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Geoscientific Model Development Discussions
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Interactive comment on "The Extrapolar SWIFT model (version 1.0): Fast stratospheric ozone chemistry for global climate models" *by* Daniel Kreyling et al.

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Dear Referee #2,

Thank you for your positive review. Please find our answers and adaptations to your remarks below.

1. *"Is the SWIFT model really robust to a large, say 30%, change in Cly?"*

   In future training data set, it is planned to include data from 30% or 50% Cly reduction scenarios, in order to cope with such conditions. Currently, an average reduction of 30% in stratospheric chlorine loading will force the polynomials

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outside the domain of definition in many areas of the 9 dimensional parameter space. 30% reduction is a significant change. It will shift the Cly-PDF (compare to Fig. 2) to far from the training PDF. Considering the current training data set, we assume that the current polynomials can handle a reduction by 10% (however we did not test this).

2. *"I do greatly appreciate that the parameterization has taken advantage of the range of chemical environments within the lower to mid-stratosphere to capture a wide range of conditions, but these different chemical families also have a certain degree of correlation. This does make me wonder how large a variation in Cly would be required before the new combination of HOx, NOy, Cly and Bry falls outside of the range of validity for the SWIFT model. Is there a simple way to test the validity to variations in Cly by using the domain-polynomial and scaling down Cly to find how rapidly parts of the atmosphere begin to fall outside the range of validity? If quantitatively testing this is not as simple as I imagine, feel free to disregard this question though I do sincerely think it would help strengthen the paper."*

   We will try to respond from two different angles:

   (a) In nature the concentrations of Cly, HOy, NOy and Bry are certainly correlated to some degree. However in SWIFT the individual chemical families originate from monthly climatologies. A change to the Cly climatology does not impact the other climatologies. Applying such a change to Cly, without adapting the other chemical families would render unphysical stratospheric conditions. They would most likely lie outside the domain of definition of the model.

   (b) If Extrapolar SWIFT is used in a simulation scenario with (e.g.) 50% reduction of chlorine loading, the training data set must include data from such a scenario (this is not yet the case). This data should originate from a full

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stratospheric chemistry model, i.e. the corresponding HOy, NOy and Bry concentrations come together with the reduced Cly concentrations. Thus the domain of definition of the model gets extended in all four dimensions of the chemical families.

3. *"Page 1, Line 9: I might suggest replacing "local ozone column" with "overhead ozone column" as this might be a bit more specific."*

   We replaced "local ozone column" with "overhead ozone column".

4. *"Page 2, Lines 6-7: the wording seems to suggest all CMIP5 models used prescribedozone and I think a more accurate representation would be 'many of the CMIP5 simulations'. A quick read of Eyring et al. (JGR, 118, 5029-5060, doi:10.1002/jgrd.50316, 2013) suggests nine of the 46 models had fully interactive chemistry and a further nine used prescribed ozone calculated from the same GCM."*

   We included the words "many of" in the sentence. It reads now: "However, a frequently used approach to represent the ozone layer in general circulation models (GCMs) is the use of prescribed zonal mean ozone climatologies, as in many of the Coupled Model Intercomparison Project 5 (CMIP5) simulations ..."

5. *"Page 2, Lines 30-32: Would it be clearer to the reader if this sentence made reference to the 'partitioning' of the chemical families being in photochemical steady state?"*

   Yes, mentioning of the keyword "partitioning" might assist the reader in understanding the sentence. But we still want to be specific about the ". . . diurnal average concentrations of the individual species ...". The sentence now reads: "In extrapolar conditions the diurnal average concentrations of the individual species within the chemical families (partitioning) mentioned . . . "

6. *"Page 5, Lines 1-4: As written I am having trouble following the argument given by: 'In the extrapolar regions the short-lived reactive species (e.g. ClOx or BrOx) are sufficiently close to chemical equilibrium determined by the local conditions (e.g. pressure, temperature, radiation and the abundance of reaction partners). Consequently, in the chemical families containing only one reservoir gas (NOy and HOy) the concentration of the short-lived species is uniquely determined by the abundance of the total family.' I agree completely with the statement that the short-lived species are in chemical equilibrium and that the partitioning within the family of short-lived species can be derived from a photochemical steady-state assumption. But I fail to see how this fact can then be used to derive the partitioning between the short-lived and reservoir species for chemical families with only one reservoir gas. For the NOy and HOy families do you not need to first divide the family into the short-lived and reservoir fractions, before chemical equilibrium can be used to further partition the short-lived species? From the way this process is described, it sounds like for the NOy and HOy families you assume local chemical equilibrium between the short-lived and reservoir species and this could be more clearly stated."*

   Yes we assume local chemical equilibrium for NOy and HOy between the short-lived and reservoir species. The sentence is appended with: "the concentration of the short-lived species is uniquely determined by the abundance of the total family, i.e. we assume local chemical equilibrium between the short-lived and reservoir species."

7. *"Page 17, Line 16: Starting here in Section 5.2 you compare a two year run of the ATLAS CTM using SWIFT where the background states for HOx, NOy, Cly and Bry are taken from daily zonal-average fields from the reference, full chemistry ATLAS run. Then in Section 5.3 you compare a 10-year simulation of ATLAS-SWIFT using monthly climatologies. Is it then possible to separate the errors that are due to the use of the monthly HOx, NOy, Cly and Bry climatologies by*

comparing the two-year period that is common to both of these runs? Perhaps just by adding an extra line to Figure 7?"

The following two plots show the comparison between the stratospheric ozone columns over Potsdam of the 2-year simulation vs. the 10-year simulation. The **Fig. 1** incorporates the 2-yeas simulation (green, 2005 and 2006) in the monthly mean time series (compare to Figure 7 in paper).

The **Fig. 2** shows the same comparison in a scatter plot. X-axis 10-year simulation y-axis 2-year simulation. Instead of monthly means the daily stratospheric ozone columns of the 2005 and 2006 are shown.

Both **Figures** show deviations of 10-20 DU between the 10-year simulation and 2-year simulation. The deviations are comparable to the deviations between ATLAS and the 2-year and ATLAS and the 10-year simulation (see Figure 8 in paper). The use of the daily averaged Cly, Bry, NOy and HOy fields in the 2-year simulation vs. the the actual monthly climatologies in the 10-year simulation does not cause any tendencies in the deviations. Thus it is not possible to attribute higher or lower deviations to either the daily averaged trace gas fields or the monthly climatologies. Since both SWIFT simulations show similar deviations to ATLAS, we do not see additional benefit in adding this information to the paper.

8. *"Page 21, Line 13: It is stated here ' An initial estimate of the increase in computation time caused by Extrapolar SWIFT is roughly 10%.' where I assume that the 10increase is relative to the ECHAM6.3 using specified ozone - i.e. no chemistry at all? Is there easily available any estimate of the increase in computation time for ECHAM6 when a full stratospheric chemistry is included that could be quoted here?"*

   We can only give very rough estimates here. The ECHAM model in our working group, which Extrapolar SWIFT was coupled to, does not have an optional full stratospheric chemistry scheme. We consulted our colleagues at FU Berlin

who are running the ECHAM MESSy Atmospheric Chemistry model (EMAC) for some estimates. For one model months EMAC with full stratospheric chemistry (MECCA) requires 60-80 min (114 cores). In the same setup (114 cores) EMAC with prescribed ozone requires 9-10 min per model month. Assumption: EMAC + Extrapolar SWIFT requires 10% more computation time, thus 10-11 min per model months. In conclusion ECHAM + Extrapolar SWIFT is 6 to 8 times faster than ECHAM + full stratospheric chemistry.

9. *"Page 21, Lines 16-17: I imagine part of the factor of 10ËĘ4 difference in speed between the full ATMOS model and the SWIFT ozone is due to the fact that SWIFT model has a significantly reduced number of advected species. It might be worthwhile to mention this as one of the factors in the reduced speed."*

   The ATLAS CTM is a Lagrangian model. The chemical species are advected along lagrangian trajectories. Thus the computation time for the advection does not scale with the amount of species. The reduced amount of species in SWIFT (coupled to / using the ATLAS advection and mixing scheme) does basically not impact the computation time.

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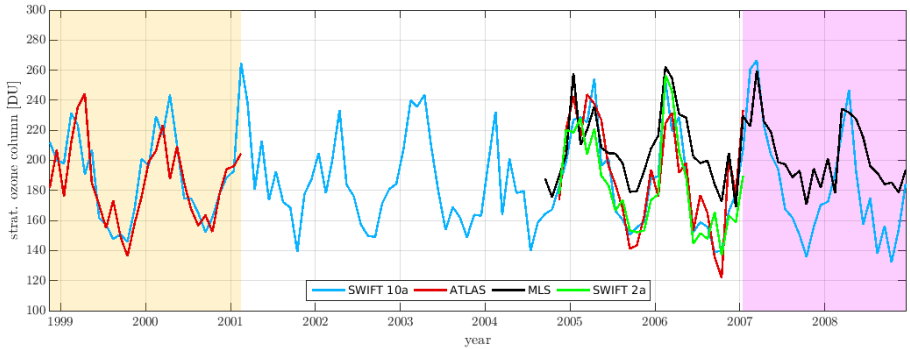

Fig. 1. Monthly mean values of the stratospheric ozone column (15 km – 32 km) over Potsdam, Germany. The bright green line shows the 2-year simulation.

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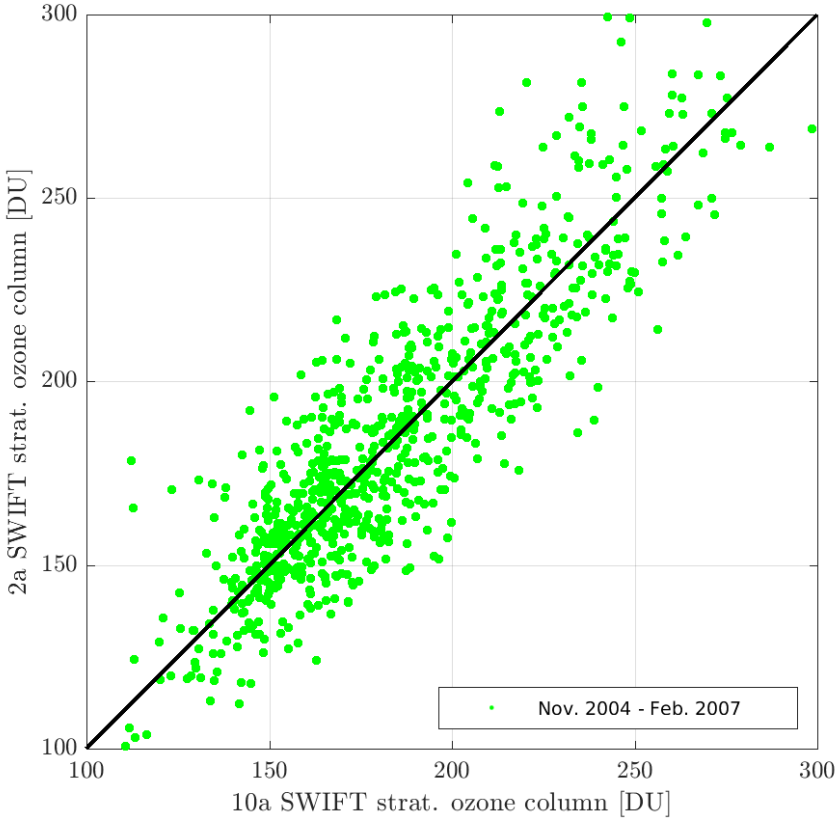

Fig. 2. Scatter plot showing daily averaged stratospheric ozone columns of the 10-year vs 2-year simulation.

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