Review of "Improving regional air quality predictions in the Indo-Gangetic Plain – Case study of an intensive pollution episode in November 2017" by Roozitalab et al. (Paper #acp-2020-744).

In this study, the authors have used the WRF-Chem to simulate the intensive pollution episode in the Indo-Gangetic Plain (IGP) in November 2017. They carried out 14 sensitivity simulations for different scenarios based on biomass burning emissions, chemical boundary conditions, and dust emissions. The model (base scenario) was evaluated for meteorological parameters (10 m wind speed and direction, 2 m temperature, and surface water vapor) with MERRA-2. The simulated AOD and $PM_{2.5}$ were compared with observations from AERONET and CPCB/US Embassy monitors, respectively. The authors have also looked into the daytime variation in ozone. The study is interesting because the authors try to simulate the $PM_{2.5}$ during November 6-13 using the emissions and aerosol-radiation feedbacks but no assimilation. This study is similar to a recent study published in JGR by Kumar et al., 2020 (https://doi.org/ 10.1029/2020JD033019). Overall, the manuscript needs a major revision. There are too many figures, which makes it hard to get the message across the reader. The labels in the figures are difficult to read. Here are my comments:

Main comments:

1. I do not understand the hypothesis behind 14 simulations and still not being able to simulate the aerosols. The authors consider FINN_VIIRS_7Xperiod2 (base scenario) as the best scenario but the bias is still high (for AOD and $PM_{2.5}$) compared to the observations. From Fig. 3, the simulated AOD is underestimated over the IGP and overestimated over the rest of India. Comparison with AERONET (Fig. 4) shows that MERRA-2 does better over Jaipur because from Fig. 3b it is evident that AOD over Jaipur is in the range 0.5-0.8. Both WRF-Chem and MERRA-2 should have the same resolution while comparing (Fig. S2) and color bar scale comparable to Fig. 3. In conclusion, I think AOD is better simulated by MERRA-2 at both Jaipur and Kanpur. Please include the statistics for AERONET vs WRF-Chem and MERRA-2 AOD.
Lines 254-255: There are no major fires during November over western India/Rajasthan (as seen in Fig. 10). Also, there is no major dust event but there is a possibility of anthropogenic dust some of it being unique to the Indian region.
Lines 270-271: I do not agree with the authors' explanation. As seen in Fig. 3, WRF-chem is simulating higher AOD values over western India/Rajasthan.
The figures do not completely agree with the statements made by the authors.

2. Why are the authors comparing the diurnal variation (Fig. 5a)? Do the emissions have a diurnal variation in the model? Why wasn't the $PM_{2.5}$ data from CPCB stations used in Fig. 5a? Fig. 6 includes data from all the CPCB stations, how was the quality check performed on the CPCB data? Please add the details on the quality check of CPCB data in the methods section. It is better to show the spatial plot along with the CPCB and US Embassy observations as a scatter. It will show if the model was able to capture the spatial variation in observed $PM_{2.5}$.
How does MERRA-2 compare with the CPCB observations?
Lines 292-293: I do not completely agree with the explanation of transported dust from the Middle East. Is PM10 high over Delhi? Looking at the CALIPSO profile data shown in Beig et al., 2019, it is polluted dust, which is different from desert dust. The authors can also look into the MISR data for dust AOD. The authors have made a statement in section 3.6 that sensitivity tests do not show a major influence of dust being transported from the Middle East.

3. Have the authors looked into the PBLH from the model and compared with the observations? You might have to derive the PBLH from radiosonde observations. The authors attribute the low $PM_{2.5}$ on Nov 8-10 to the plume rise in the model. My understanding is half of the fire emissions will be released within PBLH and the rest above it. The model is simulating higher PBLH as seen in Fig. 13. I would suggest instead of comparing at the US Embassy only, include observation data from all CPCB stations. PBLH in Delhi during November is less than 1000 m (Nakoudi et al., 2019, AMT). The days when PBLH was low (less than 1000 m) in the model (Nov 7, 11-13), the simulated $PM_{2.5}$ was comparable to the observations.

4. According to me, sections 3.3, 3.5 and, 3.6 do not add anything new to the paper. The inclusion of missing fire emissions is an important part of the simulation. Also, it is worth to include a comparison with the Kumar et al., 2020 study.

Specific Comments:

1. Line 33: Ghude et al., 2016 do not mention the long-term health impacts due to an increase in emissions based on current policies.
2. Lines 42-45: David et al., 2019 show the impact of both transport and emissions on $PM_{2.5}$ in different regions in India. The authors can add results from the study.
3. Line 78: Add reference - Kumar et al., 2020 (JGR)
4. Lines 128-129: Studies by Conibear et al., 2018, Venkataraman et al., 2018, and David et al., 2019 have shown that residential energy use is the main source of $PM_{2.5}$ in India.
5. Lines 203-204: "Irrespective …" From where did the authors get this information?
6. Multiple places the authors mention "the model was able to capture ..." (For example, Lines 230, 266, 343) – please support your statements with statistics (MB, RMSE).
7. Replace provinces with states (Lines 311, 315).
8. Line 374: Change "Fig. 1" to "Fig. 8".
9. Line 452: What are some of the other meteorological phenomena?
10. Table 2: Arrange the table as explained in the text (section 2.2).