Response to Comments of Reviewer #2

Manuscript number: acp-2019-306
Authors: Ruijun Dang and Hong Liao
Title: Severe winter haze days in the Beijing-Tianjin-Hebei region from 1985-2017 and the roles of anthropogenic emissions and meteorology

General comments:

The paper addresses the interannual variation of the severe winter haze days (SWHDs) in the Beijing-Tianjin-Hebei (BTH) region from 1985-2017 and the impacts of the anthropogenic emissions and meteorology on the variation. This study is of scientific importance and the research is well conducted. The paper is written with clear structure, good illustrations, and convincing discussions. The paper is subject to some issues described in the following. The authors are encouraged to consider these questions in their revision.

Response:

Thanks to the reviewer for the helpful comments and suggestions. We have revised the manuscript carefully and the point-to-point responses are listed below.

Specific Comments:

1. *It appears that GEOS-Chem cannot capture the interannual variation of SWHDs intensity well (see Figure 4f). In Figure 4f, R is missing. Is R statistically significant? How does this uncertainty affect the results in Figures 6, 11 and 12 and the associated discussion in the text?*

Response:

Thanks for pointing it out. Inspired by your comment #3, we realized that the simulated values of SWHD intensity in the previous version of manuscript were unadjusted, which were incompatible with the simulated SWHD frequencies which were adjusted according to the model biases. To address this issue, the intensity is now calculated as the average of daily mean $PM_{2.5}$ concentrations over all SWHDs during a winter by subtracting the mean bias (MB, as described in Section 2.4) from the original simulations. For example, for the grid at Baoding where the simulated wintertime $PM_{2.5}$ has a MB of -28.1 μg m^{-3}, the intensity of simulated SWHDs is adjusted as 249.6 μg m^{-3} (221.5 μg m^{-3} minus -28.1 μg m^{-3}) over 2013-2017. We have revised the related figures (Figure 4, 6, 8, 11 and 12) and descriptions in the text accordingly. In the revised Figure 4f, the correlation coefficient R between simulated and observed SWHD intensity is

0.58. The adjustment has a small impact on the results of simulated SWHD intensity in BTH (Figure 11) because the model has a small MB of 4.5 μg m^{-3} in simulated $PM_{2.5}$ concentrations over BTH.

2. *In the simulation experiments, biomass burning emissions are interannually variable from 1997-2016. The interannual variation of biomass burning is large globally and regionally, which may not be ignored. Therefore, the EMIS simulation may be driven by the interannual variations in both anthropogenic and biomass burning emissions, while the MET simulation is influenced by both biomass burning emissions and meteorology. Please explain.*

Response:

Sorry for the confusion. We did not describe clearly our treatment of biomass burning in the previous version of manuscript. The CTRL simulation considers variations in meteorological parameters, anthropogenic emissions, and biomass burning emissions over 1985-2017. In MET simulation, meteorological parameters are allowed to vary from 1985 to 2017, but anthropogenic and biomass burning emissions are fixed at the year 2015 levels. The MET simulation thus represents the impact of variations in meteorological parameters on the variations of SWHDs. In EMIS simulation, anthropogenic and biomass burning emissions are allowed to vary over 1985-2017, while meteorological parameters are fixed at year 1985 levels. The related descriptions in Section 2.3.3 have been clarified in the revised manuscript.

Even though the EMIS simulation includes variations in both anthropogenic and biomass burning emissions, the impact of biomass burning on SWHDs in BTH is insignificant. Large biomass burning events often occur in Southeast Asia and Russia in spring, which are reported to have significant impact on the interannual variations of air pollution in spring in the adjacent areas such as southern Yunnan province, northern Heilongjiang province, as well as part of Inner Mongolia. For other parts of China during winter, the impact of biomass burning is small (Mao et al., 2016; Lou et al., 2015).

3. *In the abstract, the authors stated: "the correlation coefficient between the simulated and observed SWHDs is 0.98 at 161 grids in China". This claim is based on Figure 4 that compares simulated and observed SWHD in terms of frequency and intensity. In section 2.4, the authors defined SWHDs for the observations and simulations. The simulated SWHD is adjusted according to the simulation biases. It is not clear if the simulated results presented in Figure 4 are the original simulations or adjusted values. If they are adjusted values, it should be stated there. Also, the claim in the abstract should be revised accordingly.*

Response:

The model results presented in the revised Figure 4 are adjusted values (see the response to your comment #1 for details).

To clarify, we have added the following sentence in the abstract: "Observed SWHDs were defined as the days with daily mean $PM_{2.5}$ concentration exceeding 150 μg m^{-3}, and simulated SWHDs were identified by using the same threshold but with adjustment on the basis of simulation biases."

We have also added the following sentence in Section 3.2 when we present Figure 4: "Note that, for each grid, the simulated intensity was an adjusted value according to the model mean bias (MB) calculated in Section 2.4. For example, for the grid at Baoding where the simulated wintertime $PM_{2.5}$ has a MB of -28.1 μg m^{-3}, the intensity of simulated SWHDs is adjusted as 249.6 μg m^{-3} (221.5 μg m^{-3} minus -28.1 μg m^{-3}) over 2013-2017."

4. *In generally, a moving average tends to smooth year-to-year variation. But in Figure 12, the author stated that using a "9-year weighted moving average" can reserve interannual variations in the SWHDs frequency and intensity. I wonder how this works. Why 9 years? Which climatic influence did the authors try to remove? Why to remove the fluctuations of more than 9 years?*

Response:

To extract the interannual component from the original timeseries, 7-10 years high pass Lanczos filters (Duchon et al., 1979) have been widely applied in previous studies (Salinger et al., 2001; Wu et al., 2010; Schneider et al., 2012; Chen et al., 2015; Wang et al., 2016; Liu et al., 2017; Sun and Wang, 2018; Wang et al., 2019), and among them the 9-year high pass Lanczos filter is the most common one. This filter can effectively remove the decadal variabilities of low frequency while preserve the interannual signals of high frequency. Therefore, in Figure 12 of this study, we selected the 9-year high pass Lanczos filter for obtaining interannual variations of SWHD timeseries.

5. *In the title, the term of "meteorological parameters" is not suitable. It is better to use term of "meteorology", "meteorological factors", or "meteorological variables"?*

Response:

We have replaced "meteorological parameters" by "meteorology" in the title.

6. *Figure 8, it is hard to identify a tick for a year in the x-axis.*

Response:

To make it clear, we have modified the length of tick marks in x-axis. The revised Figure 8 is displayed below.

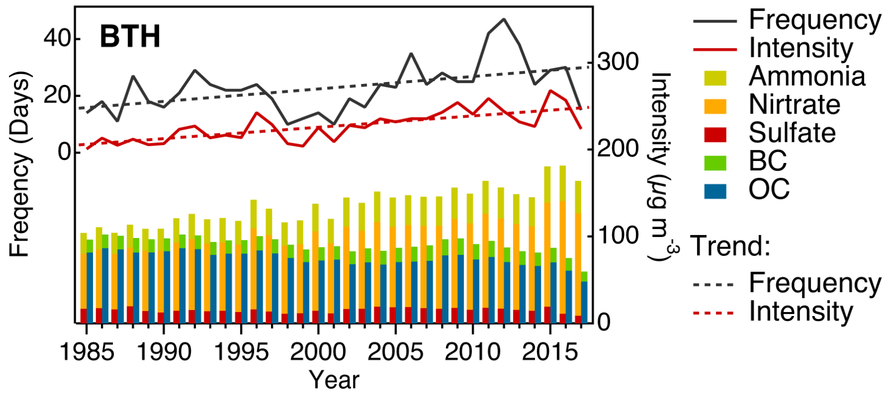

Figure 8. Simulated temporal variations in frequency (days, black line), intensity (µg m^{-3}, red line) and concentrations of $PM_{2.5}$ components (µg m^{-3}, bars): ammonia (blue), nitrate (yellow), sulfate (red), black carbon (purple) and organic aerosols (green) of regional SWHDs in BTH from 1985-2017. Also shown are linear trends (dashed lines) for frequency and intensity, which are statistically significant above the 95 % confidence level.

7. *P. 7, Line 32, replace "horizontal" with "spatial".*

Response:

Replaced.

8. *In Author contributions, delete "from all coauthors".*

Response:

Deleted.

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Thank you very much for your comments and suggestions.