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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol radiative effects and feedbacks on boundary layer meteorology and $PM_{2.5}$ chemical components during winter haze events overthe Beijing–Tianjin–Hebei region" *by* Jiawei Li et al.

Anonymous Referee #2

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The manuscript investigated the aerosol radiative effects (AREs) on meteorology and particulate matter (PM) pollution in Bejing-Tianjin-Hebei (BTH) using a fully coupled chemistry transport model. The topic is of interest and within the scope of ACP. However, there are several factors hindering publication of the manuscript at the present form.

General comments:

1) Two-page abstract is lengthy and tedious. The authors need to abbreviate it and

make it concise. I suggest that the authors put more emphases on results of process analyses. In addition, the domain average is the BTH average? I do not believe that domain average PM2.5 enhancement due to AREs is 29%, which is rather large considering that the domain covers the East Asia.

2) AREs include aerosol direct scattering and/or absorbing of incoming solar radiation and the induced adjustments of the surface energy budget, thermodynamic profile and cloudiness (direct effects and semi-direct effects), and serving as cloud condensation nuclei and ice nuclei to alter cloud properties such as cloud lifetime, reflectivity and composition (indirect effects). IPCC (2013) has use the new terminology of the aerosol-radiation interactions (ARI) to represent the combination of the aerosol direct and semi-direct effects, and use the aerosol-cloud interactions (ACI) to represent the aerosol indirect radiative effects. I am not sure whether the authors considered both ARI and ACI. Lines 309-312, they clarified that they shut off all AREs, i.e., ARI and ACI. However, ACI could not be shut off! Therefore, the authors need to clarify whether ACI were included or not. If so, which microphysical scheme is used and how to consider aerosol activation to cloud condensation nuclei and ice nuclei.

3) Model validation. Air pollutants measurement has been released since 2013 in BTH by China Ministry of Ecology and Environment. The authors only compared PM2.5 simulations with measurement at IAP, Beijing, which is not sufficient. Temporal and spatial validations of PM2.5, O3, NO2, and SO2 in BTH are necessary to warrant reasonable simulations, particularly for O3, NO2, and SO2 which are the key to sulfate and nitrate simulations. Furthermore, I am rather surprised that the authors used a two-product model to yield high SOA levels.

Specific comments:

Lines 72-73, please provide references to support your clarification.

Lines 78-79, "east China" include "north China" or vice versa?

Line 80, “haze pollution” is a little bit odd. Haze is a weather phenomenon with a horizontal visibility of less than 10 km, which might be caused by high levels of PM. Please change “haze pollution” to “PM pollution during haze days” throughout the manuscript.

Lines 88-90, please provide appropriate references.

Line 93, I do not think that Grell et al. (2005) have studied AREs on air quality and meteorology.

Line 106, “however” should read “but”.

Lines 125-128, I fairly disagree with the authors’ clarification about simulations of secondary aerosols. There still exist large gaps for SOA between simulations and observations, but models can generally well produce sulfate and nitrate.

Lines 167-169, please provide references.

Lines 167-180, which microphysical scheme is used in RIEMS-Chem? Does it consider ACI?

Lines 181-196, Does RIEMS-Chem include aerosol effects on photolysis?

Lines 197-208, how does RIEMS-Chem simulate the nitrate formation? Is the organic coating considered in calculation of N2O5 hydrolysis?

Lines 209-226, the authors clarified that they used different size distribution for inorganic, black and organic carbon aerosols. However, they also assumed completely internal mixing aerosols, which should be represented by the same size distribution. In addition, the aerosol size distribution and mixing state change considerably with development of PM pollution. The authors need at least to include discussions on uncertainties in ARE caused by variations of aerosol size distribution and mixing state.

Lines 227-231, the authors have used ISORROPIA to calculate aerosol water, so it might not be appropriate to consider the hygroscopic growth of inorganic aerosols, BC, POA and SOA.

Lines 293-298, the 60km horizontal resolution is too coarse to focus on the Beijing metropolitan.

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