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.

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 $\mu g\ m^{-3}$ 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?

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

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".

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.

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.

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.

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.

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?