**Review of the manuscript "Atmospheric impacts of chlorinated very short-lived substances over the recent past - Part 2: impacts on ozone" by Bednarz et al., EGUsphere preprint repository, 2023.**

The paper presents a modeling study using UM-UKCA with and without CL-VSLS halogens oriented to evaluate the impact of chlorinated very short-lived substances (VSLS) on recent-past stratospheric ozone trends (2010-2019). In addition, a new estimation of the ozone depletion potential (ODP) and stratospheric-ozone depletion potential (SODP) of the dominant CL-VSLS species (dichloromethane) is provided. The simulations are properly designed, the paper is well written and organized, and the analysis of the results is coherent. Their main conclusion is that CL-VSLS induce a modest but non-negligible role on the stratospheric ozone budget that counteract some of the gains achieved by the Montreal Protocol, but that the inclusion of CL-VSLS in UM-UKCS does not considerably modify the magnitude of diagnosed ozone trends in the tropical lower stratosphere. I suggest the paper is accepted for publication after the following issues have been solved:

**Main Comment:**

**Update the comparison with other modeling studies**

The authors properly compare their main results with previous publications performed by the group and/or co-authors, which are in line with the results presented here (i.e., recent ozone trends in the lower stratosphere are dominated by dynamics and not chemistry). However, a discussion with other results showing a significant contribution from VSLS chemistry (e.g., Villamayor et al., 2023) would improve the manuscript (see specific comments below). In addition, the discussion would benefit of a comparison of CL-VSLS impacts on ozone trends with respect to that arising from BR-VSLS (e.g., Sinnhuber and Meul, 2015; Barrera et al., 2020).

**Minor Comments:**

P1,L17-19: Are the values provided in the abstract "annual mean" or "springtime mean"? In case different time-averaging apply, please make it clear.

P2,L54-57: You should relate this with the recent publication by Villamayor et al. 2023 that found that inclusion of both natural and anthropogenic VSLs in a CCM results in a significant chemical signal that contribute to ozone reduction in the low-latitudes lower stratosphere.

P3,L74-75: It would be interesting to include some time of comparison between FR and SD. For example, a panel like Fig. 1c but presenting the mean +/- sigma of the latitudinal variation of ozone changes for the FR ensemble simulations. This would help to evaluate how well the FR range compares with the SD-runs values?. Note that Fig. 3 in part-1 of this paper (Bednarz et al., 2022) shows that FR simulations have a larger SGI, but similar SGI+PGI than SD simulations ... Thus I would expect a larger O3 influence for FR simulations. Is that the case?

P3,L80-81: What do you mean by “warm winters”? Are those 2-3 DU differences observed in Winter? or in Arctic spring preceded by warm winters? In addition, note the sentence only apply to high-latitudes, and is confusing as it mixes "warm winters" with "on average each year". Please rephrase and expand to make it clear.

P3,L81-81: Smaller over the Arctic, but larger over SH high-latitudes. Why is that? Due to the larger VSL-CL emissions in NH? Or could temperature changes play also a role here?.

P3,L86-87: Similar to P3,L74-75, you highlight the importance of nudging to evaluate the impact on Arctic results, but I wonder how large the difference between SD and FR simulations can be.

P4,L108-109: What is the rationale for showing ClO values at the beginning of march but O3 values at the end of march for nudged simulations? To evaluate the "cumulative" role of VSL-Cl chlorine on O3 losses?

P5,L135-136: As described, the chemical signal is negligible when SD are considered ... but induces an additional 0.5-1.0% enhancement for FR simulations (Fig S3). The authors may want to evaluate this and comment in more detail.

P5,L138-139: The results are in line with Chipperfield et al., 2018 and in contrast to Villamayor et al. 2023 ... which include both natural and anthropogenic VSLs. Note that Villamayor found also a negative trend, although not significant, when only VSLS-Cl were considered.

P5,L143-144: I suggest writing the numbers in the text as mean +/- sigma or mean +/- 2sigma ... as the range is already shown in Table 1.

P5,L151-152: I suggest mentioning explicitly that Cl-VSLS are not considered in the Montreal Protocol and/or amendments.

P5,L155-156: This absolute numbers for chlorine could be compared with VSL-Br impacts in the lower stratosphere as described by Sinnhuber and Meul, 2015 and Barrera et al., 2020.

P6,L156-157: Results summarized here apply mostly for the nudged simulations (SD), but not for the FR. A comment on this would be helpful.

P7,L192: I'm confused about the LBCs description: I thought for the case of CH2Cl2 and C2Cl4 an emission inventory (Claxton et al., 2020) was applied instead of using LBCs. Could you please make this clear?

P7,L203-204: Here you mentioned a source, but above you mentioned only LBCs were used. Please make it clear. In addition, if you provide a "perturbation value" here, it would be nice to provide the "base CH2Cl2 emission value" in previous paragraph, so one can easily estimate the magnitude of the perturbation. Similar to CFC-11, could you please provide the surface LBC for the base simulation.

**Language editing comments and Typos:**

P1,L16: Remove period "." after "time."

P4,L105: "shows that up to 25 ppt of the elevated ClO" or "shows differences up to 25 ppt for ClO". Please revise and make it clear.

P5,L158: What do you mean by "exact results"?

P7,L198: replace "ed" by "performed" or similar.

P8,L217: is it "where" or "when"

P8,L219: replace "included" by "including"

**Figures and Tables**

Figure 1: The final sentence of the caption should say annual global mean "total ozone column" changes ... similar for the "total ozone column change" magnitude that is missing in the Y axis of panel c.

Figure 2: Please use “ozone difference (%)” instead of the way it is written. Also I suggest including the dynamical tropopause line or 150 ppbv chemical tropopause line to help the reader splitting the troposphere and stratosphere.

Figure 3: Why do you show Ozone differences for 31 March but ClO differences for 1 March?

**References**


Barrera, J. A., Fernandez, R. P., Iglesias-Suarez, F., Cuevas, C. A., Lamarque, J.-F., and Saiz-Lopez, A.: Seasonal impact of biogenic very short-lived bromocarbons on lowermost stratospheric ozone between 60° N and 60° S during the 21st century, Atmos. Chem. Phys., 20, 8083–8102, https://doi.org/10.5194/acp-20-8083-2020, 2020.

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Claxton, T., Hossaini, R., Wilson, C., Montzka, S. A., Chipperfield, M. P., Wild, O., Bednarz, E. M., Carpenter, L. J., Andrews, S. J., Hackenberg, S. C., Mühle, J., Oram, D., Park, S., Park, M.-K., Atlas, E., Navarro, M., Schauffler, S., Sherry, D., Vollmer, M., Schuck, T., Engel, A., Krummel, P. B., Maione, M., Arduini, J., Saito, T., Yokouchi, Y., O'Doherty, S., Young, D., and Lunder, C.: A synthesis inversion to constrain global emissions of two very short lived chlorocarbons: dichloromethane, and perchloroethylene, J. Geophys. Res.-Atmos., 125, e2019JD031818, https://doi.org/10.1029/2019JD031818, 2020.

Sinnhuber, B.-M., and S. Meul (2015), Simulating the impact of emissions of brominated very short lived substances on past stratospheric ozone trends, Geophys. Res. Lett., 42, 2449–2456, doi:10.1002/2014GL062975.

Villamayor, J., Iglesias-Suarez, F., Cuevas, C.A. et al. Very short-lived halogens amplify ozone depletion trends in the tropical lower stratosphere. Nat. Clim. Chang. (2023). https://doi.org/10.1038/s41558-023-01671-y.