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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of monthly variation of the Pacific-North America (PNA) teleconnection pattern on wintertime surface-layer aerosol concentrations in the United States" *by* J. Feng et al.

Anonymous Referee #1

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General comments. The authors have conducted an interesting study to investigate the influence of the Pacific-North America teleconnection (PNA) on U.S. winter aerosol concentrations using both statistical methods and a chemistry model. This work contributes to our understanding of climate patterns responsible for PM2.5 variability and is appropriate for ACP. But some of the conclusions are relatively weak. So I recommend that the paper can be published only after a major revision as describe below.

1. Even though PNA is the leading circulation pattern in the North Pacific and North America in the troposphere during the wintertime, the authors should also show it is

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important for the aerosol variability. What is the fraction of temporal variability that can be explained by PNA? To show this, the authors can calculate the correlation coefficient between PNA and aerosol concentrations in each site.

2. Have the authors detrended the PM2.5 observations when they do a composite analysis between positive and negative PNA? The aerosol concentrations have shown a significant decreasing trend from 1999 to present in the United States. Won't this affect the conclusion?

3. It is hasty, with just some statistical analysis, to conclude that PBL should be the most important meteorological factor that influences the concentrations of PM2.5. Maybe the authors should do a sensitivity test by fixing the PBL height in the chemistry model. If the monthly PM2.5 variability is largely reduced, the major conclusion of this study should be correct.

4. Is there a specific reason that the authors use a very old version of GEOS-Chem model? According to the info here (http://acmg.seas.harvard.edu/geos/), the current public release is v10-01. But the authors still use v8-2 that was released five years ago. Also their model is driven by GEOS4 for 1986-2006. Why not use updated meteorological fields? How does this model treat the Secondary Organic Aerosol (SOA) and what mechanism is used? In the wintertime, SOA might still be a very important component in the south US.

Specific comments. Abstract. Why do the authors just use observations over 1999-2003 when data is available from 1999 to 2013?

Section 1. Maybe the author needs to give a literature review using more recent studies.

Section 2.1. Please specify the detrending method.

Section 2.2. Please specify if SOA is included in the chemistry model.

Section 2.3. There are many other definitions of PNA. Can these different definitions

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affect the conclusion?

Section 3.1. It is very important to include the uncertainty when calculating the PM2.5 concentration difference between positive and negative PNA. And provide details about how you calculate the uncertainty.

Section 3.2. The decreasing emission trend should have a large effect on the conclusion about the number of exceedance days. So please show Figure 4 at different timeframes.

Section 4.1. From Figure 5a and 5b, it seems GEOS-Chem largely underestimates the PM2.5 concentrations in California. What is the reason for this? Does it improve if some updated SOA mechanisms is included in the model?

Figure 5b shows the spatial correlation between the observations and the GEOS-Chem. How about the temporal correlation in different regions? Maybe the correlation is very low. Can it affect the conclusion?

Section 5.1 How important is transboundary transport of aerosols? If it contributes to a tiny amount of total PM2.5 mass, this section may be not important. Also, the authors should discuss whether the underestimate in GEOS-Chem over the west US affects the conclusion in this section.

Section 5.2 I would suggest the authors to check the impact of PNA on T, RH, precipitation and surface wind speed using NCEP Reanalysis over a much longer time period, e.g. 1948-2014.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33209, 2015.