Review: Effects of Ozone-Climate Interactions on the Temperature Variation in the Arctic Stratosphere

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I appreciate the authors efforts to isolate the impact of past Arctic ozone changes on trends in temperature and dynamics and to underline their findings with mechanical explanations. Unlike other studies, this study focuses on the extended winter season and might therefore extent our knowledge on the impacts of ozone changes from springtime to the whole winter season. However, I have doubts around the entire experiment setup (especially the way the ozone climatology is calculated) and think that the conducted simulations are not suitable to achieve the goal of this study. Moreover, the authors make use of a figure from another study without mentioning it.

Main comment:

More details should be provided on the ozone climatology that is used in the O3clim experiments. Are these daily or monthly means? 3D or zonal mean? If I understand it correctly from Fig. 1, then the ozone chemistry is still calculated in the O3clim setup, but it is not radiatively active. This information is missing in the experiment description in lines 179 ff. How many ensemble members were simulated? Moreover, Fig. 1 is identical to Fig. 1 from Friedel et al. (2022b) and the caption accompanying this figure is almost identical to the caption of Fig. 1 in Friedel et al. (2022a). This must be cited!

If I understand the experiment design correctly, I believe that the conducted simulations are not suitable to study the impact of past ozone changes on trends in temperature and dynamics. The authors contrast one simulation with fully interactive ozone for the period 1980-2020 against a simulation where they impose an ozone climatology calculated over this period. Then, they contrast trends from 1980-2000 in the two simulations to isolate the effect of ozone changes during that period on the variables of interest. I believe that this experiment design does not allow to disentangle the effect of past ozone changes, as the climatology was calculated over a period that includes ozone trends. Therefore, the ozone climatology used in the O3clim run might show lower ozone concentrations (due to an ozone decline from 1980-2000) than the ozone field in the crtl run in 1980. Hence, at the starting point of the simulations (i.e. 1980), the circulation/temperature and ozone field are not consistent in the O3clim simulation. Therefore, temperature changes might arise to adjust to the lower ozone concentrations and trends in this simulation cannot solely be attributed to dynamical and thermodynamical processes other than ozone. Moreover, with the approach used here, effects of potential long-term ozone changes and interannual ozone changes are mixed, since they are both disabled in the

O3clim experiments. When only using one ensemble member of 20 years, the role of interannual ozone variability is likely not negligible.

In order to achieve the goal posed by this study, I suggest first deriving an ozone climatology using a time-slice simulation with fixed boundary conditions (i.e. CFCs, GHGs,…) of the year 1980 with fully interactive ozone. The ozone climatology calculated this way can then be used to simulate the O3clim experiments from 1980-2000. Using this approach, the authors would be able to attribute changes in dynamics and temperature over this period to changes in ozone. However, more than one ensemble member for the transient runs would be necessary in order to isolate the effect of long-term ozone changes rather than the effect of interactive ozone variability.

Other comments:

- The introduction does not follow a clear outline. Sometimes it is unclear whether the authors are referring to interannual or long-term processes. The introduction reviews mostly literature that focuses on ozone-climate coupling on interannual timescales, but then the study focuses on long-term trends. Please tailor the introduction a bit better to the aim of this study. Some of the statements made in the introduction are also not supported by the cited literature (see detailed comments below). The title of the study is also misleading, as is suggests that this study focuses on interannual variability rather than long-term trends.
- Line 8: “increased”
- Line 15: “cooling trends in the Arctic stratosphere are …”
- Line 35: Please be a bit more precise here. Marsh et al. (2016) conclude that ozone feedbacks are not crucial for the model’s climate sensitivity in 4xCO2 forcing experiments. Maybe it is worth citing Rieder et al. (2019) in this context, who showed that ozone variations are important for the stratospheric temperature variability in models.
- Lines 44 ff: I could not find any evidence for this statement (i.e. the longwave ozone effect on stratospheric dynamics during winter) in the cited study (Strahan et al., 2013). Could you please provide further literature on the mid-winter longwave radiative effect of ozone?
- Lines 47 ff: Are you now talking about mid- or late-winter/spring? I believe this statement is only valid for springtime, when the polar vortex is already weakened. See also Haase and Matthes (2019).
- Line 85 ff: I do not really understand this sentence (starting with “Lin et al. … “). Could you please reformulate?
- Lines 91 ff: Chiodo et al. (2023) studied the impact of long-term ozone trends in the Arctic on temperature and dynamics. Might be worth mentioning this here.
- Lines 94 ff. “…, we focus on the historical long-term trends …”

- Lines 202 ff: To support this statement on the persistent cooling, you would have to calculate a trend over the whole time period. When doing this, you would probably get no trend, e.g. no significant temperature changes from 1980-2020.
- Fig. 2: How have the time series been normalised?
- The trends in Fig. 2 look a bit constructed, i.e. they seem to be very sensitive to the time chosen time frame. Especially the trends in springtime (March/April) seem to be entirely caused by the ozone depletion event in 1997.

References:

Chiodo, G., Friedel, M., Seeber, S., Domeisen, D., Stenke, A., Sukhodolov, T., and Zilker, F.: The influence of future changes in springtime Arctic ozone on stratospheric and surface climate, Atmos. Chem. Phys., 23, 10451–10472, https://doi.org/10.5194/acp-23-10451-2023, 2023.

Friedel, M., Chiodo, G., Stenke, A. *et al.* Springtime arctic ozone depletion forces northern hemisphere climate anomalies. *Nat. Geosci.* **15**, 541–547, https://doi.org/10.1038/s41561-022-00974-7, 2022a.

Friedel, M., Chiodo, G., Stenke, A., Domeisen, D. I. V., and Peter, T.: Effects of Arctic ozone on the stratospheric spring onset and its surface impact, Atmos. Chem. Phys., 22, 13997–14017, https://doi.org/10.5194/acp-22-13997-2022, 2022b.

Haase, S. and Matthes, K.: The importance of interactive chemistry for stratosphere–troposphere coupling, Atmos. Chem. Phys., 19, 3417–3432, https://doi.org/10.5194/acp-19-3417-2019, 2019.

Rieder, H. E., Chiodo, G., Fritzer, J., Wienerroither, C. & Polvani, L. M. Is interactive ozone chemistry important to represent polar cap stratospheric temperature variability in Earth-system models? *Environ. Res. Lett.* **14**, 044026 (2019).