

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 #3

Received and published: 28 February 2012

This paper provides a detailed evaluation of the WRF-Chem model for a region which is under rapid development with large increases in emissions. It can be challenging to model the atmospheric chemical composition in South Asia, e.g. due to strong convection during the monsoon season, and this region is among the least studied Asian regions. This study is a valuable contribution since it provides a thorough evaluation of the model performance in South Asia. The paper is generally well written, and I recommend the manuscript to be accepted, but some points should be addressed before publication.

First, the results presented in the paper do not give the impression that WRF-Chem

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



performs better than the global MOZART model. Although the authors claim that WRF-Chem performs better than MOZART, the reasoning is rather weak, and this should be looked into further. The other comments are to the most part minor and are presented as specific comments below.

Specific comments:

Page 6: Line 9: Which year of RETRO emissions was used? I assume year 2000 emissions were used since this is the latest year available. However, significant increases in anthropogenic emissions have most likely occurred in this region between 2000 and 2008. Have you used any scaling of the emissions to take this into account?

Line 10-11: Which regions are not covered by the INTEX-B emission inventory, and what year are these emissions from? This information should be specified.

Page 7: Line 8: Why were the biomass burning emissions emitted in the lowermost layer? In reality, lifting of the plume normally causes a large fraction of the BB emissions to take place at a much higher altitude. When everything is emitted at the surface, this could have large impacts on the results (e.g., a larger fraction of the emissions will be “trapped” within the boundary layer). This issue could easily have been dealt with since WRF-Chem includes a plume rise parameterization (see Freitas et al. (2007), ACP).

Line 25: Please specify whether the chemical boundary conditions are from climatology or real-time (e.g., updated every 6 hours).

Page 15: Line 9: In order to evaluate any over/underestimation of modelled O₃, it would be better to show in Figure 4 the absolute monthly mean ozone values instead of (or in addition to) the difference from the annual mean. Furthermore, as MOZART model results are also shown in the figure (and later explained in the text), it would be appropriate to include a very brief description of the setup of this model (e.g., resolution, emissions, simulation year), either here or in the method section (Section 2). I cannot

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



see that this has been done.

Page 16: Line 1: I doubt that wet scavenging has a significant impact on ozone levels on these scales. In fact, is wet scavenging of HNO₃ etc. included in these simulations? If this is a process which is important for the chemistry it should be mentioned in Section 2 (i.e. which wet scavenging scheme that has been used). My feeling is that the impact of cloudy conditions on photochemistry is probably more likely to suppress ozone production than the impact of rain on wet scavenging.

Line 9: How may the online treatment of meteorology and chemistry in WRF-Chem improve the results? Please explain.

Page 17: Line 6: What resolution has been used in the MOZART simulations?

Line 16: Suggest replacing “NO_x simulation” with “NO_x results”.

Page 19: Line 25: Remove “of”.

Page 22: Line 23: Could the over/underestimation of CO in WRF-Chem have to do with plume rise being neglected for the BB emissions, or lack of vertical distribution of the anthropogenic emissions?

Line 25: Remove “therefore”.

Line 26: It would be useful to compare the FINN emissions with GFED (Global Fire Emission Database) for this region.

Page 25: Line 28: Soil emissions of NO should be included in the MEGAN routine. Please check this.

Page 30: Line 23: Replace “is” with “are”.

Page 32: Line 16: Could probably also have to do with uncertainties related to the seasonal variation of anthropogenic emissions. It also seems that diurnal profiles and vertical profiles of anthropogenic emissions are not taken into account in this study,

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



and this can have an impact on modelled NO_x concentrations / NO₂ trop. columns.

Interactive comment on Geosci. Model Dev. Discuss., 5, 1, 2012.

GMDD

5, C28–C31, 2012

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

