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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What caused the extreme CO concentrations during the 2017 high pollution episode in India?" *by* Iris N. Dekker et al.

Anonymous Referee #2

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Review of Dekker et al., "What caused the extreme CO concentrations during the 2017 high pollution episode in India?"

Dekker et al. use a combination of satellite retrievals, chemical-transport model simulations, and surface observations to explore why North India experienced extreme CO concentrations during a recent pollution episode that received much popular attention. During October and November, North India frequently experiences spectacularly high concentrations of CO, PM2.5, and other primary pollutants. Public discussion generally focuses on crop-stubble (and other biomass) burning as the principal culprit. Here, Dekker et al provide evidence that this assumption may be simplistic and incorrect: instead, they suggest that poorly diluted primary CO emissions from other sources (transport, industry) etc. explains the episode.

The paper is generally clearly written and the caliber of the technical work is appropriate. I would recommend the manuscript for publication in ACP with minor edits.

The authors suggest that meteorology is a dominant explanation for the CO episodes. What I found lacking in the argument provided here was in explaining the genesis/formation of the episode. Was there a sudden shift in meteorology (e.g., reduction in wind speeds and PBLH) that coincided with the initiation of the pollution episode? Here, I concur with what Reviewer #1 has suggested, namely regarding a more detailed explanation of why the specific time periods included here were chosen for analysis. If it is possible, showing a longer time period that illustrates the formation of the episode could be helpful.

To strengthen your argument, you might also appeal more directly to the existing emissions inventories. For example, it would be useful to quantify in a table or figure the share of primary emissions (for this season) across the IGP that are attributable to the various sectors and to crop burning. You might consider distinguishing here between fire- and non-fire periods, perhaps with a sensitivity case that allows for the possibility you alluded to that fire CO emissions are underestimated.

Other Minor suggestions:

* Units: the manuscript switches frequently between using mass concentration (μg/m3, mg/m3) units and mixing ratio units (ppb) for CO. While I recognize that there are good reasons for using these alternative unit systems, it would be helpful to orient readers by providing an approximate conversion factor and when possible crafting the core narrative of the paper around one unit system).

* Health-based standards: CO is subject to national ambient air quality standards in India. It would be helpful to compare the observed concentrations to Indian guidelines and other relevant values (ie., Chinese, US, EU). I believe that Indian standards may be more stringent than most other countries for CO – unlike Indian standards for PM2.5, which tend to be quite permissive.

* Section 4.2: the role of adverse meteorology in driving North Indian pollution episodes is widely appreciated in the literature. I would suggest looking at (and citing when appropriate) the following articles:

* Guttikunda and Gurjar, "Role of meteorology in seasonality of air pollution in megacity Delhi, India": doi: 10.1007/s10661-011-2182-8

* Gani et al., "Submicron aerosol composition in the world's most polluted megacity: The Delhi Aerosol Supersite campaign", doi: 10.5194/acp-2018-1066

* Tiwari et al., "Aerosol optical properties and their relationship with meteorological parameters during wintertime in Delhi, India", doi:10.1016/j.atmosres.2014.10.003

* Tiwari et al., "Variability in atmospheric particulates and meteorological effects on their mass concentrations over Delhi, India", doi: 10.1016/j.atmosres.2014.03.027

* From the various comparisons made with conditions in China, it is evident that the authors are somewhat surprised by the severity of the pollution problem in North India. Yet the magnitude of the PM2.5 problem in North India is becoming quite well documented, especially in literature focused on remotely sensed PM2.5 levels. The evidence is now quite clear that India has overtaken China for population-weighted PM2.5 levels, while other large countries (Bangladesh, Pakistan, Nigeria, Egypt) can be even more polluted. In my view, the focus on China's pollution in the popular imagination – even when nowhere near the highest in the world – arises in part because of the rapid improvement in the availability of ground-based data there. See for example Shaddick et al, ES&T 2018: https://pubs.acs.org/doi/pdf/10.1021/acs.est.8b02864

* A discussion of data quality and measurement uncertainty for the in-situ CPCB pollution measurements would be appropriate. Do the surface measurements appear to be generally well calibrated and reliable?

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