This study attempts to estimate the performance of a newly developed emission inventory, i.e., the Multi-resolution Emission Inventory for China (MEIC) in predicting aerosols by comparing observations and simulations with the default IPCC AR5 emission inventory. The authors showed that the MEIC-driven CAM5 simulations underestimated aerosol pollution over eastern China but improved the prediction of magnitudes and seasonality of sulfate, primary organic aerosol, and black carbon (BC), when comparing to the AR5-driven one. Also, their simulations indicated that the changes in aerosol radiative forcing were significant due to the difference between the two emission inventories. This work is useful for improvement of emission inventory in China and can be published, provided that the following issues have been adequately addressed.

(1) Typically, the accuracy in global chemical transport model simulations depends on emission inventory, meteorology, and chemistry. The key features in the aerosol chemistry in China are related to very efficient secondary formation (Guo et al., *Proc. Natl. Acad. Sci. USA* **111**, 17373, 2014; Zhang et al., *Chem. Rev.* **115**, 3803, 2015). Specifically, the efficient secondary aerosol processes include aerosol nucleation and rapid growth under favorable conditions (Zhang et al., *Chem. Rev.* **112**, 1957, 2012; Qiu et al., *Phys. Chem. Chem. Phys*. **15**, 5738, 2013). It would be necessary for the authors to clearly state how those processes were accounted for in their chemistry module.

(2) The modeling setups related to meteorology constrain need to be detailed and some interpretations for the modeling results should be more accurately stated. When using "constrained meteorology" mode to run the two primary simulations driven with MEIC and AR5 emission inventories (p6, lines 139-141), besides winds, have the authors also nudged temperature and moisture, which could be crucial for the gas- and aqueous-phase chemistry? From Figures 9 and 11, it seems both temperature and moisture are nudged because the temperature and relative humidity fields are identical in the two simulations. However, according to the discussions in section 3.3 about aerosol direct radiative forcing, the temperature as well as moisture fields for the two simulations should be different from each other because the surface cooling and atmospheric heating due to aerosols in the two simulations are different. How to interpret this conflict?

3) My other concern was tied up with the first one. Did the version of CAM5 model used in this study take care of the aerosol-meteorology interactions? Aerosol impacts on meteorological fields could be significant, which might further affect the aerosol pollution condition in the lower troposphere. Also, aerosol-cloud interactions might modify temperature and moisture profiles and precipitation (Wang et al., *Atmos. Chem. Phys*. **11**, 12421, 2011), leading to potential feedback on the atmospheric chemistry. Aerosol radiative effects induced by black carbon (BC) or other aerosol components could stabilize boundary layer and thus reduce the height of boundary layer, tending to exacerbate aerosol pollution near ground (Wang et al., *Atmos. Environ*. **81**, 713, 2013). A particular important aspect is the aging of BC, which considerably enhances light absorption (Khalizov et al., *J. Phys. Chem*. **113**, 1066, 2009; Peng et al., *Proc.*

Natl. Acad. Sci. USA **113**, 4266, 2016). Obviously, the aerosol-meteorology interactions cannot be ruled out when the authors attributing the source of the discrepancy between simulations and observations and the difference between the two simulations.

5) P7, lines 200-203: what's the reason to attribute the simulated AOD maximums between 35^{o}N and 40^{o}N to the transported dust aerosol? Note that in Figure 11h, the winds over the corresponding region in early summer are quite small, which does not support the long transportation of dust aerosol here. Could it be the efficient formation of sulfate over this region at this time period, based on the relatively high concentrations of sulfate (Figure 8) and relatively high gas- and aqueous-phase production of sulfate (Figure 9) at northern China sites like Chengde or Beijing?

6) I doubt the accuracy of the statement on p9, lines 254-255, saying that the MEIC emission inventory improves BC simulation relative to the case of AR5. if only looking at the BC scattering plot in Figure 7, the dots for MEIC simulation are loosely scattered in the plot comparing to AR5 case, actually suggesting that the prediction of BC by MEIC has larger uncertainty than AR5 case.

7) Since the authors employed a global climate model, it would be necessary to consider the potential impacts of climate changes on pollution conditions in China (Wu et al., *Sci. China: Earth Sci*. **59**, 1–16, 2016).

Technical corrections

1) Why there are two green lines (it's supposed to be only one) in Figure 9?

2) Figure 3 caption for (e) panel: "haft" should be "half".

3) P10, lines 273: add refs to support the statement of "this feature is commonly seen for many climate models".