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Comment on acp-2022-360

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Referee comment on "Quantitative impacts of vertical transport on the long-term trend of nocturnal ozone increase over the Pearl River Delta region during 2006–2019" by Yongkang Wu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-360-RC1>, 2022

Wu et al. provides a novel framework for capturing the essence of nocturnal ozone increases (NOI), which is an important area of research often neglected in analysis of the ozone budget. They break the causes of NOI down into clearly discernable phenomena and present evidence that the majority of NOI events in the Pearl River Delta (PRD) are caused by Low Level Jets (LLJ). Further, they present a detailed case study of a LLJ NOI event, as well as a convective storm (Conv) NOI event.

The framework is intriguing and presents a valuable contribution to the literature, however, some modifications should be made prior to publication. In particular, the authors need to more clearly define their methodology and make a stronger case for using the K index as a proxy for convective storm events.

General Comments:

- The definition of NOI first appears in lines 118-120, where values increase by at least $10 \mu\text{g m}^{-3}$. However, this comes from a 2020 reference and not every preceding study in the introduction that mentions NOI contains findings that are consistent with this strict definition (e.g. Caputi et al. 2019). It would be helpful if the authors clarify that (I assume) this is the definition they employ specifically in their study (e.g. "for our analysis, we define NOI as ..."). Additionally, the definition needs to be clearer. For example, I am left unsure whether "values increasing by at least $10 \mu\text{g m}^{-3}$ " mean increasing from the daytime minimum, the previous hour's value, or what exactly.
- Some aspects of the methodology need clarification. In Section 3.1, are the statistics (e.g. $53 \pm 16 \text{ d yr}^{-1}$) from an aggregate of the air quality monitoring stations, and are the error values and error bars in Figures 3 and 4 calculated by a pooled standard deviation? The authors then discuss the proportions of events attributable to LLJ vs. Conv, does this come from the ERA5 data? If so, are the causes of an individual NOI

event (LLJ, Conv, LLJ+Conv, Other) determined by an instantaneous snapshot of the meteorological conditions over the air quality station, or a regional average for a given night? Please connect the dots between the different methods discussed (e.g. CMAQ, IPR, air quality stations, meteorological stations, ERA5) and where specifically they employed in the results.

- In Figure 3 (and all of the accompanying analysis), the authors take data from 2006 – 2019 and break it into two halves, with a breakpoint at 2012 for (a) and (b) and 2016 for (c) and (d). It would be nice to have some physical justification for applying a discontinuity in the linear trend analysis at these specific years. For example, did any local policies on emissions change in 2012 or 2016? If there was no specific justification in mind, the authors should clearly state this section of their research as exploratory and at least speculate on a physical cause, otherwise, this could be seen as “p-hacking”. Also, please state the statistical method used for calculating the p-values of the linear trends.
- I appreciate that the authors recognize the controversy of whether NOI increases or decreases the following days ozone concentration in lines 426-429. In lines 241-243, the authors state that “NOP is significantly positively correlated with MDA8 O3 ... implies that daytime O3 concentration levels potentially affect NOP”. To further strengthen this discussion on the relationship between daytime and nighttime ozone, I would suggest the authors look at the correlations between: 1) the afternoon MDA8 and the following night’s NOP, and 2) the NOP and the following afternoons MDA8, and explicitly report the results from both. This will help elucidate the arrows of causality between daytime and nighttime ozone concentrations in the PRD.
- As for my most significant concern, the authors use a K index (KI) > 30 as an indication of whether convective storms are occurring in the PRD on a given night. While I understand the need to make approximations when using large datasets, deep convection can occur when KI<30 and KI>30 does not guarantee the presence of deep convection, so there needs to be additional justification that KI>30 is a valid metric for what the authors are trying to capture. For example, the authors might look at a random subset of 10 nights where KI < 30 and 10 nights where KI > 30, and qualitatively compare the radar and/or satellite imagery in the PRD. Alternatively, they could look at the relationship between KI and peak vertical velocity in the ERA5 for the PRD at night, and show that KI=30 is a good cutoff for their purposes.
- On a related note to (5), the case study of a Conv event presented in section 3.6 could use some additional supporting evidence and data. Figure 9b shows updrafts of only up to 5 cm s⁻¹, which are at least an order of magnitude lower than what would be expected in convective showers and thunderstorms. While some light precipitation is indicated in Figure 9a, it would be better to also see a radar and/or satellite image of the alleged convective storms that night.

Specific Comments:

Line 45: Please explicitly introduce the chemical reaction for NO titration for unfamiliar readers.

Line 50: “around 3:00 in the morning” local time or UTC? Please specify.

Line 50: “118 μg m⁻³ in the UK” compared to roughly what values in the daytime?

Lines 51-52: "and the annual trend was found to be increasing" in terms of frequency of occurrence or intensity? Or both?

Lines 52-53: "High nocturnal O₃...pollution events" but in lines 426-429 you mention that this is controversial. Better to be consistent.

Line 54: We use the word "proven" in mathematics but not science. "Shown", "suggested", "provided evidence for", or anything similar could be used instead.

Line 63: "With an altitude of about 500 m" this is a general average cited in Stull, how well does this apply to the PRD?

Line 69: Please clarify what is meant by "dynamic variation".

Line 71: Please change "Tropospheric" to "Free Tropospheric" because we are distinguishing multiple layers of the troposphere in this study (residual layer, nocturnal boundary layer, free troposphere).

Line 96: "attributable to differences ... urbanization". Is this because of the differences in nighttime NO emissions in urban vs. rural areas? Please state.

Lines 103-104: "Moreover, high daytime ... in the PRD region". Please add citation. Also, another motivating factor would probably be the high population in the PRD and the number of people air quality affects in this region?

Figure 1: If it wouldn't create much additional work, it may be worth shading this map with terrain instead of coloring the political regions. The fill colors for the political boundaries don't add much useful information to the plot.

Line 142: Again, please avoid using the words "proof" or "proven" in a science article.

Line 164: Consider changing "explored" to "utilized"

Lines 201-205: These statistics are cutoff ranges that the EPA considers a model acceptable or unacceptable to use, correct? Please specify the purpose of introducing these values, statistics, and ranges here – it isn't entire clear.

Lines 219-220: In order to consider LLJ events a "downdraft", there must be an assumption that the LLJ is inducing turbulent mixing from the vertical wind shear it creates. Please state this.

Line 264: "Below" -> please specify "below the jet"

Lines 278 – 279: "it can bring clean marine air into the PRD region" I assume this is at the surface?

Line 297: what is meant exactly by O3 from the daytime "enters" the RL between 21:00 and 03:00? Why isn't the O3 already inside the RL from the minute the daytime boundary layer fades into the RL?

Line 299: Horizontal transport to where? And by "vertical transport" of O3 are the authors referring to dry deposition, convection, or both?

Line 319: How are individual sites classified as either rural or urban?

Figure 8: Similar to general comment (3), please provide a justification for the break at 2012.

Lines 352-353: the modeled downdraft in Figure 9b occurs *after* the observed O3 intrusion into the nocturnal boundary layer. It may be that the timing of the model is slightly off, but this should be acknowledged rather than stated as a clear cause an effect.

Line 369: "meet the criteria" -> the EPA criteria specified earlier? Please clarify.

Figure 10: The black lines (NET) is categorically different from ozone and wind because it is not a meteorological phenomena. This was a bit confusing to my eye at first because it is plotted along with ozone and wind, but in reality it relates more to the bars. Consider at least changing the circle marker to a triangle for the black NET lines.

Figure 10b: Would it be possible to plot the momentum flux in the model as well to get an indication of shear below the LLJ? Or no because this was not a large eddy simulation?

Line 428: Caputi et al. 2019 also found lower ozone the following day when more mixing of ozone from the residual layer to nocturnal boundary layer occurred overnight.

Technical Comments:

Line 91: "Long-tern" Long-term?

Figure 6b caption: reference to blue but no blue in figure, assume orange?