

Interactive comment on “Air Stagnations for China (1985–2014): Climatological Mean Features and Trends” by Qianqian Huang et al.

Anonymous Referee #2

Received and published: 22 February 2017

This study explores the long-term surface meteorology and sounding measurements in China to study the climatology and inter-decadal trends of air stagnation in China since the 1980s. Considering the current severe haze pollution in China, this topic is interesting to the community and the compiled data sets are valuable for other pollution-related researches. Therefore, the materials in the manuscript are definitely publishable. However, my major concern is that, in contrast to the abundance of data involved in this study, the analyses of the data and the exploration of physics behind the observed relationships are quite inadequate. The authors should carefully address this issue before I can recommend its publication on ACP. Some detailed comments are provided below.

1) Section 3.2, the seasonality of air stagnation is interesting, but the authors' explanation and analysis are insufficient. For example, authors simply attributed the peak

[Printer-friendly version](#)

[Discussion paper](#)



of stagnation in the summer to the “weak pressure gradient”, but failed to explain what cause the weak pressure gradient in the summertime Eastern China. How is it linked to the sub-tropical high or the monsoon circulation? Answers can be found by looking at reanalysis of meteorological fields.

2) Similarly, it is interesting to see the larger influence of upper-level wind on air stagnation than that of the surface wind (this point should be highlighted in the abstract), but the physical relationship between upper-level wind and air stagnation is vaguely described in the manuscript. It should be thoroughly discussed in the introduction part. Is the upper-level wind here associated with moving speed of high pressure systems?

3) Inter-decadal trends of air stagnation in China are derived in this study, but what drive these trends are not discussed in the paper. Greenhouse gas, anthropogenic aerosol, or some other forcing agents? I understand that this complex question may be beyond the main scope of the paper, but some discussions along this line could largely sharpen the climatic implication of this study. I suggest a more thorough literature review about recent climate changes in China, in terms of hydrological cycle and precipitation extremes (Wang Y. et al., 2016, JGR), monsoon circulations (Li Z., et al., 2016, Rev. Geos.), etc, and the authors can link the trends to the possible factors at play.

4) Would the increasing trend of air stagnation be partly caused by the accumulation of the absorbing aerosols in boundary layer and a consequent stabilization effect (Wang Y., et al., 2013, AE; Peng J. et al., 2016, PNAS)?

5) The correlation analyses in Fig. 8 and 10 use annual mean values. Why not monthly mean? That would give you more dots and larger statistical significance.

6) What is the correlation between the air stagnation and visibility measurements in China since the 1980s? I assume they are highly correlated, but it is still interesting to see their relationships in time series and spatial distribution in China.

[Printer-friendly version](#)[Discussion paper](#)

[Printer-friendly version](#)

[Discussion paper](#)

