Responses to Reviewer 2's comments

Link to review: https://doi.org/10.5194/egusphere-2024-31-RC2

We thank the reviewer for their constructive comments and feedback. Below we respond to each point raised:

`The authors use the forcing-feedback framework and a feedback analysis based on radiative kernels to examine the mechanisms causing amplified warming at higher elevations in low latitudes. The science is strong, and the manuscript is clear.`

We appreciate the reviewer's positive evaluation of the manuscript.

`I have two major and a few minor comments:`

`I see some similarity between the elevation-dependent warming described here and the overall tendency for amplified warming in the upper Tropical troposphere compared to the surface. The latter is caused by the fact that moist deep convection keeps the Tropical lapse rate close to the moist adiabat, which is steeper in warmer climates. This is somewhat akin to the finding that MSE convergence drives EDW, and both are connected to the increase in water vapour in a warming climate following Clausius-Clapeyron.`

We agree with the reviewer that there are qualitative similarities between elevation-dependent warming (EDW), i.e. along-slope warming of near-surface air, and amplified warming in the tropical upper troposphere. We have conducted preliminary analyses of the along-slope and free-tropospheric warming rates, finding that the along-slope trends vary more strongly with height. This differential warming of along-slope versus free-tropospheric air has been noted previously (e.g., Pepin and Seidel, 2005) but is not well understood; we have added a sentence to the revised manuscript highlighting this as a key question for future research (see Lines 366-368).

We also agree with the reviewer that there is likely a connection between EDW and moist convective adjustment, which constrains the vertical profile of moist static energy (MSE) and underpins understanding of amplified warming in the tropical upper troposphere. This is a question we are currently working on and initial results suggest that a theory based on the principle of convective quasi-equilibrium (Emanuel et al., 1994) predicts the correct sign for EDW (i.e., amplified surface air warming at elevation) but overestimates the strength of the historical EDW signal, for reasons that are being investigated. Our aim is to continue this work and write a follow-up paper interpreting EDW using a convective framework.

`The authors derive forcing as a residual - could you use the CO2 kernel instead, possibly with more idealized runs? Deriving forcing as a residual means there is no residual error to check the quality of the decomposition, which is unfortunate.`

As suggested by the reviewer, a CO_2 radiative kernel could be used to diagnose the influence of changing carbon dioxide concentrations on EDW over the historical period. However we are interested in the net effect of radiative forcing on EDW, and CO_2 is just one of the forcings influencing temperature over this period (anthropogenic aerosols, volcanic eruptions, and others are also important). For this reason, we decided to use the residual method to estimate the total radiative forcing in the CMIP6 historical simulations. A set of idealised, single-forcing simulations—and kernels for each forcing component—could in principle be used to directly estimate the influences of different forcing agents on EDW over the historical period; this would be an interesting topic for future work.

As discussed in the manuscript (see Lines 296-299), to check the accuracy of the residual method for diagnosing radiative forcing, for a single model (GFDL-CM4) we use the corresponding fixed-SST historical simulation from the Radiative Forcing Model Intercomparison Project (RFMIP; Pincus et al., 2016) to estimate the "effective radiative forcing" (ERF). Comparing the influence of this ERF on temperature trends with the influence of the "instantaneous radiative forcing" (IRF), estimated from the coupled historical simulations using the residual method, we find both methods give similar results (see Figure S1 in the supplement). The sum of the contributions to the temperature trends, when the forcing contribution is estimated using the ERF method rather than as a residual, is also similar to the simulated trends (see Figure R1). This similarity between methods, for a single model, underpins our confidence in using the residual method across the other CMIP6 models (note that many of the models analysed do not have corresponding RFMIP simulations).

`Minor comments:`
`17: For people who do not know the topic, the sign of EDW (larger warming at high elevation) is first mentioned halfway through the abstract, and it is not quite clear if this is a new finding or corresponds to what was known before.`

The tendency for stronger warming over elevated surfaces has been discussed extensively in the literature (e.g., Pepin et al., 2015), but this is the first study (to our knowledge) that analyses the EDW signal on large scales, in different seasons, and across a range of models and observational datasets. To better highlight this distinctive contribution relative to previous work, we have modified the first sentence of

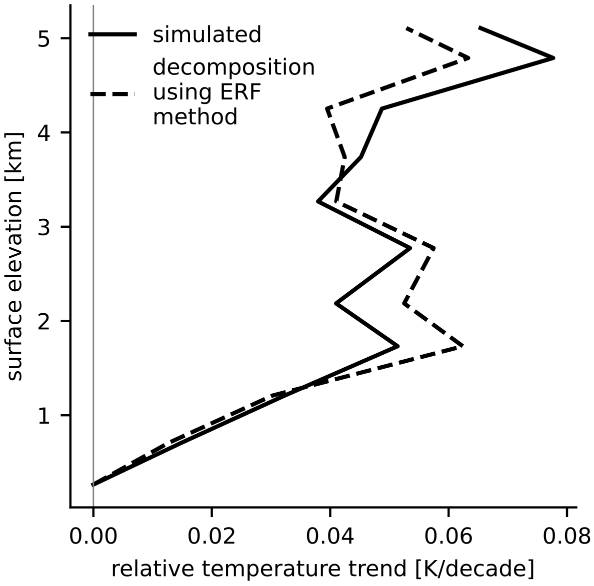

Figure R1: Simulated (solid black line) surface air temperature trends binned by elevation for the GFDL-CM4 historical simulation (1959–2014). The dashed black line shows the sum of the contributions to the temperature trends [see equation (7) in the main text], with the forcing contribution estimated using an "effective radiative forcing" estimated using the RFMIP fixed-SST historical simulation (named *piClim-histall*).

the second paragraph of the abstract so that it now reads (see Lines 5-6): *"Here we expand on previous regional studies and use gridded observations, atmospheric reanalysis, and a range of climate model simulations to investigate EDW over the historical period across the tropics and subtropics (40°S to 40°N)."*

`Fig. 1: Why use standard seasons when the data spans the equator? Wouldn't local summer/winter be more consistent?`

This is a good suggestion; we have re-plotted Figure 1b in the main text using local seasons rather than standard seasons. The difference is minimal, but using local seasons does increase the EDW index in winter.

`If you insist of changing the axis scaling within a Figure, please make a clear visual mark of that.`

We have added horizontal dashed lines to Figure 1 in the main text to identify the boundary between the linear and logarithmic regions of the scale (and have noted this in the revised figure caption). We have also expanded the range of the linear region so that only the CMIP6 outliers fall within the nonlinear region. We hope this makes the figure more acceptable to the reviewer and understandable to readers; we do not insist on this particular scaling, but find it to be a helpful way to see the central estimates and the outliers all on the same plot.

`Fig. 2 What are the gray lines?`

The grey contours mark the 1 km and 2 km surface heights, and thus outline the areas of high orography. Thank you for alerting us to this omission. We have modified the caption of Figure 2 to state this.

`Footnote p 15 - could you include this in the main text?`

Following the reviewer's suggestion, this footnote has been incorporated into the main text (see Lines 296-299).

`l. 316 - see comment above, CC-relation plays a role in MSE gradients`

We agree with the reviewer that CC-mediated changes in surface air specific humidity (along with temperature) undoubtedly influence changing patterns of MSE convergence. We have modified the final sentence of section 5.3.8 to highlight this important point (see Lines 326-327).

References

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Pepin, N., and D. J. Seidel, 2005: A global comparison of surface and free-air temperatures at high elevations. *J. Geophys. Res.-Atmos.*, **110 (D3)**, doi: 10.1029/2004JD005047.

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Pincus, R., P. M. Forster, and B. Stevens, 2016: The radiative forcing model intercomparison project (rfmip): experimental protocol for cmip6. *Geosci. Model Dev.*, **9 (9)**, 3447–3460, doi:10.5194/gmd-9-3447-2016.