "circumglobal wave events" in three different climate models. A particular focus lies on the role of the upper-level flow versus soil moisture for model biases. First it is shown that the three models in a free-running mode do a pretty reasonable job in representing the anomalies associated with these "extreme circumpolar wave" events as compared with reanalyses. Second, the authors use a valuable set of numerical experiments that was performed in connection with the ExtremeX project in order to find out whether this success is primarily related to the correct representation of the upper-level flow or to the correct representation of the soil moisture conditions. As it turns out, the upper-level flow is considerably more important in this analysis.

The topic is highly relevant, the paper is overall well written, and the logic seems straightforward in large parts of the text.

We would like to thank reviewer for acknowledging the importance of this study and the time and effort for reviewing this paper.

However, the longer I think about this paper, the less I understand what I am supposed to learn. Essentially you investigate composites of events, and the events are selected on the basis of the magnitude of one particular zonal wavenumber. Arguably this is a somewhat artificial selection, because it does not necessarily represent any physical/meteorological situation. For instance, if you select a specific wavenumber and the amplitude of this wavenumber is much larger than the amplitude of all other wavenumbers, the situation corresponds to a circumglobal wavetrain. However, if you select a specific large-amplitude wavenumber and at the same time neighboring wavenumbers are of similar (large) amplitude (something which I assume to be rather common), the meteorological situation is more likely to be a large-amplitude Rossby wave in part of the hemisphere with much smaller amplitudes in the rest of the hemisphere. It transpires that the events that you select (based on one specific wavenumber exceeding a threshold) may be a collection of rather different-looking physical situations, and they are not necessarily always associated with a circumglobal wave train. Taken at face value, this would draw into question even the title of your paper, which promises a study of circumglobal Rossby waves. In any case, if you average over many such separate events (through compositing), it is unclear (to me) what this composite is meant to represent.

We thank the reviewer for these considerations. We agree that our FFT-based approach has its limitations, as any choice of circulation metrics. If a particular wavelength is pronounced in only one part of the hemisphere, this can result in a high amplitude FFT signal. So we agree that high amplitude waves (as defined this way) do not necessarily have to be "circumglobal". They can be circumglobal or result from a regional wave pattern with high amplitude. Thus, we acknowledge the reviewer and remove the word "circumglobally" from the title. Still, the fact that we actually find pronounced (and significant) wave-patterns in our composite analyses reveals that those do not reflect 'a collection of rather different-looking physical situations', as suggested by the reviewer. The reason is that in particular wave 5 and wave 7 are subject to phase-locking: Whenever they grow in amplitude they tend to do so in the same longitudinal phase position which means that they cause temperature and precipitation anomalies in the same geographical regions. This has been reported before for observational data, highlighting the risks this creates for multiple breadbasket failures

(Kornhuber et al, 2020). The prime motivation of our study is to see how well climate models reproduce these waves, and to that end our metric is useful. We have updated the manuscript accordingly, explaining our prime motivation and the usefulness and limitations of our FFT-based wave metric (see line 380 - 389).

Possibly I have not fully understood your analysis. But maybe other readers have a similar problem. Therefore, the authors would have to be more explicit and more lucid in their interpretation and work out much more clearly what one really learns from the analysis of this set of simulations.

Thank you for the suggestion. We refer to our response to the comment above: We have updated the manuscript explaining our prime motivation more clearly as well as the usefulness and limitations of our FFT-based wave metric(see line 380 - 389).

My second issue is somehow related the previous one. A superficial interpretation of your results suggests that soil moisture is really not important; rather, all you need is to get the upper-level flow right. Maybe this is not how the results should be interpreted, but there is the danger that this impression remains (unless your interpretation becomes much more detailed and explicit). In particular, one of the authors (Sonia Seneviratne) has shown multiple times in previous publications that soil moisture is important to determine summer surface temperatures at least over certain areas over the continents. Again, on a superficial level, the current results seem to contradict these earlier results. I would be interested in Sonia Seneviratne commenting on this issue, and the reader would certainly appreciate if you could clarify.

Thanks for highlighting this. In fact, none of our results are questioning the importance of soil moisture in determining summer surface temperatures. What appears to create the confusion is that the *biases* in t2m (as shown in e.g. Fig 7) are much reduced when we nudge the upper-level circulation to the observed state, but are essentially unaffected when nudging the soil-moisture to the observed state. Based on that we conclude that most of the T2m biases originate from biases in upper-level dynamics. Also, our analyses suggest that soil moisture interactions are reasonably well represented in the climate models. We have added these subtleties in the interpretation of our results with respect to the role of soil moisture in our manuscript, explicitly stating that our results do not question the importance of soil moisture in driving near-surface summer temperatures.

Please see in line 364-377 in the manuscript:

"To be clear, our results are not questioning the importance of soil moisture as a prime driver of summer surface temperature 365 extremes in various regions throughout the mid-latitudes. Rather, our study shows that prescribing the soil moisture in the models has little effect on surface variables and upper-level variables during high-amplitude wave events. Several studies have shown that soil moisture can play an important role in maintaining large-scale circulation anomalies associated with extremely warm and dry conditions (e.g. Erdenebat and Sato, 2018). In particular, under future climate change reduced soil moisture can lead to a higher probability of heatwaves in Europe during summer via interactions between the land surface and the 370 atmosphere (e.g. Seneviratne et al., 2006). This, however, does not say anything about the imprint of soil moisture biases on biases in near-surface climate in relation to the Rossby wave events investigated in our study. Apparently, it is the state of the atmosphere, i.e. circulation or clouds and precipitation, that governs the model biases in near-surface climate and not so the state of the land surface. Further, in the interpretation of our results, one should be wary that in our prescribed soil moisture runs (AISF/AFSF), we prescribe the 'approximately observed' soil-moisture conditions (and not e.g. soil moisture 375 climatology). This implies that the turbulent heat fluxes in AISF/AFSF still depend on this prescribed soil moisture

condition. This means that during for example the heatwave period in Russia in 2010, the prescribed soil moisture will be anomalously dry, which will result in strong sensible heat fluxes. "

In my interpretation, your analysis points to a possible model bias in the sense that the models systematically under- or overestimate the spectral power in specific wavenumbers as far as the upper tropospheric circulation is concerned. In your analysis this is somewhat obscured by the fact that you focus on just a few wavenumbers. In reality, the lack of spectral power at a certain wavenumber may reappear as a surplus of spectral power at some other wavenumber. In other words, your selection of events is based on highly incomplete information in spectral space and may, therefore, obscure what is really going on. A more straightforward approach to analyse spectral model biases would be to consider the composite spectral power of all wavenumbers and determine related biases. In addition, to the extent that the meteorology in the lower troposphere is "slaved" to the dynamics in the upper troposphere, nudging the upper troposphere makes obviously a difference, but nudging the lower troposphere does not; this would imply that your results regarding the biases are somewhat trivial.

What's non-trivial and what I find highly interesting is the fact that individual wavenumbers apparently have a preferred phase. However, this is not the topic of the current paper, and the current paper does not (aim to) further contribute towards an explanation.

We thank the reviewer for this comment. We were surprised to read that the reviewer concludes that the 'preferred phase' positioning would not be the topic of our paper: Preferred phases, or phase-locking, of individual wavenumbers is actually the prime motivation of our analyses. We attribute this miscommunication to unclear formulations from our side in the earlier version of this manuscript.

We agree with the reviewer that we don't provide new explanations for the underlying physical mechanisms, but this is the first time that this phase-locking behavior is documented in several climate models. The scope of this paper is to identify if climate models are able to capture the characteristics (in terms of mean and variability) of a wide range of wavenumbers, including preferred phase positions. We focus on wave-5 and wave-7 as those were identified to be important in previous literature based on reanalyses (i.e.Kornhuber et al., 2019 & 2020; Drouard et al., 2019)..

We have updated the manuscript explaining our prime motivation in detail and the usefulness and limitations of our FFT-based wave metric (see also our response to the first comment).

Last, but not least, I have an issue with the title, and this mirrors the points that I raised above. I think the title is misleading for two reasons. (1) It suggests that you deal with "circumglobal Rossby waves", which I would argue is not true. You select events on the basis of highly incomplete information in spectral space, and this does not guarantee that your events are characterized by a circumglobal Rossby wave (see my earlier remarks). (2) You say that "small biases in upper-level circulation create substantial biases in surface imprint". To me as a reader this suggests that if you select a specific event and introduce just a small upper-tropospheric bias in that event, this results in strong changes in the surface meteorology. But again, this is not what you have shown. The concept "bias" in your analysis represents a comparison between two composites, e.g., one from a free-running model and the other one from reanalysis data. However, the underlying individual events (from which the composites are computed) may be very different and the analysis, hence, does not support the claim raised in the title (the way I understand it). I realize that you tried to summarize the whole content of your paper in the title, but with an analysis as complex as yours this is invariably going to be impossible.

We also removed the word “circumglobal” and changed the title into: “Summertime Rossby waves in climate models: Substantial biases in surface imprint associated with small biases in upper-level circulation.” We hope this satisfies the reviewer.

In summary, I am aware that the current paper uses a quite special (and maybe somewhat artificial and unphysical) setup for the analysis: namely compositing only events that have a large amplitude of a specific wave number. This very special sort of analysis may have a strong imprint on its interpretation, but it is not straightforward (for me) to see and understand this imprint. As a consequence, I am not fully able to follow your interpretations and to appreciate the merits of your analysis.

Thanks for all comments which have certainly improved the quality of our manuscript. We do not agree with the terms “artificial and unphysical”, but rather our approach has its pros and cons. We feel confident that in our updated manuscript the pros (i.e. the usefulness of this metric when studying phase-locked behaviour) and cons (i.e. the limitations of the metric) are better described with a much clearer interpretation of our findings (i.e. to do with the role of upper-atmospheric biases and the role of soil moisture). Our study is in line with previous work (Kornhuber et al., 2020) showing the importance of phase-locked waves, and that, to first order, climate models capture such wave events. This is an essential stepping stone for further research into the underlying physics of such wave events.

Minor issues

Line 50: arguably, RRWPs are not the same thing as quasi-stationary Rossby waves, so the former should not be given as “an example” for the latter.

We have rephrased the sentence “Persistent weather extremes are often induced by quasi-stationary Rossby waves.” to “Persistent weather extremes are often induced by certain Rossby wave patterns.”

Line 88: “with the duration of sfc weather conditions….”: that’s a somewhat strange formulation. Do you mean “long duration” or “persistence” here?
Thank you, we changed the word “duration” to “persistence”.

Line 137: “This added tendency term….”: this sentence is not clear to me, as well as the ensuing sentence.

We have rephrased the sentence as follows:

“By taking the differences between the model simulations and reference dataset, the added tendency term is computed.”

Line 152: Since your analysis is based on weekly averages, you could (and should) say that you are interested in rather persistent anomalies. For instance, in the abstract you could talk about “summertime persistent (or: quasi-stationary) wave events” instead of just “summertime wave events”.

Thank you for the suggestion. We have added some sentences in the abstract accordingly.

Line 157: What is an “imprint”? Please define! Here you talk only about surface “imprints”, but in the following plots you also show composite upper tropospheric meridional wind. How are the latter defined?

Imprint in our case means the characteristics, either absolute or abnormal values, of certain variables under certain conditions. In our study, this condition is high-amplified event for wave 5 and wave 7 where the wave amplitudes exceed 1.5 standard deviations from wave climatology. Thus, this term is not strictly tied only to surface variables.

Line 168: The concept of a “bias” is central to your paper (the word appears even in the title), but the “definitions” that you provide on page 6 are not clear to me. In my understanding, AISI represents a free running model run, and ERA5 represents a dataset. So what is the difference between a model run and a data set? This is way too sloppy, this must be specified much more clearly. (You probably mean the difference between the composites, not the difference between model runs or data sets, but this must be said explicitly).
Thank you for the suggestion. We have specified more in the bias definition part.

Line 190: better “variance in wave amplitude” instead of “variance in wave activity”, because “wave activity” has a special meaning in dynamical meteorology (which you do NOT want to refer to here).

Thank you for the suggestion, we agree with the reviewer and changed accordingly.

Caption Fig 2: not clear to me what the term “bandwidth” refers to here.

This information was requested before by another reviewer (See Fig. B2). The bandwidth is associated with a histogram and varies with different numbers of bins.

Line 204: Well, ERA5 shows a strong preference for one part of the hemisphere as opposed to the other part of the hemisphere. Therefore, since you have not clearly defined “phase locking”, it is not obvious to me that ERA5 is supposed to not show phase-locking for wave-6 and 8.

We do not agree with the reviewer’s comment, as we have defined phase-locking in the paragraph above in manuscript as “*a single symmetrical peak in the probability density function*”.

Lines 229/230: english? In addition, I appreciate your effort to determine statistical significance, but I was not able to extract from Fig. B12 and B13 what areas are significant. Please clarify. Where on the plots should I see “highlighted fuchsia color”?

We have changed the whole paragraph and also the Fig. B12 and B13 as follows to eliminate the potential misunderstandings:

“Figures 3 and 4 show the upper-level meridional wind (v at 250 hPa, absolute field), and anomalies of near-surface temperature (t2m), precipitation (prcp), and sea level pressure (mslp) during high-

amplitude events in ERA5 (panel a). The same variables are shown for the free-running atmosphere and soil moisture (AISI) experiments using the three climate models (panels b-d). Extended analysis for other wavenumbers revealed that the evaluated models are capable of reproducing the high-amplitude Rossby waves 4 to 8 and their associated surface anomalies reasonably well (Fig. B3 - B5). The results imply that the model is able to reproduce summertime surface anomalies associated to different wavenumber events. Using anomalies for the upper-level meridional winds, in contrast to absolute v250, gives consistent results (compare Fig. 3 and 4 and Fig. B10 and B11, respectively).”

Line 293: “almost fully….” seems somewhat overstated, I would say “to a large extent….”

Thank you for the suggestion, we changed the phrase as suggested above.

Line 304: Ref to Fig 7a seems wrong.

Thank you, it should be Fig. 5(a).

Line 316: Ref to Fig 5b seems wrong.

This is correct reference.

Line 329: what do the correlation values given in parentheses refer to?

There is only one reference used in this paper, namely ERA5, thus throughout the paper it’s always the model data compared to ERA5.

Line 330: better “climatology and variability”.

Thank you, changed as suggested.

Line 343: I think that the vertical wind in your nudging experiments is only weakly constrained by the divergence/convergence of the horizontal wind. Rather, having the horizontal wind almost right in these experiments implies the correct forcing in the omega equation and, hence, a good representation of the vertical wind. In other words, I think your statement is correct, but the reason/explanation you give may not be correct.

We did propose the explanations as potential mechanisms.

Line 347: “can be disturbed in the models….”: that’s unclear. I agree that an error in the vertical wind may lead to an error in cloudiness, but there may be other sources for errors in cloudiness (e.g., moisture advection) which may be just as important.

We agree with the reviewer.

Line 363: froced -> forced

Thank you, the typo is corrected accordingly.

Line 371/372: do you mean that they yield cirumglobal waves “by design”!?

No, that is not what we meant. We have explained the FFT approach in the response letter in “General comments”.

Line 373: What is the “stationarity of a data set”? also: I hope very much that the results do NOT depend on the method used to compute the FFT! Can you explain what you mean here?

No, the results do not depend on the method used to compute the FFT. We intended to point out some limitations of the methods and to have some critical discussions.

Line 374: I do not understand this. If you provide only the zonal mean to an FFT algorithm, then the wave amplitude would be zero for all zonal wavenumbers except s=0.

We have rewritten the whole part to make it clearer, see in line 380 – 389.

Line 376: not “the data is bigger….”, rather “the amount of data is larger by a factor….”

Thank you, changed as suggested.

Line 392: “slow to….”??? Also: a simple statistical connection between the upper-level flow and surface temperatures per so is not an “emergent constraint” (the way I understand that concept). Please clarify.

It should be “flow” instead of “slow”. We have removed the part “emergent constraint” to avoid misunderstandings.

We also would like to thank the editor sincerely for the constructive and insightful comments, which we have considered when revising this paper.

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