Review of the manuscript "On the atmospheric budget of ethylene dichloride and its impact on stratospheric chlorine and ozone (2002-2020)" by Hossaini et al., 2024.

The manuscript presents the development of a new bottom-up emission inventory for 1-2 dichloroethane (EDC) with enhanced spatial and temporal resolution compared with previous studies. An increasing trend in the global annual flux of EDC between 2002 and 2020 is determined based on regionally distributed production and consumption data. The new inventory is used in TOMCAT to estimate the Source Gas (SGI) and Product Gas (PGI) Injection of chlorine to the stratosphere, which results in a small (<1%) but not negligible impact on stratospheric ozone. The paper is very well organized, referenced and written, and certainly of interest for the community. Therefore, I suggest the work to be accepted with minor revisions. In the attached document, I provide a couple of general comments that might help to enhance the work visibility, and a list of minor and/or technical comments to be addressed.

Main comments:

- The authors mention several times that large regional and seasonal EDC enhancements are predicted with the new inventory, particularly for the Asian Summer Monsoon (ASM), and provide support by referring to the literature as well as by comparing with observations (e.g., in lines L32-37; L84-87; L266; L335-336; L439). In doing so, they should provide a stronger connection with the results published in Roozitalab et al., 2024 (cited in the manuscript) as well as to the recent ACCLIP paper from Pan et al., 2024 (https://www.pnas.org/doi/10.1073/pnas.2318716121). Most importantly, I think it would be a great idea to provide an estimate of the impact of enhanced EDC in the ASM over lower stratospheric ozone during the summer (see specific comment below).
- Given that the inventory considered bi-annual data and you performed a complete simulation for almost 20 years, it would be great to provide the mean rate of growth of EDC both for the surface emission as well as for the SGI and PGI. Those trends values (properly quantified) will be of interest for future reports on VSL influence on stratospheric ozone. In case the trends for the 2002-2020 period differ significantly to the trend during the last 4-5 years, an explicit statement and quantification could be provided.

Minor Comments:

L20: “transport of EDC (or its atmospheric oxidation products)”. Is this “or” or “and”?

L74: “… at reportedly both urban and background sites …”. Please revise text.

L78: Please indicate for which year were estimated the EDC emissions in the refernced study.

L133: “However, …”. This however seems to indicate discrepancy with previous results, but they all point in the same direction.

L146: “Although the imbalance was small compared to the large production volumes of EDC, …”. By how much? At least a percentage number should be provided.

L239: please provide a reference supporting the neglecting of EDC photolysis. Note that other studies cited in this work (Roozitalab et al., 2024) considered photolysis for EDC.

L343: I found reasonable to show the median instead of the mean for this case. Just by how much do the mean and the median differ?

L348: I completely agree with the statement and the link with Fig. 3, but I feel that this should also be linked to the spatially heterogeneous source strength shown in Fig. 2a.

L362: “Samples collected at Bachok, where the model captures the shape of the seasonal cycle well, …”. I'm really surprised about the large seasonal cycle (both observed and modeled) at this site. Why is this? Is it because the emissions also show a large variability? Is it because OH changes, is it because meteorology? Is it due to influence from continental China? Please extend about this interesting topic !!!

L414: Given by your methods description, reaction with OH is the only chemical loss in your model. Or does EDC also suffer any type of washout / dry-deposition?

L432-434: In addition to the referred work for Iodine chemistry, a simplified representation of these ice-recycling reactions has also been performed for bromine and chlorine in Fernandez et al., 2014 (https://doi.org/10.5194/acp-14-13391-2014). The impact of these and other reactions on PGI and ozone loss for the case of bromine was addressed in Fernandez et al., 2021 (https://doi.org/10.1029/2020GL091125), though no estimations was performed for chlorine PGI.

L475-477: I completely agree with the statement, and suggest that in order to advance in that area, providing an estimation of how much larger is the absolute and/or percentage ozone decrease within the ASM region would be of interest here.

L490-493: You should explicitly mention in the conclusion that you used a bottom-up approach.

L493-494: “Time-varying gridded EDC emission fields were developed and then included in the TOMCAT CTM.” It would be great if you can provide the emission inventory to the community to evaluate it in other models.

Figures and Tables

Table 4: I understand that in the last row it should say “>60°S” instead of “<” … as southern latitudes are not negative but “South”.

Figure 3: would it be possible to show an error bar (spread of data) for the observed data in the vertical profile (panel c)?

Figure 4: I do not see the shading (sc04 and sc06) but only output for the sc05 results. I think it would be very useful to provide the range here.

Figure 6: The text mention several times the importance of the high EDC emissions over Asia and the rapid transport due to the ASM, but then the figure highlights the influence during the Antarctic Spring. Wouldn’t it be nice to show also delta O3 values for the ASM region during July or August?