Atmos. Chem. Phys. Discuss., 13, C5094–C5108, 2013
www.atmos-chem-phys-discuss.net/13/C5094/2013/

Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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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Received and published: 24 July 2013

The authors present an interesting comparison of two pairs of chemistry-climate model runs with particular chemical perturbations. One pair looks at the effect of suppressed halogen activation on PSCs, the other at the effects of additional short-lived halogenated species. Effects on composition and circulation are analysed in terms of ozone, temperature, tropopause, N2O, and age of air. Overall, differences within each pair highlight aspects of the coupled chemistry-climate nature of the responses; i.e. differences cannot be explained by either composition or circulation changes alone. Usually such coupled responses are studied from a climate change perspective, the

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Interactive Discussion

Discussion Paper

setup here of using different versions of the same underlying model run for the same time period allows for a cleaner comparison and analysis. I find this aspect very useful and insightful. Coupled chemistry-climate changes are also analysed from an interannual variability perspective using MIPAS satellite data, with attempts to draw conclusion from the observations-model comparison.

We thank the referee for her/his kind assessment of our paper, highlighting the usefulness of the experimental methodology. 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.

Overall, I find the presented comparisons quite interesting and suitable for publication in ACP. Certain aspects of the model perturbations seem well suited for a detailed analysis of coupled chemistry-climate responses to such perturbations. However, I find most presented elaborations and interpretations too speculative - most of them are not backed up by relevant evidence. For example, the BDC structure is only shown schematically in Fig. 1 but there are frequent inferences about changes in the different branches without showing direct measures of the circulation (age of air arguably qualifies for this but represents an integrated measure, which makes it hard to distinguish BDC branches). Additionally, referring to complicated structural changes/differences in the BDC between the runs makes it hard to follow the line of arguments, at least I had a hard time at places to connect all the dots. I think the paper would strongly benefit from a more mechanistic understanding of the described responses to chemical perturbations.

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).

As argued more specifically below in my minor comments, I think a lot of the described differences between the model runs (maybe more so for pair A than pair B) can be more simply explained by seasonal shifts - even though mentioned here and there in the paper, I think this argument is not pursued enough.

Our apologies that we did not manage to make this clear enough: The detail we present does describe how the seasonal shift is realised in the model. We hoped opening 5.2 (our final subsection) with "Much of what we describe does not allow inference of causality. It merely points to a consistent change of the seasonal BDC development in both model integrations with enhanced ozone depletion in Antarctic spring." would wrap this up nicely and make this point clear. In the revised version we will mention this more strongly in the introduction and at various points in the paper. In addition we feel that the format of many figures (e.g. Figure 3) highlights the seasonal changes and potential shifts well.

A final major concern to me is that the comparison to observational data is not very conclusive, in fact I don't fully see the benefit of including the observational analysis in the paper. The similarities between interannual differences in the satellite data and the differences between the model runs are very limited (see minor comments for more details) - depending on what you look at, the differences are not similar at all. More importantly, simply finding a somewhat similar anomaly structure in interannual variability does not convincingly demonstrate that this happens because of similar underlying causes/triggers - I don't agree that this gives "confidence that the modelled BDC change is relevant for the real atmosphere" (page 8469, line 18). Chances are that for almost any sensible perturbation in the model, you can find more or less similar structures from interannual variability in observational data. That by itself is not very helpful. To me, understanding the kind of changes and responses that come about from the applied chemical perturbations improves our understanding of coupled chemistry-climate interactions, which makes the results relevant even without comparison to observations.

I understand the reviewer's reservations. It is never straightforward to compare observational data (from a certain period) with data modelled by a climate model in an idealised experiment. Nevertheless we believe that two take home messages exist without stretching this framework too far: 1. The seasonal development of age-of-air gradients in high latitudes agrees well between model and observations, even though absolute values differ (Figure 9). 2. There is an observed case in which the N2O anomalies resemble the climatological differences of our model integrations (Figures 7 and 10). We did not imply that this happens for the same reason. The point is: Natural variability allows such a pattern in observations; therefore we conclude that the solution that our model produces in an equilibrium climate (caused by a systematic perturbation) might be possible in the real world, nothing more. We are happy to strengthen this notion in the final manuscript.

Minor Comments: Abstract, last sent.: the last point here (line 19) is by now well established, I don't think it needs specific mentioning in the abstract

We feel it is a useful reminder and hope the referee agrees with a compromise: ... and that coupled chemistry-climate models allow a better assessment of future

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ozone and climate change than recent CMIP type models with prescribed ozone fields.

page 8457: lines 1/2: please explain in more detail - how ozone change leads to temperature change, leads to circulation change

We will insert: A possible example could start with an ozone anomaly after winter: Lower high latitude ozone in spring leads to smaller shortwave heating rates. Temperatures stay lower for longer and the meridional temperature gradient remains steep. Winds adjust to this temperature structure and the resulting flow leads to changes in tracer transport, including ozone.

line 12: suggest change 1) "derived" to "estimated" to highlight the uncertainty when using tracer data to infer age (FYI, a detailed discussion of this is presented in Garcia et al., 2011, JAS), 2) "have not been able to confirm" to "have not confirmed"; I think it's also important to state that these observations have been taken at particular points in space

We will change this and add some more detail.

lines 13-15: note, this has also been inferred by Boenisch et al. (2011, ACP), who in particular highlight a potential difference in trends between the different branches of the BDC

See previous point.

page 8458, lines 22/23: using "details" and "detailed" here seems to contradict above "do not provide a detailed analysis"

We will deleted "detailed".

page 8459 lines 2-4: a brief summary of the updates since CCMVal would be useful

We will clarify that the major update is the underlying climate model (as indicated).

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line 20-24: A and B are used twice in the Fig. - please change (or drop) for individual experiments

We have adapted the font to distinguish between the experiments and the pairs (bold).

page 8460 line 11: why shorter for pair B? this surely has an effect on statistics - your sample size for interannual variability is 10 for pair B, which is very small (even 20 for pair A is on the small side)

Pair B is also part of a different experimental suite in which all other integrations have been 10 years long. We do not believe that this mismatch in length impacts the overall conclusion, namely that the seasonality can potentially shift and does so in a similar way. Note that both models are in equilibrium, because they have been started with a 'realistic' set of initial conditions and have been run for a ten year spin-up.

lines 12-17: the only quantity listed here directly related to circulation is age - why is there nothing shown on residual circulation or wave forcing?

We are mostly talking about the effective transport facilitated by the BDC. Therefore tracer data seems the most adequate source of information to use. We analyse both age (an idealised model tracer with linear increasing source) and N2O (a model tracer closer tied to reality through its boundary conditions) data. So we believe the notion that we are using age only is not quite correct. We do mention the consistency of TEM with our results, but we do not believe that it adds anything to the point we want to make, namely the changing seasonality of the "tracer BDC" in different altitude ranges. TEM was introduced as a theoretical concept to derive tracer behaviour; we have the tracer data readily available. As mentioned above, a forthcoming companion paper will argue for stratosphere-troposphere links using TEM in experiment pair A.

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page 8461 line 4: why not the more common significance level corresponding to two standard deviations? also, this is the only place where this is mentioned, please also mention in the Fig captions; for pair B: with only 10 years (=samples) it'll be very hard to say anything about significant differences (see above comment)

Invariably the significance in pair B will be low, we agree. We are happy to provide the meaning of the stippling in individual figure captions. The main aim is to highlight consistency and the possibility of seasonal shifts in both pairs of experiments. We do not believe that this point hinges on the details of the significance threshold.

line 10: "for one experiment" - please be more specific

We now spell out the cause of the ozone loss.

line 22: and somewhat during autumn and spring (not just winter)

True – the sentence was not meant in an 'exclusive' sense and we will add the note above.

page 8462 lines 12/13: but these are mostly not statistically significant

True – we will add this to the sentence.

line 15: suggest either "deeper than average troposphere", or "higher than average tropopause"

Thank you! Will be fixed!

line 22-26: I find it somewhat confusing to put the tropopause as the cause here - it's the dynamics that lead to anomalously high tropopause that also prolong the low ozone episode

The sentence was meant to convey causality focusing on the 'thermal structure' of which the tropopause height is one indicator. Certainly, once the tropopause

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is high, ozone (in a column sense) has a tendency to stay low. We will take the sentence apart to avoid any misunderstanding.

line 25: I was surprised that radiative effects (due to ozone changes) are not mentioned here - clearly these must play some role during spring

Radiative effects play a role as well Therefore we used the phrase 'thermal structure', indicating that adiabatic and radiative effects are important. As mentioned above, we will change this sentence.

page 8463 lines 1-4: this sounds like this means the polar O3 perturbation in this case is purely dynamical? probably not what you're trying to say, in which case a slight reformulation may be good

Thank you. This sentence was meant to emphasise the unique response in December/January. We will change the sentence accordingly.

lines 16/17: yes more halogens, but short-lived - so will largely change O3 in tropics? as shown in AoA there seems general slow-down in circulation in Pair B, which must play an important role

This is a new equilibrium state. Even though the VSLS and their breakdown products enter the stratosphere through the tropical tropopause, there is a general increase of reactive Bromine levels in the stratosphere. The overall burden that can be achieved will certainly depend on the age-of-air.

line 22: 1) insert "interaction" after "composition-climate", 2) "difficult to disentangle cause and effect" - this is an unsatisfactory statement, as a reader I'd like to know/understand more about this part of the response

The word 'interaction' will be inserted. Part of the explanation is following lower down in the same paragraph; we will clarify this in the updated manuscript.

lines 24/25: re strat-trop-exchange - has this been quantified? The statement makes sense but is still speculative based on the provided evidence.

Not in this particular model runs, but it is a feature of our model that we have encountered and investigated many times. Morgenstern et al. (2013) includes a table showing ozone STE and burden for different climate change scenarios (Table 2), including the number for ozone recovery: 20% change in STE (which is the relevant number for A). This will now be referenced in the paper.

page 8464 line 4: "more closely to transport": motivated this way it appears surprising to look at temperature - in some ways O3 is a better proxy for transport than temperature, and certainly AoA is better suited to answer this question, which is discussed later; Temperature certainly describes part of the climate response and is useful to look at radiative responses from the O3 perturbations and should be motivated accordingly

This sentence was meant to introduce both, the temperature (climate changes) and the age-of-air (transport) discussion. We will clarify this sentence.

line 13: why is there enhanced decent, and why does that not impact T further below? To me, simplest interpretation is delayed vortex breakup, i.e. strong downwelling shifted to later in year (btw, note cold anomalies in mid-lat's and decreased age there – how do you explain those?)

We are not sure we understand the point. Enhanced descent and (strong) downwelling are sort of the same, so we cannot see the alleged discrepancy. That the descent/downwelling might not be enhanced for all altitudes is exactly the important point that the summary Figure 11 tries to convey by showing a 25 and 15 km slice, strengthening the notion of changed seasonality. We think this explains our approach.

lines 15-19: similar to other places, these are hand-wavy statements that are not backed up by quantitative analyses - in the given context they really are just speculation

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We partially disagree: The first part is back-of-the-envelope chemistry and well established. The second part is basically highlighting the complex coupling. The later sentence is just summarising all mechanisms that contribute. We will make this clearer by changing the sentence slightly.

page 8465 lines 1-6: again, given this is primarily a modeling study it is unclear at this point why AoA (or other more direct circulation diagnostics) is not brought in - any chemical tracer suffers from not purely representing transport

Neither TEM nor age-of-air data are directly observable quantities - N2O is! We will strengthen this notion in the first sentence, also highlighting the very long lifetime of N2O. We note that the age discussion follows in the same section p8466, line 8.

line 9: it would have been more natural to me to start the discussion on the left side of the plot (July/August), instead of in the middle

We actually tried this and the consensus was that it worked better this way. I hope the referee will grant us some license.

line 10: there is not much BDC downwelling in the summer - I find this confusing

We agree – we merely point out that the shape already indicates the seasonal transition to autumn when the downwelling becomes stronger. This is also in-line with the general seasonality of TEM derived from assimilated meteorological data see for example Seviour et al., QJRMS, 2012. We will now cite this paper to clarify matters.

line 14: is this rapid increase due to the shut-down of the BDC into the summer? Please provide explanation

This happens when TEM diagnostics indicate that the region of tropical up-welling is furthest to the south, with downwelling in high latitudes weakened. We will link this to the above change (line 10).

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line 16: I'd suggest starting a new paragraph, since now you talk about the anomalies

Done!

lines 18-20: why would the mesospheric branch change? What explains the other changes in the BDC? This is all very hand-wavy further interpretations of N2O: I don't quite understand all of these inferences - isn't the transport more related to the slope of the N2O isolines in the Fig? please provide more detailed and physical interpretations and inferences why is N2O constant with height between âĹij20-30km during SH summer? why does it slope down more strongly during SH autumn than during winter/spring? can a seasonal shift explain this?

We feel that this is the main point of the referee's comments. We have largely addressed this in the reply to the second comment above. In our model the overall high latitude descent changes throughout the stratosphere and mesosphere, even though the magnitude is small in the lower mesosphere. As discussed before, the general descent is related to the slope of the isolines as stated a few lines up. Here we talk about differences in N2O and how they relate to changes in circulation (e.g. the slope of N2O isolines).

N2O is depleted most efficiently by photolysis between 20 and 40 km and a strong vertical gradient between high tropospheric and low mesospheric values is established. This behaviour is well understood and can be simulated in simple models, see for example Prather, Science, 1998. Note that we show a high latitude average as a function of month and altitude. Looking at individual month we see low south polar N2O in November throughout the height range. In December some vertical structure evolves and N2O generally increases. So the notion of near constant N2O is only true for November, following the prolonged descent throughout winter and spring that is also evident in the TEM (not shown here) and can be rationalised with the temporal development of stratospheric zonal mean zonal winds, which will now be included in the paper (see comment

by referee 2).

lines 26/27: as mentioned before to me this seems to provide the simplest explanation/interpretation of the results and doesn't require all the complicated and speculative individual circulation changes; at the least it seems to provide a simpler framework from which to interpret the results

We explain the details of the seasonal shift, highlighting the different height region. With the additional detail we provide in response to the earlier comments we hope that the details we try to convey become clearer. Yes, the details are complicated but as the reviewer correctly notes the overarching idea of a seasonal shift is simple.

page 8466 line 5: what do you mean by "velocities...more poleward"?

We will change this to 'the poleward residual meridional velocities are enhanced'.

line 6: really? or is this simply due to the upward shift in tropopause and the associated upward shift in larger N2O values?

Both factors play a role, but the high latitude altitude difference is not that large (see figure 4) and the model complements such a tropopause response with more meridional exchange just above the tropopause. We will clarify this in the text.

line 9: remove "tropical" line 9: hard to understand AoA differences w/o seeing annual mean changes, or at least some other seasons

Done!

line 12: again, I think this is simply because the tropopause is higher

See above, we will now state the interplay between the two effects.

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lines 20-22: why is this important here? this seems out of place

It was just an example illustrating the point in the previous sentence, but we are happy to remove it.

line 28: suggest to change "north of" to "equatorward of"

Done!

line 29: what explains these general differences?

We don't believe there is a general cause.

page 8467, lines 17/18: see above, it appears simpler to think of upward shifts in tropopause

See comments above.

page 8468 line 13: why those specific latitudes (80 vs. 70)? please provide motivation

It is the region in which the strong change in ozone gradient is observed (see Figure 3), will be explicitly mentioned in the text.

lines 15-18: it seems very hard to explain this from a BDC point of view, which would give you a zero gradient at most. First, is this a statistically significant difference of means? Second, one possibility is that tropopause is higher at 80S than at 70S, leading to more frequent tropospheric air at those altitudes?

We are not sure we understand the referees postulate. The models mean tropopause has a monotonic downward slope towards polar latitudes in this region for all months (including October) and we are describing an age difference. All we are highlighting is that there is a process that allows for younger air to be at more polar latitudes. We are not arguing about statistical significance in this context, because we have the observations for 'only' 10 years with some missing data.

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line 21: "very old mean age" could be misleading if this is really related to errors in "age retrieval"

We will make this clearer.

page 8469 line 13: N2O anomalies in the model are located much further down and show a different seasonality; O3 anomalies are shifted down in MIPAS compared to model with very different structures in the troposphere - not sure they are all that similar (this depends strongly on what specifically "similar" refers to)

We were specifically referring to the negative anomaly relative to the upward sloping isolines of the climatology. This will now be clarified.

line 18: aren't halogen changes more important for long-term trends, as opposed tom related to interannual variability? In this sense, it may be very hard to draw any conclusion form this comparison.

We compare time-slice integration with a 10 year climatology and argue about an episode. We agree, in the long-term halogen changes are important, but not for our purpose.

page 8471 line 12: is this delayed vortex breakdown because of radiative anomalies due to smaller ozone?

This consistency exists in the model. We are careful with causalities, because we talk about a 'new; modelled equilibrium state, but this statement is correct.

line 14: what explains this weakening?

We don't know.

lines 24/25: remove either "also" or "as well"

Removed 'also'.

page 8472, line 22: please provide reference(s)

Done!

page 8473, line 2: suggest: "IPCC-type climate models" - note this has improved in the new CMIP5

Done!

Fig 3: suggest "Difference" instead of "Change" in label (also for other Fig's)

Done!

Fig captions: either explain what you mean by "cold" and "warm" colours, or simply refer to the actual colours

Colour theory clearly defines the terms warm and cold colours. We do not really think it is necessary to explain this term in the captions.

Fig 5: is this based on mixing ratio or concentration? please clarify in caption

On volume mixing ratio, information added to caption.

Fig 6: line contouring stops near 50 lat - is this on purpose?

Yes, the lines are used to provide structural detail for the much smaller equatorial changes. Note will be added to caption.

Fig 7: left shows pair (A) - typo

Done!

Fig 9, caption: it may be helpful to provide a factor for the difference in scales, since they differ quite strongly

We will explain in the caption what magnitude 'orange' represents.

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