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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.

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We thank the reviewers for their positive and constructive comments on our manuscript. Point-by-point answers and descriptions of the performed changes are given in the following:

Reviewer Nr. 1

The paper by Garny et al. describes, or better attributes, the drivers of ozone return dates. In doing so it tries to explain the obvious asymmetry in hemispheric ozone return dates, with northern hemispheric return dates being earlier. The paper argues that the

hemispheric asymmetry in the Brewer-Dobson circulation (BDC) alone is not enough to explain the modelled behaviour and argues for important chemical changes. This is an important point to be made. I conclude that the paper is a well written contribution suitable for publication in ACP. However, I would like to ask the authors some questions and hope that by receiving answers to those questions the paper will improve further:

Is all the statistical detail necessary? In the beginning a long argument is developed, that basically just says that it doesn't matter for the general hemispheric asymmetry how return dates are defined. For the sake of readability and having more space for the main point (chemistry matters), I would recommend to shorten this section and to put the details in an appendix.

Thank you for this comment. The tests using different methods for the ozone return date calculation were introduced to ensure the independence of the conclusions on the methods used, so we would like to keep the analysis using different methods in the paper. But we agree with the reviewer that the readability and the main point gets somewhat lost in all these details, so we followed the suggestion to move the details into an appendix and just keep a brief statement in the main part of the paper. In addition, we re-arranged the order of the method sub-sections to better match the flow of the paper (see comment by Reviewer Nr.2).

What is the role of a possible tropopause height trend? Here, I have to admit, I got slightly confused. The authors state the importance of the ozone trend in the lower-most stratosphere (e.g. 32839, line 27). I am not sure how this would be reflected in the terms of their budget equation [2]. Certainly an increase/decrease in tropopause height could be linked to a general decrease/increase in partial column ozone, even though it would be neither direct transport nor change in chemistry (it would be more a question of which chemical regime is seen but what part of the atmosphere). More explanation here would be helpful for the reader.

This is a good question and we checked how a trend in the tropopause height might

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affect the results: First, the partial columns shown in Fig. 3 are calculated with the time-varying tropopause. When using a constant tropopause height instead (i.e. the mean tropopause of the first decade, here the 1960s), the return date differences in the tropospheric and lowermost stratospheric columns did not change for the multi-model mean. Individual models were affected, and the overall spread between models is smaller when using a fixed tropopause height. The return dates of the model E39CA, that we use for the trend attribution in the lowermost stratosphere and troposphere, do not change considerably when using a fixed or time-varying tropopause. Overall, these results indicate that trends in the tropopause height do not contribute in the multi-model mean to the hemispheric differences in the ozone return dates. A note on the role of tropopause changes is added to the paper.

For the attribution of ozone trends to chemistry and transport, we show trend profiles (Fig. 7), thus here a tropopause trend might be reflected in a height shift of the ozone profile. We added the mean tropopause height of the first and last decade to the plots in Fig. 7 to elucidate this point. Indeed, the strong positive chemically-induced ozone trends, and counteracting transport-induced trends in the NH (and to a lesser degree in the SH) are located just at the tropopause. This might indicate that the upward shift of the tropopause has some contribution to the trends. To test this, the trends were calculated from profiles that are shifted in height with respect to the tropopause. Compared to the profiles in height above the ground, small differences are found: the decrease in relative ozone trends close to the tropopause are reduced (see Fig. 1 in this reply), as expected from the upward moving tropopause. However, the large increase in ozone due to changes in chemistry remains, and we conclude that the results of our study remain valid. We added a discussion of this point to the manuscript in Sect. 5.1.

What happens to water vapour? Water vapour, undergoing microphysical change and being sensitive to temperature, could change as well. Again, transport would not need to change much, but water vapour could change due to cold point tempera-

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ture changes, with consequences for the chemistry. This term would presumably be reflected in the diagnosed chemical change?

Changes in stratospheric water vapour have an impact on ozone chemistry, and the reviewer is right in that we only marginally discuss this point (on page 32848, lines 5-9). Generally, water vapour is simulated to increase in CCMs, resulting from enhanced transport from the troposphere and increasing methane concentrations. These increases in available water vapour generally lead to enhanced ozone destruction (e.g. Stenke and Grewe, 2005). In our study, this effect can be seen by negative ozone trends induced by HOx chemistry in the lower stratosphere (see Fig. 11). We added a sentence on these strong HOx-induced ozone trends. However, as the HOx chemistry does not play a large role in inducing the hemispheric asymmetries in ozone trends (but rather counteracts it), discussing the HOx chemistry in more detail is not the scope of the paper.

What is the role of simple tropospheric chemistry schemes? Are the schemes too simple for a conclusive assessment? Please explain possible caveats.

The results of the paper indicate that tropospheric ozone trends might play a role for the asymmetries we see in the evolution of total column ozone. We argue that this result calls for an improved treatment of tropospheric chemistry in the CCMs, and it will be necessary to repeat similar analysis with such models to validate our findings (see e.g. last paragraph in Conclusions). However, at the same time the prescribed emissions of NOx likely play a large role for tropospheric ozone trends, and to improve their representation is probably as important as the chemistry scheme. We revised our discussion on these points in Sect. 7.2.1, also including more recent literature on tropospheric ozone trends.

Answering the questions above will strengthen the main message of the paper, namely that there is more to the asymmetry than BDC changes.

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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 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 ïňĄrst 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.

Thank you for this comment, we agree and accordingly switched the order of the subsections in Sect. 3, and added a paragraph at the beginning explaining how the different methods are used. Further, we shortened the section by moving parts to an appendix (see Reviewer Nr. 1). Hopefully this should improve the readability of the paper.

Minor comments

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

Done.

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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?

The "numerical artefacts" that are included in the transport terms are, as the reviewer states, likely mostly due to excessive numerical diffusion, but can also result from numerical errors in the calculation of chemical ozone tendencies. Estimating this term in the context of the current study would be rather hard. To get a grip on the numerical diffusion, one could think about a comparison with Lagrangian advection schemes. However, one can argue that the diffusion should be included in the transport tendency – even though it might be excessive compared to the real world, this is seen by the model ozone fields, and thus can be described as "mixing" of ozone, therewith being part of ozone transport. The results of this study are affected only as much as the simulation of ozone (and any tracer) fields are affected by numerical diffusion. We added a sentence on the matter.

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.

Good point, and we changed the description accordingly.

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.

True, and the part has been rewritten in the process of restructuring the section.

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

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reader to understand the logical flow of the manuscript better.

Good point, this description of Sect. 4 was removed, and the description of Sect. 4 at the end of the Introduction is extended to include the part on the BDC connection.

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

This paragraph was moved to the beginning of Sect. 3 and rewritten following the major comment of the reviewer. Hopefully it becomes more clear now how the methods are combined.

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.

The reviewer is right in that the upper boundary of the "LSTR" region at 10 hPa is somewhat arbitrarily chosen. In this altitude range, the transition from the dynamically controlled region to the purely chemically controlled region takes place. In Fig. 9 it can be seen that the transport contribution to ozone trends approaches zero at 10 hPa, while at 20 hPa transport and chemistry contributes about half each. Above 10 hPa, the transport contribution to trends remains negligible. Thus, 10 hPa appears to be a good choice to separate the region where dynamics might still have an impact in driving ozone trends from the purely chemically controlled region above. We added a sentence to motivate the choice of the 10 hPa upper boundary.

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).

Good point and done.

9) Page 32840, line 12: This has been done to compile Figure 3 right? Or does this

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sentence refer to results shown in Figure 4?

This statement was aimed at Fig. 4 but we see that it is rather confusing at this point. Furthermore, it might be more clear to first discuss what is shown in Fig. 4 before arguing what the individual regression coefficients can contribute to the hemispheric asymmetries. Thus, we rearranged the order of the following paragraphs. Hopefully this should make the overall text flow more clear.

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

Thank you for this comment, and this might indeed be a confusing quantity. The "partial column ozone at each level" is the partial column ozone calculated over the model layer that is centered at this level. TOZ is calculated as Thus, with TOZ calculated as TOZ = constant*Sum(O3(pi)*dpi), the value we show are part.OZ(i) = constant*O3(pi)*dpi. We added a short description to clarify this point.

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.

I apologize for the poor quality of the figure, it will be improved in the revised version of the manuscript (also following the reviewer's comments below). It should then become apparent that there are indeed hemispheric differences in the trends also between 100 hPa and 10 hPa.

However, the reviewer points out correctly that it might seem contradictory that the trend differences appear larger in the troposphere, but still it was argued before that

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the LSTR plays a more important role for the return date differences. The problem here is that the magnitude of the trends at different layers are not necessarily reflected linearly in the magnitude of the contribution to return date differences. Thanks to the reviewer's comment, we realized that this issue needs more work and discussion. In doing so, we found that while the trend is responsible for shifting the return dates forward, it depends also on the sensitivity of ozone to Cly how strong this forward shift will be. Therefore, even identical trends in the SH and NH can lead to hemispheric differences in the ozone return dates. This effect can explain about half of the total hemispheric differences in the ozone return dates. The other half is due to the stronger trend in the NH than in the SH. This does not change the Conclusion of the paper, that chemistry-induced changes are the major contributor to hemispheric differences, but rather adds another aspect on how the trends translate to return dates. We added a new figure and discussion in Sec. 4.1 on this point, and added remarks in particular in the Abstract, Sec. 4.2 last paragraph, Sec. 7.1 and the Conclusions.

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 ïňĆux are and the transport resulting from chemically altered background ozone.

True, the transport-induced changes in NIWA-Socol might seem contradictory to the residual mass flux changes shown in Fig. 5 (esp. when comparing the profiles in Fig. 9 and Fig. 5). The overall positive transport-induced trends in ozone in both Hemispheres are consistent with an increase in the circulation strength. In addition to the mass flux changes, the transport-induced trends are positive because chemically-induced trends are negative (i.e. less chemically produced ozone is available for transport away from the region). This compensating effect of chemistry-induced changes is

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probably the reason why the hemispheric differences in the transport-induced trend are contradicting the hemispheric differences in the residual mass flux trends. We added a reference to Fig. 5 and some discussion on the differences of Fig. 5 and Fig. 9.

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

True, we changed the text to refer to the label instead of the color to avoid confusion.

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

Good point, this was meant to refer to the differences only, so we changed the sentence accordingly.

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.

True, at 100 hPa transport effects were found to be of importance. However, in the partial column of both the LSTR and the column below the chemistry effects were found to dominate (in contradiction to what was assumed in previous studies) so we would like to keep this point.

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.

Done. Furthermore letters marking the panels were included to clarify the order.

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".

The colors were changed and the caption revised.

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

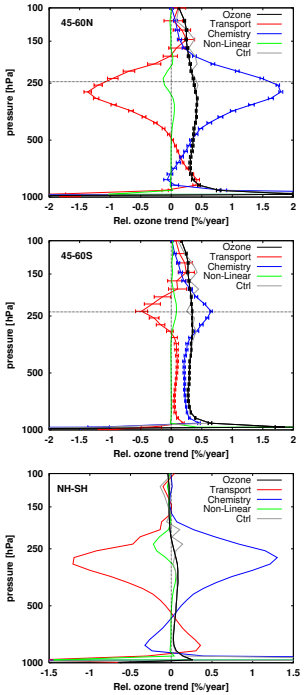

Fig. 1. As Fig.7 in the paper, but trends calculated from profiles that are shifted relative to the tropopause height.

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