A review report on the ACPD manuscript entitled "WRF-Chem simulated surface ozone over South Asia during the pre-monsoon: Effects of emission inventories and chemical mechanisms" by Sharma et al., 2016.

General

The study investigates simulated ozone over South Asia, using several simulation scenarios, composed of different inventories and chemical mechanisms. The simulation results were evaluated using data from an in-situ monitoring network. Among the findings of the study is that simulated daytime ozone maximum differ significantly between different emission scenarios, by as high as -22%, in contrast to the 24h mean values, which are more consistent. The results are not surprising, especially on local scale, given that measured ozone is primarily photo-chemically formed. However, a major issue here is that the authors use different temporal emissions (2010 for HTAP, 2006 for INTEX-B) form different emission inventories and are trying to validate the model simulations of 2013 (using reanalysis ECMWF product) with measurements from completely different temporal period (e.g, 2004 or before, and 2009-2013), except for 4 stations. The authors should clarify the significances of these results in this context, especially in this very active developing region? Impacts from biomass-burning emissions are not adequately discussed. The authors proclaim similar results between different emissions scenarios despite the different temporal periods. However, these claimed similarities should be only a warning of some compensating effects that cancel the interesting differences caused by the emissions annual trends and variability.

The study sounds scientifically interesting and well written, but still need more consistent analysis and casual discussions on the driving factors of the differences between these scenarios.

Specific comments

Page 1, lines 32-33. The conclusion that the SEAC4RS-RADM2 scenario preforms better than the others does not sound novel scientific information. I think that it is important here that the authors shed some light on why this specific scenario works better than the others.

Page 3, lines 103-: The authors mentioned high pollution loading and biomass burning as reasons for the intense ozone photochemical formation during the pre-monsoon period. It would be also very interesting if the authors could investigate how biomass burning emissions and transport affect ozone photochemical formation in the study's domain.

Page 4, lines 139-141: Could the authors elaborate on the difference between the two aerosol modules used, the (MADE/ SORGAM) vs GOCART, and how this would affect their results?

Page 4, lines 142-145: Also, how the different photolysis schemes Fast-J and F-TUV may affect the results?

Could the authors employ the same aerosol and photolysis scheme for each scenario (using different emissions and chemical mechanism), so that casual factors for the differences can be determined?

Page 4, line 152: What is the effect of using year 2010 HTAP emissions as opposed to experimental observation date and model reanalysis of 2013? How this may affect their conclusions?

Page 5, line 160: What is the effect of using year 2006 INTEX-B emissions as opposed to experimental observation date and model reanalysis of 2013? How the authors account for using emissions from different years?, especially in this high-pace developing region?

Page 6, lines 198-200: But how the comparison would make sense given that the emissions are from different years and are also different between different inventories?

Page 6, line 204: No, that too much difference, I do not think the authors can use (2004 or before) ozone measurements to validate model simulations for years 2013 using emissions from different temporal periods?? I think the authors need to reconsider all these comparisons..

Page 6, lines 219-220: Could the authors provide quantitative numbers for this similarity between HTAP, INTEX and S4RS scenarios (e.g., r^2)? To me, they look quantitatively different..

Page 7, lines 241-250: Again, it is important to address here if the differences in the ozone production rates between different emission scenarios are related to using different temporal periods for the emission inventories or related to different emission inventories as it appears here?

Page 8, lines 304-318: So, are these differences related to chemical mechanism, or the constrained different overhead ozone column, or photolysis rates (Fast-J vs F-TUV) or different aerosol modules (static vs dynmic)?

Page 11, lines 403-406: The authors claim interesting similar results despite the use of different temporal emission, but I think that shows only possible compensating effects that lead to the claimed similar results despite different emissions… I think that the authors should seriously address this issue as it significantly affect the credibility of the results.

Page 11, 420: Again, I still not convinced by the “overall agreement”, given that the model is constrained to emissions from different temporal periods than the measurements as well as the model simulations (using reanalysis products from year 2013).