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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying errors in surface ozone predictions associated with clouds over CONUS: A WRF-Chem modeling study using satellite cloud retrievals" *by* Young-Hee Ryu et al.

Anonymous Referee #2

Received and published: 14 December 2017

This is a review of the manuscript titled "Quantifying errors in surface ozone predictions associated with clouds over CONUS: A WRF-Chem modeling study using satellite cloud retrievals". Overall the manuscript is well written and presents a very interesting analysis of integrating observed clouds into the WRF-Chem model to help correct errors in model simulated clouds. I agree with the comments already submitted, and only have a few additional comments to make regarding the manuscript. I don't see any reason to hold up publication of the manuscript once the comments/suggestions have been addressed.

General comments:

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The authors mention several times in the manuscript the useful of this technique to improve ozone forecasts. I find this odd, since the technique described uses satellite observed clouds to correct model errors. How would this benefit forecasts? Is the assumption that these satellite data could be assimilated in near real-time, improving the near-term forecast of ozone? Some clarification seems necessary here to explain exactly what the authors have in mind for improving forecasts.

I'm also curious about the meteorological performance, although I realize that the cloud assimilation technique only applies to clouds as they affect photolysis. Since WRF tends to underpredict clouds in some regions and overpredict clouds in other regions, does that underprediction/overprediction manifest itself in the meteorological performance (e.g. surface temperature)? If so, this would imply to me that while assimilating clouds to improve photolysis is clearly important, improving clouds in WRF itself, and thereby hopefully improving the overall WRF performance, would be the ultimate goal, since surface temperature (and other meteorological variables), play an important role in not just ozone chemistry but in aerosol chemistry as well. More of thought than something that needs to be addressed in this article.

Specific comments:

Line 14: What is meant by "attributed to that in cloud predictions"?

Line 45: Is surface ozone hourly? Perhaps specify if it is.

Line 206: Change "over CONUS" to "over the CONUS".

Line 208: Remove "the" before central California.

Line 211: Change "in supplementary" to "in the supplementary material".

Line 288: I would be a little careful calling this 8-h average O3, since commonly 8-h average O3 refers to calculation of finding the maximum O3 across a number of 8-h averages throughout the day, whereas it appears the authors are simply using an afternoon average consisting of 8 hours. This might cause some confusion to some

readers.

Line 361: This should be changed to say "partially corrected". It would be presumptions to assume that the cloud fields have been fully corrected. It is a big step in the right direction though.

Line 410: Remove "relatively" before greater.

Fig 5. What is the cause of the very large reduction in O3 over the great lakes in the GOES simulation? Is that due to an improvement in clouds over the lakes themselves, or is it the result of improved clouds over the land and advection of O3 over the lakes? High O3 over the great lakes is a persistent problem in many air quality models, so the resulting improvement warrants some additional discussion in my opinion.

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