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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Consistent circulation differences in the Southern Hemisphere caused by ozone changes: a chemistry-climate model and observational study" *by* P. Braesicke et al.

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This manuscript investigates through idealised model experiments the complex chemistry-radiation-dynamics coupling and how a chemical change can impact the BDC branches in the SH in different ways also affecting composition. I consider this work interesting and worth to be published in ACP, moreover it is well-written and clear. I am sorry for taking much time in the review dealying its possible publication. These mechanisms have to be investigated expecially in the view of interpreting climate projections performed with models do not including an interactive chemistry scheme.

We thank the referee for her/his kind assessment of our paper, highlighting the importance of such mechanisms for understanding climate change projections better. In the following we will respond point-by-point to the issues raised and suggestions made. For convenience we keep the referees comments and insert our replies.

My major concern is the lack of a few more diagnostics related to the circulation changes such as zonal mean zonal winds, meridional circulation, resolved waves acceleration and deceleration of the mean flow, wave fluxes, streamfunction. I am not suggesting to add extra 10 figures, but maybe one more figure could render the interpretation of results stronger (even if shown into an appendix). Specific suggestions are inlcuded below.

After careful consideration of the referees' comments we have added the temporal evolution of the zonal mean zonal wind in the stratosphere (annual cycle and deviations). This is a nice simple diagnostic adding a lot of meteorological information to our interpretation. By definition the transformed Eulerian mean circulation (TEM) and its changes are consistent with the polar night jet strength modelled and changes of it. Unfortunately the full set of TEM diagnostics is only available from pair A (our model versions require a pre-selection of output). Using this output we have been able to check that the consistency assumption is true. A companion paper by Keeble et al. (in preparation) will use the full set of TEM diagnostics to investigate stratosphere-troposphere coupling and surface climate effects (in pair A and other related runs), including the height varying structure and the attribution of heating rate changes. Here we deliberately constrain ourselves to one "real" (N2O) and one "idealised" (age-of-air) tracer to illustrate the consistency of circulation changes (in particular the change in seasonality) in different model versions due to changes in (primarily) ozone, thus arguing for the robustness of the response in a particular season and hemisphere (spring, polar, SH).

Introduction: could you also refer at the Birner et al papers (J CLim 2010 and/or ACP 2011)?

The revised paper will cite Birner and Boenisch, ACP, 2011.

Method: which are the known biases of this model w.r.t the other ccmval models (they are just briefly mentioned in section 5.2 and just concerning the breakout time of the vortex in the model)?

This model is a relative of UMUKCA-UCAM in the CCMVal-2 report. The dynamical core has changed. For CCMVal-2 we used version 6.1 and our new integrations use version 7.3. This has improved the tropopause height biases identified in CCMVal-2 considerably. Nevertheless we still suffer a small high bias in column ozone. We will add a small note to the manuscript.

Figure3: have you seen if there are more SSWs in the activated PSC in Pair A? This could be consistent with + ozone anomaly. A curiosity, what would it happen to the wbester relatinship in the PairA simulations? it would be nice to repeat that figure for this experiment, what do you think? (see webster et al grl, 2003 and weber et al, ACP, 2011)

SSWs are one mechanism to realise a basic relationship: Strong vortex -> low polar ozone, weak vortex -> high polar ozone. A link between heat fluxes and vortex strength exist in the model as well. We will add a plot to the revised paper (30 or 10 hPa annual cycle of the zonal mean zonal wind and differences from the annual cycle between runs) to explain this is in more detail. Please see reply below.

The positive anomaly in PairB is less than 1std, clearly not significant, it is not given that the polar vortex really shows n earlier breakup in this case. Howevere in general 1-std for a gaussian distribution does correspond to the 66

We agree – we are not arguing primarily for statistical significance but physical

consistency. We employ t-tests as well (not shown). They do not change the statistical significance for pair A, however they affect the significance for pair B to some extent. Basically, we are just using stippling to indicate regions were deviations are sizeable and we try to avoid strong statements about significance. We will add a comment to the paper.

Figure 4: the strongest signal is in DJF SH, this is consistent with Son et al 2010 interpretation of SH O3 long term changes and changes in the tropopause height, is it possible to discuss the chain of mechanisms here more clearly here? (Is this figure really necessary?) No stippling in this figure, why?

We are not sure that we would argue for a chain of mechanisms. The model goes into a new quasi-equilibrium state, from this point of view a strict causality might be misleading. We would be happy to state that the result is consistent with Son et al. 2010. The pdf file we have uploaded to the ACP(D) web page does contain stippling. We check that it will be there in the final version.

Figure5: why December only? maybe the DJF average would be more clear?

It was a slightly arbitrary choice. In terms of ozone changes a DJF mean would not change the story. Because we show the seasonal change/progression anyway we selected a particular month and went for December where a significant strengthening of the stratospheric jet occurs.

Figure 6: In order to interpret the positive signal above the cooling in the polar region, have you also looked at the changes in the winds? I would expect that the cooling is consistent with stronger westerlies that imply an increase of the wind shear at and below the jet core; at the stratopause, the net momentum flux gravity waves could therefore become more negative (filtering), this implies in the mesosphere a deceleration of the westerlies leading to an increased circulation.

We have looked at the winds and we now provide this additional information in

a figure (see above) – thank you for the suggestion. As you point out, there is good agreement (consistency) between modelled wind, temperature and ozone changes.

Looking at the three months separately would also be interesting: Dec, Jan, Feb. How is the evolution within the season for the T (and possibly U winds)?

The cold anomaly around 20 km is lowest before December. The warm anomaly above 25 km maximises in December or after. This is reflected in the wind anomaly with large increases in November December (will now be shown in the paper).

page 8465, lines 10-15. Why not referring here at contours of the streamfunction within the model itself?

Our aim was to describe the tracer as to make the reference to the MIPAS data easier. TEM is only available from pair A. On balance we decided it would be better to explain that the models TEM and tracer are consistent. We will strengthen this notion in the text.

page 8467, line 8, would it be possible to look at w* if available? Is it possible to link any change of the circulation (unfortunately U is not shown) to changes in the EP flux convergence, this could nicely confirms where (at which alt and lat) acceleration anomalies drive the trends in the circulation?

We thank the referee for the suggestion. The zonal wind evolution (see above) will now be included and discussed in conjunction with the ozone change (we will highlight the consistency with the TEM changes verbally). A complete assessment of the TEM (for pair A and related experiments) will follow in another paper focussing on the surface effects.

Case 2006/2007: I think this section (comparison with MIPAS) is very important, however instead of choosing one single year, why not an average of "similar" years? You

have chosen one specific year where the anomalies in the N2O were similar to the case A anomalies. Would it be possible to fine more "special" years and then multi-average these cases insetad than one only year? Especially because this specific MIPAS year may have different T and O3 values. Moreover, even if the anomalies are similar to the model, is it also true for the mean values? For example, if the n2o anomaly for the observations in 2006/2007 were identical to the anomaly in the model case, but the mean value of the n2o in the model were say 50

We will strengthen the comparison of the N2O climatologies (model vs satellite) in the paper. Unfortunately only two cases exist showing a negative N2O anomaly in the region of the rapid upward slope of the N2O volume mixing ratios (between October and December). The other case has very little data; therefore we went for just the one case. We are not implying anything more than the model state is somewhere in the range of natural variability and that this state can be realised by the atmosphere.

Figure 9: is the 70S too close to the vortex edge? why not averaging over two regions one 70-80 and one 60-70 or even southern? however it is interesting to see such a difference in the observations (not published before, that is correct?)

Thank you! We have tried to highlight the similarity between the model and the satellite observations. The latitudes have been chosen in accordance with the latitudinal extend of the ozone anomaly (e.g. Figure 5).

I liked the summary section and mostly appreciated the implication section

Thank you!

As a final suggestion, I would simplify the title: 'Circulation anomalies in the Southern Hemisphere caused by ozone perturbations

We will change the title to: "Circulation anomalies in the Southern Hemisphere caused by ozone changes"

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8455, 2013.