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Geoscientific
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Discussions

Interactive comment on "Simulations over South Asia using the weather research and forecasting model with chemistry (WRF-Chem): chemistry evaluation and initial results" *by* R. Kumar et al.

R. Kumar et al.

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Reply to the comments of Anonymous Referee #2

We are thankful to the reviewer for carefully evaluating our manuscript and appreciating our work. All the comments raised by the reviewer are addressed below one by one with reviewer's comment appearing in regular font and our reply in bold font characters.

1) The manuscript falls short of showing that WRF-Chem contributes significantly to improve MOZART global scale (and resolution) results. The authors state (page 17, lines2-4) that "the performance of WRF-Chem is better than MOZART". This is not clear at all from all the figures presented in the manuscript. Within the manuscript the

authors speculate in some parts (for example lines p.17, lines4-9 and 18-21) about possible reasons for differences between MOZART and WRF-Chem but do not investigate any further. While it is clear that regional models such as WRF-Chem are needed for smaller scales (for example urban) since the coarse resolution of a global model does not allow for resolving relevant topographic/meteorological/emission features, the results presented in this manuscript may be cause for concern that this is the case at the continental scale over South Asia. In fact, based on figures 4-6, I would argue that MOZART performs slightly better than WRF-Chem at quite a few stations. I really believe that the WRF-Chem evaluation is valid only if the authors can show that the model outperforms the global model.

In the revised manuscript, Figure 4 has been modified (with absolute values of ozone) to illustrate better performance of WRF-Chem over MOZART. These figures now depict the comparison of WRF-Chem, MOZART and observations in terms of absolute mixing ratios instead of deviation from the mean values. It is clear from Figure 4 that WRF-Chem is better than MOZART at reproducing summertime lower ozone levels at all the sites except Nainital. It is already discussed in the manuscript (and shown in the supplemental material) that WRF-Chem can also simulate summertime low ozone at Nainital if a higher resolution is employed. WRF-Chem is also much better at capturing surface ozone variations from September to December at all the sites.

2) Following the first point, I would encourage the authors to compare the equivalent of figure 17 but with MOZART results. The question of the rather odd seasonal variation (spring ozone concentrations lower than in autumn and winter) is whether it is a WRF-Chem feature, a feature of the (MOZART) boundary conditions or emissions. Again, I think it is important to address this question before evaluating WRF-Chem.

As suggested, the equivalent of Figure 17 from MOZART results are compared with WRF-Chem and shown as Figure 18 in the revised manuscript and discussed in Section 4.5. We would like to mention that, the seasonal variation of ozone over the Indian region is different from those typically observed over North America and Europe. Several observational studies (e.g. Lal et al., 2000; Naja and Lal, 2002; Naja et al., 2003; Nair et al., 2002; Beig et al. 2007; David and Nair, 2011) have reported a decrease in surface ozone levels from winter to spring over the sites located in western and southern India. In contrast, surface ozone observations from Northern India (Kumar et al., 2010) show an increase in ozone levels from winter to spring. These observational features have been reproduced qualitatively well by WRF-Chem. The decrease in ozone over southern and western parts of India could be due to change in wind patterns. It can be clearly seen from fig. 17 that near surface winds blow from land to ocean during winter (January) while they reverse to onshore during spring (April). The reversal of winds is also evident from the increase in water vapor mixing ratios (Figure S3) over all the regions except North India. Therefore, it is suggested that mixing of continental air with cleaner marine air masses might be reducing ozone levels during spring.

3) I would also encourage the authors to use the potential of an online model that WRF-Chem offers. As the authors correctly state, there are model studies over South Asia which use the offline approach. These offline models "may miss important information about short-term atmospheric processes due to inherent decoupling of the meteorological and chemistry components". However, on page 16, lines 9-11, the authors only speculate on the impact of the online approach on better model results. Apart from not being very convincing (the offline approach also allows for photolysis reduction due to clouds), the questions on these "important atmospheric processes", how an online model like WRF-Chem deals with them and how the model results contribute to a better understanding of the overall atmospheric characteristics would be an appropriate approach for GMD but are not being addressed by the manuscript.

We really appreciate the suggestion of the reviewer to explore the potential of an online model. But we feel that and as the reviewer also correctly states (next point) that this idea is worthy of a separate study, this is not attempted in the present manuscript.

4) Finally, to include all three of the above mentioned points might be too ambitious for one publication. My suggestion would be to include points 1) and 2) and take out any

speculations on the impact of the online approach.

Thanks, Now, the speculations on the impact of online approach have been taken out from the Introduction section in the revised manuscript.

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