**Atmospheric
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Discussions

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Interactive comment on "Effects of dust aerosols on tropospheric chemistry during a typical pre-monsoon season dust storm in northern India" *by* R. Kumar et al.

Anonymous Referee #2

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The manuscript by Kumar and co-authors tempted to investigate the impacts of a pre-monsoon season dust storm event on the regional tropospheric chemistry over India. In order to achieve their goal, they further developed the WRF/Chem model by incorporating a number of heterogeneous chemistry reactions and considering the effects of dust particles on photolysis rates of major gas species. They found by including those model treatments, the discrepancies between model and observations can be reduced. The paper also examined the impacts of various model treatments such as RH on heterogeneous reaction rates and aerosol coating effects. The topic of the work is of scientific interests to the journal and contributes to the scientific understanding of

the complex interactions between gases and aerosols in the regional model. However the following comments/suggestions need to be addressed before it can be accepted for publication.

Major comments:

One of my major concerns about the model treatments is the neglect of products of the heterogeneous reactions especially for those are associated with nitrogen and sulfur species. There are many recent studies including laboratory experiments, field measurements, and global/regional modeling studies (e.g., Tang et al., 2004; Fairlie et al., 2010; Li et al., 2010; Manktelow et al. 2010; Li et al., 2012; Wang et al. 2012) that have found the significant impacts of dust associated heterogeneous chemistry on the formation of aerosol nitrate and sulfate especially over Asia (with very high anthropogenic SO2 and NOx emissions and frequent dust storms), which in turn can modulate the aerosol thermodynamics. The authors did a few sets of hypothetical simulations assuming the dust particles coated with nitrate and sulfate, however those simulations can be done better by explicitly adding the aerosol products or aerosol precursor gases such as HNO3 into the model treatment. This limitation needs to be discussed or be addressed through sensitivity simulations.

Another concern is about the approach to determine the tuning factor C in the dust emission equation. A more conventional way to set its value is to conduct a set of sensitivity simulations by matching the total generated dust emissions with dust climatology data. I can understand that the dust climatology data over the selected domain might not be readily available or requires more work to obtain. This study tries to constrain the value of C by comparing the simulated AOD and Angstrom exponent with AERONET data, which may introduce additional uncertainties to the estimated dust emissions since those optical properties are also heavily affected by anthropogenic aerosols besides dusts. This limitation should be discussed somewhere in the paper. In addition, the simulated AOD/ Angstrom exponent should not be evaluated against the same set of AERONET data anymore since they are anyway highly correlated be-

cause of the approach taken in this study.

The authors evaluated the model results by using NO2 column data from the OMI instrument, which also measures O3 and SO2. Both species are highly related to this study. I would suggest adding the evaluation of those two species to make the evaluation more solid. The evaluation also falls short with the lack of aerosol comparisons (by using observed dust or even PM2.5 or PM10 concentrations). If those data are indeed not available, an acknowledgement that such a comparison has not been done is needed.

Specific comments:

Page 1116, line 22-26: I don't think this re-noxification process is a recent finding, since it has been reported in much earlier studies such as Knipping and Dabdub (2002).

Page 1117, line 4: for "absorbed phase", should "aerosol phase" be more accurate?

Page 1118, Section 2: More information should be provided in this section regarding WRF/Chem model configurations (e.g., model inputs and physical/chemical schemes) and dust emissions schemes in this section since this is standalone paper.

Page 1118, line 8: need some details regarding this bulk aerosol scheme such as the aerosol size distribution, aerosol speciation, and major aerosol processes such as condensation and aerosol thermodynamics etc.

Page 1118, the description of dust scheme, line 15: how is S determined? line 18-19: the formula for ut should be provided; line 19: Are there any physical meaning of C or is it simply a tuning factor? A factor of magnitude difference seems to me to too very large.

Page 1119, line 1-5: A brief description regarding all those information should be provided, since this is a standalone paper.

Page 1119, line 19-20: How about aerosol nitrate and ammonium? Are they treated

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at all? Such information is expected when mentioning GOCART bulk aerosol module earlier.

Page 1125, line 7: how is gamma determined for this Dust_JH_NO3_SO4 simulation?

Page 1126, Section 3.2: Are level 2 or level 3 retrievals used?

Page 1128, line 11: any reasons for the increase of NOy during the indicated time period? Doesn't dust storm supposed to decrease the total NOy?

Page 1134, Section 4.5: The paper examined the effects of RH on gamma values, which is not commonly considered by other regional models and the effort is appreciated. However, the gamma values associated with various RH values in this study differs around one order of magnitude or less, which is generally less than uncertainties of gamma values reported by literature. A set of sensitivity runs by using lower and upper bounds of gamma values would be more of scientific interests and can provide a range of estimate of impacts of dust associated heterogeneous chemistry on regional chemistry over the modeling domain.

Page 1139, line 22-23: how about measurements of aerosols?

Fig. 10: what about the decrease and increase over northeastern corner of the domain in both NO3 and SO4 coating simulations? This needs to be clarified.

Technical notes:

Page 1122, line 5: H2 should be HO2?

Page 1125, line 5: 70% in?

Page 1130, line 17: the largest. There are other similar typos and should be fixed.

Page 1130, line 20: I suggest using O3 to denote ozone throughout the text to be consistent with other gases.

Page 1131, line 25: likely less

Page 1134, line 10: double check the range, I think it should be in the range of 70-90%.

Page 1135, line 14: Dust_JH_NO3.

References:

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Knipping and Dabdub (2002), Atmos. Environ., 36, 5741-5748.

Li et al. (2010), Atmos. Chem. Phys., 10, 463–474.

Li et al. (2012), Atmos. Chem. Phys., 12, 7591–7607.

Manktelow et al. (2010), Atmos. Chem. Phys., 10, 365–382.

Tang et al. (2004), J. Geophys. Res., 109, D19S21, doi:10.1029/2003JD003806.

Wang et al. (2012), Atmos. Chem. Phys., 12, 10209–10237.

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