Manuscript: WCD-2021-48

Response to the Reviewer' comments on

Summertime Rossby waves in climate models: Substantial biases in surface imprint associated with small biases in upper-level circulation

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 the reviewer for reviewing the paper again and is satisfied with our efforts and revisions from last round. We find most of the suggestions are constructive and will help us to finalize the manuscript. We address each comment point by point below. The reviewer's comments are given in **black** and our responses in **blue**.

Reviewer #1

General comments

This is the revised version of a manuscript that I have reviewed before. My original concerns have been addressed to a large extent by the reply to my earlier comments as well as by the revisions.

The paper sort-of "validates" three state-of-the-art climate models in terms of the statistical behavior of selected episode during which a few selected Fourier components of the upper tropospheric meridional wind have strong amplitude. The motivation for those specific Fourier components stems from an earlier paper in which the authors have shown an interesting connection to summer heat extremes. The main point here is that the models represent these large-amplitude wave-5 and wave-7 events sort of OK in terms of the upper tropospheric circulation statistics, while there is a substantial underrepresentation (and partly misrepresentation) in terms of associated surface variables. The situation is analyzed in more detail with the help of model simulations in which certain model variables are nudged towards observed conditions. In particular, the underestimation close to the surface almost disappears when the upper tropospheric flow is nudged towards observations.

The analysis seems to be sound, the topic is relevant, and the results are intriguing. I have a few further issues which should be taken into account when preparing the final version of this manuscript.

We thank again the reviewer for the acknowledgment of the improvements we made to the manuscript during the last round of revision. We are delighted to know that the reviewer found our results intriguing and also gave a nice summary of the essence of this paper.

Major issues

1.) The selection of episodes (both in reanalyses and in the free-running model simulations) is based on the large amplitude of a specific wavenumber of the upper tropospheric meridional wind. This procedure does not imply that the episodes in the reanalyses and in the model simulations represent the same "events". The use of the term "event" is, therefore, somewhat misleading.

The situation changes fundamentally when the upper tropospheric flow in the model simulations is nudged to the reanalyses: now the different episodes, indeed, represent the same (or at least very similar) events. In particular, the associated surface anomalies now turn similar to the observed ones (from reanalyses) to the extent that the upper tropospheric flow determines the lower tropospheric meteorology, which is true to a considerable extent.

I think it would be helpful to make a clear distinction between "episodes" and "events".

We thank the reviewer for this comment, and we agree with the reviewer that the distinction between "episodes" and "events" should be made in the manuscript. We have changed "events" to "episodes" when referring to free-running model experiments and followed the terminology throughout the whole manuscript. Thus, for AISI and AISF runs, we use the term "episode" whereas for AFSI and AFSF runs, we use the term "event".

2.) Although the biases of the free-running EC model in terms of the upper-tropospheric flow are, in my eyes, quite substantial, they are referred to by the authors as "minor" or "small". I think it would be fair to acknowledge that they are not just "minor" or "small". This, in combination with the fact that the composite from the free-running model simulations is not composed of the same "events" as the composite from the reanalyses, renders the result somewhat less surprising: namely that nudging the upper tropospheric circulation improves the situation substantially.

We thank the reviewer for mentioning this point. We agree that the usage of "minor" or "small" can be somewhat subjective, thus we tried to eliminate subjective wording during this round of revision when referring to the biases in upper-level circulation (v250) and put the terms in context when needed. For example, our findings show that the biases in upper-level atmospheric flow is smaller compared to those at the surface.

Minor issues

Line 156, 209, 370: What do you exactly mean by the term "imprint"? Does it suggest a causal relationship? It seems that the latter is unproven at this point.

No, it does not imply a causal relationship and we also do not imply causality in the manuscript. We have also explained the term "imprint" in our response letter from last revision. See below:

"*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.*"

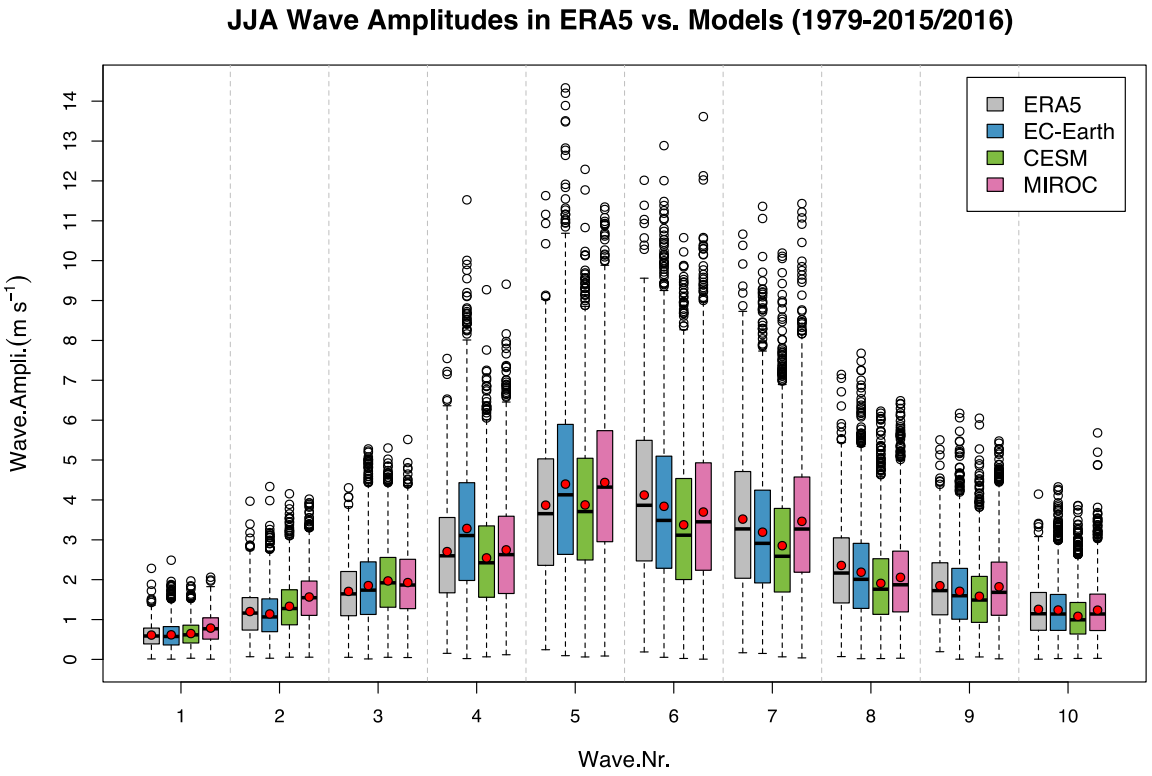
Line 605, caption of Fig 1: if the whiskers indicate the maximum and minimum values that have occurred, then all values are taken care of, right? In this case there should be no "outliers that are not shown in the plot". Have I missed something?

Thank you for spotting this, indeed there is some confusion here that needs to be explained better. Between upper and lower bars is the 1.5 times interquartile range (1.5 IQR).

The caption of Fig.1 has been modified as follows:

Figure 1: Boxplot for wave amplitudes in AISI climatology runs for climate models EC-Earth, CESM, and MIROC, as well as reanalysis data ERA5 for the period of June, July, and August in 1979-2015/2016. Red dots indicate the mean, and thick black lines represent the median. The lower hinge of each box is Q1 quartile (25th), and the upper hinge for Q3 quartile (75th). The upper and lower bars are based on the 1.5 times interquartile range (IQR) value. The outliers are not shown in the plot.

Below is the same as Fig.1 but with outliers plotted.



Line 210: this sentence is somewhat non-sensical: even if the wave gets phase-locked in a not-preferred position, I would expect a prolongation of surface warm anomalies.

Thank you for this comment. We agree with the reviewer and have removed this whole sentence.

Line 212: Here you talk about “events”, suggesting that your method singles out specific meteorological situations. However, as argued above as well as in my previous review, this is not the case, rather your selection of “events” may contain a collection of rather different-looking meteorological situations. Therefore, the use of the term “event” is misleading. Rather, you single out specific “episodes”, not specific “events”.

Thank you for this comment. We agree with the reviewer and changed “events” to “episodes”. Please also see our response in the section “Major issues” part 1).

Line 264 ff: The episodes that you study are selected using an upper-tropospheric criterion. For me, it therefore does not come as a surprise that the associated "bias" in the upper troposphere is less than in the lower troposphere. Can you comment, i.e., give the reader some guidance at this point?! Moreover, as stated above, I do not really find the biases in the upper troposphere to be "minor" or "small".

We basically agree that this is not necessarily surprising, it's simply the findings of our work. For the wording issue of "minor" or "small", please see our response in the section "Major issues" part 2). We have also rewritten the sentence, see below:

"One common finding from both wave-5 and wave-7 events and episodes is that the biases (n.s.d. ≥ 0.75) in upper-level circulation are smaller compared to more-pronounced biases in the surface anomalies t2m, prcp, and mslp."

Line 269: these two figures do not show biases, but composite anomalies. The biases can, at best, be inferred from these plots.

We agree with the reviewer and have rephrased the original sentence as follows:

"Next, we aim to infer the biases from composites of anomalies shown in Figs 3 and 4 for the upper-level wind and surface fields."

Line 285: unclear what the word "this" refers to.

We agree with the reviewer. We have rewritten the sentence as follows:

"As for wave 5, the free-running EC-Earth (AISI) ..."

Line 326: "individual Fouriermodes" would be better that "amplified Rossby waves".

Thank you, changed as suggested.

Line 333: again, you should not talk about "events" here (as detailed above). I you were more precise and talk only about "episodes", the result (namely that surface variables do not necessarily show concomitant anomalies) would appear somewhat less surprising.

Similarly, the fact that these "biases" disappear when the (fairly substantial!) upper tropospheric biases are removed through nudging, turns less surprising, because you make a transition to the same "events" (as defined through upper tropospheric dynamics").

Thank you for the suggestion and comments, please see our response above in the section "Major issues" part 1) and 2).

Line 344: "… too weak in the models": why?

Thank you for the question. We have added one sentence after "… too weak in the models." See below:

"As this subsidence might not be well represented in the models, because of biases in the upper-level flow."