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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 #2

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- 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 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 stratosphere-troposphere 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.

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 some-

times 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.

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 CO2 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.

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.

III Specific comments

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

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

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

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.

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

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.

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

12. p.23904, l. 19: As stated above, it is not clear how radiative cooling will affect PV. On the next page, it is not clear why more wave activity flux would lower the PV.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23895, 2010.