

Interactive comment on “Simulation of trace gases and aerosols over the Indian domain: evaluation of the WRF-Chem model” by M. Michael et al.

M. Michael et al.

mary@iitk.ac.in

Received and published: 28 May 2014

Response to the suggestions of Referee #1

General Comments: This paper focuses on the evaluation of the WRF-Chem model performance primarily in northern India. Most of the paper concentrates on aerosol comparisons between the model and AERONET, lidar and in situ measurements. The pre-monsoon months of May and June for three years (2008-2010) are studied. The authors repeatedly use the phrases ‘good agreement’ and ‘quite well’ in Section 5.3.1 in describing the comparisons between model simulations and measurements of aerosol parameters. However, the statistics the authors present in the paper (Table 4) show that there is little to no correlation between the modeled aerosol optical depth (AOD) and the AERONET measured values at several sites, with the percent of variance explained

(R2) ranging from only 1% to 27%, with most sites having less than 15% of variance explained. Therefore the authors do not make a convincing argument for the model performance being 'good' except for somewhat groundless claims with seemingly misleading words. If the correlation statistics the authors present do not matter then they failed to explain this, nor did they present other metrics to support their conclusion of good agreement. The simulated AOD is even anti-correlated with the measured AOD at some sites ($R^2 = -0.19$ at Ghandhi College) yet the authors fail to mention this in the text. An equally serious shortcoming of this paper is the lack of analysis regarding other noted issues such as the 40% underestimation of simulated AOD in the Indo-Gangetic Plain. In the context of these large underestimations there is no analysis or discussion on what factors are most likely to be responsible, such as emission data bases, simulated gas-to-particle processes in the model, RH and cloud effects on aerosols in the model, or other meteorological issues in the simulations. Therefore the analysis presented in this paper provides no information on how it might be best to proceed in order to improve the current aerosol simulations in this region. Based on these issues I cannot recommend this version of the paper for publication in GMD.

Ans. The ms will be re-structured and more data will added to the final ms.

Other aspects of the paper where I noted problems or issues that need to be addressed are discussed below in the Specific Comments.

Specific Comments: Page 436, lines 23-24: In Figure 1 please define AWS station in the caption. Also, please note that surface albedo is not static since it varies from dry winter to wet monsoon seasons primarily as a result of vegetation (crops and natural) phenology.

Ans. Will be addressed.

Page 437, line 8-19: Please provide a clearer explanation why emissions for 2006 were utilized to simulate gases and aerosols for different years (2008-2010).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Ans. Will be addressed.

Page 439, line 1-12: Table 2 should be Table 3 in line 5 and this is a table of AOD statistics, not details about the stations. Additionally, on line 3 you say that AOD data were observed at various AERONET stations at 550 nm. However, AERONET does not measure AOD at 550 nm, the data for the closest wavelengths to 550 nm are available at 500 nm and 675 nm. You need to interpolate to the study wavelength of 550 nm (by using Angstrom exponent or more accurately the 2nd order fit of AOD versus wavelength in logarithmic coordinates), and you need to provide details of the interpolation method in this paper.

Ans. Will be addressed.

Page 441, line 12-14: Figures 1a and 1 b should be 2a and 2 b.

Ans. Will be modified.

Page 442, line 7: Figures 2a and d should be 2a and d. Also please mention that slope of RH here is far off with larger bias at higher RH.

Ans. Will be modified.

Page 442, line 17-18: Please explain the auto-correlation in the simulated data in more detail, as this is not very clear.

Ans. Will be discussed in more detail.

Page 446, lines 4-5: You say that in Figure 8 the AOD at Kanpur is mostly produced by anthropogenic activity. However, you provide no evidence for this. In fact for the months of May and June the AOD is dominated by coarse mode dust at this site (see Eck et al., 2010) largely from natural sources in arid lands to the west. Additionally, throughout this section you mention coarse mode aerosols yet provide no quantification of the breakdown of the AOD into fine and coarse mode components. It would be useful to analyze the AERONET product of Level 2 fine and coarse mode AOD at 500 nm

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

from the Spectral Deconvolution Algorithm (SDA), and compare these to modeled fine and coarse mode AOD. It would also be useful to provide some measure of relative particle size in the tables and text, such as Fine Mode Fraction (FMF) from SDA or the Angstrom exponent (440-870 nm).

Ans. More detailed discussion will be included.

Page 447, lines 8-9: Please explain how you can say that WRF-model simulated the locally generated aerosols well at Jaipur when the correlation with AERONET AOD is so poor ($R^2 = -0.08$). This is very unclear, and if correlation does not matter then please explain why, and also provide some other metric to support your claim of good agreement.

Ans. Will be discussed in more detail.

Page 448, lines 6-8: With only 8 days to compare then there is not a significant sample size and should not be included in the paper.

Ans. There is more data for the month of May and 8 days of data is available for June. As we are making the evaluation for 2 months, I request you to let us include this figure for completeness.

Page 449, lines 6-8: It is not acceptable to compare simulated AOD at 550 nm with measurements at 675 nm. It is easy to interpolate AERONET measured AOD to 550 nm (see comment above regarding Page 439), and in fact this is always done in the refereed literature.

Ans. The interpolated data will be used.

Page 449, lines 13-19: This section on vertical profiles is much too brief. Either expand upon it or delete from the manuscript.

Ans. Will discuss in more detail.

Page 449, lines 24-26: Please explain why the averages of simulated and measured

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



BC agree well in 2009 yet show poor agreement on 2010.

Ans. Will analyze more and be discussed in the paper.

Page 450, lines 23-25: The symbols for the observed and simulated values are the same in Figure 17a making it very hard to interpret.

Ans. Will be modified.

Page 451, lines 9-11: Please discuss possible reasons for the poor simulation at altitudes 3 km.

Ans. More discussion will be added.

Page 452, lines 7-8: I disagree with this statement that you claim: "It is shown that the transported dust is not captured well by the model." I argue that you have not really shown this in the paper since you did not compare coarse mode AOD with simulated coarse mode AOD in the text or figures.

Ans. More analysis will be included.

Interactive comment on Geosci. Model Dev. Discuss., 7, 431, 2014.

GMDD

7, C652–C656, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper