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Interactive comment on "Kinematic and diabatic vertical velocity climatologies from a chemistry climate model" *by* C. M. Hoppe et. al.

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We acknowledge the reviewers for the helpful comments on our manuscript. All comments were taken into account in the revised version and we feel that the manuscript has improved greatly through these revisions. Below, we reply point-by-point to the referee comments:

1. Discussion of vertical velocities (Section 3): The discussion of differences in kinematic versus diabatic velocities is certainly interesting and worthwhile showing. I feel that it could be improved by the following points: a) Presentation: - move the monthly Figures 4/5 to Appendix / Supplement. Instead, I would find more illustrative of the relevant differences (e.g. shift of tropical pipe) a latitude-time plot at relevant heights

(e.g. 500 K,..), including a difference plot.

We moved the monthly figures to the appendix and replaced them by a seasonal plot of DJF/JJA.

- I find it hard to see the relevant features in Fig. 6 (in particular in association with the discussion in Sec. 4). Maybe you can think of a different way to illustrate the relevant differences (see above).

We tried different ways of plotting but came to the conclusion that the figure in its present way is most clear.

b) Attribution: While the differences are discussed in great detail, little explanation is given. I understand that this not an easy task, but some discussion should be added. For the diabatic velocities, it should be possible the separate the impact of the different processes. In this respect, the analysis in the Appendix are very interesting, and I encourage the authors to discuss those results more (and maybe move them to the main body of the paper). How large are the differences caused by using different convection parametrizations as compared to the differences between Kinematic to diabatic velocities? Can those sensitivities reveal anything about the causes of differences kinematic vrs. diabatic velocities?

We followed the suggestion of the reviewer and moved the discussion of the effect of the different convection schemes to the main part of the paper. Additionally, we compared the diabatic vertical velocities resulting from the Tiedke scheme with the vertical velocities resulting from the two other convection schemes. We produced new difference plots (Fig. 4) using the same units and color scale as in Fig. 5.

2. Age of Air (Section 4): Again, the investigation of the AoA differences resulting from the different vertical velocities, as well as different advection schemes is certainly relevant. While some explanations are given, and connections are made between the differences in the vertical velocities and AoA differences, they are on a rather specu-

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lative level. For example, in the conclusion it is stated that "there is a clear correlation between vertical velocity and age of air" - However, no correlations or robust connection is made. Possible additions could be: - If in any way it is possible to run either of the advection schemes with the other vertical velocity this would be a great comparison, and a clean separation of the two factors causing differences in AoA. - Otherwise, analysis of the differences caused purely by the different vertical velocities could be obtained by an analysis of residual circulation transit times, as done in the cited papers by Garny et al., 2014 and Ploeger et al., 2015.

We address this point by adding the RCTT analysis to the age of air discussion.

- Introduction, page 29942, line 5 to 10: If I'm not mistaken the vertical velocity in a model like ECHAM is a purely diagnostic variable, i.e. it is calculated for output, but not used in the calculation of the dynamics. So the fact that "spectral" and "FSSL" vertical velocities differ is important to keep in mind when analysing model output, but not an inconsistency per se in the model, correct? The fact that w_spec is purely a diagnostic output should be mentioned somewhere.

We agree and added this information in the section 'Kinematic vertical velocity'.

- page 29942, line: 26-27: "transport and mixing..." is not a good terming, as mixing is also a transport process. Change to e.g. "residual transport and mixing"

This has been changed as suggested by the reviewer.

- page 29945 / Section 2.1: can you comment on how relevant the discussed issue of different velocities in the dynamical core and the advection scheme is in general for climate models - is this an issue in almost any climate model (that uses an FSSL-like scheme?), or a specific "problem" in Echam ?

This is a general issue in climate models, but it is not a problem since the differences are small (see Jöckel et.al., 2001).

- Fig. 1: Is the difference shown the relative difference of the mean values, or the mean

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of the relative differences for e.g. each month?

It is the relative difference of the mean values for the year 2005, calculated using the formula on p. 29945, l.21 (page and line number of old manuscript).

- page 29944, line 18: can you give an equation for the FSSL vertical velocity, i.e. the tracer continuity equation, to make it more clear how w_FSSL is obtained?

An explanation how w_FSSL is obtained is given by Lin (2004) and Lauritzen et. al. (2011). We have added the references here.

- page 29949/59: Do you expect an (computational) error from the transformation of diabatic velocities to w_Theta, and do you expect it to contribute to the comparison to the kinematic velocities? Probably in the continuity equation, the vertical velocity is a small residual compared to the other terms? (so the same problem arises as in the calculation of kinematic velocities).

We have done the following test, which shows that the error is small (see Fig. 1 in this comment).

- Fig. 7: Something is wrong in Fig. 7: My guess is that the difference shows EMACF-SSL - EMAC/CLAMs and the label is the wrong way round. This would be consistent with the text and the last sentence of the Figure caption. It would also be interesting to show the relative difference here.

The color in this figure is correct, see also answer to Reviewer #1.

- page 29944, line 7: "than the vertical.." (insert the)

This has been changed as suggested by the reviewer.

- page 29946, line 20: redefining -> redefinition (?)

This has been changed as suggested by the reviewer.

- page 29949, line 3: E.g. -> For example

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This has been changed as suggested by the reviewer.

- page 29952, line 4: above the equator -> at the equator

This has been changed as suggested by the reviewer.

page 29952, line 18: not so -> less

This has been changed as suggested by the reviewer.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29939, 2015.

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Interactive Comment

To calculate $\dot{\theta}$ from $\Omega = \dot{p}$, the following relation was used

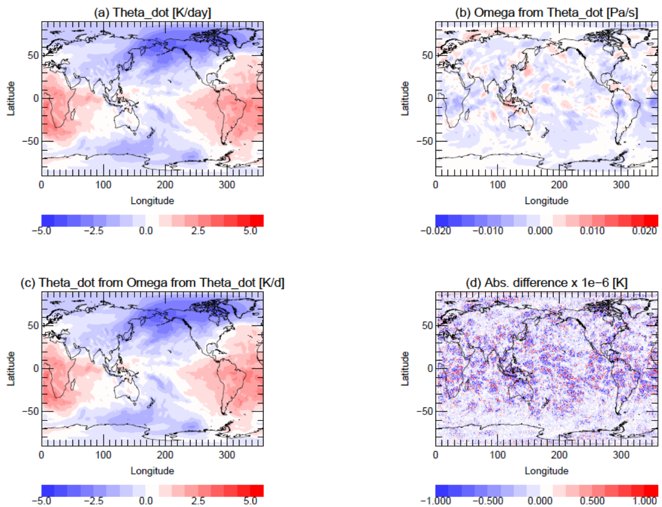
$$\dot{\theta}(\dot{p}) = (\partial_t + u\frac{1}{a\cos\phi}\partial_\lambda + v\frac{1}{a}\partial_\phi + \dot{p}\partial_p)\theta \tag{1}$$

with the horizontal velocity (u, v), λ/ϕ - longitude, latitude and a the Earth's radius. The inverse transformation is given through

$$\dot{p}(\dot{\theta}) = (\partial_t + u\frac{1}{a\cos\phi}\partial_\lambda + v\frac{1}{a}\partial_\phi + \dot{\theta}\partial_\theta)p\ . \tag{2}$$

Thus, to check the numerics, the relations (1) and (2) can be inserted into each other, i.e.:

$$\dot{\theta} = \dot{\theta}\left[\dot{p}(\dot{\theta})\right] \tag{3}$$



(a) $\dot{\theta}$ at model level 40 (ERA-Interim, 1.01.2006, 12 UTC) with mean pressure around 44 hPa and mean θ around 520 K. (b) Using relation (2), $\dot{\theta}$ is transformed to $\Omega = \dot{p}$ (c) Starting from this $\dot{p}$, $\dot{\theta}$ is calculated by using the relation (1) (d) The difference between (a) and (c) is shown which is smaller than 10^{-6}K.

Fig. 1.

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