Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1145-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatological study of a new air stagnation index (ASI) for China and its relationship with air pollution" *by* Qianqian Huang et al.

Anonymous Referee #1

Received and published: 23 January 2018

Review of "Climatological study of a new air stagnation index (ASI) for China and its relationship with air pollution" by Huang et al. (MS ID: #ACP-2017-1145)

Summary: This study has improved the ability of an air stagnation index to measure the atmospheric conditions of the air pollutions over China. The result is valuable and interesting and the paper is well written. I think this manuscript can meet the scope of ACP. I recommended it to be published in ACP after the following issues addressed clearly.

Specific Comments: 1. As indicated in the stage of quick comment, I have pointed that this study was quite similar with a newly published paper in the journal of "Bulletin

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of the American Meteorological Society" (Wang et al. [kcwang@bnu.edu.cn], PM2.5 pollution in China and how it has been exacerbated by terrain and meteorological conditions, BAMS), at least, the definition of the air stagnation index. However, I have not got the available information related to the difference between them, though authors have cited this newly study. So, I still suggest the authors clarify it in the introduction. 2. It has been also pointed out in the quick comment but no reflection has been reached. The hourly PM2.5 data of Beijing in January 2013 from the US Embassy is used here. However, in general, this sort of data cannot be used in the open published paper because this monitoring is not a regularly site-observation. 3. Some newly works in this aspect should be reviewed. For example: (1) Cai WJ et al., 2017: Weather conditions conducive to Beijing severe haze more frequent under climate change. Nature Climate Change, doi:10.1038/NCLIMATE3249. (2) Yin ZC et al., 2017: Understanding severe winter haze events in the North China Plain in 2014: roles of climate anomalies. Atmos. Chem. Phys., 17, 1641-1651. (3) Han ZY et al., 2017: Projected changes in haze pollution potential in China: an ensemble of regional climate model simulations. Atmos. Chem. Phys., 17, 10109-10123. (4) Wang HJ et al., 2016: Understanding the recent trend of haze pollution in eastern China: roles of climate change. Atmos. Chem. Phys., 16, 4205-4211. 4. Some methodology explanations should be added. For example, "temperature profiles from radiosonde are linearly interpolated to 1-m vertical intervals", "Wind profile from 1200 UTC (i.e., 2000 BJT) sounding data is also interpolated to 1-m vertical grids,"..... how to complete it? Different results may be obtained when different methods used. 5. In this study, just 66 stations are used across China which is resampled into 2*2 grids. Obviously, there are many grids that even have no stations, resulting in misleading to readers for the information over these grids. Of course, some descriptions in this MS are not precise. For example, there is just one station in Tibet Plateau and the information of the spatial patterns for the ventilation, CAPE etc. are just the interpolation results, cannot represent the actual distribution. So, the authors say that the ventilation (CAPE etc.) is largest over Tibet Plateau may be not correct. 6. "Another discrepancy is the high ASI in October and November in

Urumqi, corresponding to relatively lower API values." I suggest the authors to check the variation of each component of ASI. May be it resulted by one of the components. 7. "In order to exclude the influences of emissions as much as possible, the investigation only covers data of winter half-year (i.e., October–March) when domestic heating requires more energy consumption." This sentence confused me. 8. In this study, the newly developed ASI is compared with the original one and the results indicating a better performance for newly index to capture the air stagnation days. The correlation coefficients should be shown in the text that can increase the readability. 9. From the comparison between the newly and original ASI, we can find that there are generally peak stagnation days in summer from original ASI but winter from newly one. Why? 10. From Figure 12, we can see that the numbers of stagnation days are generally much larger from the newly ASI than the original one. Why? 11. Some figure captions are not clear. Please check it. For example, what's the mean of the whisker in Figure 11.

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