

## ***Interactive comment on “Air quality in the Kathmandu Valley: WRF and WRF-Chem simulations of meteorology and black carbon concentrations” by Andrea Mues et al.***

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Below we address the comments of reviewer #1 and questions raised during the open discussion of the paper “Air quality in the Kathmandu Valley: WRF and WRF-Chem simulations of meteorology and black carbon concentrations”. We would like to thank the reviewer for the time and effort reviewing the paper. We feel it has improved thanks to the constructive comments. We have listed all reviewer comments below and our answers are provided in [blue](#). A “track changes” version of the revised manuscript is provided as a supplement with all changes to the manuscript highlighted.

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The paper is recommended for publication in GMD, however several comments and suggestions as listed below should be considered during the revision. Title: Air quality is used in more general context, and it is expected to have study of more pollutants, while here focus is only BC. Possibly it would be better to revise the paper title as e.g. WRF-Chem simulations of meteorology and black carbon concentrations in Kathmandu valley

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We agree with the reviewer's point and changed the title into "WRF and WRF-Chem v3.5.1 simulations of meteorology and black carbon concentrations in the Kathmandu Valley" (see also comment of the executive editor).

Page 3, l.29: what is the motivation behind preventing sea salt emissions from the small in land lakes? What is the effect on results presented, if this change is not made in the model?

In the simulations shown in this paper, we use the sea salt emission scheme recommended in the WRF-Chem v3.5.1 user's guide for the selected chemical mechanism RADM2/SORGAM (seas\_opt = 2). This sea salt scheme does not distinguish between ocean and freshwater grid cells (lakes) resulting in unrealistic emissions from lakes in the Himalayas when running the model at high horizontal resolution (at low resolution, the inland lakes are not resolved). The correction applied here is more of cosmetic nature as sea salt aerosol does not play an important role in the Kathmandu Valley.

Page 6, l.16 and section 3.1.1: How does model winds compare with the reanalysis near surface (e.g. 800 hPa)? as compared to those currently presented (500 hPa)

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We added discussion of the 800 hPa winds to section 3.1.1 (zonal and meridional wind fields) and added the corresponding figure to the supplementary material (figure S1).

Page 7, l.24-28: this should be part of data description (2.3.4), and not the evaluation metrics.

As suggested, we moved the corresponding paragraph from section 2.4 (evaluation metrics) to section 2.3.4 (radiosonde data).

section 3.1.2. The text is too general. Please discuss by using average values and standard deviations at few representative pressure levels.

We extended the discussion of the vertical profiles in section 3.1.2 focusing on some selected pressure levels.

3.1.5. and Fig. 9: if there are sufficient observational data to show the comparison separately for winter (Jan-feb) and pre-monsoon (March-June), that would be more informative. Singh et al. (Atmos. Chem. Phys., 2016) using this model found overestimation of boundary layer especially towards the pre-monsoon in northern India /Himalayas. It should be mentioned if it is similar / or different at Kathmandu.

Following the suggestion of the reviewer, we extended section 3.1.5 (mixing layer height) and now discuss winter (January-February) and the pre-monsoon season (March-June) separately. A comparison with the results from Singh et al. (2016) has also been included and figure 9 has been updated accordingly.

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3.2.2 It would be possibly useful to discuss the uncertainties among different available emission inventories of BC in this region, and that whether the modified emission flux (based on observations) is within these uncertainties or not. Sharma et al. (Atmos. Chem. Phys. Discuss., 2017, in this special issue) showed large differences among different recent inventories of pollutants over South Asian regions.

The observation-based emission estimate ranges between 64 and 248 ng m<sup>2</sup> s<sup>-1</sup> depending on the time of the year. In contrast, the BC emission flux in the Kathmandu Valley in the EDGAR HTAP emission database, which is based on the REAS data in this region, ranges between 19 and 28 ng m<sup>2</sup> s<sup>-1</sup>. The INTEX-B emission dataset gives 21 ng m<sup>2</sup> s<sup>-1</sup> for the Kathmandu Valley. Such a comparison is already discussed in Mues et al. (2017) and we think a repetition of this discussion is not needed here. Instead, we added a reference to this discussion in Mues et al. (2017) as well as a reference to the work of Jayarathne et al. (2018) to section 3.2.2 (discussion of the observation-based emission estimates for black carbon). We think the measurements of numerous emission factors in Nepal by Jayarathne et al. (2018) is an important contribution to reduce the large uncertainties in future emission inventories for this region.

Are there BC observations at other stations too in Kathmandu to evaluate further the simulation using observation-based emission flux?

Besides Bode, BC concentrations were also measured at Pakanajol, a site near the center of the Kathmandu Metropolitan City about 9km (aerial distance) to the northwest of the Bode site. The BC concentrations at both sites were found comparable in all seasons (Putero et al., Aerosol Air qual. Res., 2015; Putero et al., Atmos. Chem.

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Phys., 2018) and have therefore not been used in this study. We added this information to the revised manuscript, section 2.3.1 (SusKat-ABC field campaign).

Page 17, l.35, page 18: 1-2: Could authors show a comparison of model and observational BC concentrations separately for day and night, to indicate which sources could particularly be underestimated.

We extended the corresponding paragraph in section 4 (summary and outlook) by adding a discussion on the difference between night-time and all-day biases in the modeled BC concentration.

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