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Geoscientific Model Development Discussions

Interactive comment on "A Comparative Study of Two-way and Offline Coupled WRF v3.4 and CMAQ v5.0.2 over the Contiguous U.S.: Performance Evaluation and Impacts of Chemistry-Meteorology Feedbacks on Air Quality" *by* Kai Wang et al.

Anonymous Referee #1

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General Comments:

The manuscript 'A comprehensive study of two-way and offline coupled WRF v3.4 and CMAQ v5.0.2 over the contiguous U.S.: Performance evaluation and impacts of chemistry-meteorology feedbacks on air quality' written by Kai Wang presented the comprehensive comparison of offline (i.e., traditional) CMAQ and two-way coupled CMAQ over the CONUS. To promote our understating on the interaction between meteorology and air quality, the approach of two-way coupled modeling is necessary, and this manuscript can contribute to this purpose. The authors claimed that long-term

simulations on both (two-way and offline) models over the CONUS is important point in this study, because the previous studies have been limited in many aspects (different chemical options, difference meteorological options, or, limited in time to focus on episode analysis). Although I would like to recognize the importance of this study, the evaluation is not well conducted in depth to make the best use of this long-term simulation. Please consider to address the following one major point, and also check the minor comments to improve the manuscript.

Major point:

I would like to disagree the evaluation framework of long-term simulations conducted in this study. The authors stated that "more robust assessments" through five-year simulations; however, the evaluation is only conducted by averaging the five-year dataset. This does not take advantage long-term simulations, and does not provide deep understanding of two-way coupled and offline models comparison. In addition to the averaged field of climatological type data, the comparison should be furthermore focused on trends in five years (if detected from observed facts) or year-to-year variations of both meteorology and air quality. Based on this extended evaluation, it could be finally proved the importance of two-way coupled model. Without such kind of evaluations, this study of long-term simulations will be less important.

Minor points:

1. L26: "modes" is typo of "models"?

2. L178-180 (and abstract): Are this chemical ICON/BCON considered year-to-year variation simulated by CESMv1.2.2/CAM5? Did this model perform well compared to other model(s)? If this model had superiority, please note how this model is important. Without any specific reasons, I feel it is no need to mention this model in the abstract.

3. L183-L185: In my best knowledge, inline dust scheme implemented in CMAQ version 5.0.2 is not the scheme reported by Foroutan et al. (2017) (see, also

https://www.airqualitymodeling.org/index.php/CMAQv5.0.2_Wind_blown_dust_updates). In addition, this statement contradicts to the discussion in its evaluation (L427-429). Please address this issue. If the authors implemented the scheme by Foroutan et al. (2017) in this study, exact explanation is required because this is model development paper.

4. L212: “PM10” will include PM2.5, hence the expression of “coarse particulate matter” is not appropriate. Or, did the authors calculate PM10-PM2.5 to represent coarse-mode particulate matter?

5. L222 (and related to Section 3.2.3): “paired with the satellite retrievals” means the deficit grid points in satellite observation are applied for model results? Please clarify. I guess that some satellite products provide averaging kernel, but how did the author apply averaging kernel for better comparison between model and satellite measurements? The detail seems to be dropped here. Please specify.

6. L443-446: The review paper by Emery et al. (2017) (used for ozone evaluation in this study) also presented the model performance goal/criteria for aerosols. Why these criteria is not used?

7. Figure 10: For gas species, differences are seen along latitude (approx. each 3-4 deg.) over western U.S.A. and Mexico. What is this difference?

8. Author contribution (L720-723): The contributions of all authors are not explicitly described here. Is it accepted in this journal style? (see, https://www.geoscientific-model-development.net/submission.html#manuscriptcomposition)

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