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Geoscientific Model Development 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.

Anonymous Referee #2

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The manuscript is basically an evaluation of the WRF-Chem model over South Asia. Aiming at establishing the model's credibility, the evaluation has been carried out very thoroughly and indeed would be a good basis for model users to refer to when applying the model over the region. The manuscript is generally well presented and its results are scientifically sound. However, for the manuscript to be within the scope of GMD I would strongly recommend to include the following points:

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, lines 2-4) that "the performance of WRF-Chem is better that MOZART". This is not

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clear at all from the figures presented in the manuscript. Within the manuscript the authors speculate in some parts (for example lines p.17, lines 4-9 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 that 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 that WRF-Chem at quite a few stations. I really believe that the WRF-Chem evaluation is only valid if the authors can show that the model outperforms the global model.

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.

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.

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.

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