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Interactive Comment

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

M. Rex et al.

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Dear reviewer,
thank you for reviewing our paper and your helpful comments!

General comments

- 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."

We do not want to imply that the current version of SWIFT is ready for operational use in a GCM. We added a sentence to the introduction that the current version is a "proof of the general concept" and that a future version for operational use is in development (including extra-polar processes). We added some more discussion about the implementation into a GCM in Section 2.

- 1) "...have tendency to present SWIFT as an improvement over the existing linearized ozone schemes ..."

  Our point is that the method of using a system of differential equations based on physical and chemical considerations is a promising alternative approach compared to using a linearization around a mean state. We added some more discussion to the introduction and Section 2 to clarify this.

- 1) "It is clear that further extension of SWIFT to extra-polar processes will be necessary before this new method can be implemented in GCMs."

  We agree. It is stated on page 31610, line 16 that an extra-polar module is in development.

- 1) "My second concern is that the paper lacks technical details with regard to the practical implementation of SWIFT into 3D models."

  We have added a paragraph explaining the future implementation of SWIFT into a GCM at the end of Section 2.

- 1) "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."

  This is a misunderstanding and we have clarified this point in the paper: SWIFT calculates the chemical ozone change rates and the transport of ozone has to be

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done within the GCM. For the other prognostic variables (which are not needed outside the SWIFT module and are never communicated to the GCM) the comparably small transport related changes are included in the the SWIFT module and these species do not need to be transported by the GCM. Added some more discussion of this to this paragraph. Further information is found in the new paragraph about the future implementation.

- 1) "On a related aspect, it is not clear how many tracers are needed (if any) for SWIFT to work in a GCM environment."

  The only species that is transported and represented as a 3D field at every grid point is ozone. The other species are represented as vortex averages (one single value per level) and transport effects are implicitly included in the fit parameters. So only one tracer is needed in the GCM, i.e. ozone. This information was missing in the manuscript. We added some discussion of this at the end of Section 2 to clarify what we have done.

- 1) ". . . I do not think the model will be any faster . . . "

  Since we only employ 1 tracer, the computing time for the transport will be comparable to other schemes. We do not state that our scheme is faster than other schemes in the manuscript and we agree that it is not clear if our scheme is faster.

- 2) We have included a new figure showing a range of results from other winters.

Specific comments

- Page 31609, lines 4–7: The introduction makes a clear separation between CCMs (Chemistry Climate Models) with a detailed stratospheric chemistry model and "pure" GCMs without a full chemistry model. This is explained from page 31608, line 21 to page 31609, line 3. The models mentioned in Austin and Eyring et al. are CCMs and not GCMs. Hence, there is no confusion here: CCMs do not use prescribed ozone fields by definition. In contrast, most GCMs still do use prescribed ozone fields. To make that more clear we have now added a sentence that most of the models used in the IPCC reports use prescribed ozone fields and added the current IPCC report as a reference. In addition, we changed the sentence “generally referred to as chemistry-climate models” to “. . . generally referred to as chemistry-climate models (CCMs) in contrast to GCMs without a chemistry scheme . . . ”.

- Page 31609, lines 15–16: Removed “urgent”. We do not want to suggest that fast O3 schemes do not exist and do not state that in the manuscript. Since we introduce the existing fast models in the next paragraphs, it is not clear to us how this impression could come up. The sentence refers to the fact that most GCMs still use prescribed ozone and none of the existing fast O3 schemes (with no preference to our scheme).

- Page 31609, lines 17–24: We have included the reference in the introduction.

- Page 31610, lines 14–16: We agree. We have now rephrased some sentences in the introduction and added some more discussion in the introduction and Section 2, see the reply to your general comment 1.

- Page 31611, line 3: Done.

- Page 31611, line 4: We have changed “are” to “represent” and added some further explanation in the next sentence. Vortex averages are not calculated from underlying Eulerian gridpoints in this simplified model version, which is not a submodule in a GCM, but there ist just one single-valued variable representing a vortex average. The slightly different approach when implementing SWIFT into a GCM is now explained in a paragraph at the end of Section 2.

- Page 31611, line 19: We have now added some discussion on the future implementation of SWIFT into a GCM to Section 2. The future implementation will also include modified fit parameters (to exclude transport in the rates of change).
- Page 31612, line 11: Added the information in the text and figure caption.
- Page 31615, line 1: It improves the agreement at the end of the winter without too much computational cost. Term G cannot be removed without removing Term B, since the effect of the reactions represented by these two terms cancels to some amount.
- Page 31615, line 9: This is based on calculations in Wohltmann et al. (2013). Added reference.
- Page 31619, lines 12–22: Added that these values are derived for the 460 K level.
- Page 31622, lines 2–5: The fit optimizes the difference between observations and model results, which will always leave an unexplained residuum between the observations and the model. In particular, since the observations of HCl, ClONO2 and ozone rates are all prone to errors, they will never be perfectly compatible to each other. Note that the fit to the ozone rates lies mostly inside the displayed error bars (since these are one sigma values, some values outside the error bars have to be expected).
- Page 31622, lines 6–13: The figure has been changed and now ends in 2008.
- Page 31622, lines 22–23: There is a misunderstanding here, because the details of incorporating the SWIFT model into a GCM were not described in the manuscript. We have now added a paragraph to Section 2 that describes the future implementation of SWIFT into a GCM.

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The idea is that vortex-averaged rates of change of ozone are calculated on a small number of GCM vertical levels (say 5). The rates of change are then added to the ozone values of the grid points at these vertical levels which are situated inside the vortex. This way, the calculations have only to be performed 5 times. Even if the calculations would be performed for all grid points, our estimation would be some 10.000 grid points (based on 2 degrees resolution) and not some millions. Note that ozone is the only transported species and that the other species are represented as vortex averages.

- Page 31623, lines 5–6: We agree that the linearized ozone schemes also will to some extent cope with changing climate and do not state that this is not the case. But note that the temperature term in the linearized models does not refer to heterogeneous chemistry in the polar vortex and that heterogeneous chemistry has to be included by an extra term in these models. Our model calculates heterogeneous chemistry in the vortex, so that no direct comparison is possible here.

  The main difference to the linearized models is hat the equations in our model are based on a more “physical” approach (compared to the “mathematical” approach of a Taylor series) which lets us expect that our equations behave more realistically when conditions move away from the mean state. Added some discussion along these lines in the introduction.

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