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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of the stratosphere on the tropospheric zonal wind response to CO_2 doubling" *by* Y. B. L. Hinssen et al.

Anonymous Referee #1

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General comments

This study examines the influence of changes in stratospheric zonal wind resulting from a doubling of CO2 on the tropospheric circulation using a PV inversion. Calculations are carried out using two different models. This has not been done before to my knowledge, and therefore this study has potential, but suffers from some problems described below.

I do not find the link to variations in heat flux is shown very convincingly. Firstly, the authors should use timestep output from the models to calculate the heat flux and not monthly means. Secondly, there is no clear physical discussion of where and how

much changes in heat flux might be expected to alter stratospheric PV. Lastly, the authors make qualitative comparisons of the changes in heat flux and the changes in stratospheric PV, and find apparent consistency in some cases and inconsistency in others. They conclude that the heat flux variations explain the changes in PV in the former case, but that other mechanisms are important in the latter. But since there is no clear expectation of where and how much we would expect the heat flux variations to change the PV, this is hard to assess.

This study discusses PV inversion, but does not point out that this approach is equivalent to 'downward control' calculations. For example, Thompson et al. (2006) showed that downward control can explain much of the tropospheric response to variations in stratospheric wave driving, though not the full zonal structure. Thompson et al. examine stratosphere-troposphere coupling in the context of variability rather than the CO2 resopnse, but their study remains relevant.

Specific comments

Ln 23897, ln 7: This is related to an increased vertical gradient of the wind in the tropopause region, not increased westerly wind itself.

Pg 23898, ln 3-5: Poor English. Rephrase.

Pg 23898, Ln 13: HadAM3 is a version of the Unified Model, not the other way round. Also what is meant by 'based on' here? Are the authors saying that the model used was HadAM3 coupled to a slab ocean (this model is called HadSM3), or that the model used is based on a such a model? If it's the latter, then more details need to be given on the model used. How many levels did it have? What changes were made compared to HadSM3? The authors cite Gillett et al. (2003), who used a 64-layer version of HadSM3 – but it's not clear whether this is the model that the authors are referring to. If only a 19-level UM version was used, then some justification needs to be given for using a model with limited vertical resolution in the stratosphere.

Pg 23902: Explain somewhere here how the inversion is carried out based on stratospheric PV changes only.

Pg 23903, ln 4-6: v'T' should be calculated from timestep output of the model, not from monthly means. Using monthly means only considers the stationary component of the heat flux, and not that associated with transient eddies.

Pg 23904, ln 19: Looking at the definition of PV, uniform cooling won't cause an increase in PV – the PV change must depend on the vertical and meridional gradient of that cooling. Is this just the gradient from tropospheric warming to stratospheric cooling?

Pg 23905, ln 1-3: This expected influence of the wave forcing on PV should be clearly explained at the start of this section (how would changes in eddy heat flux in the mid-latitudes be expected to change stratospheric PV?).

Pg 23905-23906: Comparing Figures 2 and 3 it is hard to see a clear link between the heat flux changes and the PV changes – in some cases there appears to be a link (NH winter heat flux and PV changes), but in other cases there does not (ECHAM shows a large decrease in heat flux in the SH, but this doesn't have a clear influence on the PV response). I did not find this section wholly convincing.

Pg 23905, ln 7: I would dispute this. Just because the the stationary wave component of the heat flux has a similar seasonal cycle to the transient component of the heat flux, this doesn't mean that the two will respond in the same way to a doubling of CO2. For example the previous paragraph cites literature suggesting that an increase in CO2 will enhance baroclinic wave generation – this will manifest itself mainly in the transient eddy heat flux and not in the stationary wave component.

Pg 23910, ln 28: I don't think the authors have demonstrated that the PV response is strongly coupled to the change in heat flux. I think a more accurate conclusion would be that they seem to be consistent in some cases and not in others.

Pg 23911, ln 28-29: This has not been clearly demonstrated. To do this, the authors would have to estimate the change in stratospheric PV associated with radiative forcing (e.g. from a fixed dynamical heating version of each model), and then difference this with the change predicted by the full GCM to derive the dynamical component.

Pg 23915: It is hard to interpret the superposed contours. If retained, it would be better to show the climatology and changes in two separate plots. However, the climatology is only discussed to compare it with ERA-interim, but the ERA-interim PV is not shown. I would recommend just showing the response to CO2 doubling.

Pg 23916, Caption, ln 5: Replace 'axis' with 'NH'.

Pg 2319: Consider using red shades for positive, blue for negative in the lower set of panels here and in the other figures. This clearly differentiates between positive and negative changes.

Pg 23921-23922: It is confusing to have south to the right on these plots and north to the right on the previous two. Reverse the direction of the x-axis on these plots.

Pg 23921: This model does not appear to show a poleward shift in the SH extratropical jet in response to the CO2 doubling, which is seen in almost all other models? Is this correct?

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