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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Drivers of hemispheric differences in return dates of mid-latitude stratospheric ozone to historical levels" *by* H. Garny et al.

Anonymous Referee #2

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The authors address the question of the hemispheric asymmetry of the return dates of total column ozone to 1980 values as projected by CCMs. The paper is well written and includes a detailed analysis of the transport and chemically driven contributions to the hemispheric asymmetries. The paper is suitable for publication in ACP after addressing the following comments.

Major comment: The paper uses two different approaches, an attribution method based on ozone sources and sinks as estimated by the model and a linear regression, separating between Cly and a linear term as long-term drivers of ozone changes. During the first part of the paper the line of arguments and how the two different methods

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are combined is somewhat confusing. It might be helpful to start section 3 with short paragraph about the different nature of the approaches and about how they will be combined later, i.e., in the first part of the paper the regression is used to identify the height dependence of the linear term and the Cly term. The major argument made in the second part of the paper is based on analyzing the linear trend (derived with the regression model) of the transport and chemistry attribution terms (after concluding in the first part of the paper that the Cly term can be ignored when trying to explain the hemispheric differences). Potentially the method description in Section 3 could be switched to better match the order of arguments made in the paper.

Minor comments

1) Page 32829, line 8: Please give a short description of REF-B2.

2) Page 32831, line26: Is it possible to estimate the magnitude of these numerical artifacts? How realistic is the mixing within the models? If mixing is found to be too strong how could this affect the analysis presented here?

3) Page 32834, line 10: To better understand how Figure 1 is based on Equation 2 it might be helpful to state that p1 equals 1960-1969 and p2 is each single year of the time series from 1960 to 2050, respectively.

4) Page 32837, lines 13-20: This is repetitive. A large part of the argument has been made before on page 32835, line 1-6.

5) Page 32837, line 27: Description of section 4 is misleading. There is no real discussion of the method impact in Section 4 (apart from pointing out this dependence once or twice). Discussion of the method impact was done to some extend at the end of Section 3 (based on Figure 2). A short and clear description of what Section 4 is about (attribution to different altitude regions and connection to BDC changes) might help the reader to understand the logical flow of the manuscript better.

6) Page 32838, line 1-2: Maybe turn the order of the two methods so that the sentence

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fits the following text.

7) Page 32839, lines 12-14: How were the altitude regions of the partial ozone columns chosen? Why not use for instance 20 hPa as done in earlier studies? Please motivate.

8) Page 32839, Figure 3: Please state the method used for results in Figure 3 in the text (and not only in the figure caption). Since there was such a detailed introduction of the different methods the reader might expect this issue to be picked up somewhere and is confused if not even the method chosen is clearly stated (see also my comment above on the description of Section 4).

9) Page 32840, line 12: This has been done to compile Figure 3 right? Or does this sentence refer to results shown in Figure 4?

10) Page 32840, line 27: How is a partial ozone column assigned to an individual level?

11) Page 32841, line 8: It seems like that MMM is quite symmetric above 100 hPa? But maybe this is not the case and just a problem of the quality of the figure? However, if the liner trend in NH and SH would be very similar above 100 hPa this would contradict arguments made before (that the hemispheric differences of TOZ return dates must be caused by the variability associated with the linear trend and that the LSTR plays an important role). Here it could help to spend some words on how this new analysis compares to the results from Figure 3 and how combined results from 3 and 4 add to the main argument of the paper.

12) Page 32846, line 1-4: Are the transport induced changes in line with BDC changes as displayed in Figure 5 and 6 (upper panel)? It seems that BDC changes in the LSTR are stronger in the NH in most models. Or are the transport changes more a response to changes in chemistry as discussed earlier for the LMSTR? It seems that (in a perfect world) one would like to distinguish between the transport changes resulting only from changes in the mass flux are and the transport resulting from chemically altered background ozone.

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13) Page 32847, line 10: There are two green bars in Figure 10.

14) Page 32848, line 1: The first part of the sentence is only true for above 20 hPa.

15) Page 32852, line 24-28: But only for the region 50-70 hPa, right? Not below 70 hPa where transport might still be very important.

16) Figure 3: Panels 6 and 7 should be switched to link the order of the first 4 panels with the order of the last three panels.

17) Figure 4: Red versus Pink? Blue versus light blue? Both are hardly distinguishable. Caption: Replace ‘’at each pressure level” with ‘’over all pressure levels”. Replace ‘’2049 is shown” with ‘’2049 are shown”.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 32825, 2012.

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