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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Technical Note: SWIFT – a fast semi-empirical model for polar stratospheric ozone loss" *by* M. Rex et al.

Anonymous Referee #2

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This paper describes a new semi-empirical model for computing polar O3 loss from a simplified set of equations. A stated aim of this study is to develop a fast yet accurate method to derive ozone loss in the general circulation models used for future ozone depleting substances and greenhouse gases emissions scenarios.

This is "technical paper" in the sense that the article is essentially oriented towards the description of a new tool, rather than to advances in scientific knowledge. I would have preferred this paper to be submitted to Geoscientific Model Development, which is a perfectly suitable EGU journal for this kind of study, but I respect the decision of the Editor to treat is a Technical Note in ACPD.

This study presents an original and elegant way of rapidly computing the polar O3 loss

occurring each winter/spring in polar vortices. The paper is concise and well written, and the results look promising when compared to state-of-the-art experimental data. Therefore I recommend this note to be published in ACP, after proper consideration of my two general comments and my specific comments listed below.

GENERAL

1) I find the general tone of the introduction to be overly optimistic with regard to the direct application of SWIFT to general circulation models. I think it is fair to say that there is still a long way to go before this model can be implemented to GCMs: first, SWIFT only deals with polar ozone loss. This is recognized by the authors, but they have tendency to present SWIFT as an improvement over the existing linearized ozone schemes, which have yet the advantage of representing both polar and extra-polar O3 changes. It is clear that further extension of SWIFT to extra-polar processes will be necessary before this new method can be implemented in GCMs. My second concern is that the paper lacks technical details with regard to the practical implementation of SWIFT into 3D models. Because the transport-related changes are included in the fit parameters, it is said that the model "should not be used in combination with a model of stratospheric transport". This point must be clarified, as it implicitly excludes the possibility of coupling SWIFT to a GCM using O3 as a transported tracer. On a related aspect, it is not clear how many tracers are needed (if any) for SWIFT to work in a GCM environment. If SWIFT requires four prognostic tracers to be transported (such as O3/ClONO2/HCl/HNO3), I do not think the model will be any faster than the existing linearized O3 schemes for GCMs, which usually employ only 1 or 2 tracers.

2) The year-to-year variability of the modeled total O3 loss in Figure 4 looks good, but the absolute agreement is obtained after fitting the the loss at 460 K to the observed ozone column loss. This is recognized by the authors, but I think the demonstration would be much strengthened and the paper much improved if it showed the results of SWIFT at 460K for a selection of winters other than those on which it was trained, in the same way as Figure 2.

SPECIFIC

comments listed in the (page,line) coordinate system

(31609, 4-7)

"The effects of ozone on climate are therefore usually incorporated in GCMs by prescribing ozone..". This statement is confusing since the GCMs mentioned in the previous sentence (Austin, Eyring) do not use prescribed ozone fields. In general, I find that little credit is given to the CCMVal GCM experiments in this paragraph.

(31609, 15-16)

"there is an urgent need for fast stratospheric schemes…". This suggests that fast O3 schemes for GCMs do not exist. As the authors themselves show in the following paragraph, this is not true. Rephrase please (at least, remove "urgent").

(31609, 17-24)

The authors should cite the paper by Cariolle and Teyssedre (ACP, 7, 2183, 2007) describing an improved version of the original Cariolle's scheme, in particular for polar regions.

(31610, 14-16)

"polar ozone depletion…which is by far the strongest perturbation of the state of the ozone layer". I agree, but this is only a fraction of the picture when it comes to coupled chemistry-climate simulations. Tropical and mid-latitudes changes in O3 are at least equally important in future stratospheric climate. As it is, the introduction suggests that the current version of SWIFT would be sufficient for realistic GCM simulations of GHG and ODS scenarios. I do not think this is true. SWIFT may represent a progress in computing accurately polar O3 loss in GCMs, but for the moment it cannot compete with current fast O3 schemes due to the absence of representation of extra-polar processes. This point should be made more clear in the introduction.

(31611,3)

define ClOx in the body of the text (in addition to the abstract)

(31611, 4)

"All variables are vortex averages of the respective species". I am not sure to understand the meaning of this.

(31611,19)

"the model (SWIFT) should not be used in combination with a model of stratospheric transport". This seems incompatible with a future use in a GCM. Clarify.

(31612,11)

what is the isentropic level chosen for the calculations in Figure 1 ? 460 K ? please indicate on the Figure.

(31615,1)

It is not clear to the reader why the G term must be taken into account if it is not important for the O3 loss.

(31615,9)

"reaction R4 is efficient only at temperatures well below the PSC formation threshold". A reference is needed in support of this assertion.

(31619,12-22)

again the isentropic level at which these values have been derived (460 K?) should be mentioned explicitly.

(31622, 2-5)

In Figure 2, the modeled O3 loss appears consistently smaller then the Match rates between days 20 and 40, whereas ClOx seems in line the observations. This must be explained or commented.

In Figure 3, the "observed" ClOx is substantially larger than the model ClOx for most of the 2006 winter. On the other hand, SWIFT and Match O3 loss rates are fairly consistent at the beginning and at the end of the winter, but tend to disagree just at the time when the SWIFT and "observed" ClOx agree best (days 200-230). Comments on those two issues?

(31622,6-13)

Why are the SWIFT results not shown after year 2008 in Figure 4 ?

(31622,22-23)

"SWIFT is extremely fast. . .several hundreds of year per second". This statement must be tempered by the fact that the simulations are done for only one point (vortex-average) and one single level. When incorporated in a GCM, the calculations will be performed on millions of grid points, and may require the explicit transport of several tracers. In those conditions I am not sure that SWIFT will be faster than the existing linearized chemistry schemes.

(31623,5-6)

"SWIFT will still give a good representation. . .in a changing climate". I agree, but this is also the case of existing linearized O3 scheme that include the temperature in their Taylor expansion of the O3 continuity equation.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31607, 2013.