We sincerely thank all reviewers for their constructive comments and suggestions. All comments have been carefully considered and addressed in the revised manuscript. Detailed point-by-point responses to the reviewers' comments are provided below, where the authors' responses are highlighted in blue, and newly added or revised text in the manuscript is highlighted in red.

Reviewer #1：

Overview

This work uses formaldehyde-to-NO_2 ratios (FNRs) from the Geostationary Environmental Monitoring Spectrometer (GEMS) to classify ozone formation regimes over eastern China during the warm season (April-September) from 2021-2023.

This version of the manuscript is much improved from previous versions. Specific concerns that were addressed in the new version included:

1. Clearer definitions of:
    a. Temporal extent of the study.
    b. O_3 sensitivity regimes based on FNR values.
    c. How the point data was compared to the gridded satellite/meteorology data.
2. Additional literature support for:
    a. Seasonal trends in O_3 formation.
    b. Satellite retrieval algorithms for GEMS and TROPOMI.
    c. Consistency between surface O_3 and GEMS tropospheric O_3 VCDs.
3. A quantitative comparison between TROPOMI and GEMS for the NO_2 VCDs, HCHO VCDs, and FNRs.
4. Additional statistical support for relationships between the satellite observations and meteorology.

There is still room for improvement here (see the Specific Comment section below).

The changes made to this newest version of the manuscript has addressed almost all of the concerns from the previous round of revisions. I suggest that this paper be published with consideration of the following comment and suggested technical corrections.

Response: We thank the reviewer for the positive evaluation of this study and for recognizing the improvements made in the revised manuscript. We are pleased to see that the current version has satisfactorily addressed the major concerns raised in the previous review round, including the study period, data-matching methodology, incorporation of relevant literature, comparison between GEMS and TROPOMI, and the statistical relationships between pollutants and meteorological factors. We also sincerely appreciate the reviewer's further suggestions and technical comments. These comments have been carefully considered, and corresponding revisions and additions have been made point-by-point to further improve the clarity, rigor, and overall persuasiveness of the manuscript.

Specific Comment

Line 431: I think the statistical relationships between pollutants and meteorology were much improved in Section 3.1 but are not present in Section 3.4. From my interpretation, the statistical

analysis between pollutants and meteorology in Section 3.1 (i.e., Table S1) is based on all data in the study region. Are there differences in the statistical relationship among the four cities/regions of interest? In Section 3.4, the differences in NO_2, HCHO, and meteorology between the cities are already qualitatively described, so including statistical information for each city would help to strengthen the discussion even more, just like it did in Section 3.1.

Response: We thank the reviewer for this valuable suggestion. We agree that the original Section 3.4 was mainly based on qualitative discussion and lacked the statistical support provided in Section 3.1. Therefore, in the revised manuscript, we further supplemented the Spearman correlation analyses between pollutants and meteorological factors for four representative cities, namely Beijing, Nanjing, Chengdu, and Guangzhou (Tables S2–S5). The newly added results further demonstrate that the statistical relationships between pollutants and meteorological factors differ substantially among cities, which provides stronger support for explaining the differences in ozone formation mechanisms across regions. The related discussion has been added to Section 3.4 and the Supplementary Material (Tables S2-S5).

Suggested Technical Corrections

Line 72: capitalize the author's name in the reference.

Response: We thank the reviewer for pointing out this issue. The original "mahajan et al. (2015)" has been corrected to "Mahajan et al. (2015)" in the revised manuscript. (Line 80)

Line 247: there is some sort of error with the text here.

Response: The original text has been revised accordingly (Lines 266-269) as follows: "Figure 2, based on GEMS satellite data and ground monitoring observations, shows the daily variations in atmospheric pollutants across China during the warm seasons (April–September) from 2021 to 2023, including the tropospheric column concentrations of HCHO and NO_2, as well as ground-level O_3 concentrations."

Line 315: same error as above.

Response: The original text has been revised accordingly (Lines 334-335) as follows: "Figure 4 displays the daily relationship between HCHO and NO_2 concentrations and ground-level O_3 utilizing LOESS fitting method to highlight trends and high O_3 concentration events."

Reviewer #2：

一、This paper is much improved over the initial version. I remain excited about it for the data and not the analysis, but it should be published and I don't want to hold it up further. The paper remains extremely parochial, with frequent references to the recent publications of Chinese scholars for ideas that are 30-50 years old. The effort to put these results in the context of research on China air quality is to be commended. The omission of context about what we already know from research in other parts of the world is glaringly obviously and undermines the credibility of this paper as a serious contribution to the literature.

Response: We sincerely thank the reviewer for these valuable comments. Corresponding revisions

have been made in the manuscript, particularly in the Introduction section.

(1) Regarding the concern that the manuscript is "parochial" and lacks connections with studies from other regions of the world.

1) The reason why this study cites a relatively large number of studies from Chinese regions (e.g., BTH, YRD, PRD, and the Sichuan Basin) is that the research objectives and discussion of this work are primarily focused on China and its major urban agglomerations. Therefore, it is necessary to discuss the results within the context of recent ozone pollution and ozone sensitivity studies in China in order to better highlight the scientific questions and practical significance of the study region.
2) In response to the reviewer's suggestion regarding the lack of international context and comparison, we have retained the original discussion involving other regions (e.g., South Asia, North America, Japan, and South Korea) and further incorporated additional studies from North America, Europe, and Mexico in the revised manuscript. These additions strengthen the comparisons and connections with previous international studies, thereby improving the international context and overall credibility of the manuscript. In addition, we have increased the citation of both classical and recent international studies.

(2) Regarding the frequent citation of recent Chinese studies when discussing classical concepts.

Following the reviewer's suggestion, we revised the Introduction by replacing some recent references with earlier and more fundamental classical studies in order to more accurately present the development background of ozone formation mechanisms. Meanwhile, we further supplemented the manuscript with several recent advances in ozone formation research, including the sustained role of OVOCs in radical cycling, aerosol–photochemistry coupling, and the UFU plume mechanism, which have received increasing attention in recent years. These additions help to better reflect current research directions and recent developments in ozone formation mechanisms.

二、More technical, it looks to me like the LOESS fitting approach substantially biases the authors conclusions about the FNR transition point from NOx Limited to VOC limited. This is visible in Fig 4 and 6 where the LOESS fit looks like it underestimates the peak at every hour. Seems like it'd be better to have a peak finding algorithm and then do a separate fit on the shape of these curves above and below the peak.

I leave it to the authors whether to make any changes in response to my comments. I do not wish to review this again.

Response: We thank the reviewer for this valuable suggestion regarding the potential peak-smoothing effect of the LOESS fitting approach. We agree that the LOESS curve may smooth local peak structures in the O_3–FNR relationship, particularly near regions with high O_3 occurrence frequency. This characteristic mainly results from the locally weighted averaging nature of the LOESS method, which is intended to reduce the influence of local fluctuations and uncertainties in satellite observations .

We acknowledge that the peak-finding and segmented fitting approach suggested by the reviewer could provide additional information regarding local peak positions. However, the primary

objective of this study is not to identify a single exact peak value, but rather to characterize the overall nonlinear relationship between O_3 and FNR and to identify ozone formation sensitivity transition regimes at the regional scale. In this context, the LOESS approach provides a stable and flexible representation of the overall variation trend without imposing a predefined functional form.

In addition, the ozone formation sensitivity regimes in this study are defined based on the high-response interval ($f(FNR) \geq 0.9\times$ f_max). Therefore, small variations in local peak position or magnitude are not expected to substantially affect the regime classification results.

To further evaluate the robustness of the fitting results, we additionally applied a bootstrap resampling method (1000 iterations) to quantify the uncertainty of the LOESS fitting and estimate the corresponding 95% confidence intervals. Following the reviewer's suggestion, we have added further clarification regarding the applicability and limitations of the LOESS approach in Section 2.2 of the revised manuscript (Lines 256-263).

三、Some specific comments and examples below.

(1) From lines 40-59 the text reads as if the conclusions are new and provides recent references. However, all of those statements are generic and have been known for decades. Many of them could have been written in the 1960's and are well documented in introductory atmospheric science textbooks. Others were likely in the original proposal for GEMS. The authors should be clear what, if anything, they are saying has been learned recently. I have not read many of these references, but if they present the findings as described in this paragraph as new findings, I am deeply disappointed with peer review.

Response: We thank the reviewer for this important comment. We agree that several descriptions in the original Introduction could give the impression that well-established atmospheric chemistry concepts were being presented as recent findings. In the revised manuscript, we have therefore clarified the distinction between long-established knowledge in ozone photochemistry and more recent developments in atmospheric chemistry research.

Specifically, we revised the relevant text and references to better acknowledge the foundational understanding of ozone formation mechanisms established in earlier studies, while highlighting several recently emphasized research topics, including the role of OVOCs in radical cycling, aerosol–photochemistry interactions, and the "urban–forest–urban (UFU plume)" mechanism. We also adjusted the wording to more clearly distinguish established atmospheric chemistry principles from recent advances and applications in satellite-based ozone sensitivity analysis.

The corresponding revisions have been incorporated into the Introduction section (Lines 38-67).

(2) Line 64-65. These references are not the first to show this point. Fine to use them, but then what they did that adds to the literature on FNR needs to be highlighted. It is unreasonable to present this as if they are the first or best references for this point.

Response: We thank the reviewer for this valuable comment. Following the reviewer's suggestion,

we have revised the reference list and replaced several recent citations with earlier and more fundamental classical studies to more accurately reflect the development of ozone formation regime classification and the FNR approach.

The corresponding revisions have been made in the Introduction section (Lines 69-73).

(3) Line 92-100. Again, these can't possibly be the first or best references for these very generic points about temporal variation in ozone production rates. In the remainder of this paragraph where the text explicitly refers to analyses for China then the references seem more appropriate although even with my limited knowledge I suspect that the references given are not the first to say some of the statements given. Moreover, I wonder what is being said here that is not obvious. Doesn't the slowing of photochemistry in winter require that there be some transition from summer chemistry regimes to a VOC limited everywhere in the world for much of Fall, Winter and early Spring?

Response: We thank the reviewer for this important comment. Following the reviewer's suggestion, we have supplemented the manuscript with earlier foundational references as well as addtional studies from other regions to better reflect the international development of ozone formation regime research.

We agree that seasonal transitions in ozone formation regimes, including the tendency toward more VOC-limited conditions under weaker photochemical activity in autumn and winter, are well established in previous studies. The purpose of this paragraph is not to present these seasonal characteristics as new findings, but to summarize the established understanding of temporal variability in ozone formation sensitivity across seasonal and monthly scales, which provides the necessary background for this study.

In this context, we have clarified in the revised manuscript that the novelty of this work lies in the extension to the diurnal scale based on hourly GEMS observations, enabling the characterization of continuous sub-daily variability in ozone formation sensitivity that has not been systematically resolved by previous satellite-based or ground-based studies.

The corresponding revisions have been made in the Introduction section (Lines 100-108).

(4) Line 231-244: I think the earlier text implies this fitting is for data at a single hour of the day without reference to earlier hours. That point should be restated here.

Response: We thank the reviewer for this important suggestion. We agree that the description of the hourly fitting procedure in the original manuscript was not sufficiently clear. In the revised manuscript, we have clarified that the LOESS fitting was performed independently for each hour from 09:00 to 16:00 LST. Specifically, for each hourly analysis, only the O_3 concentrations and FNR samples corresponding to that specific hour were used, without incorporating data from other time periods.

This clarification has been added in Lines 244-248 of the revised manuscript.

(5) I would prefer to see maps like Fig 7 and 8 for the cities instead of the very uninformative ones currently in the manuscript. The conclusion at line 556-558: “that many urban areas transition from VOC-limited conditions in the morning to transitional or NOx-limited regimes in the afternoon.” Seems like one that could’ve been made from the existing surface observations. It undersells that value of the mapping component of the remote sensing data.

Response: We thank the reviewer for this insightful comment. We agree that the spatial representation of the results is important for illustrating differences across urban regions, and that surface observations alone can capture part of the diurnal transition signal in ozone formation sensitivity.

However, the satellite-based GEMS observations provide additional spatially continuous information that complements ground-based measurements by revealing regional-scale patterns across multiple cities simultaneously. In the revised manuscript, we have further clarified these spatial patterns already presented in Fig. 7 and Fig. 8 through improved textual description to better guide interpretation of the observed spatial heterogeneity.

We also note that more detailed high-resolution mapping of urban-scale variability in ozone formation sensitivity is an interesting direction for future work.