Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC3
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Comment on gmd-2022-13

Anonymous Referee #3

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-RC3, 2022

Review of Transport parameterization of the Polar SWIFT model (version 2), by Ingo Wohltmann and Daniel Kreyling, gmd-2022-13.

This manuscript describes a parametrization for transport to obtain from a previous chemistry parametrization the amount of stratospheric ozone change inside the polar vortex. The method presented provides polar stratospheric ozone information for GCMs that require both simplified ozone chemistry and simplified atmospheric transport schemes.

While it is clear that the method provides a computationally fast option for models unable to include more complex ozone descriptions, several aspects need further clarification before final publication. The main ones are related to application and implementation of the method, as well as to validation and comparison of results. Other specific comments included below also need to be addressed.

Main comments:

Application and implementation of the method:

-What type of GCM will benefit from this parametrization? Why do the GCMs mentioned in the paper require a transport parametrization to be able to use the Polar SWIFT ozone?

-How do GCMs using this method consider ozone outside the polar vortex?

-How can be the limited number of vertical levels considered by this parametrization be enough for modern GCMs to simulate realistic stratospheric ozone links with meteorological variables? This is particularly concerning in the case of ICON-NWP which is not a climate model but an NWP model.

-More details about the implementation of the parametrization in GCMs should be added to Section 3.3

Validation and Comparison:

-Comparison of the parametrization results against observations is limited to one figure in the manuscript, and the comparison against ATLAS full-chemistry or ATLAS-SWIFT is also limited. This type of comparisons should be included across figures for a clearer quantitative assessment of the parametrization performance.

-A more quantitative discussion of comparison results should be included in the discussions, at several points the manuscript does this only in a vague qualitative way (see specific comments below).

Specific comments:

Abstract: It needs rewriting to summarize more clearly the work discussed here. In its current version it reads more about Polar SWIFT itself than about the transport parametrization described in this paper.

Lines 12-14: These studies on ozone linear schemes that should also be included here: McCormack et al (2006), Monge-Sanz et al (2011; 2022). The latter showing results from implementation in ECMWF runs and improvement over the Cariolle's scheme performance.

Lines 14-15: This manuscript does not compare the performance of Polar SWIFT to the mentioned schemes, and neither did Wohltmann et al. (2017). Therefore, the sentence needs to be rewritten for instance as "Polar SWIFT was developed as an alternative to these schemes...", otherwise a comparison to those schemes' performance would need to be provided.

Line 18: does the ICON-NWP model require a transport parametrization in the stratosphere to account for the evolution of the polar vortex? This sentence makes the reader assume it does, better rephrase and clarify please.

Lines 26-27: Changing preposition “by” to "from" would make the sentence clearer, same at the end of the sentence for "from Polar SWIFT".

Lines 37-38: Authors need to further justify how these 5 levels can provide enough information on polar ozone for global GCM runs. Or explain what limitations there will be for GCM runs using this scheme.

Line 51: Rewriting as "The vortex-averaged concentrations for these species..." would be clearer.

Line 53: Do you use daily climatologies or seasonal climatologies? Are the daily initializations done with the corresponding seasonal climatology, am I understanding correctly? Please clarify and rewrite.

Lines 54-56: How is mixing climatologies from observations and model runs affecting self-consistency and how do you deal with this?

Lines 61-64: if the parametrization is derived from full chemistry ATLAS and MLS observations, it is not fully clear to me why additional consideration of long-term change in chlorine content needs to be included. Please include further explanation

Line 66: As earlier in the paper, this limited number of vertical levels needs further explanation and how this can affect the parametrization performance in a global GCM needs to be discussed.

Line 79: Please quantify this statement. How small is the sensitivity to the choice of vortex dates?

Line 83: The date for the closure of the Antarctic ozone hole shows high interannual variability, how is this vortex breakup fixed date for all years going to affect the parameterization performance in the SH for a GCM?

Figure 1: This figure should provide context for these results by including comparison at least with another validated model, for instance with ATLAS full chemistry.

Lines 92-93: From the figure, this linear approximation only holds in the long term, for shorter timescales high non-linearity can be seen, especially in the NH but not only. How will this affect ozone-meteo links in a GCM operating on the shorter timescales?

Lines 120-122: Please explain and write more clearly what you mean. It reads as if "deliberately choose to simplify" could go against "find a well-working empirical relationship". What aspects are you deliberately simplifying?

Figure 3: Results shown in Figure 3 should be further discussed in the text, and this figure should also show a comparison with data other than the parametrization.

Line 154: Please quantify "reasonable agreement".

Line 159-160: If this was the reason, shouldn't it be easier for the parametrization to simulate results in the SH than in the NH? What about the non-linearity between ozone and temperature in the SH vortex?

Section 3.3: Is temperature the only variable needed from the GCM when using this parametrization? This seems to be indicated by Eq 3 but there is no specific mention in the text. Please add a specific description in the text and add more information about implementation in a GCM.

Figure 5: The comparison with ATLAS-SWIFT should also be added to previous figures, in particular Figure 3, and corresponding ones in the supplement.

Lines 190-192: what about the % difference against observations? And is a 10 % order of magnitude good enough for the ICON-NWP model?

Line 211: Delete "The" at start of sentence.

Lines 212-213: Why is the difference so small? This needs to be further discussed, isn't the purpose of the parametrization to improve this?

Figure 6: Interannual variability in the NH is far from MLS observations. In the SH both the interannual variability and the mean value are far from MLS observations. Please develop further the discussion on these points in the paper.

Lines 216-218: Please quantify these statements. By how much does the model overestimate ozone in cold winters and what does "relatively well" exactly mean for warm winters?

Lines 220-221: Then shouldn't the parameterization allow for a different vortex breakup date for warm and cold winters?

Lines 224-225: OK, but please add a reference documenting the performance of Polar SWIFT compared to observations please.

Lines 227-230: Key references should be added into this sentence documenting validity of linearity assumption, transport in ERA5 and chemistry performance of Polar SWIFT that can back up or strongly suggest these hypotheses.

Line 234: However, these three models do have complex transport schemes. Please summarize here clearly why they need this type of transport parametrization.

Line 238: "slightly better" needs to be quantified. The agreement with observations and ATLAS is very different in NH and SH from the results you show in the paper, indicating the parametrization may not be fully suitable for SH polar ozone.

Lines 238-240: Please add some text to summarize why it performs better in the NH than in the SH. Same for the contribution of the transport and temperature terms mentioned in the last sentence.

Acronyms: Most acronyms throughout the paper (abstract and main text) are not spelled out. If possible please do so on first appearance.

Figures S28 and S56: What percentage of the total ozone does this change represent? This information should be added to these figures.

References:

McCormack, J. P., Eckermann, S. D., Siskind, D. E., and McGee, T. J.: CHEM2D-OPP: A

new linearized gas-phase ozone photochemistry parameterization for high-altitude NWP and climate models, Atmos. Chem. Phys., 6, 4943–4972, https://doi.org/10.5194/acp-6-4943-2006, 2006.

Monge-Sanz, B. M., Chipperfield, M. P., Cariolle, D., and Feng, W.: Results from a new linear O_3 scheme with embedded heterogeneous chemistry compared with the parent full-chemistry 3-D CTM, Atmos. Chem. Phys., 11, 1227–1242, https://doi.org/10.5194/acp-11-1227-2011, 2011.

Monge-Sanz, B. M., Bozzo, A., Byrne, N., Chipperfield, M. P., Diamantakis, M., Flemming, J., Gray, L. J., Hogan, R. J., Jones, L., Magnusson, L., Polichtchouk, I., Shepherd, T. G., Wedi, N., and Weisheimer, A.: A stratospheric prognostic ozone for seamless Earth system models: performance, impacts and future, Atmos. Chem. Phys., 22, 4277–4302, https://doi.org/10.5194/acp-22-4277-2022, 2022.