Response to Reviewer 01:

This study evaluates the variation in tropospheric ozone levels, trends, photochemical regimes, and radiative effects using the global chemistry-climate model ECHAM6–HAMMOZ and satellite data from 1998 to 2019. It examines how anthropogenic emissions of nitrogen oxides and volatile organic compounds affect ozone production. The global trend in tropospheric ozone is increasing, with simulations showing strong agreement with satellite data, which is in accordance with previous studies. The study also explores how changes in pollution emissions impact ozone trends and photochemical regimes. Doubling emissions of nitrogen oxides and volatile organic compounds leads to different ozone trends compared to halving these emissions, with region-specific responses observed in different parts of the world.

The manuscript explores an important and timely topic regarding the climate and health impacts of tropospheric ozone, which holds great relevance. To further enhance the clarity and impact of the work, however, it is recommended revisiting some language elements to improve readability. Additionally, some of the figures would benefit from improvements in quality; they appear somewhat small, with low resolution, and the axis labels and legends could be more legible.

Response: We sincerely thank the reviewer for the meticulous review, constructive comments, and valuable suggestions, which have significantly enhanced the quality of the manuscript. As suggested by the reviewer, we have carefully addressed all suggestions and incorporated them. Additionally, we have corrected grammatical and typographical errors throughout the manuscript to ensure clarity and readability. Figures have been modified to improve their readability. We appreciate the reviewer's time and effort in improving our work. Changes are indicated in track mode version of the manuscript at a line number indicated in the replies.

Content-wise, there are several key points that also need to be revised to bring the manuscript to publication standard. Addressing those specifically will be essential to enhance the clarity and impact of the results:

1. The introduction lacks structure. Model and observational data from the literature are presented in a mixed way, as are global and regional findings. Chemical symbols such as NOx, NOy, and VOC need to be defined or explained right from the beginning. In addition, the abbreviations used for the sensitivity experiments are not well chosen, as they could be mistaken for names of chemical species.

   Response: We appreciate the reviewer's suggestions. We have revised and reorganized the introduction to improve its overall structure and clarity. Additionally, we have defined chemical symbols at their first appearance to enhance readability and prevent ambiguity. To avoid confusion, we have also refined the abbreviations used for sensitivity experiments as (2) doubling anthropogenic emission of NOx globally (DoubNOx), (3) reducing anthropogenic emissions of NOx by 50 % globally (HalfNOx), (4) doubling anthropogenic emissions of all VOCs globally (DoubVOC), (5) reducing anthropogenic emissions of all VOCs by 50 % globally (HalfVOC). (See L252-258 in section 2.5).

2. The manuscript does not specify which emission scenario from ACCMIP (Representative Concentration Pathway - RCP) is used. A rationale or justification for the selection of sensitivity experiments regarding NOx and VOC emissions is currently missing, and providing this context would strengthen the study's approach. Are there real-world examples for this? What exactly is being investigated beyond the well-known fact that these are the primary drivers of tropospheric ozone?

   Response: We appreciate the reviewer's concern and clarify that our model simulations use the RCP 8.5 high-emission scenario from ACCMIP (Van Vuuren et al., 2011), chosen for its relevance in representing strong anthropogenic impact. The ACCMIP inventory includes emissions from multiple sectors, making it a robust dataset for evaluating atmospheric composition changes. This is already mentioned in the manuscript [Line No:257-258].

   Our sensitivity experiments, doubling and halving global NOx and VOC emissions aim to quantify ozone, ozone photochemical regime and radiative forcing changes due to anthropogenic emission changes rather than just reaffirm their known role. This approach of increase/decrease of emissions is important for nonlinear response of ozone to emissions. These experiments are helpful for designing emission implementation strategies (e.g., Zhang et al., 2021; Wang et al., 2023). These points have been incorporated into the revised introduction for clarity and context (L146-149).

3. It would be important to differentiate regionally more when describing the relationships between NOx and VOC development. In addition, a more detailed discussion of the effects of different VOC species would also be helpful. And, it is worth considering whether natural VOC emissions in different geographical regions might play a significant role. Despite their considerable contribution natural VOCs are not addressed at all.

   Response: Thank you for your valuable comment. We acknowledge the importance of regional differentiation and the role of natural VOC emissions in ozone formation. However, our study focuses primarily on the impact of anthropogenic NOx and VOC emissions on global ozone trends and photochemical regimes. We believe that adding additional analysis of natural VOCs will lose the focus of the manuscript and make it very lengthy. Therefore we think that this should be the focus of a separate study where the ozone sensitivity to natural VOCs is investigated in detail.

4. It is well-known that temperature and humidity have a significant impact on the life cycle of ozone. The most obvious explanation for the positive ozone trend - climate warming - is not discussed in this study, which is a major shortcoming.

   Response: We acknowledge the reviewer's concern regarding the role of climate warming in tropospheric ozone changes. However, our study primarily focuses on assessing the impact of anthropogenic emission changes on tropospheric ozone over the 21-year period from 1998 to 2019. Given this relatively short time frame, the direct impact of climate change on ozone levels is expected to be minimal.

   According to the IPCC AR6, the rate of global temperature increase during our short study period is approximately 0.3 to 0.4°C. As highlighted by Zanis et al. (2022), an ozone climate penalty, where higher temperatures contribute to ozone increases, typically emerges only after global temperatures rise by 2–3°C. Even then, this penalty is observed primarily at the surface in high-emission regions. Furthermore, climate change-induced increases in water vapour generally reduce ozone lifetime in remote regions, leading to net ozone reductions rather than increases.

   While we recognize the importance of climate change in shaping future ozone distributions, our study is focused on identifying the more immediate and dominant driver, that is, changes in anthropogenic emissions over the past two decades, and their impact on ozone photochemical regimes and trends. The conclusion section has been revised in the manuscript to include the above points (L1152-1168).

   Zanis, P., Akritidis, D., Turnock, S., Naik, V., Szopa, S., Georgoulias, A.K., Bauer, S.E., Deushi, M., Horowitz, L.W., Keeble, J. and Le Sager, P., 2022. Climate change penalty and benefit on surface ozone: a global perspective based on CMIP6 earth system models. Environmental Research Letters, 17(2), p.024014.

5. The conclusions section is more of a listed summary of findings rather than a true conclusion. What would be the interpretation of your findings, for example regarding current and future mitigation measures in a warming climate and changing natural sources?

   Response: We appreciate the reviewer's suggestion. We included a summary of the results as we believe it is important to mention the key points of our assessment of trends, photochemical regimes, and radiative effects. In addition to this, we have now also revised the Conclusions section to include a discussion paragraph that provides further interpretation of our results. This discussion elaborates on the implications of our findings regarding current and future mitigation strategies in the context of a warming climate and evolving emission sources.

Response to Reviewer 02:

The manuscript by Fadnavis et al. presents an analysis of transient simulations of ozone over 1998 – 2019 simulated by the ECHAM6-HAMMOZ model. To assess the simulations, the authors compare absolute amounts and trends for tropospheric column amounts of ozone, NO2 and HCHO with a number of satellite observations and find reasonable agreement. The analysis focuses on a set of four sensitivity experiments where the emissions from anthropogenic sources of either NOx or VOCs are alternatively increased by 100% or decreased by 50%. The response of simulated surface ozone and tropospheric column from these four sensitivity experiments are used to calculate whether photochemical generation of ozone in different regions around the globe is limited by the availability of NOx or VOCs, and the derived sensitivity is then used to calibrate the transition between NOx and VOC sensitivity given by the widely used ratio of HCHO/NO2. The authors also present estimates of the radiative effect of tropospheric ozone (TO3RE) and the changes in TO3RE across the different sensitivity simulations.

Given the horizontal resolution of typical global chemistry climate models I would expect significant limitations in being able to resolve the full spectrum of NOx and VOC sensitivity that would occur in a particular region. While that is a limitation that will affect this work, it is also true that chemistry climate models are widely used to understand interactions of climate change and air quality, and project future regional or global scale concentrations of ozone. On that consideration I find the research presented here to be an interesting addition.

Response: We thank the reviewer for appreciating our work and useful suggestions. We have incorporated all the suggestions given by the reviewer. Changes are indicated in track mode version of the manuscript at a line number indicated in the replies.

My most significant concern is the focus on deriving NOx and VOC sensitivity using, what I must assume are, annual average fields of ozone and the change in ozone in response to imposed emission changes. Given how different the response to NOx and VOC changes would be between summer and winter, why were the results not broken down by season? The winter season at mid-latitudes is a period with very weak photochemical activity and very limited ozone photochemical production. A significant part of the chemistry in the winter at mid-latitudes is dominated by ozone titration near regions with strong emissions of NOx, followed by NOx oxidation and removal. I am not completely sure how one derives NOx and VOC sensitivity under conditions with only very weak local ozone photochemical production. Given how weak ozone photochemical production is in the winter the analysis of annual averages really dilutes and, arguably, confounds the separation into NOx or VOC limited regimes. I believe the authors must rework the analysis to separate the summer season in each hemisphere from the full year.

Response: Thank you for this point. We do agree that ozone photochemical regimes exhibit distinct seasonal variations, particularly due to differences in photochemical activity between summer and winter. Therefore, the seasonal variation of photochemical regimes is already discussed in section 5. Also, we have derived seasonal FNR threshold and listed in Table -4.

In addition, Section 5 provides more insight on the seasonal variations, with a particular focus on urban and semi-urban regions identified in Figure 9. We wish to clarify that in our analysis we have also separated the summer/winter season in each hemisphere (see section 5).

While not as central to the main findings of the paper, for the comparison of HCHO and, especially, NO2 vertical columns with satellite observations, was time of day accounted for? The Boersma et al. (2016) paper mentioned in the manuscript does investigate different strategies for comparing models and measurements of total column amounts from UV-Vis instruments, but all the different cases investigated in Boersma et al. included matching time of day. From the text it would appear that full-day averages were used from the model to compare with the satellite observations.

Response: Thank you for this point. We note that TROPOMI/OMI monthly means are valid for clear-sky situations, whereas the model simulations are all-day all-sky averages. In previous studies (Boersma et al. (2016) and references therein), it was shown that NO_2 is typically 15 – 20 % lower on clear-sky days than under cloudy situations due to higher photolysis rates, and faster chemical loss of NO_2 (L407-411).

As mentioned in the manuscript at line L409-411, we sampled 24-hour mean ECHAM-HAMMOZ tropospheric NO2 and HCHO columns. We agree that it would have been better to sample ECHAM-HAMMOZ tropospheric columns close to the OMI and TROPOMO local overpass time of 13:30 hrs, and for clear-sky situations only, therefore ensuring that the comparison of modelled and satellite observed tropospheric NO2 and HCHO columns in section 3.1 is free of representativeness errors. To acknowledge that our comparison does suffer from such representativeness errors, we now use the Boersma et al. (2016) results to provide an estimate on the penalty of not sampling ECHAM-HAMMOZ for clear-sky only.

Boersma K. F., Vinken G. C. M., and Eskes H. J., Representativeness errors in comparing chemistry transport and chemistry climate models with satellite UV–Vis tropospheric column retrievals, Geosci. Model Dev., 9, 875–898, 2016, doi:10.5194/gmd-9-875-2016

Additional minor comments are given below.

Lines 39 – 41: 'The global mean simulated trend in Tropospheric Column Ozone (TRCO) during 1998 – 2019 is 0.89 ppb decade−1. The simulated global mean TRCO trends (1.58 ppb decade−1) show fair…' The text here is confusing because there are two near identical references to the TRCO trend but with two different values given. Reading on a bit, I assume the second trend (1.58 ppb per decade) is for the 2005 – 2019 period of the OMI/MLS observations but the text should be clearer.

Response: thank you for pointing this out. We have now modified the sentence in the revised manuscript to clearly distinguish between the full study period (1998–2019) trend and the coincident period with OMI/MLS observations (2005–2019) L40-41

Lines 54 – 55: 'The impact of anthropogenic NOx emissions is higher on TO3RE than VOCs emissions globally.'. Given how different VOCs and NOx are I am not sure how one can interpret this statement. Is it per unit mass of emissions or a fractional perturbation? And, given how non-linear the chemistry is, I would worry about comparing the effects of NOx and VOCs when the perturbations are as large as they are here.

Response: The above sentence is re-written as "Our simulations show that emissions changes in anthropogenic NOx cause higher changes in TO3RE than anthropogenic VOCs (L56-57)

Lines 62 – 63: 'short-term climate forcer', I think, is more widely accepted as 'short-lived climate forcer'.

Response: While both terms have been used, we decided to adopt the terminology used in the IPCC reports, namely 'Short-lived Climate Forcers'

Line 80: To avoid confusion IAGOS should really be 'In-service Aircraft for a Global Observing Network'

Response: Thank you. Corrected in the revised manuscript [L83].

Lines 98 – 100: The reference to Archibald et al. (2020) is to the TOAR paper on tropospheric ozone budget and burden, but the text supported by the reference discusses only UKESM1 results. Should the reference be to Archibald et al. Description and evaluation of the UKCA stratosphere–troposphere chemistry scheme (StratTrop vn 1.0) implemented in UKESM1. Geosci Model Dev 13: 1223–1266?

Response: Thank you for pointing this out. However, these lines are deleted as per suggestion of Community comments.

Line 117 – 118: I am not sure it is accurate to suggest that in a NOx-limited regime there is 'no impact from VOC perturbations'. Only that the ozone production is more sensitive to changes in the concentration NOx than for changes in VOCs.

Response: Thank you for pointing this out. It is now modified as below.

"The regime is called NOx-limited if the ozone production is directly related to a change in NOx, rather than from VOC perturbations." L122-123.

Line 518: The year 1955 in 'during 1955–2017 over South and East Asia' should be 1995.

Response: Thank you for pointing this out. Correction is made in the revised manuscript. L529

Lines 572 – 573: 'The surface ozone trend shows a large negative trend over Europe and South Asia, while a positive trend over the US, China, and Australia (Fig. 5g).' It is not always clear if the trends in the sensitivity experiments, shown in Figure 5, are the trends in the experiment or the difference in the trend between the sensitivity experiment and the control. Phrases like lines 572 – 573 seem to suggest it is the trend calculated directly from the sensitivity experiment which can be compared with the trend in the control. But other places, such as the titles on the panels of Figure 5, the text seems to imply it is the difference in trends that is plotted and discussed.

Response: Sorry for the confusion. The anomalies are computed as CTL - DoubNOx, CTL - DoubVOC, etc. Then trends are estimated using the anomalies. It is now clarified in the revised manuscript in section 3.3. for example, see L556-557. Figure 5 c–d shows trend in surface ozone and TRCO estimated from anomalies obtained from DoubNOx - CTL simulations.

Lines 577 – 579: 'Similarly, the trend from anomalies of NOx in the HNOx-CTL simulations is positive over the US and Europe while negative over India and China (Fig. S1a-b).' Is this because in the control simulation NOx emissions are decreasing in the US and Europe, but increasing in India and China? Everything else remaining the same, decreasing NOx emissions by 50% will have produced a weaker trend, but of the same sign as the underlying trend. But I am not completely in agreement with the argument that is being made here. The trend of NOx emissions in the HNOx simulation would still be negative over the US and Europe, just not as negative as in the control simulation. And the resulting trend in ozone may have been negative as well, only not as negative as in the control simulation. So it is not quite an accurate representation of the situation to state (Lines 579 -580) 'The strong positive trend in both VOC and NOx might have resulted in the observed positive trend in surface ozone over the US…' because there is not necessarily a positive trend in surface ozone in the HNOx simulation, but a less negative trend. This confusion connects with the comment on Lines 572 – 573, that it is not clear what quantities are being shown in Figure 5 and discussed here.

Response: Thank you for your insightful comment. We acknowledge the need for a clearer interpretation of the NOx trends in the HalfNOx-CTL simulation. To improve clarity, we have revised the paragraph in the manuscript to ensure that the response of ozone to imposed NOx and VOC changes is properly conveyed.

Lines 656 – 662: The box and whisker plot shows the range of emissions over different regions. Are these emission fluxes at the grid resolution of ECHAM or from a higher resolution dataset such as the original ACCMIP dataset? The figure caption should state this.

Response: The emission fluxes are at the grid resolution of the ECHAM model. This is stated in the figure caption of the revised manuscript (L580-608).

Lines 770 – 771: I am having trouble seeing how the definition of NOx limited is formulated correctly with d[O3]/dENOx in 'for the conditions d[O3]/dENOx < 0) (NOx limited) and (d[O3]/dENOx > d[O3]/dEVOC > 0) (VOC–limited) (Fig. 8b)'. For the NOx limited case, an increase in NOx emissions should increase ozone, dENOx> 0 results in d[O3] > 0 giving d[O3]/dENOx >0. Likewise, for NOx limited conditions, a decrease in NOx emissions (dENOx<0) should produce a decrease in ozone (d[O3]<0) so that d[O3]/dENOx > 0 as well. Reading a bit more, I see the cases are reversed in the Figure 8 caption, which may be the problem here.

Response: Thank you for pointing this out. We acknowledge the inconsistency and have corrected the text at L 772-774 as below calculating cumulative probability from this data for the conditions $d[O_3]/dE_{NOx} < 0$) (VOC limited) and ($d[O_3]/dE_{NOx} > d[O_3]/dE_{VOC} > 0$) (NOx–limited) (Fig. 8b), where $d[O_3]/dE$ represents the change in ozone corresponding to a change in emission of either NOx or VOCs.

Lines 971 – 974: The caption for Figure 12 needs a brief description of what each panel is presenting.

Response: Thank you for pointing this out. As suggested by the reviewer, we have added a brief description in the figure caption to clearly explain the content of each panel in the revised manuscript.

Lines 1054 – 1055: Can the statement 'The minor differences in the estimated global mean TO3RE from the model and satellites are due to different time periods of observations/simulations.' be supported? The TO3RE for 1998 – 2019 is 1.21 W/m^2, which is very close to the observational estimates. I would think moving towards the period of the satellite observations (2008 – 2017) would result in a larger value of TO3RE since TRCO has a positive trend over the 1998 – 2019 period.

Response: This sentence is rewritten as "The values reported by Pope et al. (2024) are comparable with our CTL simulation (e.g. IASI–FORLI: 1.23 W m^{-2}, IASI–SOFRID: 1.21 W m^{-2}, IASI–IMS: 1.21 W m^{-2}, ECHAM6: 1.22 W m^{-2})." L1054-1056.

Replies to comments by Owen R. Cooper (TOAR Scientific Coordinator of the Community Special Issue) on:

Influence of nitrogen oxides and volatile organic compounds emission changes on tropospheric ozone variability, trends and radiative effect

Suvarna Fadnavis, Yasin Elshorbany, Jerald Ziemke, Brice Barret, Alexandru Rap, PR Satheesh Chandran, Richard J. Pope, Vijay Sagar, Domenico Taraborrelli, Eric Le Flochmoen, Juan Cuesta, Catherine Wespes, Folkert Boersma, Isolde Glissenaar, Isabelle De Smedt, Michel Van Roozendael, Hervé Petetin, Isidora Anglou

This review is by Owen Cooper, TOAR Scientific Coordinator of the TOAR-II Community Special Issue. I, or a member of the TOAR-II Steering Committee, will post comments on all papers submitted to the TOAR-II Community Special Issue, which is an inter-journal special issue accommodating submissions to six Copernicus journals: ACP (lead journal), AMT, GMD, ESSD, ASCMO and BG. The primary purpose of these reviews is to identify any discrepancies across the TOAR-II submissions, and to allow the author teams time to address the discrepancies. Additional comments may be included with the reviews. While O. Cooper and members of the TOAR Steering Committee may post open comments on papers submitted to the TOAR-II Community Special Issue, they are not involved with the decision to accept or reject a paper for publication, which is entirely handled by the journal's editorial team.

Comments regarding TOAR-II guidelines:

TOAR-II has produced two guidance documents to help authors develop their manuscripts so that results can be consistently compared across the wide range of studies that will be written for the TOARII Community Special Issue. Both guidance documents can be found on the TOAR-II webpage: https://igacproject.org/activities/TOAR/TOAR-II

The TOAR-II Community Special Issue Guidelines: In the spirit of collaboration and to allow TOAR-II findings to be directly comparable across publications, the TOAR-II Steering Committee has issued this set of guidelines regarding style, units, plotting scales, regional and tropospheric column comparisons, and tropopause definitions.

The TOAR-II Recommendations for Statistical Analyses: The aim of this guidance note is to provide recommendations on best statistical practices and to ensure consistent communication of statistical analysis and associated uncertainty across TOAR publications. The scope includes approaches for reporting trends, a discussion of strengths and weaknesses of commonly used techniques, and calibrated language for the communication of uncertainty. Table 3 of the TOAR-II statistical guidelines provides calibrated language for describing trends and uncertainty, similar to the approach of IPCC, which allows trends to be discussed without having to use the problematic expression, “statistically significant”.

Response: We sincerely thank the reviewer for the meticulous review, constructive comments, and valuable suggestions, which have significantly enhanced the quality of the manuscript. As suggested by the reviewer, we have carefully addressed all suggestions and incorporated the necessary revisions. We appreciate the reviewer’s time and effort in improving our work.

General comments:

In the list of authors, please check the spelling and affiliation of co-author Eric Le Flochmoen

Response: Spelling and Affiliation is corrected in the revised manuscript [L5].

“America” is a vague term. Please specify if you are talking about North America, Central America, or South America, or a sub-region.

Response: The term ‘America’ is replaced with ‘North America’ in the revised manuscript [L48].

Line 80 “Global Observing System” should be, “In-service Aircraft for a Global Observing System”

Response: Corrected in the revised manuscript [L83].

Line 83 Given that the IAGOS record only extends back in time to 1994, Fiore et al. 2022 could only base their assessment of long-term trends (1950-2014) on the model simulations, and a few limited surface ozone records.

Response: This sentence is corrected in the revised manuscript [L87].

Lines 87-90 With only 5 years of OMI/MLS data available, Cooper et al. (2014) did not assess trends over such a short time period. But they did assess the average tropospheric ozone burden by latitude.

Response: This sentence is corrected in the revised manuscript [L92].

Line 97-111 Global ozone trends were sufficiently summarized at the beginning of the Introduction, based on the findings of IPCC AR6. This particular paragraph then repeats some of the IPCC findings by citing some of the same papers summarized by IPCC. It also cites the trends reported by Cooper et al. 2014, which are now out of date. The discussion in this paragraph is also fairly disorganized. As the paper is already quite long, I recommend that this paragraph be deleted.

Response: This paragraph is removed from the revised manuscript.

Lines 132-134 The most up-to-date estimates of ozone ERF are provided by Forster et al. 2021 and Forster et al. 2024, so why report the out-of-date findings by Myhre et al. 2013 and Skeie et al., 2020?

Response: The lines quoting the results of Myhre et al. 2013 and Skeie et al., 2020 are removed in the revised manuscript.

Line 137 It would be more accurate to say that your analysis addressed ozone's radiative effect, rather than radiative forcing. Lu et al. (2024) just submitted a paper to the TOAR-II Community Special Issue, and it should be available for the open comment period very soon. This paper is relevant to your study as it uses models to understand the drivers of increasing ozone across East and Southeast Asia. The reference is listed below. Another recent submission to the TOAR-II Community Special Issue that is relevant to your analysis of ozone's radiative effect is Collins et al. (2024).

Response: The term 'Radiative forcing' is replaced with 'radiative effect' in the revised manuscript [L145].

Figure 3 This is a very interesting figure, but it is ignoring ozone changes over the oceans. Why leave out the atmosphere that lies above the oceans, which cover 2/3 of the surface of the Earth? I think most readers would like to see what happens to ozone downwind of East Asia, for example, and these oceanic regions should be shown, as has been done for Figure 4. I have similar comments regarding Figure 5.

Response: As suggested by the reviewer, the Ocean mask in Figures 03 and 05 are removed in the revised manuscript.

At the time of this writing, one of the anonymous referees has posted a set of comments, which are thorough and constructive. However, I have to disagree with this comment: "The most obvious explanation for the positive ozone trend - climate warming - is not discussed in this study, which is a major shortcoming." Fadnavis et al. only assessed trends over a short 21-year period (1998-2021), and according to the global temperature rate of increase assessed by IPCC

AR6, this corresponds to a relatively small temperature increase of about 0.3 to 0.4 C. As shown by Zanis et al. (2022), an ozone climate penalty doesn't emerge until global temperatures increase by 2-3 C, and even then its only at the surface in high emissions regions. Given that higher temperatures increase the water vapor content of the atmosphere, which reduces ozone's lifetime, the main impact of climate change is to reduce ozone in remote regions.

Response: We appreciate the reviewer's thorough assessment and valuable insights regarding the role of climate warming in ozone trends. Their careful evaluation has helped refine our discussion, and we have incorporated these points into the revised discussion section.

References

Collins, W. J., O'Connor, F. M., Barker, C. R., Byrom, R. E., Eastham, S. D., Hodnebrog, Ø., Jöckel, P., Marais, E. A., Mertens, M., Myhre, G., Nützel, M., Olivié, D., Bieltvedt Skeie, R., Stecher, L., Horowitz, L. W., Naik, V., Faluvegi, G., Im, U., Murray, L. T., Shindell, D., Tsigaridis, K., Abraham, N. L., and Keeble, J.: Climate Forcing due to Future Ozone Changes: An intercomparison of metrics and methods, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-3698, 2024.

Lu, Xiao, et al. (2024), Tropospheric ozone trends and attributions over East and Southeast Asia in 1995- 2019: An integrated assessment using statistical methods, machine learning models, and multiple chemical transport models, submitted to ACP

Zanis, P., Akritidis, D., Turnock, S., Naik, V., Szopa, S., Georgoulias, A.K., Bauer, S.E., Deushi, M., Horowitz, L.W., Keeble, J. and Le Sager, P., 2022. Climate change penalty and benefit on surface ozone: a global perspective based on CMIP6 earth system models. Environmental Research Letters, 17(2), p.024014.