Response to referee comments on the first revision of "The interaction of tropical and extratropical air masses controlling East Asian summer monsoon progression" by Volonté et al.

wcd-2021-12-R1 - A. Volonté on behalf of all authors, 7 April 2022

Dear Editor,

We thank Prof Michael Reeder and an anonymous referee for their reviews and for the positive and constructive comments on our manuscript. We believe that addressing those comments has given us the chance to further improve our study. Below we give point-to-point responses to the comments by Referee #1. Our responses are written in *green italic* font, while all edits in the tracked-changes version of the manuscript are in red.

Comments on WCD-2021-12-R1

Summary

Thanks to the authors for their efforts in revising the original manuscript. The revised version has been largely improved and the authors' responses have cleared most of my concerns. Particularly, the authors provided a more thorough literature review on frontal detection methods and expanded their analysis for a more comprehensive survey of insights revealed from the frontal detection algorithm proposed in this work. My comments in this round are mainly clarifications, focusing on making sure the message is clear and direct to the readers. I recommend the paper for publication in the journal after the authors address these comments.

1. I think the authors should highlight the most novel element in the title of their manuscript. In my view, the most novel thing in this work is that they proposed a frontal detection algorithm and validated its fidelity in capturing the climatology and interannual variability of the EASM. So, I suggest changing the title to something like "Understanding the seasonal evolution and interannual variability of the East Asian summer monsoon through a novel frontal detection algorithm"
Many thanks for this suggestion and for acknowledging the novelty in this study. We agree on including a reference to the frontal detection approach used. However, we would argue that the focus on the interaction between airmasses is a prominent feature of this paper and should be retained in the title, arguably at the expense of mentioning "seasonal evolution and interannual variability". Therefore, we changed the title to "Characterising the interaction of tropical and extratropical air masses controlling East Asian summer monsoon progression using a novel frontal-detection approach".

2. Line 6. Please clarify here you meant "interannual variability"
Done, thanks. The same change has been applied to the relevant sentence in the conclusions (see text highlighted in red) and the same applies to our replies to comments 3-6. Contextually, we have also replaced "EASM evolution" with "EASM progression" in the abstract and in the rest of the manuscript, where applicable.

3. Line 9. I think you should highlight that Mei-Yu is the primary stage of the EASM.
Done.

4. Line 10. This sentence should be revised and clarified. What does "These forcings" mean? The authors seem to argue that the low-level southerly flow is modulated by the seasonal evolution of the South Asian Monsoon system, not in part due to WNPSH? In my view, both the South Asian origin and those along the edge of the WNPSH contribute to the southerly flow.
The sentence has been changed to "These forcings act to steer the southerly advection of low-level moist tropical air, modulated by the seasonal cycle of the Asian monsoon." STWJ and WNPSH are mentioned in the previous sentence and are thus the forcings we are referring to. We think that the updated sentence is consistent with the results in this study, showing that both STWJ and WNPSH locations and patterns are instrumental in the path of the low-level southerly advection of tropical air, which is indeed modulated by the overall cycle of the South Asian monsoon (see for example the origin and path of "warm" trajectories in Figures 11 and 17 and the low-level wind field in many other figures).

5. Line 13. Clarify "the midlatitude flow impacting on the northern side", in the spirit of being concise and clear, suggest changing to "extratropical northerly flow"
We replaced "midlatitude flow" with "cool extratropical flow". We would avoid inserting the term "northerly", as our study (particularly the Lagrangian analysis) shows that this airstream has an important zonal component, particularly when travelling on the northern side of the Tibetan Plateau.

6. Line 17. Clarify "the regional flow"; suggest being specific and clear which regional circulation systems drive the airmasses converging at the front; also, is it true that only "the low-level airstreams" matter? The authors also showed in the paper that the northerly flow from higher levels is needed for the formation of the Mei Yu front.
Thanks for pointing this out. We added "over East Asia" after "the regional flow" (this is a summarising sentence, so we don't think we should mention again STWJ and WNPSH). We also changed "low-level airstreams" to "low- and mid-level airstreams" to include the "cool" flow.

7. Line 38. Provide the full name of EASMF as it is the first time it appears in the main text.
Done, thanks for spotting this.

8. Line 42-44. This statement is oversimplified and might be misleading. The whole sentence is a paraphrase of the texts below Figure 4 from Wang and LinHo (2002). And if I understand correctly, Wang and LinHo (2002) is not about how the whole Asian monsoon (including the Indian Summer monsoon, East Asian summer monsoon, and the WNP summer monsoon) affects the EASM, rather, they discuss how the Asian monsoon system as a whole progress across the season. So, I don't think it is reasonable using this single paper as a reference for arguing tropical influence on EASM. The authors mentioned other tropical influences before closing that paragraph; it is more appropriate to include those factors (ENSO, tropical convection) into the opening sentence of the paragraph. And you can elaborate how the EASM is related to other components in the Asian monsoon system after that.
We have clarified that the eastern component of the Asian monsoon is characterised by a multi-stage northward progression starting from tropical latitudes, sustained by low-level southerly and southwesterly winds. This makes the EASM a tropical phenomenon, at least partially. This progression is described in Wang and LinHo (2002) and thus we would argue that this reference is appropriate. We would also argue that tropical modes of variability like ENSO should not be moved to the opening sentence of this paragraph, as the Asian monsoon (including the EASM) would exist (and progress) even in the absence of ENSO.

9. Line 45-46. Suggest removing the redundant information and rephrase to "…, the poleward transport of moisture over the South China Sea is associated with the Western

North Pacific subtropical high (WNPSH)."
Done.

10. Line 53. Provide full name of "ENSO"
Done.

11. Line 56-57. I don't disagree with the authors that diabatic heating over the orography can affect the meridional position of the STWJ, but the current description is too vague. Please provide references (if there is any) that have discussed effect of the springtime diabatic heating on the northward migration of the STWJ. Also, the seasonal cycle of insolation and the resulted changes in the meridional temperature gradient should be more instrumental in driving the northward progression of the jet from spring to summer.
We tried improving the clarity of this discussion by referencing the work by Li and Yanai (1996) and specifying that, given the height of the TP, these diabatic processes are able to trigger a reversal of meridional temperature gradient in the upper troposphere between the TP and the Indian Ocean, therefore acting on the regional circulation.

12. Line 57. I think the authors meant the diabatic heating over the TP, but please be more specific and clarify what are "These processes" as the preceding sentences have covered both STWJ and the diabatic heating.
We replaced "These processes" with "These diabatic processes" (mentioned in the previous sentence).

13. Line 85-86. Can the authors clarify the blocking anticyclones over which regions are of particular importance in causing the dry air intrusion that may affect EASM?
Yihui and Chan (2005), referenced in the manuscript at the beginning of the sentence considered in this comment, state that "The continuous southward intrusion of cold air and accompanying frontal systems (the so-called Meiyu-Baiu front) is excited by the development and prevailing of blocking highs in the mid-and high latitudes over Eurasia. The dual blocking high situation, one located over the Ural Mountains and another located over the Okhotsk Sea, is the most favorable situation for prolonged Meiyu-Baiu heavy rainfall". We have added a few more words at the end of the sentence, explicitly mentioning this preferred blocking pattern.

14. Line 87-89. The original papers of the Silk Road Pattern should be cited (see references in Hong et al). Suggest adding one sentence to briefly introduce what is the Silk Road Pattern, otherwise readers who are not familiar with this pattern might have a hard time appreciate the summarized studies of Hong et al.
Thanks for pointing this out; a reference to Lu et al. (2002) has been added. Starting from the explanation given in the conclusions of the article referenced, we also specified that the Silk Road Pattern is a wave train trapped along the North African and Eurasian jet streams.

15. Line 90-92. Did the author mention the interaction between the tropical dynamics and the TP in the introduction section?
We appreciate that the role of the TP is mainly discussed in relation to the STWJ. Therefore, we modified the sentence, that now reads "… the complexity of EASM progression, influenced by both tropical and midlatitude dynamics, and by the interaction with the TP".

16. Line 105-106. Suggest rephrasing to "…we focus on the role of … masses in the EASM progression"
Done, thanks.

17. Line 106 and in other related lines. Is it possible to replace “a Parker et al. (2016)-like framework” with a more physical description? The author cited Parker et al. (2016) as one motivation for understanding the dynamics of the EASM from the perspective of the interaction of tropical and extratropical air masses. But this study used very different approach or methods throughout, so I don’t suggest the authors to use “Parker et al. 2016-like framework or Parker et al. 2016-like approach” to describe this motivation.

The quote (now at line 114) now reads “a front- and airmass-centred Parker et al. (2016)-like framework”, and the same addition has been made in the first paragraph of the Conclusions. We would argue that there are substantial methodological similarities between Parker et al. (2016) (and Volonté et al., 2020) and this study, such as the focus on frontal progression and on the location and evolution of the 3-dimensional boundary between different air masses. Furthermore, Parker et al. (2016) used a trajectory approach to contrast the tropical and mid-latitude origin of air at different stages of the ISM onset, as in this study. We now explicitly mention this at lines 110-112. In this study, we have assessed the validity of such an approach (front- and airmass-centred) to the progression of the EASM. We copy here a couple of sentences from the conclusions that summarise this assessment. “There are fundamental differences between the Indian summer monsoon and the EASM, caused essentially by the different geographical location of the main actors of the two monsoon systems. However, analogies can be drawn. The progression of the Indian summer monsoon is modulated by the balance between moist low-level tropical advection and mid-level subtropical dry intrusion, in turn influenced by tropical modes and extratropical dynamics, respectively. For the EASM, the interaction between tropical and extratropical airstreams is particularly pronounced in the Mei Yu stage, when also most of the northward migration occurs.”

18. Section 2.2.1. Since the main purpose of this section is to introduce the previously proposed algorithms and justify the choice of using the horizontal gradient of theta_e in the EASMF detection, I think it would make more sense to first introduce the methods based on the baroclinicity/SOM/rainband summarized in Line 160-170, then introduce the thermodynamic definitions as mentioned in Line 140-159, and then continue in Line 171-187 to justify the parameter used in this work.

We understand this point of view, but we would argue that the description of methodologies based on thermodynamic functions should be placed first, given that those are the methodologies most commonly used. To better highlight the two main choices to be made when devising a front identification method (physical quantity and mathematical function) their descriptions are now placed in two different sub-sections.

Other minor comments in this section include:

a. Suggest using a title such as something like “Existing front detection algorithms in the literature”

We changed the title of Section 2.2.1 to “Front-detection algorithms in literature”. Contextually, we modified the title of Section 2.2.2 to “Description of the algorithm used in this study”.

b. Line 135. Suggest rephrasing to “… have been proposed in the literature to identify …”

Done.

c. Line 147-150. To make it more concise, suggest replacing “While this… Being dependent on moisture content, ” with “As a result,” because the description of Line 152-155 have covered similar information; also suggest replacing “this front” in Line 149 with “the EASMF”

Although we did not implement the changes exactly as suggested, the modifications we have made follow the spirit of this comment. We also made a few small changes earlier in the paragraph to improve its clarity.

d. Besides providing the list of references in Line 160-165, can the author add more information for each study (or each group of studies), no need to be long, simply describing XXX used XXX methods to study fronts or airmass boundaries over which region or to study which problem (something like this) would be helpful.
We added a sentence at the end of this part, specifying that the breadth of different methods reflects the variety of subjects tackled by those studies, ranging from global reviews, climatologies of Mei Yu and mid-latitude fronts, to single Mei Yu case studies (and we included relevant references in the manuscript text).

e. Line 165-166. I did not get the point here. Can the authors reword?
Sentences reworded, thanks.

f. Line 168. Suggest deleting "subtropical, or even" because the focus in this study is on the interaction between the tropical and extratropical airmasses.
Done.

g. Line 170. I think the authors also discussed higher level circulations including the STWJ and the extratropical northerly flow.
We agree with this observation. However, in this case the sentence refers specifically to rainfall and low-level winds because those are the key quantities to the "other approaches to front identification" described in the paragraph.

19. Line 198. Did you use a uniform 850 hPa as the threshold? Not sure whether this is a reasonable estimation because the terrain in East Asia is not that uniform, though I agree that the terrains in East China are not that high.
We confirm that the front detection algorithm was applied at the 850 hPa pressure level, following most of the literature presented in Section 2.2.1, including studies focusing on the EASMF. In addition to being the most practical option (ERA5 data on pressure levels), we would argue that the 850 hPa level is far enough from the terrain in East China to allow a safe use of the algorithm, as opposed to (e.g.) the 925 hPa level. The topography of the region is presented in Figure 2, along with dekadal climatological frontal lines, and in Figures 11,13, 17 and 18. These figures show that only the region west of 110°E display a non-negligible terrain influence. This point is made at the end of Section 2.2.2, along with an explanation of how many missing values determine longitudes being skipped.

20. Line 203. How sensitive are the results obtained from this algorithm to the selected threshold? If this has already been discussed in previous literature, suggest the authors add that information here as well.
The gradient threshold chosen in this manuscript is consistent with the relevant studies referenced in Section 2.2.1. To our knowledge, the key points of discussion on frontal identification concern the quantity to be used and the mathematical function to be applied on it, rather than the actual threshold value. However, a preliminary investigation on selected case studies has shown that 0.05 K km^{-1} is close to the 90^{th} percentile of the horizontal gradient of equivalent potential temperature and this seems to agree with Figure 4 in Thomas and Schultz (2019a).

21. Line 214. I am curious about how these captured "other large-scale fronts" look like. I am asking because though these fronts might not look like a typical EASMF that we expect from the climatological rainband progression, but this reasoning might not be convincing enough to exclude their role in contributing to the EASM rainfall. Can the authors comment on this?
The statement on excluding "other large-scale fronts" is related to the choice of restricting the latitudinal domain to a 20° latitude interval centred on the mean gradient-weighted latitude of all points exceeding the gradient threshold on that day. For example, a daily mean gradient-weighted

latitude of 30° N would exclude all points below 20° N and above 40° N. Thus, this could exclude fronts related to tropical activity in the South China Sea and/or fronts associated with higher-latitude Rossby waves, when the EASM front is over the Yangtze river. Figure 1c provides a partial example, with above-threshold values present at around 15° N and not considered by the algorithm. It is clear that those frontal regions are not part of the EASM front and do not contribute to EASM rainfall. In the same figure, the EASM front splits into two bands over land, with the northern one selected by the algorithm. Those bands are not removed by the algorithm step just discussed and their treatment is explained in detail in the next comment.

22. Another question related to the algorithm. Figure 1c shows frontal line over land along the northern front, while the southern land-based front is not considered as front though it appears to be part of the large-scale zonally coherent fronts extending from the land to the ocean. What is the reasoning for this?

In Figure 1C the front, clearly oriented on a single zonal band over the ocean, splits into two bands over land. The southern "land band" lies around the same latitude of the "ocean band". However, our algorithm identifies the northern "land band", a few degrees further north, as front. The reasoning behind this is that the northern band has a higher mean θ_e gradient than the southern one and thus, as the algorithm starts drawing the line over land, this is the band that will be picked up first. If instead the algorithm were to start from the ocean, the southern land band would be chosen, as (at all points after the start) the search for bands is firstly performed in a 5° interval centred on the previous latitude identified. As explained in Section 2.2, since the existence of small-scale multiple fronts within the EASMF was found more likely over land than ocean, starting longitudes selected in this study cover the inland longitudinal extent of the region affected by EASM progression, every 3° from 108°E to 120°E. The modal value of the latitudes identified is then taken at each longitude.
In summary, in cases like the one shown in Figure 1c, the algorithm will prioritise the most intense band over land, as long as this band is located within the 20° latitude interval centred on the mean gradient-weighted latitude of all points exceeding the gradient threshold on that day. This is consistent with our aim of identifying and characterising the general progression of the monsoon front. While the identification of a coherent band is a key feature of this study (and therefore we inserted the 5° interval step), we are choosing to prioritise intensity rather than minimising all latitudinal jumps, which would come at the cost of following less intense (and arguably secondary) frontal bands.

23. The current shading scale in Figure 1 makes it a bit difficult to see the black contours of theta_e. I wonder whether regions where the theta_e gradient is lower than 0.03 can be blanked out.

Thanks for pointing this out. After some experimenting with contours and colour maps, we decided to increase the width of equivalent potential temperature contours (now dashed) to improve their visibility. We did not blank out low-gradient regions because we wanted to keep them distinct from masked and out-of-domain areas.

24. Equations (3) and (4): suggest replacing "1000 hPa" with "surface"

We see the merit of this comment, given that the vertical integration starts from the surface at all grid points where surface pressure is equal to or less than 1000 hPa. However, in all other grid points the integration starts from 1000 hPa, and the choice of levels used for vertical integration is explained in the text. Therefore, we make no changes.

25. Please provide references for ERA-Interim, GPCP, and APHRODITE when these datasets are mentioned.

References added, thanks.

26. For the frontal deformation analysis (Figure 6 and Lines 370-380), can the authors show the F1 and F2 separately as well, and discuss how they tell us about the front and the role of tropical and extratropical air masses?
As mentioned in Section 2.5, F1 and F2 are the two deformation components, with an angle of 45° between each other. Given that the dilation axis is nearly zonal, F1 = ∂u/∂x − ∂v/∂y is dominant over F2. This point has now been added to the discussion.

27. Line 433. Please clarify what it means by saying these southerlies are "monsoonal"
This has been clarified by specifying that the air considered "retains a moisture content typical of South Asian monsoon airmasses".

28. Figure 10 caption, clarify it is zonal wind speed shown in the green contour
The quantity indicated by the green contours in Figure 10 is not zonal wind speed, but the magnitude of horizontal wind speed. This is now specified in the caption (and in the caption of Figure 8). We inspected the text of Section 4.1 to find possible misleading sentences on the matter but couldn't find any. Therefore, no other changes have been made.

References

Parker, D.J., Willetts, P., Birch, C., Turner, A.G., Marsham, J.H., Taylor, C.M., Kolusu, S., and Martin, G.M. (2016). The interaction of moist convection and mid-level dry air in the advance of the onset of the Indian monsoon. Quarterly Journal of the Royal Meteorological Society 142, 2256–2272.

Wang, B. and LinHo (2002). Rainy Season of the Asian–Pacific Summer Monsoon*. J. Climate 15, 386–398.