

Interactive comment on “Evaluating Trends and Seasonality in Modeled PM_{2.5} Concentrations Using Empirical Mode Decomposition” by Huiying Luo et al.

Anonymous Referee #4

Received and published: 19 June 2020

Comment on “Evaluating Trends and Seasonality in Modeled PM_{2.5} Concentrations Using Empirical Mode Decomposition”

General comments: This paper introduces a new approach for process-based model evaluation of speciated PM_{2.5}, which allows for the assessment of the performance of regional-scale air quality models like CMAQ on the intrinsic time-dependent long-term trend and cyclic variations in daily average PM_{2.5} and its species. The authors tested the method with time series data at three sites. The data are generally sound, whereas some results and discussions of the study are still lack of persuasion. One major concern is about how well the current approach’s performance is compared with

C1

the previously published methods and some over-interpreted conclusions. The other is that it is not sure that the difference between the model and the new approach evaluation results can be simply explained by the inadequate description of nitrate or organics in the model. As the authors noted, they obtained abnormally low correlations of synoptic scale NO₃ at ATL and calls for a better representation of nitrate partitioning and chemistry. What about the results for the other two sites? The authors need to provide more information on such issues to make the conclusion robust.

Specific comments:

1. Introduction: “Evaluation of ten-year averaged monthly mean of PM_{2.5} simulated with WRF/Chem . . .” how does the model performance of PM_{2.5} compositions simulation should also be summarized to provide an intact view on the previous results.
2. Line 36: “and other natural species...” what do natural species refer to?
3. Line 47: “monthly or seasonal means” means of speciated PM_{2.5}?
4. Line 48: what do you mean by “ten-year averaged monthly mean”?
5. Line 51: “with a phase shift of few months” please explain phase shift.
6. Line 55-57: “. . . long-term trends or interannual variations driven by climate change, emission control policies or other slow varying processes...” what is the main reason? Are there any previous results?
7. Line 68-74: I do not think this paragraph is necessary for the manuscript.
8. Line 311: “RENO is located close to the border with California and is affected by large wildfire breakouts in the western U.S. . . .” Is there any evidence for this demonstration?
9. Line 327-: “To sum up, the long-term trend at QURE is well simulated by the model.” This is unlikely consistent with the data presented in Table 1.

C2

10. Lines 333-335: “the available dataset may also play a considerable role in driving the agreements or disagreements between model simulations and observations of total PM2.5” What are the contribution of these species to PM2.5 at the studied sites?

11. Lines 367-368: “Both observed and simulated annual cycles at the RENO site are largely contaminated by the extreme events lasting for several months that are not properly simulated” is it possible to remove the data of extreme events before simulation, in order to eliminate the contamination?

12. Lines 384-387: “Specifically, the anti-correlation likely points to an inaccurate representation of the seasonal variation of the non-carbonaceous portion of organic matter due to an improper representation of organic aerosols in the model version analyzed here; this problem has since been corrected in more recent releases of the CMAQ model.” This sentence needs to be rewritten for clearance. And what does the non-carbonaceous portion of organic matter refer to?

Minor:

Line 17: “chloride (Cl) organic”

Line 311: “U.S. as can been seen”

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1079>, 2020.