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**Geoscientific
Model Development
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.

R. Kumar et al.

manish@aries.res.in

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Reply to the comments of Anonymous Referee #1

We thank the reviewer for careful and thorough evaluation of the manuscript. All the comments raised by the reviewer are addressed below one by one with reviewer's comment appearing in the regular font and our reply in the bold font characters.

1. P. 15 & Fig. 4 Ozone seasonality plots should be provided not only in the form of ΔO3 but also in the absolute concentrations. It is very informative if the model gives the characteristics of either overestimate or underestimate of O3 mixing ratio. Although there are not many papers showing the comparison between the model output with

observational data in India. Ellingsen et al., (Atmos. Chem. Phys. Discuss., 8, 2163-2223, 2008) shows much of overestimate by global model for N. India. Since this issue is also related to the suspected underestimation of NOx emission as compared to the satellite data discussed later, some discussion is necessary here on the comparison of absolute concentration.

We have replaced these deviation plots with the absolute value plots following suggestion from the reviewers. Additionally, it can also be noted that the absolute value plots are better at illustrating superiority of WRF-Chem over MOZART. In the former version of the MS, We had used ΔO3, ΔCO and ΔNOx as the time periods of some of the observations were different.

2. P. 15-16 Although it is evident that the summer low of ozone in India is mainly due to the intrusion of the pristine marine air just as same as in East Asia, the problem here is that the discussion is too much focused on cloud and rainfall as a factor of reducing ozone mixing ratio. Even though cloud would reduce the local photochemical ozone production in wet season, it is not the major reason to give low ozone either at coastal and inland sites, and the major reason is the long-range transport of oceanic air. It should be noted that "rainfall" repeatedly mentioned in the manuscript as a cause of low ozone due to suppressing photochemical production is not correct and suggested to be removed. It has been well established that ozone itself is not scavenged by rain at all, wet scavenging of NO2/HCHO would not be very effective to affect O3, and the wet removal of HNO3 is not sensitive to O3 concentration since photolysis rate of HNO3 is too slow to affect local O3. If the authors are interested in, it may be worthwhile to evaluate the impact of wet scavenging of NO2/HCHO and HNO3 on local O3 concentration.

Yes, we agree that lower summertime ozone values over the Indian region are mainly due to long-range transport of ozone-poor marine air-masses with some contribution from reduced photochemical production and wet scavenging of ozone precursors. Now, the MS text has been revised to present it more clearly. We would also like to men-

tion that the present simulations did not include washout of precursors. Therefore the reduced ozone in the model is only due to transport and reduced photochemistry. We feel that the evaluation of the impact of wet scavenging of NO2/HCHO and HNO3 on local O3 concentration can be a separate study in itself and is therefore not attempted here.

3. P. 17 and Fig. 5 Although it is mentioned that "The variations in MOZART CO and NOx values are similar to WRF-Chem except for NOx variations at Mt. Abu", Fig. 5 shows, the agreement between MOZART and WRF-Chem for NOx at Mt. Abu is as good as others. Instead, WRF-Chem ΔNOx in January is unusually high. If the data is correct, what gives the anomaly? Further, discrepancy between the WRF-Chem data giving much higher values in February-April at Gadanki, and observations/MOZART should be discussed in the text.

We thank the reviewer for pointing this out and apologize for the mistake made in preparing ΔNOx plot (WRF-Chem) in Figure 5 where we erroneously plotted WRF-Chem Mt Abu data for Gadanki and vice-versa. Now, this figure has been changed to absolute values plot (Figure 5) and now we can clearly see differences in WRF-Chem and MOZART NOx variations at Mt Abu, and do not see much higher WRF-Chem values at Gadanki during February-April. The WRF-Chem NOx is found to be unusually high for Ahmedabad and not for other sites used for comparison. Analysis of the INTEX-B anthropogenic emissions showed that NOx emissions for Ahmedabad in the model are unusually high during January ($\sim$120 mol km-2 hr-1) as compared to other months (20-30 mol km-2 hr-1). Now, this is mentioned in the revised manuscript (page 21).

4. P. 23-27, Fig. 13, 15 & 16 Underestimate of NO2 tropospheric column density by WRF-Chem against OMI and GOME-2 by a factor of 2-3 (up to 5) is puzzling. If it is real and could be ascribed to the underestimate of NOx emission, it is interesting to see how much O3 concentrations would be increase by the increase of NOx emission say by a factor of 2. In this regard, model comparison with surface O3 data in absolute

concentrations is interesting and should be mentioned as noted in comment 1 above, although comparison with TES retrieved O3 is presented in Fig. 8 in surface-500 hPa.

We analyzed the differences between WRF-Chem and satellite (OMI and GOME-2) retrievals again. The criterion for selecting the satellite retrievals for comparison with model is further tightened by removing the nighttime data. Earlier, we also added the ghost column to tropospheric column NO2 retrieval following the online documentation (http://toms.gsfc.nasa.gov/omi/no2/OMNO2_readme.pdf). The communication with OMI team at KNMI revealed that ghost column is already included in the tropospheric column NO2 retrieval from KNMI. Therefore, we revised the entire analysis presented for comparison between WRF-Chem and satellite NO2 retrievals by excluding the ghost column retrievals. It is found that most of the underestimation over the model domain was due to addition of ghost column to tropospheric column NO2 retrieval. The revised analysis enhanced the magnitude of difference between model and satellite retrievals over the Indo-Gangetic Plain region during all the seasons, while it reduced the magnitude of underestimation over other parts of the domain. However, the model still underestimates the satellite retrievals by a factor of 2-3 over most parts of the domain during all the seasons. Such an underestimation by a factor of 2-5 is not surprising and is consistent with results from previous studies over other Asian regions [e.g. Akimoto et al., 2006; Uno et al., 2007] indicating towards higher uncertainties of Asian emissions. The underestimation over the biomass burning regions (e.g. Burma) is also seen in the revised analysis. A sensitivity simulation including the plume rise parameterization revealed that absence of plume rise parameterization in the present study could be an important factor contributing to the underestimation over the fire regions. The manuscript has been revised accordingly to incorporate these revised results.

5. P. 27-31, Fig. 17, Fig. 17 shows a very peculiar spatial distribution of O3 by season in India. As discussed in the paper, summer low is reasonable, but it is very unusual that surface O3 concentration in spring is lower than winter and autumn. There is some

suspicion in the figure that O3 in Burma in spring where NOx and CO concentrations are higher than other seasons due to biomass burning (Fig. 9&13), is even lower than in winter. In winter and autumn some coastal oceanic area of India surface O3 concentrations are even higher than the inland. This is very strange since even the outflow of O3 is efficient in these seasons, O3 net production is much higher in the terrestrial area than the oceanic areas as shown in Fig. 18. Therefore, the concentration of O3 in the outflow region is expected to be lower than the source region in general. Some explanation after checking the data would be necessary.

The decrease in ozone mixing ratios from winter to spring has been reported in many observational studies over southern and western parts of India (e.g. Lal et al., 2000; Naja and Lal, 2002; Naja et al., 2003; Nair et al., 2002; Beig et al. 2007; David and Nair, 2011). In contrast, ozone concentrations show an increase from winter to spring over Northern India (Kumar et al., 2010). These observational features have been reproduced qualitatively well by the WRF-Chem model. The decrease in ozone over southern and western parts of India could be due to change in wind patterns. It can be clearly seen from fig. 17 that near surface winds blow from land to ocean during winter (January) while they reverse to onshore during spring (April). The reversal of winds is also evident from the increase in water vapor mixing ratios (Figure S3) over all the regions except North India. Therefore, it is suggested that mixing of continental air with cleaner marine air masses might be reducing ozone levels during spring. The underestimation of CO and NOx by the model during spring over Burma (Figures 9 and 13) could be the reason for lower springtime ozone there in the model. Higher ozone levels simulated by the model over oceanic region than over land are consistent with INDOEX observations (e.g. Lal and Lawrence, 2001; Stehr et al., 2002) and possible reasons for this are already mentioned in the manuscript (P. 27, l. 24-30 and P. 28, l.1-5).

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