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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.

Anonymous Referee #1

Received and published: 10 June 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 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.

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.

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.

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.

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

page 8457:

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

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

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

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

page 8459

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

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

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)

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?

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)

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

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

page 8462

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

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

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

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

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

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

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

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

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

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?)

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 specu-

lation

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

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

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

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

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

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 $\sim$20-30km during SH summer? why does it slope down more strongly during SH autumn than during winter/spring? can a seasonal shift explain this?

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

page 8466

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

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

line 9: remove "tropical"

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

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

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

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

line 29: what explains these general differences?

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

page 8468

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

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?

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

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)

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

page 8471

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

line 14: what explains this weakening?

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

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

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

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

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

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

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

Fig 7: left shows pair (A) - typo

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

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