

Responses to Reviewers' comments

To the esteemed Editor Dr. Jerome Brioude and Reviewer Dr. Dani Caputi,

We would like to thank Dr. Jerome Brioude for handing this paper. We would also like to thank Dr. Dani Caputi for the time and efforts in reviewing our manuscript. We have accordingly revised the manuscript.

Please find the point-to-point responses to the reviewer' comments as follows:

Reviewer' comments are in black.

Author's responses are in blue color.

Changes in the manuscript are in red color.

Sincerely,

Weihua Chen

On behalf of the authors

Response to Reviewer #1:

Referee #1 comment:

The quality of the manuscript has significantly improved, and I am overall pleased with the authors responses to my comments. I would be happy to recommend the article for publication after the following outstanding issues are addressed.

Response:

We thank the reviewer's positive comments. We have incorporated all your constructive comments and suggestions in the revised manuscript.

General Comments:

For my general comments, I will retain the same numbering as in my first review.

1. I am still left a little bit confused as to the exact definition of a NOI event. Let's suppose the following are hourly time-series data of ozone concentration in $\mu\text{g m}^{-3}$ within the 21:00 – 6:00 LT range. Please verify that my understanding is correct:

43 54 51 40 = NOI, the concentration increased by >10 (43 to 54) and subsequently decreased by < 10 (54 to 51)

43 54 65 40 = NOI, the concentration increased by >10 (43 to 54) and subsequently didn't decrease (54 to 65) even though in the following hour it decreased by > 10 (65 to 40)

43 47 54 51 = Not NOI, the increase of > 10 happens over more than 1 hour

43 54 42 41 = Not NOI, even though there's an increase of > 10 (43 to 54) there is a subsequent decrease of > 10 (54 to 42)

Also, I assume that the "previous hour" and "next hour" can include 20:00 and 7:00 LT, respectively?

Response:

The reviewer is correct. As the reviewer said, the "previous hour" and "next hour" include 20:00 and 7:00 LT, respectively. We have clarified it in Lines 157-158:

'..., with an increase in levels of at least $10 \mu\text{g m}^{-3}$ compared to the previous hour (include 20:00 LT) and a decrease of less than $10 \mu\text{g m}^{-3}$ in the next hour (include 07:00 LT).'

2. The methodology is much clearer in this version, and I have no additional concerns.

Response:

Many thanks.

3. I appreciate the authors relating the breakpoints of 2012 and 2016 to changes in urbanization and policy. Given the ambiguity of the exact years that these changes went into effect, I do think that one could still make a case that choosing the specific years of 2012 and 2016 to split the data is a form of p-hacking, i.e., finding breakpoints that would make the trends statistically significant rather than strictly testing a pre-determined hypothesis. However, exploratory analyses are an important component of observational field studies and distinguish this type of research from controlled laboratory experiments, where statistical controls are much more rigorous but creativity is highly limited and the scope of work is more narrow. I believe the trends found in this study are important findings to report.

My only additional recommendation on this point is to change “was related to urbanization” to “was likely related to urbanization”.

Response:

Thanks for bringing this issue to our attention. We have modified it in Line 276:

‘..., which was likely related to urbanization.’

4. The authors did excellent work in following my suggestion to plot MDA8-NOP and NOP-MDA8, and the results are highly intriguing. I agree with their conclusion that it suggests a complex interplay between daytime and nighttime dynamics.

Response:

Thanks for providing this constructive suggestion to make the results clearer and more convincing.

5. Based on the new analyses presented, I accept the use of cloud top temperature as a proxy for deep convection, and the authors now make a satisfactory case that KI is a valid metric of Conv events in the PRD.

Response:

Many thanks.

6. I appreciate the Conv case study being supplemented with CTT. I also appreciate the citation of Ploeger et al. (2021) showing comparable vertical velocity. It may be that the WRF model cannot resolve the core

updrafts in deep convection with 3 km resolution, or that the convective scheme used may not be designed to do this – though I am not a modelling expert. Regardless, the overall picture of convection occurring on this night is clear with the WRF, CTT, and KI taken as a whole.

Response:

Thanks for the reviewer's approval.

Specific Comments:

Lines 167-168 (originally #22 - lines 219-220):

I apologize that my original comment was likely unclear. I was suggesting the authors clarify why LLJs can be considered downdrafts for unfamiliar readers. A sentence such as “We assume that LLJs cause downdrafts because of the vertical wind shear the jets induce, which creates mechanical turbulence” would suffice.

Response:

According to the reviewer's suggestion, we have modified this sentence in lines 167-168:

‘We assume that LLJs cause downdrafts because of the vertical wind shear the jets induce, which creates mechanical turbulence’

Lines 415-418 (originally #29 - lines 352-353):

I accept the authors correction, except I am not sure that “model bias” is the correct term. Would “model uncertainty” or “model imperfection” be more appropriate?

Response:

Thanks for pointing out this issue. We intended to say that the modeled downdraft was half an hour later than the observed O₃ intrusion, which was related to the model errors. Therefore, “model errors” would be more appropriate and we have modified it in Line 417:

‘... due to the model errors, ...’