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Interactive comment on "Evaluation of modeled surface ozone biases as a function of cloud cover fraction" *by* H. C. Kim et al.

Anonymous Referee #2

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Review of "Evaluation of modeled surface ozone biases as a function of cloud cover fraction" by H. C. Kim et al., submitted to GMDD

The authors describe a new method for evaluating air quality models. They present an observational constraint on the surface ozone/cloud relationship for the continental USA, using observations from the AirNow air quality network and cloud data derived from the satellite-mounted MODIS instrument. New ways to evaluate models are always welcome, and this is an interesting addition to our evaluation arsenal. Ultimately, I think that the study could be a good addition to the literature, but I feel that the authors claim too much for the method, and their conclusions should be more circumspect. It would also benefit from further statistical analysis. I have comments related to this below.

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Interactive Discussion
Discussion Paper

(Note: My line numbering refers to the pdf version I downloaded from the reviewer log in page, rather than the GMDD article.)

General comments

1. Interpretation. Ozone chemistry is very complicated and depends on many things, which is something the authors themselves note in L164. As such, I don't think that attributing x% of the model bias to cloud fields can be done (L182). How can one disentangle this bias from (say) a bias in the emissions? If the emissions biased things one way, the cloud bias might correct it or intensify it. Instead I think that this technique potentially adds another useful constraint on model performance, but one that should be used in conjunction with other evaluation methods (MDA8, pdfs of monthly stats, long term climate relationships etc).

In addition to these comments, there is a distinct lack of statistical rigor in the interpretation of the relationships. The authors should at least quote uncertainties on the regression coefficients for (e.g.) Figure 3 – are they in fact statistically different from zero? Also, what is meant by "correlation slope"? Slope from the linear regression perhaps?

Regarding correlations, the authors might like to see if there is a significant correlation between CF and MDA8, for both the "standard" (Pearson) correlation and a rank correlation. They will likely need to be careful in their interpretation of the significance here since, depending on spatial autocorrelation, each site will likely not represent an independent sample.

Finally, do the authors think that these relationships would be broadly applicable to other regions, or even for global models?

2. Introduction. I'm not sure that the introduction sets up the paper all that well: - It would be useful to mention the other techniques that are used to evaluate AQ models to give some context for this work (and something to refer to in the conclusions) - The

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first paragraph of the introduction talks about the importance of aerosols for photolysis rates, but my understanding is that CMAQ (in common with most other models) does not consider aerosol scattering when it is adjusting the photolysis rates. It would be a good idea to mention this I think. - The authors also might like to think about what photolytic processes are most important here: jNO2, jO3P and jO1D, or others? - Finally, the introduction could also mention some of the work that has looked at the potential role of clouds (through photolysis) in interannual variability of tropospheric composition (e.g. Voulgarakis et al. (2009), ACP, doi: 10.5194/acp-9-8235-2009).

Line by line

L14. Is this “clear” correlation significant?

L31. “For instance. . .” before “Studies. . .”

L81. Define CONUS

L124. “serious” is rather vague

L144. “August 2014”

L147. I’m not sure that I “readily expect” anything from the basics of ozone photochemistry. Would be good to have a citation here.

L195. See my general comments. I’m afraid I don’t think the study demonstrates how “crucial” it is

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Interactive comment on Geosci. Model Dev. Discuss., 8, 3219, 2015.