Geosci. Model Dev. Discuss., 5, C1–C3, 2012
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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.

Anonymous Referee #1

Received and published: 18 January 2012

This is a counterpart of the first paper of the same authors with subtitle, "set-up and meteorological evaluation," and presents the validation of chemical species, CO, NO2 and O3. The paper is presenting comprehensive regional view of ozone and its precursors in South Asia. It is in general well written but there are some suspicious points, which should be cleared before the publication.

Some of the comments that would be useful for revision are as follows.

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

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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 models 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.

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.

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 this anomaly? Further, discrepancy between the WRF-Chem data giving much higher values in February-April at Gadanki, and observation/MOZART data should be discussed in the text.

4. p. 23-27, Fig. 13, 15, & 16 Underestimate of NO2 tropospheric column density by

GMDD
5, C1–C3, 2012

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 increased 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.

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 of O3 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 area 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.

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