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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 #3

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This manuscript examines the correlation between the Pacific-North America (PNA) phases and the wintertime surface-layer aerosol concentrations at the United States (US) on monthly scale. It uses the observation (1999-2013) from the Air Quality System of Environmental Protection Agency (EPA-AQS) and the simulation (1986-2006) from Goddard Earth Observing System chemical transport model (GEOS-Chem). Both the observation and simulation are composite to months with positive and negative PNA phases. The mean aerosol loadings are found to be higher for PNA+ months. The authors tried to understand the mechanisms for the PNA impacts on aerosol concen-

trations with the chemical transport model (CTM). Cross-boundary fluxes are found to be responsible for the PM2.5 increase in PNA+ months. With the pattern correlation coefficients (PCCs), they quantified the relative influence on local aerosol abundances from temperature, precipitation, relative humidity, wind, and boundary layer height.

The manuscript is well organized, and within the scope of ACP. Overall, I found that the observation-based analysis on the PNA impact on aerosol was more valuable than the model-based attempt on understanding the driving mechanisms. It can be published after addressing the following questions/comments.

1. Although it is mentioned that chemical reactions affect the aerosol concentrations, it seems assumed that large-scale circulation and/or meteorological factors dominate the chemical factors. Some references showing the relative contributions of dynamics, physics, and chemical impacts on aerosols are helpful.

2. It is known for years that the GEOS-Chem model has high biases in OH (chemistry is too "hot", resulting for example too short methane lifetime). Is this problem improved in the GEOS-Chem version used here? What is the CH4 lifetime in the model? The oxidizing capacity of the troposphere is largely determined by OH, which is a chemical sink for the aerosols studied here. It is hard to represent aerosols correctly in the model without reasonable OH, unless the chemical impact is trivial. In fact, the biases in the chemistry simulation may explain some of the model-obs differences denoted in Sect. 4.1.

3. Sect. 5.2 uses the PCC to quantify the relative contribution of different meteorological variables to the aerosol changes due to different PNA phases. It finds some correlations. But more sophisticated analysis is required to infer the driving mechnisms.

Specific comments:

1. Sect. 2.2, besides noting the methane (or other species that can infer OH concentra-

tion) lifetime, it is worth mentioning how many layers are in the PBL as the focus of the study is the aerosols in the surface layer, which is largely impacted by the presentation of PBL structure in the model.

2. P33218, L24, what are the numbers if restrict to the sites that pass the t test? Are they significantly different from current ones? Try to get a sense on how different sampling changes the results.

3. Sect. 3.2 can be removed. It is very short and does not add much, if any, to the paper. The results shown in Fig. 4 are mostly numbers close to 0. No significance test is done for those numbers, so I am not sure statistically how significant those numbers are. If the authors would like to keep this section, I'd highly recommend the significance test. The tools used for extreme weather/climate can be borrowed, which often rely on shifted (towards high tail) probability distribution functions.

4. P33222, L10-18: How much of the underestimated modeling PM2.5, SO4, NO3, etc. is caused by "too hot" chemistry in the model as mentioned above? The authors seem believe that it is mainly because of the observational uncertainties. Even it is the case, a factor of up to 13 differences in different measurements is worrisome. Such large error bars of the observations bring up the question if those observations are suitable for scientific applications.

5. P33223, L5-10: I understand why the GFED OC and BC lines are added to those plots. I suggest adding one or two sentences explaining the reason for it, so it would read logically smoother.

6. Sect. 4.2: Figures 3 and 6 are compared extensively. It would be better if the corresponding panels (abs and relative differences) use the same color bars. Otherwise, it is difficult to see some of the features noted in the text.

7. Table 4: There is no point to show the numbers that fail the significance test. I'd suggest changing them to NaNs or dashes.

8. Figure 2: It is redundant showing panels (a) and (b) only because different years of PNA phases are marked. I suggest that either (a) or (b) be deleted, but marking the PNA+/- months for the whole period (1986-2013). It will allow comparing the NCEP2-PNAI and GEOS4-PNAI for more years than the current form.

9. Figure 3: The caption should mention the results are for monthly averages (it says in the text, but should be clear in the caption as well). Also, the test is to confirm if the two averages (PNA+/-) are significantly different. The caption should be revised to be more specific about this.

10. Figure 5(b): Many panels show clearly two regimes. Does it make sense to fit a single line? Maybe two lines are better?

11. Figure 5 caption: Space is missing "DMof"

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