Comment on acp-2022-299
Anonymous Referee #2

This study evaluates NOx emissions and ozone production in Texas, which covers several interesting questions, including model-satellite comparison, top-down NOx emission estimates using EMG approach, and HCHO/NO2 for identifying the ozone production regimes. Overall the results are clearly presented, and the figures are informative. There are several interesting findings, which could well be three independent studies, but my concern is that the findings are a little disconnected, and it is not easy to tell the major take-aways from the study. I’d suggest the authors better frame the core questions, and show how each section is connected. Below are some major comments:

- It is not clear to me why the authors compare two different versions of TROPOMI NO2. Satellite retrieval products are updated routinely. The newest version should certainly be better, and the older version is already replaced with newer one. I don’t think there is much scientific value of such comparison. Evaluation of different versions should be in the technical document of TROPOMI NO2. I’d suggest the authors stick to the newest, publicly available version.
- Related to previous question, I’d suggest the authors use newest version TROPOMI HCHO and NO2 data in Section 4.2 to evaluate ozone sensitivity. The authors have shown better performance of v2, but switch to v1 in 4.2. It is also not clear to me why the authors use an outdated version of TROPOMI HCHO (v1.1) while a newer version is already available.
- It’s also not clear to me why satellite HCHO is less sensitive to the application of AK. While it may not help improve the spatial patterns of HCHO, application of AK may resolve the overall difference between modeled and satellite HCHO. It’d be great if authors can show a figure or two to illustrate this point.

Specific comments:
- Figure 3 (right): What does the red line mean? Which shape profile?
- Figure 4: There is no explanation of the right figure in the caption. Why is it continuous? Are these generated for selected pixels?
- Page 16 Lines 5 to 10: The authors show TROPOMI NO2 is significantly lower than modeled NO2, and they attribute this to issues with TROPOMI. While it may be true, but could this also be due to model unable to capture the sub-grid processes?
- Page 20 Line 12: This sentence is confusing.
- Table 3 shows the derived NOx emissions are sensitive to NO2 lifetime. How much confidence do you have with the NOx lifetime in EMG approach? Does it agree with the NO2 lifetime simulated from model? And how does the seasonal variation in NO2 lifetime affect the calculation of emissions?
- Figure 12: It’s interesting to see there is almost no diurnal cycle with HCHO. I think both biogenic emissions and photolysis rate vary diurnally. It’d be interesting to illustrate why HCHO is relatively constant, and what this would mean for ozone production.
- Figure A1: There seems to be a large discrepancy between CAMx and observed NOx and NOy. I’d suggest the authors investigate why CAMx is biased too low, and how this would affect the interpretation of other findings.