Geosci. Model Dev. Discuss., 8, C1047–C1054, 2015
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Geoscientific Model Development Discussions

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

**Anonymous Referee #1**

Received and published: 9 June 2015

General Comments: This paper addresses a very important issue: the source of ozone forecast bias in the NAQFC model. As this model is used extensively for operational air quality forecasting in the US, information of this type is certainly timely. The paper provides what is essentially a "back of the envelope" calculation of the impact of cloud cover on ozone forecasts. Since I have seen no prior work that addresses this issue systematically, it is a welcome addition. My main criticism is that the authors are claiming too much for their study. It can stand alone as a ïňĄrst rough guess calculation but probably isn't strong enough to support some of the conclusions in the paper. I think the paper is certainly worth publishing if the authors add some clariïňĄcation concerning the limits of their conclusions. SpeciïňĄc Comments: p. 3221, line 6: re, trends in "frequency of photolysis", do you mean "rates" of photolysis? p. 3222, line 15: Cloud fraction (fc) as diagnosed in the model is a function of RH, but it would be useful to



know exactly what that function is as used in the experimental NAQFC. p. 3224, line 2: It would be interesting to know if the changes to the experimental NAQFC noted at this line reduced the bias of the model by a magnitude that is more or less than that by cloud fractions as estimated in this paper. p. 3224, line 6: The cloud fraction difference is estimated at 1:30 LT but the metric of interest for ozone is the 8-hour running average. This raises a few questions that probably should be addressed in the text. For example, is an instantaneous measure of cloud fraction an accurate metric with which to compare the cumulative effects of clouds and sun over an 8-hour averaging period? Is it a good measure for high, stratiform clouds more than for low level buoyancy driven clouds? Is it possible that the NAQFC model "catches up" with cloud fraction as the day increases? The NAQFC, based on the NAM, uses a boundary layer parameterization scheme that may, or may not, produce low level cloudiness at the proper time in the diurnal cycle. p. 3225, lines 20-23 and Figure 3: I didn't ïňĄnd this ïňĄgure to be very enlightening. The text notes that Figure 3a shows "a clear separation of ground level ozone for each cloud fraction". _fig. 1 below

To be honest, I don't see much clarity in the scatterplot. Perhaps if a linear best ïňĄt line was superimposed? I ïňĄnd it useful for all "busy" scatterplots to include some ïňĄtted line in the ïňĄgure along with the best ïňĄt equation and r and r2 values in a legend. That makes it more convenient for the reader who otherwise has to jump back a forth in the text – a table would be second best. In any event, while the NAQFC shows less cloud cover, particularly in the near-overcast range, it is worth the effort to see whether there is a statistically signiïňĄcant difference between the two samples. Because we are looking at one month of data, a bit more statistical rigor would be very helpful. It's clear that cloud fraction effects are important but a little more information on the uncertainty of the estimates (that used mean values) would be very useful. It would also be good to mention if there was anything unusual with respect to the climatology of the CONUS during August of 2014. I'm not certain that it affects the results shown here, but for the bulk of the CONUS, the summer of 2014 was a historically low ozone year – similar to 2013. As a result, the critical cases for air quality forecasts - those

in the high end of the distribution (e.g., Code Orange), were scarce in 2014. _figure 2 below

The very clean conditions in 2014 may be a function of changing emissions but may also be associated with large scale weather patterns. See images below suggesting a cooler than normal August with large OLR anomalies (ïňĄgures via, http://www.esrl.noaa.gov/psd/data/composites/day/). _figures 3 and 4 below With reference to the statistical analysis, Figure 3d shows an extremely broad standard deviation band for all ranges of cloud fraction difference. As the conclusions that follow in text lean heavily on mean values to express model sensitivity, this is a little troubling. It is what it is, of course, but the authors should point out that the data is very noisy so that later calculations that make use of them should be taken with a grain of salt. In this case, the "very rough" results at p. 3226, line 20, should be further qualiïňĄed. For example, what is the range of possible sensitivity across the distribution of O3 differences? _figure 5 below

In this regard, it might be worthwhile to choose a subset of data, perhaps set of monitors in a region, and see how this cloud fraction bias works on a local or regional level. p. 3226, line 5: -10.5ppb100%-1 looks like a typo. p. 3226, line 10: The use of the term "brighter" is a bit confusing here. "Brightness" is kind of a term of art in many other applications and may not be meant the same here. Is what the authors mean to say is that the model has fewer clouds? Conclusions: This is a good paper on a very interesting and timely subject. It should be published with revisions. In particular, the authors should qualify a few of their conclusions and better describe the underlying uncertainty of the data and the metrics used to estimate sensitivity, in particular the use of mean values in a noisy field of data.

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



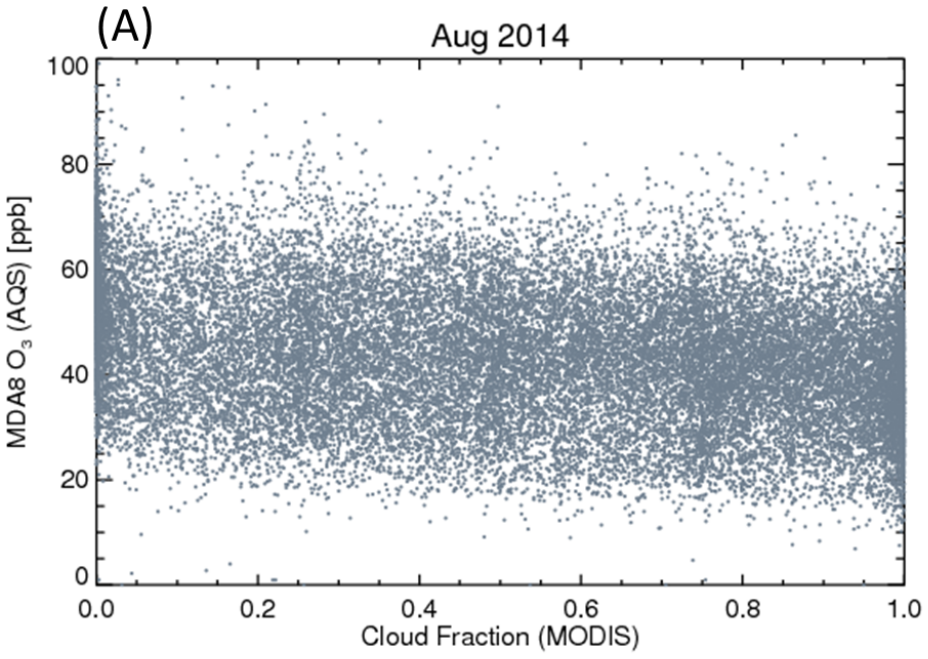


**Fig. 1.** Figure 3a from text

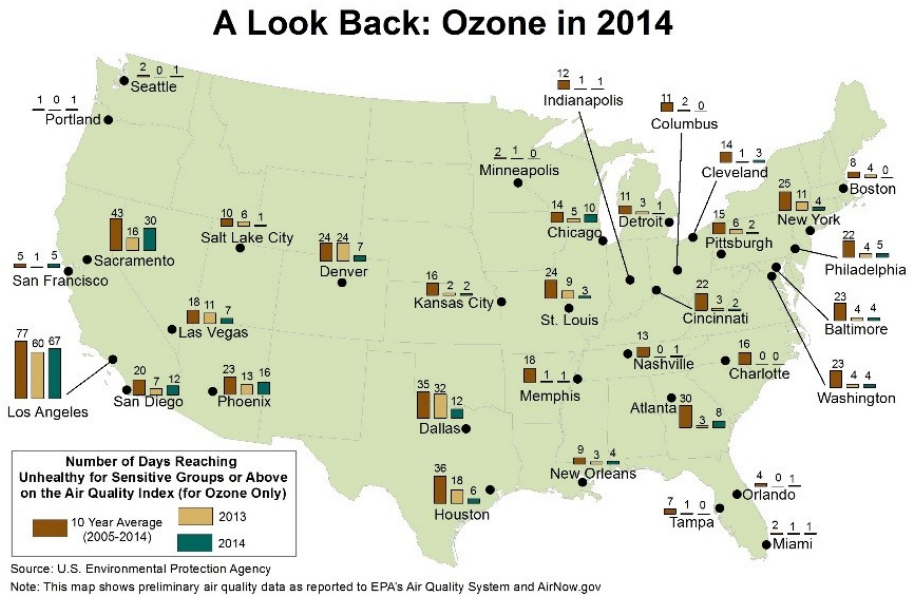


**Fig. 2.** EPA 2014 summary



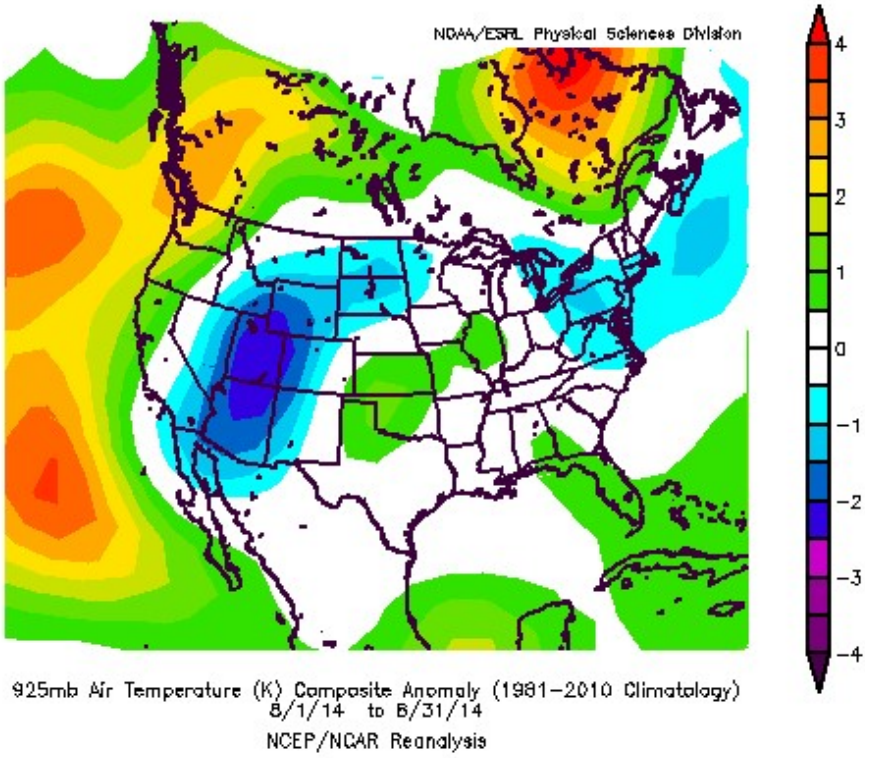


**Fig. 3.** august 2014, temp anomaly

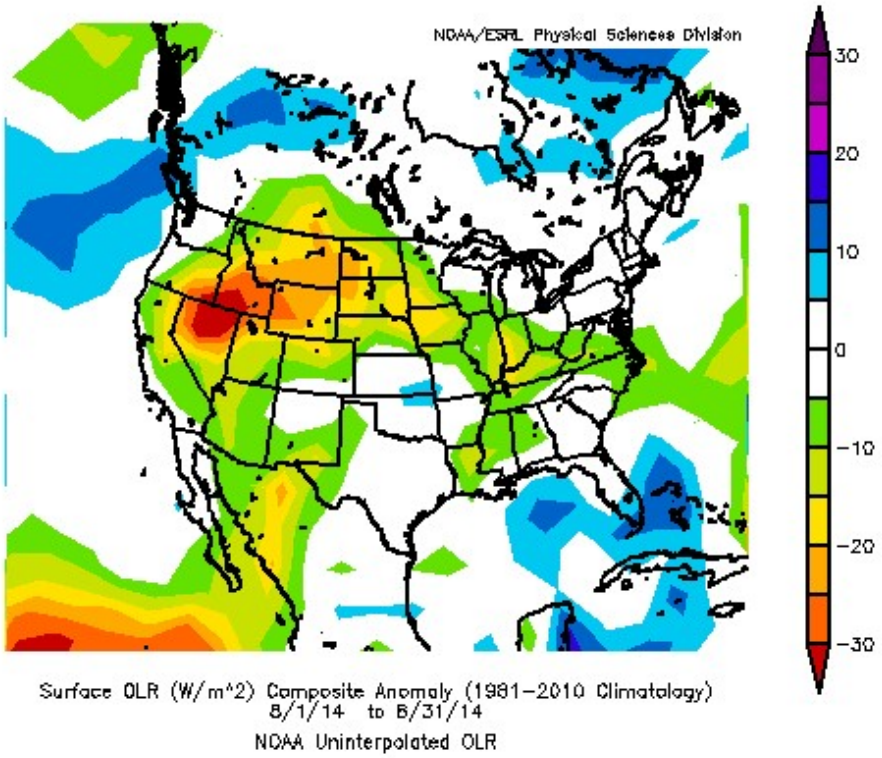


**Fig. 4.** august 2014, OLR anomaly



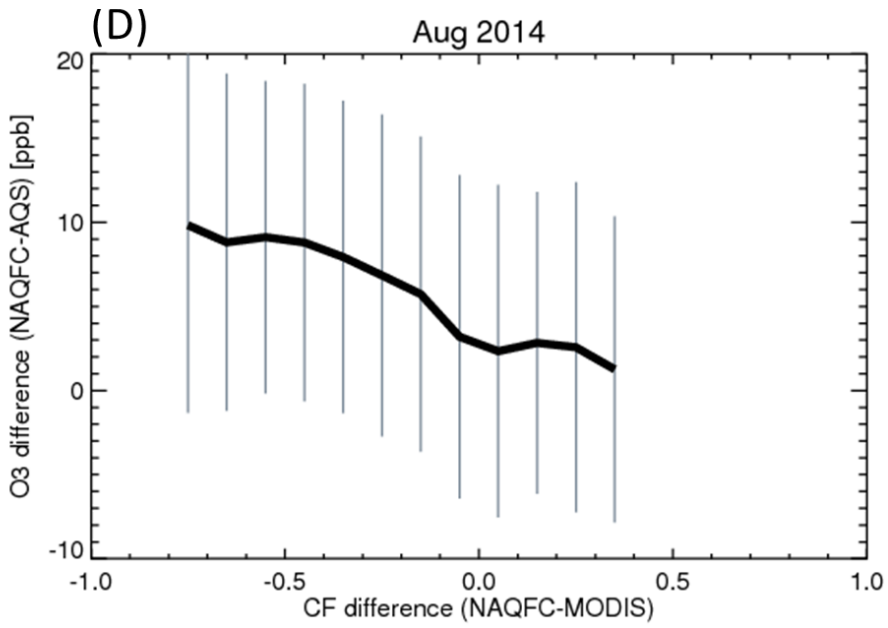


**Fig. 5.** Figure 3d from text