Comments on "Attributions of meteorological and emission factors to the 2015 winter severe haze pollution episodes in Northern China" by T. Liu et al.

Anonymous reviewer

18 November, 2016

General Comments:

This manuscript is an interesting study of the relative roles of emission and meteorology for air pollution. Through data analysis and numerical experiments, the authors conclude that unfavourable meteorological condition was the main reason for the 2015 winter severe haze episodes in Northern China, and emission control alleviated the air pollution. In general this manuscript is well organised and provides some new insights. However, some conclusions are not adequately supported by the modelling and data analysis. Many important details (e.g. model validation and uncertainty quantification) are not presented, and some method and model results need to be further discussed. There are a few major issues and concerns that should be addressed.

1) The authors conduct numerical experiments to investigate the effectiveness of meteorology versus emission. To ensure convincing results, the well-simulated meteorology and particulate matter is pre-requisite. The model skill can be evaluated using multi-site measurements. The simulated meteorological parameters and particulate pollutants should be compared with observing data in temporal contrast, and statistical analysis is helpful for the validation. Besides, the uncertainty quantification of the results needs to be performed.

2) A large disparity was found between the mean observed and simulated PM2.5 concentration (in Table 3). How about the time and space variations of the simulated bias? What were the main sources of error?

3) The limitations and uncertainties in the approach of quantifying of meteorological contribution needs to be addressed. For example, one of the underlying assumptions for the approach is that amount and spatiotemporal distribution of the precursor sources emission are accurate; but it is well documented that emission inventory in Asia/ China could associated with considerable uncertainty. How about the emission data used in the modelling? The potential impact of the emission uncertainty on the quantification results needs to be discussed.

4) Please re-evaluate the reliability of quantifying the contribution of emission change since the indirect method may have pronounced uncertainty.

5) Traceability is important for a scientific paper. Many important details (e.g. model configuration) should be included in the manuscript.

6) How were the wind speed convergence lines (WSCL) calculated? Could model re-produce the observed WSCL? Please provide the supporting materials for the WSCL analyses.

Specific comments:

1) Abstract: The authors are advised to present the method and quantitative results in brief here.

2) Line 17: “meteorology” —> “meteorological”

3) Page 3, Line 6-8: Zhao et al is not found in the reference list. If possible, please add references on the association of haze with ENSO.

4) The authors are advised to limit the number of cited non-English references.

5) P8, Line2: In my opinion, “wind convergence line” is more accurate than “wind speed convergence line” (WSCL).

6) -, 2nd paragraph: The analyses in this paragraph need sufficient supporting materials, and the conclusions need further discussed. The association of ENSO with east asian winter monsoon and shifting of the WSCL can not be simply established in the analysis.

7) P8, L19: “Research [*Si et al*., 2016]” -> “Si et al. (2016)”

8) P9, L2: How to draw the conclusion “the cold front in 2015 could not extend to the degree as in 2014”? It is not completely reasonable. The mesoscale cold fronts are embedded in the extratropical weather systems (i.e. baroclinic waves and the associated extratropical cyclones). Usually, the cold fronts exhibit fast movement and low occurrence frequency (about 5~10 days per time) in the mid-latitude region. Hence, the cold front locations are not suitable to be monthly averaged for this analysis. As far as one case was concerned, the cold front in 2015 could extend to that degree/location.

9) PBL height is an important meteorological parameter for atmospheric transport and dispersion. The meteorological factors will be more comprehensively considered if the analysis of PBL height is added.

10) P13, L13: Please briefly address the “2010 HTAP emission inventory data” and spatiotemporal variations of “hourly-gridded data”.

11) -, L14-17: Many model’s parameters (e.g., grid number, vertical levels and model initialization) should be clarified.

12) References: Check and correct the reference format. Non-English reference should be given clear indication of the language (for example, “in Chinese”).

13) Some new papers (e.g., Wang et al., 2016; Yang et al., 2016) are closely relevant to this work. The authors are advised to cite or discuss these works.

References:

1) Wang, X., Wang, K. and Su, L.: Contribution of Atmospheric Diffusion Conditions to the Recent Improvement in Air Quality in China, Sci Reports, 6, 36404, doi:10.1038/srep36404, 2016.

2) Yang, Y., Liao, H. and Lou, S.: Increase in winter haze over eastern China in recent decades: Roles of variations in meteorological parameters and anthropogenic emissions, J Geophys Res Atmospheres, doi:10.1002/2016JD025136, 2016.