The manuscript entitled "*Large Reductions in Satellite-Derived and Modelled European Lower Tropospheric Ozone During and After the COVID-19 Pandemic (2020–2022)*" presents satellite records of European tropospheric O3 from pre-COVID period to mid-2023 (post-COVID). In addition to providing evidence of O3 reduction during the COVID-19 pandemic – consistent with findings from previous studies – the authors report an even larger reduction in O3 in 2022, after the pandemic. This finding could hold scientific importance if further investigation into its underlying causes is conducted. Alongside the analysis of satellite products, the authors utilized a chemical transport model to separate the contributions of emissions and meteorology to O_3 reduction from 2020 to 2021. Their results suggest that meteorological factors, particularly stratosphere-troposphere exchange, play a crucial role in O_3 reduction. However, I find that the conclusions drawn in this section lack robustness due to the following reasons: 1. There is no O_3 budget analysis, which is necessary for a comprehensive evaluation of contributing factors. 2. A more thorough discussion of uncertainties related to the scaling factor, model resolution, and other relevant aspects is needed to enhance the scientific rigor of the study. Overall, the manuscript may be suitable for publication after major revision.

General comments:

1. Depth of scientific analysis: The current manuscript primarily presents data analysis and modeling results but does not sufficiently explore the underlying scientific implications. The discussion/conclusion sections are brief and lack a deeper interpretation of the findings. I recommend expanding these sections to provide more context, including possible explanations, comparisons with previous studies, and insights into the broader scientific relevance of the results. For example, the finding that the largest O3 reduction occurred in 2022, after the COVID-19 period, could hold scientific significance if potential explanations for this trend were explored in greater depth.

2. Uncertainty analysis and discussion: The analysis presented in this study generally lacks an assessment of uncertainty, which is crucial for interpreting and validating the results. Including uncertainty analysis in Section 3 and discussing limitations in the conclusion would enhance the robustness of the findings. Currently, the conclusion drawn from the modeling analysis is limited due to absence of uncertainty analysis. I recommend a more thorough discussion on uncertainties related to the scaling factor used, model resolution, etc. These additions would significantly improve the scientific rigor of the study.

Specific comments:

1. Line 136: If the authors have done a literature review but still lack of an emission scaling factor for 2022, running a BAU case with 2022 meteorological data could provide

valuable modeling insights into the causes behind the observed large O3 reduction in 2022. This would significantly enhance the scientific impact of this study.

2. Line 156: The implementation of the STE tracer is somewhat difficult to understand. It seems unusual that the only chemical sinks, aside from photolysis, are reactions with HO2/OH and H2O through O1D produced from *the tracer itself*. Could you provide more explanation on how this tracer could represents the O3 budget or its contribution to tropospheric O3? Alternatively, citing previous studies that use a similar tracer implementation could help, too.

3. Fig2(a): In examining the sub-col O3 time series from the two satellite products, I noticed that, in addition to a generally lower values across the observation period – which the authors suggest might be due to instrument biases – GOME-2B appears to have a one-month phase compared to the IASI data. For instance, GOME-2B consistently shows a lower trough at the end of 2015, 2016, 2017, 2018, and 2019, whereas IASI shows this trough at the beginning of each respective year. Could you offer some potential reasons for this discrepancy?

4. Line 223 (Figure 3(b)): Given that the represent an average of three simulations, I suggest adding error bars to indicate the standard deviation in the plot alongside the averaged values. This would help illustrate the significance of the differences more effectively.

5. Line 251: "*These values represent the contribution of emission reduction.. and the corresponding contribution of meteorology*" – do you mean that the ratio represents the contribution of emission reduction, while (*1- ratio)* represents the contribution of meteorology? Please consider rephrasing this sentence to improve clarity and readability.

6. Line 260: As mentioned previously, the definition of STE is not very clear. Could you elaborate further on why it represents the contribution of O3 from STE? Specifically, can the STE tracer defined in this study be transferred as contribution of O3 from STE? For instance, Griffiths et al., 2020 validate their derived STE using O3 budget: STE = LO3+DO3-PO3. Could you adopt a similar method to demonstrate that the STE used in this paper aligns with this O3 budget equation? Additionally, I recommend including a detailed O3 budget analysis to provide a more robust and concrete evaluation of contributing factors.

7. Line 267: Concluding that other factors are likely neutral or even positive based solely on the STE tracer anomaly being larger than the O3 anomaly may not be very robust. An O3 budget analysis could provide a clearer understanding of the contributions from various

factors, such as chemical production, chemical losses and deposition losses, all of which are influenced by meteorological parameters and affect O3 concentrations.

References:

Griffiths, P. T., Keeble, J., Shin, Y. M., Abraham, N. L., Archibald, A. T., & Pyle, J. A. (2020). On the changing role of the stratosphere on the tropospheric ozone budget: 1979–2010. Geophysical Research Letters, 47, e2019GL086901. https://doi.org/10.1029/2019GL086901