Response to Reviewer 01:

Before publication, the box plots in Fig. 6, in which the NOx values are very small and difficult to read, and Fig. 11f, which will appear too small in the printed version, still need improvement.

Response: We sincerely thank the reviewer for the valuable suggestions. As suggested, Figure 6 has been modified to improve its readability in the revised manuscript.

The reviewer may kindly note that the annual mean FNR part (Figure 11) is removed as per the suggestions of the reviewer-II. Since the reviewer-II has suggested to remove discussion of annual FNR.

Response to Reviewer 02:

Having reviewed the original version of the paper, I will not begin the review with the usual brief synopsis of the work. I will, instead, focus on one of the criticisms of the original paper that is still of concern in the revised version. I am referring to the use of the response of annual average ozone to emission changes to derive the NOx and VOC sensitivity, which is then related to the annual average HCHO/NOx ratio. In Section 4 the authors state that their method for comparing the ozone response to emission changes and the HCHO/NO2 ratio follows that used by Jin et al. (2017). Looking at the Jin et al. paper I will admit that the original analysis did use data from both summer and winter seasons, but they also used once-daily data taken from between 1:00 and 2:00 PM local time to derive daily values of dO3/dE and daily NO2/HCHO ratio. Note that because GEOS-Chem is a chemical transport model with repeated meteorology for each experiment, it was possible to compare day-by-day for the base case and each emission perturbation experiment. Using this day-by-day comparison they are able to show the seasonal transition from NOx limited (warm season) and NOx saturated (cold season) conditions for model grid points close to a number of large cities (their Figures 8 – 10) and how the model derived NOx limited and NOx saturated conditions relate to the HCHO/NO2 ratio. While the Jin et al. analysis does use data from the entire year, the response of ozone to the emissions perturbation and the relationship of the ozone response to the HCHO/NOx ratio is derived from once-daily data, with the seasonal transition in ozone sensitivity captured by seasonal changes in the HCHO/NO2 ratio. My concerns with the analysis presented in the manuscript under consideration is that it seems that the changes in annual average ozone from the emission perturbation tests were compared with annual average HCHO/NO2 ratios to derive the thresholds for NOx and VOC sensitivity. This seems to be the case from the statement on Lines 730 – 733 of the revised manuscript: 'The procedure to obtain FNR involves two steps: (1) obtaining the annual and seasonal ozone response from emission sensitivity simulations (here, HalfNOx and HalfVOC simulations) by considering only the polluted cells over the study region and plotting it as a function of FNR (Fig. 8a),'

Looking at Figures 8 – 10 of Jin et al. shows the extent of the strong seasonal changes in NOx saturated and NOx limited conditions. While the relationship between O3 sensitivity and the HCHO/NOx ratio is assumed to be constant throughout the year, the Jin et al. analysis derives this relationship from near-instantaneous values sampled throughout the year. I have difficulty

understanding how relating the annual average response of ozone to NOx and VOC emission perturbations to the annual average HCHO/NOx ratio can be used to derive a relationship that correctly represents the strong seasonal variations in NOx and VOC sensitivity. It is entirely conceivable that for certain model grid points the HalfNOx experiment would have resulted in one change in ozone in the summer and a very different change in ozone in the winter (the Jin et al. results show exactly this), yet these responses would have been averaged together to derive an annual average ozone response to the NOx emission decrease. The HCHO/NOx ratio also may have a fundamentally different nature in the summer and winter seasons because of many reasons, including increased HCHO from biogenic hydrocarbon oxidation during the summer.I appreciate that the authors do present the HCHO/NOx indicator for different seasons, but I have serious doubts about the validity of deriving relationships between ozone sensitivity and HCHO/NOx ratios from annual averages and believe these should be removed from the paper and replaced with a focus on an analysis and presentation of sensitivities for separate seasons.

In discussing the comparison of NO2 and HCHO columns from the model with satellite data, the authors have added a reference to findings from Boersma et al (2016) on differences that could arise between cloudy and clear sky conditions. Uncertainties that arise between cloudy and clear sky conditions would be, I think, an underestimate of the differences that would arise from comparing midday NO2 from TROPOMI with NO2 averaged over the full 24-hour diurnal cycle. A recent paper (Edwards et al., Atmos. Chem. Phys., 24, 8943–8961, 2024, https://doi.org/10.5194/acp-24-8943-2024) comparing TROPOMI observations, models and the GEMS geostationary satellite observations of column NO2 shows time-of-day variations can be on the order of 50% or more. While I think this is an important caveat that should be acknowledged, the assessment of NO2 and HCHO columns against satellite observations are not a critical part of the paper and the comparisons as is can stay.

Response: We sincerely thank the reviewer for the meticulous review and valuable suggestions.

The reviewer may kindly note that the monthly mean ozone to emission changes were used to derive the FNR thresholds for identifying NOx and VOC sensitive regimes. However, this was not explicitly stated in the original manuscript.

As suggested by the reviewer, the annual mean FNR part is removed from section 4, and the seasonal change in FNR discussed in section 5 is merged into section 4 in the revised version. Accordingly, figures, tables and section numbers are revised, and the concluding points are modified.

A line highlighting the result from Edwards et al., 2024 is added in section 3.1 in the revised manuscript [Line no: 389-391].