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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.

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We thank the referee for his positive words and constructive comments regarding our manuscript. Below we give a point-by-point response to the referees' comments, where the comment is repeated, and the response is given directly below each comment.

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.

1) I do not find the link to variations in heat flux is shown very convincingly.

a. Firstly, the authors should use timestep output from the models to calculate the heat flux and not monthly means.

It would also be interesting to perform our study with timestep output from the models, but unfortunately this information is not available in our model-output (which was not generated by ourselves), and therefore we use monthly means. A sentence is added to the conclusions (section 5) to address that this would be an interesting subject for further research.

b. Secondly, there is no clear physical discussion of where and how much changes in heat flux might be expected to alter stratospheric PV.

We agree with the referee that such a discussion is not given in the current paper, but it is described in detail in Hinssen and Ambaum (2010). This reference and a short summary of their findings is now added to the end of section 2.

c. 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.

Hinssen and Ambaum (2010) find that on average about 50% of the interannual variability in the state of the Northern Hemisphere stratosphere can be related to variations in the 100 hPa heat flux. This indicates that there is a quantitative relation between the stratospheric PV and the heat flux, but that other processes also affect the stratospheric PV. In the present study we merely want to point out that it is possible (and taken together with the Hinssen and Ambaum study we even think that it is likely) that the changes in the stratospheric PV are related to changes in the heat flux. Further

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study in line with the work of Hinssen and Ambaum would be needed to examine the relation between the stratospheric PV and heat flux under climate change in more detail. This could give more certainty about the influence of changes in the heat flux on the stratospheric PV for the different hemispheres and different seasons. Timestep output from the models would indeed be needed for such a study. This is now also proposed in the concluding section as an opportunity for further research.

2) 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.

A sentence has been added to the end of the introduction to acknowledge the similarity between the downward control framework and PV inversion.

Specific comments

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

This is indeed true, and corrected in the revised manuscript.

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

This sentence is removed in the revised manuscript, since it does not add much information. The sentences that follow already describe the work of this paper, and the new aspect of our work follows from combining this with the sentences before the removed sentence.

5) 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.

Data were indeed used from the model HadSM3, similar to the 64-level version used by Gillet et al (2003). This is clarified in the text of section 1.

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

The text of section 2 (second last paragraph) is adjusted to explain the stratospheric PV inversions.

7) 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.

As noted in the response to comment 1), using timestep output from the models would be preferable, but is, unfortunately, not possible given the data we have at our disposal. We agree that the v'T' flux based on monthly mean does not present a complete view of the heat flux, but in the present study we want to use this part of the heat flux to point out that variations in the heat flux are possibly related to variations in the stratospheric PV.

8) 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?

We agree that the definition of PV does not indicate that a uniform cooling will cause a

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change in the PV, but CO2 doubling results in cooling of the stratosphere that increases with height (see e.g., Bell et al 2010) and hence results in a change in the stability and the PV. The relation between the diabatic heating and PV is given by the PV evolution equation as given in, for example, equation 3 of Delden (2003). Equation 6 in Delden (2003) indicates that due to the strong increase of PV with height in the stratosphere, a decrease or weak increase of the diabatic cooling with height will lead to an increase in the PV. A more detailed study of the diabatic heating is beyond the scope of this study, but the equations in Delden (2003) indicate that it is not trivial to say how a diabatic cooling will affect the PV, especially in a PV stratified region like the stratosphere. It would be worthwhile to investigate this in a future study, by quantifying the different terms in the PV evolution equation. This would lead to more insight into the effect of diabatic heating on the PV. This is clarified in section 3 of the manuscript.

A possible mechanism for additional PV changes could be as follows: The tropospheric warming and stratospheric cooling lead to an increased horizontal temperature gradient near the midlatitude tropopause, this corresponds to increased vertical wind shear, which can again influence wave propagation to and within the stratosphere, which could again affect the stratospheric PV.

9) 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 midlatitudes be expected to change stratospheric PV?).

This PV-flux relation is described in Hinssen and Ambaum (2010), which is now explained in section 2.

10) 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.

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See the response to comment 1)c.

11) 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 CO_2. For example the previous paragraph cites literature suggesting that an increase in CO_2 will enhance baroclinic wave generation – this will manifest itself mainly in the transient eddy heat flux and not in the stationary wave component.

You are right. The sentence has been adjusted by saying that the monthly mean data might be suitable to obtain an estimate of the seasonal cycle of the heat flux. As pointed out in the response to comment 7), further research with more detailed model data is needed to be able to draw firmer conclusions about the heat flux changes due to climate change.

12) 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.

The conclusions have been adjusted to indicate that there is a link between heat flux and PV response, but that more research is needed to clarify this aspect.

13) 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.

We agree with the referee that at the moment no firm conclusions can be drawn about this aspect, but that the present results combined with the work of Hinssen and Ambaum (2010) indicate that wave effects play an important role in the Northern Hemisphere. The conclusions are adjusted to indicate this.

14) Pg 23915: It is hard to interpret the superposed contours. If retained, it would

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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.

We choose to retain the superposed contour-plots to illustrate the seasonal cycle in the PV anomaly and changes therein due to CO2 doubling. We decided not to show difference plots since these are rather noisy, likely due to the interpolation to isentropic coordinates and the change of sign in the PV anomaly in autumn and spring.

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

The term "x-axis" is replaced by "horizontal axis" to indicate that the months noted on this axis are for the NH.

16) 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.

We have considered your point, and experimented with the colorscale. However, we had some difficulties with finding an appropriate scale that improved the figures, and more importantly, this would mean 2 additional colorbars to indicate the legends for the PV and wind difference, and we do not think this will make the figure more clear. Therefore we leave the figure as it is and refer to the title, colorbar and caption of the figure to indicate that the lower panels display differences between the double CO2 and the control run.

17) 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.

We choose to have the pole to the right of the plot for both hemispheres to ease comparison between the hemispheres. It is indicated in the text and in the figures whether the Northern or Southern Hemisphere is considered.

18) Pg 23921: This model does not appear to show a poleward shift in the SH extrat-

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ropical jet in response to the CO2 doubling, which is seen in almost all other models? Is this correct?

This indeed seems correct, although an increase in the wind speed is found somewhat poleward of the climatological subtropical jetstream (but the wind speed at the location of the jet also seems to increase).

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