Reply to Reviewer 3

We thank the Reviewer for the careful reading and evaluation of the manuscript and the good comments which helped a lot to further improve the paper. In the following, we address all comments and questions raised (Reviewer's comments in italics). Text changes in the manuscript are highlighted in color (except minor wording changes).

Besides several specific comments, we see two common main concerns raised by both Reviewer's, regarding (i) the presentation quality and clarity of figures, and (ii) the discussion of specific climate model characteristics. A short overview of the related changes in the revised manuscript is:

(i) To enhance the **presentation quality and clarity** we modified all figures in the revised manuscript. In particular, we chose different color schemes, reduced the number of contours, changed to difference plots for certain quantities and improved the model-correlation visualization (a detailed list of figure changes is in the reply to the specific comment below). We are confident that these changes clearly improve the presentation of our results.

(ii) To relate our results to specific **model characteristics**, we included a new subfigure Fig. 3 (e, f). This figure shows the inter-model correlation for wind velocities averaged over the region of maximum signal, and includes information on horizontal and vertical resolution of the models by highlighting models with relatively high number of horizontal grid points with horizontal lines and models with high number of levels with vertical lines. Based on this simple model classification, no clear relation of resolution differences to the simulated water vapour bias is found. These findings are briefly discussed at the end of the discussion section and described in more detail in the Methods section 5.1.5.

A more detailed reply to all comments and description of the changes in the revised manuscript is given below.

Overall comment:

Ploeger et al. report on the impact of moisture biases in the Pacific UTLS on regional circulation patterns at the tropopause level, such as the anticyclone Monsoon circulation and its zonal extent over the Pacific. They show that a modified Lagrangian scheme improves a ubiquitous deficiency of the EMAC model, in terms of the UTLS climatology of water vapor (too "zonal" and moist bias over W-Pacific) and this improvement also has effects on the circulation, such as on the Monsoon anticyclone structure (reducing the zonally too broad and strong Monsoon anticyclone, and too strong westerlies over the tropical Pacific). I find this study really interesting, and the paper is well structured. I only have some minor suggestions. I recommend prompt publication upon addressing them.

Thanks for this positive evaluation of the manuscript!

General comments:

1) I think that for CLAMS, the role of stratospheric water vapor (SWV) can be indeed nicely isolated with the suite of experiments presented in the paper. However, I am less convinced about the "generalization" for all CMIP6 models: I am not entirely sure that the "cross correlation" of U-wind vs SWV (Fig. 4) can be really taken as "proof" that moist biases in other models have the same effects on the circulation as demonstrated for EMAC. This is essentially shown in Fig. 4c and 4d. I see quite a difference, for example, between the effects of CLAMS in EMAC (panel b - this should be the "impact of SWV improvements") and the relationship between inter-model spread in SWV and U in CMIP6 (panel d) - the location of these correlations is quite different. Can the authors maybe test the correlation between SWV and U (on e.g. inter-annual time-scales instead of "inter model") within CLAMS and EMAC directly, to support their inferences about dynamical impacts of SWV biases in other climate models?

We do fully agree with the reviewer that the CMIP inter-model correlation alone should not be taken as a "proof" for the effects of moist biases on circulation. In the EMAC model experiment, on the other hand, the effect of stratospheric water vapour changes can be well isolated, and the EMAC control minus modified–Lagrangian differences (e.g. Fig. 3 a/b) unambigously show the effects of stratospheric water vapour differences in the model experiment. Hence, our argumentation is always based on the similarity of inter-model correlation and the model experiment. If both show similar patterns, also the mechanism is expected to be similar. In the second paragraph of the revised discussion we try to explain this line of argumentation more clearly and also clearly state that the results for CMIP6 are not a strict proof but more a hypothesis: "Based on similarity between CMIP6 inter-model correlations with the effects of the Pacific moist bias in the EMAC model experiment, we hypothesize in the following that CMIP6 model differences could partly be related to the moisture content in the Pacific UTLS."

Furthermore, we also see that for the Pacific zonal wind the correlation is somewhat weaker and the patterns show larger differences. We added the new Fig. 3 e–f to show the model correlation in the relevant region (tropopause region above the Pacific) even clearer. Also, we included a cautious note when discussing the figure: "Furthermore, CMIP6 inter-model correlations between zonal wind and Pacific UTLS moisture show a significant correlation in the Pacific subtropics around the tropopause (Fig. 3 d, f), although weaker than for meridional wind at the eastern monsoon edge (Fig. 3c)."

Analysing inter-annual variability further could be interesting, but would optimally need longer simulations than the 20 years we have available, and also the existence of many other additional variability factors will likely obscure a clear picture. Instead, we present the correlation for wind velocities averaged over a larger region in the new Fig. 3 e/f. Although also this correlation is not too compact, it is clearly significant, and we use it to discuss the applicability and limits of the approach as well as effects of certain model characteristics, as suggested by the other reviewers.

2) While the role of SWV can be indeed nicely explained, the role of other radiatively active species in the stratosphere is a lot less clear. Among them, ozone is another major heating source in the tropical stratosphere, but it's not discussed at all. I would expect the implementation of CLAMS to also affect the ozone in e.g. indirect ways. Have the authors looked into changes in ozone between the regular EMAC and EMAC-Clams? Would these be big enough to also play a role in the differences seen in terms of the large-scale circulation?
Considering other trace gas species besides water vapour is definitely a very good suggestion. However, so far the coupling of Lagrangian transport in EMAC to radiation and dynamics only works for water vapour. Extending this approach to other chemical tracers (e.g. ozone) is the focus of ongoing model development work. On the other hand, it could also be seen as an advantage of the current simulation set-up for the present paper, that it allows the effects of stratospheric water vapour to be unambigously isolated, without interference with the effects of other trace species. To briefly mention these aspects we added a short sentence about the effects of other trace species besides water vapor at the end of the revised discussion: "Differences in model transport could also affect other trace gas species with large gradients, like ozone. However, as water vapour shows particularly steep gradients in the UTLS also the associated effects on circulation are expected to be comparably strong."

3) While the role of the westerly wind duct is clear (at 100 hPa) in linking SWV biases and the anticyclone circulation, the role of other (prominent) dynamical features of the lower stratospheric circulation are less clear... such as, for example, the QBO jets, the cold point tropopause, the tape recorder, etc. I would recommend the authors to give a "broader" view of the effects of the implementation of CLAMS, aside from the localized effects at 100 hPa.
As explained in the introduction (last paragraph) the focus of the present manuscript is on regional water vapour biases and regional circulation effects. Global effects on the zonal mean state of lower stratospheric water vapour changes have been discussed in the recent paper by Charlesworth et al. (2023), including effects on subtropical and eddy-driven jets, stratospheric circulation upwelling (related to the tape recorder) and the cold point tropopause. We precised the related text in the revised manuscript so that these issues become clearer (in the second paragraph of the introduction): "Charlesworth et al. (2023) showed that lower stratospheric water vapour, in particular the model moist bias in that region, exerts a first order effect on the zonal mean atmospheric circulation, with water vapour increases causing a strengthening of the stratospheric circulation, upward and poleward shifts of the subtropical jets, and a poleward shift of the tropospheric eddy-driven jet."

Specific comments:

L4: *I'd recommend adding a specific altitude range when talking about "regional circulation systems" (this also applies to L8).*
We added "...regional circulation systems in the UTLS" to make the altitude range clear.

L132: *I'm not entirely convinced about the causality... as many things change across different CMIP6 models. What about, for example, the role of vertical resolution across them?*
We fully agree with the reviewer here that also other factors likely play a role besides UTLS water vapour. Therefore, we chose a weak formulation here: "overestimated westerly ducts... appear to be related, at least partly, to the model moist bias ...". In the following, new paragraph in the revised version , related to the new subfigure Fig. 3 e–f we also briefly discuss resolution effects (see general comment (ii) above).

L143: *What about the effects of ENSO on the water vapor? WOuld that relationship also change in the CLAMS version*

of EMAC?
This is a good question. ENSO substantially affects the moisture budgte in the UTLS and it could well be that with a different representation of moisture transport in the model (as implemented here) also the simulated transport effects due to ENSO change. Focussing on inter-annual variability would be an interesting topic for future work, but would also require to extend the simulation length beyond the 20 years considered here.

Figure 4: *Would it be possible to also see the lines for EMAC and EMAC-CLAMS in this figure?*
We agree that it would be helpful to include some comparison between the EMAC model experiment and the CMIP6 simulations. However, these different model simulations are not exactly comparable as the EMAC experiment doesn't use exactly the same configuration as the historical CMIP6 simulations considered (as described in the appendix). Hence, a too detailed comparison between the EMAC experiment and CMIP6 could be misleading.
Nevertheless, we include the relation between u- and v-wind speed and the Pacific UTLS moisture from the two EMAC simulations (control and modified–Lagrangian) into the new figure Fig. 3 e–f together with the relation for CMIP6 models. On the one hand, this figure shows that also for the modified-Lagrangian simulation the agreement with the observaions is not perfect. On the other hand, the difference between the two EMAC model versions agrees well with the mean relation for CMIP6 (black dashed line), providing additional support that a similar mechanism is at work.

Why is 250 hPa the lower boundary chosen for CLAMS? Are results sensitive to this choice?
The level 250 hPa (more precisely, the model level closest to 250 hPa) as the lower boundary for the Lagrangian water vapour calculation has been chosen to ensure that it is only stratospheric water vapour that differs between the control and the modified–Lagrangian simulations. In pricinple, any level could be chosen, and as long as that level is below the tropopause the results for the lower stratosphere will be largely insensitive. In the model set-up used here, the Lagrangian transport calculation also extends all down to the surface, but below 250 hPa it is overwritten with the EMAC control water vapour field in each time step. This guarantees that both simulations have exactly the same water vapour field in the troposphere below about 250 hPa.

General recommendation for 5 Appendix → I think this info should be moved into the main text, as lots of essential information is "packaged" into the Appendix, Since there are no length limitations for this journal that I'm aware of, I'd strong recommend restructuring and move all this info into the main paper.
We understand that for ACP–Letters it is required to have all data and method information in the Appendix. Also, there is a length limit of 2000 words for the main part of the manuscript. Hence, we stay with the structure as is.

General comment: while the impact of the diffusive transport scheme is clear and nicely demonstrated, it would be nice if the authors could comment on the role of other features on SWV, such as convective overshooting. Are there any changes in the Monsoon Anticyclone that are also driven "from the troposphere" or do all the differences only originate in the stratosphere?
We totally agree that in general the UTLS moisture budget is affected by many different transport processes, including advection, mixing, convection, etc. In the control EMAC simulation in the UTLS, however, the water vapour distribution is largely controlled by advection and dehydration. This was shown from an additional EMAC sensitivity simulation with all water vapour tendencies output, as plotted in supplement Fig. 2. At least in EMAC at these levels the contribution of convective transport is minor. These sensitivity results are discussed in the last paragraph of the "EMAC model simulations" appendix section. Related to the reviewer comment we added the note: "This EMAC sensitivity analysis does not exclude a more significant effect of convection in other models or in the real atmosphere."