Review of “Severe winter haze days in the Beijing-Tianjin-Hebei region from 1985–2017 and the roles of anthropogenic emissions and meteorological parameters” by Dang and Liao

This 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 provides an enhanced understanding on this topic. In the meantime, the paper is subject to some issues described in the following. The authors are encouraged to consider these questions in their revision.

The following are some questions for the authors to consider when revising their paper.

Major issues:

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?
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.
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.
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 SWDs 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?

Minor issues:

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”.
6. Figure 8, it is hard to identify a tick for a year in the x-axis.
7. P. 7, Line 32, replace “horizontal” with “spatial”.
8. In Author contributions, delete “from all coauthors”.