Review of "Air quality in the Kathmandu Valley: WRF and WRF-Chem simulations of meteorology and black carbon concentrations" by Mues et al.

General comments

This paper presents evaluation of the WRF simulated meteorology and WRF-Chem simulated black carbon mass concentrations in Kathmandu Valley. Air quality has been degrading rapidly in the Kathmandu Valley and thus needs immediate attention so that the public health can be protected from acute air pollution episodes. In this direction, this study takes a much-needed step and advances the community efforts by complementing intensive observations collected during the SusKat-ABC filed campaign with chemistry transport modeling. The Kathmandu Valley is a very difficult area to model mainly due to complex topography and wide range of emission sources active in the Valley. Therefore, modeling studies like this are essential to establish credibility of the models before they can be employed in design of pollution control strategies. The paper is very well written and easy to understand. However, I think this paper can benefit from a few WRF sensitivity experiments (see my first specific comment) and thus recommend major revision. My recommendation does not mean that the paper has scientific or technical flaws but it is to assure that the authors have sufficient time to perform and analyze the suggested WRF experiments.

Specific comments

I have third specific comments listed below.

First, I was surprised to see large differences between WRF and ERA-Interim wind fields (Figure 2) because WRF is driven by the ERA-Interim itself. Since the model runs are a month long, I think WRF is drifting away significantly from the large-scale forcing provided by the ERA-Interim. Thus, I suggest the authors to conduct a model experiment by nudging the WRF meteorological fields towards the ERA-Interim above the planetary boundary layer, and examine if that helps in reducing the bias. In case, the authors are not aware of the nudging option in WRF, here are the steps to run analysis nudging in WRF (http://www2.mmm.ucar.edu/wrf/users/wrfv2/How_to_run_grid_fdda.html). In addition to this, I think the authors also need to examine the sensitivity of model results to land use in WRF. The USGS land-use category used here is representative of 1994 and Kathmandu Valley has changed dramatically since then. Thus, I suggest conducting a WRF simulation with MODIS land-use. MODIS land-use is representative of 2003 but this experiment should still help us understand the sensitivity of model results to land-use representation.

Second, I think section 3.2.3 needs further detailed investigation. I believe that this event is potentially driven by open burning of agricultural crop residue in northern part of India and forest fires in Himalayas. The failure of the model to capture this event should not be attributed only to the anthropogenic emissions in Kathmandu Valley. It is important to understand the relative importance of local vs. non-local sources in this event as well as uncertainties in biomass-burning emissions. I realize that such an exercise can be time-consuming and can lead to another paper in itself. Thus, I recommend deleting this section. However, I suggest the authors to include a discussion about the potential impact of uncertainties in open biomass burning emissions and long-range transport on black carbon mass concentrations in the Kathmandu Valley.

Third, I recommend the authors to quantitatively assess the model performance by comparing their statistical metrics for temperature and wind speed against the benchmarks by Emery (2001). This is important for this paper as the focus is on evaluating the meteorological parameters that are highly relevant to air quality.

Minor Comments:

Page 1, Line 20: Change "long-term" to "extensive" because 6 months is not long-term.

Page 3, Line 8: I think it is important to state how different regional and global models have performed in simulating BC mass concentrations in South Asia. This will nicely connect the present study to literature. Here are few studies that employed regional and global models to simulate black carbon mass concentrations in South Asia [e.g., Ganguly et al., 2009; Nair et al., 2012; Moorthy et al., 2013; Pan et al., 2015; Kumar et al., 2015a, 2015b, Goverdhan et al., 2016]

Page 9, Line 28: This is probably a typo here because there is no panel corresponding to station "1206" in Figure 5.

Section 3.1.6: I suggest adding a map of the WRF and TRMM precipitation for February and May so that readers can visualize if the model is able to simulate the precipitation in right places.

Table 1: Please name the inventory used to represent biomass burning emissions.

Figure 12: Should the last legend read as "WRFchem_BC_min/max"?

References

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