Reviewer comments on Hou et al., "Future tropospheric ozone budget and distribution over East Asia under a Net Zero scenario"

This study presents an assessment of the impact of net zero emission policies on tropospheric and surface ozone concentrations over east Asia using the Community Earth System Model (CESM). Changes in ozone are simulated between 2015 and 2060 using the SSP119, with emissions over eastern China from the Ambitious pollution-Neutral-goals scenario from the Dynamic Projection model. A tagging method is used to analyse the impact on ozone concentrations from different regional and sectoral source of NOx. Net zero policies are shown to reduce both the tropospheric ozone burden and surface ozone concentrations in the future, with small increases due to stratospheric exchange and climate change. Local NOx emission sources show a large reduction in the contribution to surface ozone concentrations in the future with contribution from biogenic sources increasing. The study shows the importance of future emission reductions to decreasing ozone concentrations and the importance of net-zero policies.

I found this paper well written with some interesting results, particularly the tagging, that cover an important area of policy. I think the manuscript is suitable for publication once the following comments have been addressed.

Major Comments

- At various points in the manuscript (introduction and results) more reference could be made to more recent CMIP6 studies e.g. Zanis et al., (2022) and Allen et al., (2020). See minor comments for more details.
- I think some more comments about the influence of climate change on the results would be useful. Firstly, there is no mention of what climate change signal is simulated by this model in the future e.g. what is the future temperature change globally and regionally over East Asia by 2060 in this future scenario? This would allow some context to be placed on the climate change signal in relation to the results of other studies e.g. Zanis et al., (2022). Also, it would be good for the author to comment on whether a simulation period of 15 years is long enough to be able to simulate a climate change signal in comparison to internal variability, particularly in a future pathway with a smaller assumed signal. Given that this is a single model study there is also likely to be larger differences between models in the simulation of the future climate change signal. Furthermore, is there an impact from using the different model configurations when calculating the impact of climate change on the results? A lot of mention is made of climate change influencing STE and ozone concentrations. Is this the only important mechanism of climate change impacts on ozone, what about temperature effects on chemical reaction rates?
- I think when talking about the results of the tagged ozone simulations it would be good to emphasise that the results are only due to changes in NOx and not other ozone precursors such as CO, VOC and CH4. In the results section of the manuscript, it could be interpretated that these future changes in NOx from different sources are the only influence on future ozone concentrations. It would be useful in the manuscript if more comment is made about the impact of changes in CO, VOCs and CH4 on ozone concentrations in future.
- The tropospheric ozone evaluation section could be expanded to include some of the reasons that the model might not reproduce the observations of ozone in both the troposphere and at the surface. Also is there a reason, apart from better meteorology, that the offline simulation is better able to reproduce observations than the online simulations (line 250)?

Furthermore, can the authors comment on the impact that model biases could have on the projection of future ozone concentrations in this study (see Liu et al., (2022b) for methods to correct biases)?

- The results presented here are from using a single model to simulate future changes in ozone concentrations. Given the range of projections made in previous multi-model assessment could more comment be made in the discussion on the impact of only using output from a single model on the simulated changes in future ozone concentrations?
- It would be good to provide more comment and discussion on whether the scale of the emission reductions are considered feasible in the SSP119 pathway, especially if they are shown to return tropospheric ozone burdens back down to pre-industrial levels? As part of this it would be good to place these results in more context with those of the more studied pessimistic pathway of SSP370, particularly the influence of climate change.

Minor Comments

Line 24 – need to highlight here (and other places) that the tagging results are only for NOx sources

Line 36 – could mention that these effects are important at the surface

Line 40 – shorter timescales than long-lived greenhouse gases

Line 57 – not just dependent on climate mitigation measures but air pollution policies

Line 66 – development storylines or socio-economic pathways? Also mention anthropogenic radiative forcing

Line 76 – what scenario are these increases for?

Line 91-92 – Can you say what has led to the large differences between the results of these studies?

Line 147 – You are using the 2015 ssp119 as present day emissions for 2015? How representative are these compared to other inventories for 2015 e.g. MEIC, especially when using the DPEC for future scenarios over China.

Line 159-160 – Related to the point above. How different are the DPEC emissions over China compared to SSP119 and why are they different? What is the added advantage of using this regional emission inventory compared to the global one?

Line 179 – Is it worth showing the aerosol precursor emission changes (SO2, BC, OC) in Table 2 if they do not directly impact ozone concentrations and the main message of the paper?

Line 186 – why only tagging NOx and not VOCs?

Line 225 – Could figure 2 also include a different plot to highlight the overestimation?

Line 265-267 – need to look at CMIP6 reference of Zanis et al., (2022)

Line 269-271 – why is the decrease in photochemical production less over East Asia than global?

Line 271-272 – can you define what you mean here by net outflow region? Is this ozone produced over East Asia is more favourably exported to other regions?

Line 278 – In Table 4 you could also include up to date numbers from the CMIP6 study by from Griffiths et al., (2021) https://doi.org/10.5194/acp-21-4187-2021

Line 306-309 – It would be good to show this comparison of emission inventories for both present day and future in section 2

Line 311-313 – It would be good to make more comparisons to Zanis et al., (2022) which shows a consistent increase over East Asia in these multi-model responses whereas here the difference shows a reduction in JJA over Eastern China due to climate change. Can you explain the differences here or is it due to a smaller climate change signal? Also see major comment on this.

Line 313-314 – Linked to the above. Is this all due to the STE increases? What about other effects e.g. temperature on surface O3 response. This could be expanded to include more discussion on other impacts.

Line 327-328 - is this changes in the tropopause height significant as hard to see on the figure?

Line 338-341 – Could this be expanded to say the decrease in anthropogenic sources shifts the seasonal cycle from summer towards spring, which is more dominated by STE?

Line 349-352 – Reference could be made Liu et al., (2022a) showing similar seasonal effects in another model (https://doi.org/10.5194/acp-22-1209-2022) and also a preprint by the same author studying Net Zero policies over China (https://doi.org/10.5194/egusphere-2023-230).

Line 355 – Figure 6 needs to be clearer here on what the bottom panel of figure c and d is and why is this included next to NOx concentrations? Could make NOx separate panels?

Line 361 – Could more be explained on what the dominant biogenic sources of NOx are? Also would biogenic sources of VOCs not be more important in future conditions for ozone formation?

Line 365 - Is the biogenic source enhanced of has the relative contribution (as anthropogenic reduced) increased?

Line 367 – Linked to the above point. Do the biogenic sources change between the present day and future scenarios?

Line 371 – Could the percentage change be linked to the actual ppbv change in the rest of this section like it is done here?

Line 359-374 – Could a comparison also be made of these to local vs external contributions to those in Fig 5 of Turnock et al., (2019)?

Line 377 – Can more detail be included in the caption of Figure 7 to say what simulations these results have been derived from and also to make clear that these changes are only due to NOx.

Line 389-391 – Is there a substantial change in the external sources to China in the future SSP119 pathway and does this influence ozone concentrations in East Asia?

Line 392 – Could more be made in the conclusion section to try and link the results to the impact on air quality and health?

Line 426-427 – Need to make sure this is clear that it is biogenic NOx sources considered here and also that they are not changing in this study.

Line 427-430 – I found this to be quite a broad statement that net zero policies are sufficient to mitigate surface ozone pollution over East Asia, especially in summer. Does this mean that there won't be any issues from ozone in summertime in the future under this scenario?

References

Allen, R. J., Turnock, S., Nabat, P., Neubauer, D., Lohmann, U., Olivié, D., et al. (2020). Climate and air quality impacts due to mitigation of non-methane near-term climate forcers. Atmospheric Chemistry and Physics, 20(16), 9641–9663. https://doi.org/10.5194/acp-20-9641-2020

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