

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

### **Anonymous Referee #2**

Received and published: 16 April 2014

This manuscript presents the application of the WRF-Chem model to an inter-annual (2008, 2009, and 2010) simulation over India. Model is evaluated for pre-monsoon seasons by applying a wide set of experimental data, ranging from ground-based observations to vertical profiles. In particular, authors performed a deep investigation of aerosol optical properties (AODs) by comparing model results to AERONET Lidar measurements.

I agree with all the comments of Anonymous Referee #1 and I believe that he/she gave a very nice review and lots of helpful suggestions. In addition to his/her recommendations, I would propose a considerable restructuring of the manuscript.

In the results section authors often used phrases like “good agreement”, “very well”, etc. even though the correlation between model and observations is poor or sometimes

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they are anti-correlated. Moreover, authors do not make any convincing debate on the model performances being “good” except for some isolated cases.

They do not show any useful conclusions on the influence of model parameterizations (included or not in the simulation) on simulated results. Indeed, authors generally do not prove the sources of model deficiencies (e.g. temperature overestimations/underestimation, RH underestimations/overestimation, over prediction of ozone, under prediction of AODs etc. etc.), but they simply commented model results. When the agreement is poor, please explain why with convincing argumentations.

Furthermore, the analysis of the evaluation of the results is too descriptive, while it should be more technical. Authors illustrate how model performs for the selected species without citing and describing performance indicators in the text (e.g. MB and RMSE), but constantly repeating “good agreement” or simply referring to the Tables without explaining them.

From the format point of view, authors should make efforts to highly improve the format of the article (see list of technical corrections).

In this sense, I recommend the publication of this work only after the authors provide a deep discussion of the results, and they improve the format of the paper (typos, acronyms, tables, references, etc.).

Technical corrections and comments:

Page 433 line 22: Jai Devi is the right name. Here and everywhere else.

Page 434 line 4: There are two different versions of WRF-Chem found in the manuscript. The model is defined “WRF-Chem” in the abstract and “WRF-chem” here. Please choose one of these versions.

Page 434 line 6: Giorgi et al. (2012) is missing in the references.

Pages 436-437: You did not mention the initial and boundary conditions for chemistry.

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Please provide information about these.

Page 436 line 13: Jai Devi et al. (2013) is missing in the references.

Page 437 lines 8-25: What about natural emissions (e.g. dust, biogenic volatile organic compounds, etc.)? Did you include them in the simulation? WRF-Chem has on-line dust, sea salt and biogenic emission modules. Did you activate these modules in your simulations?

Page 437 line 4: Jai Devi et al. (2012) is missing in the references.

Page 437 lines 13-14: Please explain the reason of using 2006 emission inventories for other simulation years (2008, 2009 and 2010).

Page 439 line 9: Giles et al. (2012) is missing in the references.

Page 440: RMSE and R2 are used in the text, but they are not defined here. Please provide formulations.

Page 440 lines 22-24: I disagree with this statement. WRF-Chem provides instantaneous outputs at the user-defined timeframe (e.g. hour, day, month, season or year). In this sense, WRF-Chem cannot simulate averaged parameters over long timeframes.

Page 441 lines 16-23: I suggest a restructuring of the text, I found it a bit repetitive. You constantly use the word “agreement”. Furthermore, you do not describe this “good”, “not very good” or “similar” agreement in text, but you simply state it. How can you affirm there is good/poor agreement? Since you are referring to the spatial pattern distribution, please describe similarities or differences of the space plots in the text, and prove this agreement with some metrics.

Page 441 lines 16-18: At the beginning you state that “parameters show a good comparison over land” with ECMWF data, while immediately after you sustain that “RH is not in very good agreement with ECMWF data”. I think it’s a bit misleading.

Page 442: Please explain the possible reasons of model biases for temperature and

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RH.

Page 442 lines 7-12: Please substitute Figure 2 with Figure 3. Here and everywhere else.

Page 442 lines 22-23: Please explain the statement “exist an auto-correlation in the simulated data”. It is not clear in the text.

Page 443 line 26: Please comment Table 2. Here and everywhere else.

Page 444 lines 13-15: Quantify the positive bias of ozone mixing ratio with some metrics. Moreover, please explain the possible sources of model overestimations.

Page 444 line 27: Carbon monoxide is under-predicted in June, it is not over-predicted.

Page 445 line 10: Since SO<sub>4</sub> is satisfactorily simulated by the model, can you give some indications on SO<sub>2</sub> over-prediction?

Page 446 lines 12-22: It can be useful a comparison to the Angstrom exponent (440-870 nm) in order to verify the presence of coarse particle (dust) in the measured values, and whether this presence can be connected to the model under-predictions.

Page 446 lines 19-20: You attribute the model under-prediction to the dust module used in the MADE/SORGAM aerosol scheme, but you didn't mention the activation of this module in the text.

Page 447 lines 8-9: There is scarce correlation between model and observed data in Jaipur ( $R^2=-0.08$ ). Moreover, from June 8th to 14th 2009 model simulates quite high AOD values (higher than 1), while AERONET data are lower than 0.5. Please explain how you can say that “WRF-model simulated the locally generated aerosols well” with other performance indicators that support your statement.

Page 447 lines 27-28: Once again I think you can verify the presence of dust in measurement data looking at the Angstrom coefficient and comparing it with model results.

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Page 448 lines 24-28: Please try to prove these statements with Angstrom coefficient comparisons.

Page 449 lines 13-19: The vertical profile section is a bit too succinct. If you can't go into depth on the matter, please remove the section.

Page 451 lines 1-9: You repeat "good agreement" twice, but you don't demonstrate this agreement with performance indicators.

Page 452 lines 11-19: I suggest a restructuring of this part of the conclusions, avoiding statements like "good agreement", "well captured", "simulated well", etc. while including some metrics. Furthermore, in the "conclusions" section please furnish some useful general indications on the sources of model biases.

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

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