Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-134-RC2, 2017

Geoscientific Model Development Discussions
Open Access
EGU

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

Anonymous Referee #2

Received and published: 30 October 2017

Comments on Kreyling et al., The extrapolar SWIFT model...

The manuscript describes the development and testing of a parameterized ozone for use in general circulation models that can be used to provide ozone fields for use in the model radiation that are consistent with the model circulation. The parameterization expresses the total chemical tendency of ozone for the extrapolar mid to lower stratosphere as a function of nine variables that are either derived directly from the model or specified from an external climatology. The paper is very well written, the methodology used to develop the SWIFT parameterization is logically laid out and well described and the approach to reducing both the parameters and the terms in the polynomial

Printer-friendly version

Discussion paper

functions appears to be quite rigorous.

Overall, I only have minor comments. Although I do have one nagging question that I cannot quite figure out for myself that perhaps could be addressed more explicitly in the article. As discussed in the manuscript, there are existing approaches to specifying ozone fields that are consistent with the model circulation, such as Linoz or the Cariolle scheme. These schemes rely on linearization of the ozone chemistry around a reference climatology and, for the example of Linoz, only account for variations in temperature, local ozone concentration and overhead ozone column. One of the significant advantages of SWIFT would be the explicit inclusion of variations due to the major chemical families that affect ozone, including HOx, NOy, Cly and Bry. To ensure that the SWIFT parameterization does not extrapolate outside of the range of conditions for which it has been developed, there is a rigourous check on local conditions using an additional polynomial – the domain-polynomial. The problem of a large-scale shift in the conditions for which the SWIFT model has been designed is discussed in the paper and the example of a decrease in Cly of 50% (Page 11, Line 30) is given. Is the SWIFT model really robust to a large, say 30%, change in Cly? 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.

My other minor comments are given below.

Page 1, Line 9: I might suggest replacing ‘local ozone column’ with ‘overhead ozone

column' as this might be a bit more specific.

Page 2, Lines 6-7: the wording seems to suggest all CMIP5 models used prescribed ozone 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.

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?

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.

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?

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 10% increase 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?

Page 21, Lines 16-17: I imagine part of the factor of 10ˆ4 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.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-134, 2017.

Printer-friendly version

Discussion paper