Second revision - final author comments - Circulation responses to surface heating and implications for polar amplification (egusphere-2023-3066)

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We would like to thank the reviewers again for their feedback. Point-by-point responses to the comments below. Reviewers' comments are in blue, our replies are in black. We highlight the line number changes in the track-changed manuscript in red.

Reviewer 1: Review of "Circulation responses to surface heating and implications for polar amplification"
General comments:

The authors have adequately addressed my previous comments. I recommend the manuscript be approved for publication on the condition that they address my follow-up comments below.

#1. Thank you very much.

Specific comments: (line numbers refer to the tracked changes version)
Line 114-115: Here you state u and E are individually vertically integrated quantities and also that div(uE) is the divergence of vertically integrated moist static energy. This implies that div(<u><E>)=div(<uE>), where < > denotes the mass-weighted vertical integral. I don't think this is generally true. I suggest you explicitly show where the vertical integral operator is in the equation and remove the prefix "vertically integrated" when describing u and E.

#2. Thank you for raising this mistake. The vertical integral should be outside of the product of u and E (i.e., firstly calculate the product of u and E at all individual levels, and then take the vertical integral). Nevertheless, we have treated this term as the residual so the results will not be changed. We have now modified the presentation of the equation. Please see Equation 3 and lines 113-115 in the revised manuscript with track changes.

Line 117: Moist static energy does not include kinetic energy, hence the label "static" energy.

#3. This was also a mistake. We removed the kinetic energy within the bracket. See line 117.

Line 241-243 and 269-272: The way these statements are phrased implies a direction of causality that the weaker circulation response drives the larger radiative cooling response. Do you have evidence to support the direction of causality? If not, I suggest rephrasing to eliminate the implication of causality; e.g., "The weaker circulation contribution in the high latitudes is associated with a stronger radiative cooling contribution.

#4. We do not intend to imply causality between the circulation and radiative processes. We have now modified a few phases to eliminate the implication of causality. Please see line 239 and line 265.

Reviewer 2: Review of “Circulation responses to surface heating and implications for polar amplification”

The authors have addressed all my comments in a satisfactory way. I find the paper appropriate for publication in WCD, except for a few minor issues elaborated below.

#5. Thank you very much.

Specific comments

1) I haven’t noticed it in the previous round, but I think equation (1) is missing something. Looking at the Q-flux in Thompson and Vallis (2018) I see that they set it to a constant negative value in all the places where the expression in equation (1) is negative. Is that done also here? Because otherwise it will become increasingly negative with the distance from the heating center, and this doesn’t seem to be the case.

#6. Thank you for catching this. We set the Q-flux perturbation outside of the paraboloid function (i.e., the negative values) to be zero. We have modified line 84 to reflect this in the revised manuscript (with track changes).

2) In the text explaining equation (3) it says that u is the vertical integral of the zonal and meridional winds and E is the vertical integral of moist static energy, so that the second term on the RHS of equation (3) is the transport of the vertical integral of moist static energy. This equation is wrong, because there should be a contribution from product of the vertically-varying u and E. The vertical integral of the energy transport should be taken after multiplying u and E. For example, the energy transport by the Hadley cell is mostly due to the correlation between the meridional wind, which changes from equatorward to poleward with height and the MSE which increases with height. Taking the vertical integral before multiplying u and E would result in a much weaker energy transport. I understand that this term was calculated as a residual, so this mistake has no effect on the results, but nonetheless it should to be corrected.

#7. This was a mistake also noted by reviewer #1. Please see response #2. We have modified Equation 3 and the relevant text.

3) Figure 6: The subtitle of the second row should be “Vertical potential temperature advection” instead of “Vertical temperature advection”.

#8. We have now modified the figure. See the new Figure 6.

4) The last paragraph of section 3.4: The authors argue that because in the low-latitude heating case the vertical heat transport is more efficient, the radiative cooling to space is also more efficient, providing a negative feedback to the surface warming. It's not obvious to me why. I guess the authors are implying a lapse rate feedback. Are you sure that with the gray radiation scheme in this model and in the absence of clouds the lapse rate feedback is still valid? Maybe, but I don’t think it’s trivial. If the authors use this argument, I think it requires some more explanation.

#9. Although the gray radiation model has no cloud, the model still has prognostic water vapour that can evaporate from the surface, condense and immediately precipitate out in the troposphere. Although the radiation in the model does not interact with the water vapour, the latent heat released during condensation can still warm the troposphere and change the lapse rate. Previous studies show that the lapse rate feedback is important to shape the temperature response in such a gray radiation model (e.g., Henry and Merlis 2019; Feldl and Merlis 2021).

5) In the same paragraph change “balanc” to “balance”.

#10. Thank you and we have now made the changes. Please see line 254.

References:

Henry, M. and Merlis, T. M.: The role of the nonlinearity of the Stefan–Boltzmann law on the structure of radiatively forced temperature change, Journal of Climate, 32, 335–348, https://doi.org/10.1175/JCLI-D-17-0603.1, 2019.

Feldl, N. and Merlis, T. M.: Polar amplification in idealized climates: The role of ice, moisture, and seasons, Geophysical Research Letters,
48, e2021GL094 130, https://doi.org/10.1029/2021GL094130, 2021.