

## ***Interactive comment on* “The influence of the stratosphere on the tropospheric zonal wind response to CO<sub>2</sub> 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.

- Does the paper address relevant scientific questions within the scope of ACP? Yes, the paper focusses on atmospheric processes connected to climate change in the stratosphere. - Does the paper present novel concepts, ideas, tools, or data? Yes, the analysis is novel in this context. - Are substantial conclusions reached? Conclusions are not very strong but are defensible. - Are the scientific methods and assumptions

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valid and clearly outlined? The methods are clearly described but their validity is not fully established. - Are the results sufficient to support the interpretations and conclusions? The paper is weak in this respect, interpretation is not necessarily valid. - Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes - Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes - Does the title clearly reflect the contents of the paper? Yes - Does the abstract provide a concise and complete summary? Yes - Is the overall presentation well structured and clear? Yes - Is the language fluent and precise? Yes, with some issues of interpretation. - Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes - Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The paper's text requires clarification. - Are the number and quality of references appropriate? Yes - Is the amount and quality of supplementary material appropriate? Not applicable.

## II General comments

1) Piecewise PV inversion has been used widely in the meteorological literature to decompose the dynamics of circulation systems (e.g. synoptic systems and stratospheretroposphere coupling events) that involve strong spatial coupling. PV inversion is applied here in a novel way to the problem of the stratospheric and tropospheric response to climate change, in order to see whether some aspects of the tropospheric response is of stratospheric origin. My evaluation of this paper is that the analysis undertaken is largely technically correct, but that the interpretation of the results is weak. Some of this weakness can be improved by revision, but some of this weakness stems from the ambiguities in the piecewise PV inversion technique. The authors have shown convincingly that the PV response to climate change in the stratosphere is in balance with significant wind anomalies in tropospheric winds via PV inversion. But this does not explain unambiguously the part of the tropospheric response that is caused by stratospheric change.

Indeed we limit our study to that part of the stratospherically-induced tropospheric change that is due to changes in the stratospheric PV. We show the balanced response to PV changes in the stratosphere and show that the tropospheric winds change due to adjustment to thermal wind balance. This involves the total stratospheric PV changes. These PV changes in the stratosphere can be related to radiative effects in the stratosphere, but also to a change in wave forcing from the troposphere, so part of the stratospheric changes may originate from tropospheric changes.

2) It would be useful for the authors to state more clearly what they are trying to explain. The key figures are the two right panels in the bottom row of Figs. 5-8, and Figs. 10-11. It is clear from Figs. 5-8 that the stratospheric PV accounts for the stratospheric winds, but what does it really explain about the tropospheric PV field? Sometimes the stratospheric PV inversion matches well with the tropospheric wind response, and sometimes it doesn't. For example, in three of the four cases in Figs.10-11, the seasonal cycle of the tropospheric wind response is not at all well captured by the inversion. This seems to imply that the \*tropospheric\* PV response is an important contributor to the tropospheric wind response in most cases. The authors state, rather weakly, that the stratospheric PV related wind anomaly is comparable to the wind response (p. 23910, line 10). This is not surprising given the papers on strat-trop coupling by McDaniel, Black and others. But the overall lack of agreement between the pattern of the wind response and the pattern of the PV inverted wind anomaly is disturbing and leaves one with the sense that little has been explained.

We agree with the referee that we cannot explain the total tropospheric response to CO<sub>2</sub> doubling with the PV changes in the stratosphere, and we note this by mentioning that the structure of the total response in the troposphere differs from the structure of the response in the troposphere found from the stratospheric PV inversions. It would of course have been interesting if the structure of the total response and the response of the stratospheric PV inversions had been more similar in the troposphere, but that is not what we find in the present study. So the tropospheric PV response and/or

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feedback mechanisms like a change in the wave propagation are indeed important in forming the tropospheric response. What we want to point out with the present study is that it can however be important to involve the stratosphere in climate studies, since changes in the stratosphere can modify the wind response in the troposphere. The conclusion (section 5) has been adjusted to clarify what our findings are.

3) The paper does not make clear the different roles of the stratosphere in PV inversion and in the response to external perturbations to the stratosphere. These really aren't the same thing and yet they are spoken of as two aspects of the same problem. This becomes really confusing when talking about the long-term response to climate change. In Transformed Eulerian Mean theory (Haynes et al. 1991), diabatic heating changes in the stratosphere will generally lead to only very weak wind response in the troposphere; a change in vertically integrated wave driving is required to change the tropospheric winds in the absence of other effects in the troposphere. This implies that the authors should make clear two things: first, a change to CO<sub>2</sub> radiative heating alone will have no impact on the troposphere. Second, a change in wave driving in the stratosphere could lead to a PV response in the troposphere along with a PV response in the stratosphere. So in that case it is unclear why a piecewise PV inversion, with non-zero PV in the stratosphere and zero PV in the troposphere, is something useful to think about.

It is not our goal to separate changes in the stratosphere caused by internal processes from changes caused by external processes/changes in the troposphere. Equation 3b in van Delden (2003) indicates that diabatic heating affects the PV. Hinssen and Ambaum (2010) further show that wave forcing of the stratosphere affects the PV. In the present study we only examine the total PV changes in the stratosphere, and do not examine separate effect of radiative heating caused by CO<sub>2</sub> doubling alone. From the present study we can therefore not conclude that this radiative forcing alone will have no impact on the troposphere. Equation 3b in van Delden (2003) indicates that the horizontal and vertical structure of the diabatic heating are of importance for the

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effect on the PV, and that even a uniform diabatic heating will affect the PV due to the increase of PV with height in the stratosphere.

As PV is a fundamental dynamical quantity and the inversion is a powerful method to derive nonlocal effects we believe that this method is a powerful way to investigate the coupling between the stratosphere and troposphere.

4) Another concern is that the authors have not really shown quantitatively how the PV response in the stratosphere is related to radiative and wave driving responses. The arguments based on the seasonal cycle of PV response versus seasonal cycles of wave driving responses are weak. A useful starting point would be to state more clearly what the direct radiative response of PV is expected to be. E.g. on p. 23904 line 19, it is not clear why an enhanced cooling to space will lead to an accompanying increase in stratospheric PV. I don't think this point is trivial or necessarily correct. Then, on top of this, the wave driving response will further modulate the PV response, presumably in different ways in the different models. The review discussion on p.23897 is also quite weak dynamically.

The relation between the eddy heat flux and stratospheric PV and its physical basis is described/studied by Hinssen and Ambaum (2010). This reference and a short summary of their findings have been added to the end of section 2. With respect to the comment that the wave driving response will further modulate the PV response we want to mention that we already take this wave driving response into account in the PV response. The effect of waves on the stratospheric PV gives a stratospheric PV change, which is captured in the total stratospheric PV change. So we do not separate the stratosphere from the troposphere, but also take changes in the stratosphere that are caused by (wave) changes in the troposphere into account.

III Specific comments

5) p.23896,l.14: What is meant by "westerly influence"?

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A westerly influence on the tropospheric winds means that the tropospheric winds will be more westerly than without this influence (this can mean stronger westerlies or a switch from easterlies to westerlies). This is clarified in the abstract and in the text when the term “westerly influence” is used for the first time.

6) p.23897, l.4: "sensitive to" -> "reflects"

The term “is sensitive to” is replaced by “reflects”.

7) p.23897, l.8: What is rationale for saying "but with a possible downward influence towards the surface"?

This sentence has been adjusted to indicate that a change in the vertical gradient of the wind near the tropopause might affect the tropospheric winds.

8) p.23897, ll.11-14: Perhaps it would be better to say that the BDC dynamical response modulates the radiative response, sometimes attenuating it, sometimes amplifying it.

We want to point out here that a stronger Brewer Dobson circulation, as is seen in most climate models in enhanced CO<sub>2</sub> experiments, would warm the Arctic lower stratosphere.

9) p.23898, l.3: more \*generally\*

This sentence is rewritten and the term “general” is no longer used, so this comment is no longer applicable.

10) p.23902, l.1: The justification for the lower boundary condition is weak. What is the sensitivity to changing the lower boundary condition? This is important because the tropospheric wind anomaly will be sensitive to this aspect.

The results are of course sensitive to the lower boundary condition, but zero thermal wind is believed to be the best lower boundary condition for the stratospheric inversions. There is no knowledge about the wind or temperature at the surface before the

inversion, and we want the wind to be able to adjust to the stratospheric PV, so we don't want to set the wind equal to zero at the surface.

11) p.23902, l.24: It is not explained why wave fluxes are now being talked about.

This is now made more clear by adding the reference to Hinssen and Ambaum (2010) which indicates a relation between PV and eddy heat flux.

12) p.23904, l. 19: a. As stated above, it is not clear how radiative cooling will affect 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. This is clarified in section 3 of the manuscript. The equations in Delden (2003) however indicate that it is not trivial to say how a diabatic cooling will affect the PV, and that it would be worthwhile to investigate this in a future study.

b. On the next page, it is not clear why more wave activity flux would lower the PV.

The relation between PV and heat flux is described in detail in Hinssen and Ambaum (2010) and this paper is now referenced in section 2.

13) p.23907, ll.8-10: I did not understand this. Could this behaviour be demonstrated with a simple example?

Considering, for example, the subtropical jet, the maximum wind speed is found near the tropopause, so the wind speed increases if you move from the surface to the tropopause. In the doubled CO<sub>2</sub> climate this is also the case, but now the surface has warmed by 3K so in isentropic coordinates this means the jet has shifted upwards by 3 K. This means that below the old jet maximum the windspeed will have decreased in isentropic coordinates merely because the jet shifted with respect to the vertical axis. A sentence has been added to clarify that the increase of potential temperature with

height is an important aspect of the argumentation.

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