Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-13

Anonymous Referee #1

Referee comment on "Transport parameterization of the Polar SWIFT model (version 2)" by Ingo Wohltmann et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-13-RC1, 2022

The manuscript presents a very computationally light method for calculating the time evolution of the average ozone concentration inside the polar vortex during springtime, the period of time when ozone loss is significantly enhanced by heterogeneous chemistry on polar stratospheric clouds. The pre-existing Polar SWIFT model had been designed to calculate the chemical loss of ozone, but required several prognostic tracers (as described in Wohltmann et al., Geosci. Model Dev., 10, 2671-2689, 2017) to calculate the effect of transport. Here a parameterization of transport is developed to allow for the calculation of a vortex average ozone concentration directly and remove the need for the addition of advected tracers.

I have no serious concerns with the method used to derive the transport parameterization. Although the description of the previous implementation of Polar SWIFT, the one requiring additional tracers advected by the model, and how this differs from the revised version would be helpful. In particular, while there is a discussion of how Polar-SWIFT is implemented in ATLAS (Section 2.2), it is difficult to get an idea of how exactly Polar-SWIFT with the newly developed transport parameterization is implemented in a GCM. What prognostic variables does the model require and how does this differ from the previous version that required the explicit calculation of transport. As far as I can tell, this information is provided in the introduction (around line 20) but is mentioned in passing and is not part of any coherent description of the implementation.

One other, overarching question I am left with is how does the GCM specify the concentration of ozone outside of the polar vortex when using this version of the Polar SWIFT ozone parameterization? This should be described here.

Other than these 'editorial' suggestions, I only have minor comments on the manuscript. These are detailed below.

Lines 4 – 5: 'Many GCMs do not include a usable general scheme for the transport and mixing of chemical species in the stratosphere.' As many GCMs and Earth System Models now contain a prognostic treatment of aerosols, I would think a serviceable transport scheme would be more generally available. Is it perhaps more true that many GCMs do not specify a high-enough model lid and a sufficient number of model levels in the stratosphere to adequately resolve the dynamics of the stratosphere? Isn't this point also a little beside the point because the parameterization being presented here is for the vortex averaged transport effect and would not be the kind of quantity readily calculated by a 3-D advection scheme in a GCM?

Lines 23 – 34: The section beginning 'When using the Polar SWIFT model…' is actually describing the present work but it is a bit disorienting to the reader because it appears in

the introduction and really does not describe previous versions of Polar SWIFT or the motivation for the current work.
Lines 65 – 76: It took me a couple of readings to understand what is going on – just too many variations of 'SWIFT's. Maybe an introductory sentence around Line 67 would help, stating that the transport parameterization is derived from an analysis of the total and chemical tendencies of ozone from a simulation of the ATLAS-SWIFT model?
Lines 132 – 133: 'For the temperature variable ΔT_{fit} , we use the vortex-averaged temperature difference in a layer at a given date compared to the start date (vortex formation date, see Table 1)'. Since the ΔT_{fit} term also involves subtraction of the estimate of the time-evolving radiative equilibrium temperature, as discussed a bit later, what is the reason for including the (somewhat arbitrary) temperature at the vortex formation date? Given the radiative relaxation timescale, the temperature at the vortex formation date I think would become irrelevant as an estimate of dynamical forcing with increasing time since the start date. And, as pointed out at lines 133 – 136 'Equation 2 suggests that the difference in temperature to the start date roughly corresponds to the deviation of the temperature on this day from the radiative equilibrium temperature by the effects of the Brewer-Dobson circulation. According to Equation 2, this would be exactly true when the temperature at the start date would be the radiative equilibrium temperature.' And, of course, there is no guarantee that the temperature at the start date will be the radiative equilibrium temperature.
Lines 221 – 223: 'The differences between Polar SWIFT and MLS cannot be explained by the transport parameterization, since they are much larger than the differences of about 0.2 ppm between the transport parameterization and the transport term of ATLAS discussed in the last section 4.1.' But in Figure 6, particularly for the Northern hemisphere the Polar SWIFT model without the transport parameterization (the blue line of Figure 6) does a good job of estimating ozone for the two cold years. Is the argument that there should always be some positive contribution from transport so that the chemistry-only simulation should be even lower than it is, particularly for cold years? Do the authors have any reason to believe the chemistry parameterization underestimates the amount of ozone chemical destruction?