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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.

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Reply to the comments of Anonymous Referee #3

We thank the reviewer for careful and thorough evaluation of our manuscript. 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.

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

Now, the revised manuscript shows Figures 4 and 5 in terms of absolute ozone mixing ratio (rather than deviation from the mean values), which clearly shows that summertime lower ozone values are reproduced better in case of WRF-Chem than MOZART (Figure 4). WRF-Chem also better captures surface ozone variation from September-December. We have also added a plot of MOZART surface ozone spatial distribution (Figure 18) in the revised MS. The comparison of WRF-Chem and MOZART surface ozone distribution shows that WRF-Chem is better at resolving the small scale features.

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?

Yes, latest emissions from RETRO are representative of the year 2000. RETRO emissions cover only a small part of our domain (regions west of Pakistan). This has been clarified on page 6 of the manuscript.We have not applied any scaling factor to the RETRO emissions. However, we did perform a short sensitivity simulation with higher NOx emissions (section 4.2.3) to examine whether estimated increased emissions would explain discrepancies between modeled and OMI-retrieved NO2 column amounts. This has been discussed in section 4.2.3.

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.

INTEX-B inventory do not cover the countries west of Pakistan and emissions in this inventory are representative of the year 2006. Now, this information has been added to the revised manuscript.

Page 7: Line 8: Why were the biomass burning emissions emitted in the lowermost

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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).

We agree with the reviewer that emissions from the fires can reach to higher altitudes. However at the time of simulations for the present work, the coupling of FINN biomass burning emissions to the plume rise module in WRF-Chem (V3.1.1) was under development. Now, FINN can be used with plume rise and we have conducted a sensitivity simulation with inclusion of plume rise in the model. The sensitivity simulations are conducted for the month of April since South Asian region experience wide spread biomass burning activity during April. It is seen that the inclusion of plume rise increases tropospheric column CO and NO2 by 10-40% and 10-50% respectively over biomass burning regions in India. The increase is more than 100% over some parts of Burma where biomass burning is most intense. This is now mentioned in the revised manuscript (section 4.2.2 and 4.2.3).

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

The chemical boundary conditions are real-time and are updated every 6 hours. Now, this is specified in the manuscript.

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

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see that this has been done.

Figure 4 is now shown in terms of absolute concentrations instead of difference from the annual mean. A brief description of MOZART has also been included in the revised manuscript in section 2.

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.

Yes, the wet scavenging of trace species is not included in the simulations. This sentence was written to mention that wet scavenging of HNO3 and other species could contribute to observed low summertime ozone over this region. However, this part has been rewritten in the revised manuscript to avoid any confusion.

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

The online treatment of meteorology and chemistry would capture the atmospheric processes occurring typically at the time step of less than an hour such as winds, rainfall and cloud formation at every model time step (3 min in the present case) which is not the case in offline models where meteorology is updated once or twice per hour. During summer/monsoon season when South Asian region is mostly covered by the clouds, WRF-Chem is expected to be better at capturing cloud cover induced changes in solar radiation and thus ozone photochemistry. It can be seen that summer-monsoon time observed surface ozone levels are in better agreement with the WRF-Chem when compared with the MOZART. Please note that statement on online treatment has been removed from the revised manuscript following the suggestion of reviewer #2 to remove the speculation of online model impact.

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

MOZART simulations are conducted at the horizontal resolution of 2.8o x 2.8o. This is now mentioned in the revised manuscript.

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

This is done in the revised manuscript.

Page 19: Line 25: Remove “of”.

Thanks. This has been removed.

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?

Yes, the absence of plume rise parameterization may contribute to the model-observation discrepancy. Please see response to specific comment #3 also.

Line 25: Remove “therefore”.

This is removed now.

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

It has been shown that FINN (V1) emissions are in good agreement with GFED (V3.1) database [Wiedinmyer et al., 2011]. This is now mentioned in the revised manuscript (section 2, page 9).

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

We apologize for the mistake. MEGAN calculates soil NOx emissions but these emissions estimates are highly uncertain. Now, we have revised the manuscript accordingly (section 4.2.3).

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

Thanks. This is replaced as suggested.

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

Yes, we agree. These factors may also contribute to the large differences between model and observations during spring and are now mentioned in the revised manuscript (section 4.2.3).

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