



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-1260-RC1>, 2023  
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## **Comment on egusphere-2022-1260**

Anonymous Referee #1

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Referee comment on "The future ozone trends in changing climate simulated with SOCOLv4" by Arseniy Karagodin-Doyennel et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-1260-RC1>, 2023

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Review for Karagodin-Doyennel et al., "The future ozone trends in changing climate simulated with SOCOLv4", submitted to ACP

Here authors use the SOCOLv4 Earth System Model (ESM) to simulate future ozone evolution. Results from two model simulations (based on two potential Shared Socioeconomic Pathways (SSP) scenarios: SSP2-4.5 and SSP5-8.5) are presented. SOCOL\_v4 predicts a decline in tropospheric ozone around the 2030s for SSP2-4.5 and after 2060s under SSP5-8.5 scenarios primarily due to decrease in ozone precursors such as NO<sub>x</sub> and CO. As expected, models also predict ozone increase in the upper/middle stratosphere globally and high-latitude lower stratosphere. Model also predicts that under SSP5-8.5, the stratospheric ozone increases are largely due to stronger stratospheric cooling as more greenhouse gases lead to more cooling that slows ozone loss. On the other hand, both the simulations predict ozone decrease in the tropical lower stratosphere as strengthening of BD circulation transports more ozone to the mid-high latitudes.

As confirmed by various recent studies, the model does not predict any decrease in lower stratospheric ozone at mid-latitudes. Consistent with our understanding, SOCOLv4 predicts an increase in stratospheric ozone in the 21st century due to decreases in the ODS and increases in the GHGs.

Overall, this is a well organised manuscript with some room for improvements. So I would recommend the manuscript for the publications if authors can address minor comments suggested below

- I am struggling to find clear motivation. What are the largest sources of uncertainties in our understanding about the future evolution of ozone layer . What was done in earlier

SOCOL studies and what was missing and how this manuscript is able to improve those biases. There are several papers like Morganstern et al., Dhomse et al., Keeble et al, highlighting the role of GHG in explaining decreases in tropical ozone. So, what is new here?

- Why there is a paragraph about the VLSL if there are no plots showing its impact on the ozone layer?
- Can you explain why sAOD term is included in DLM model though there is no volcanic eruption in the simulations
- Line 198: Usage of DLM to model simulated data is still unclear. Please include some clear information explaining why this type of analysis should use DLM rather than multivariate (or ordinary least square) regression or simpler composite analysis. I strongly feel that using DLM for the analysis of observational data is OK as we have just one realisation about the past atmosphere. But as you have 3 ensemble members for each type of simulation, does DLM provide unique insight in model world compared to simple averaging and smoothing?

Technical

Line 115: Hu et al. (2015),

Line 163: Only GHGs (prescribed ODSs are identical)?