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Interactive Comment

Interactive comment on "Use of North American and European air quality networks to evaluate global chemistry-climate modeling of surface ozone" *by* J. L. Schnell et al.

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We thank anonymous referee #1 for their useful review and encouraging comments. The minor comments relating to odd constructs or grammatical errors have been fixed to follow the referee' suggestions. Below we address the major and other minor comments made by referee.

Major comments:

"My only major comment relates to the use of the UCI model in this analysis. While it does make sense for the UCI developers to see how their model fares compare to the

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broader community of models, I have found that very little information was added to the paper by the inclusion of this model. I would therefore suggest removing this model, and focus solely on the ACCMIP results, since this is a consistent set of models. A better use of the UCI model results would be to understand better other model biases, but this is not achieved in this paper."

Response: We have carefully considered this suggestion, but ultimately chose to keep the UCI CTM in the analysis for the following reasons. While the UCI model is driven by hindcast meteorology and is therefore unlike the ACCMIP climate models, we do not exploit those differences here, but instead treat all models in terms of their climatological ozone patterns. Thus, the diurnal and seasonal climatologies of surface ozone developed here provide critical diagnostics for either type of model. In addition, the use of the UCI model highlights that although a model may perform well in daily correlations (see UCI CTM in Schnell et al., ACP, 2014) and seasonal cycles (here) it may totally fail in another metric like diurnal cycles (here). We have our work to do on the UCI CTM, now with clearly defined metrics. Finally, since the UCI model is driven by hindcast meteorology, it aids in understanding the individual effects of inter-annual meteorological variability and observed nonstationary precursor emissions on interpretations of model evaluation results.

"The discussion on the NOx dependence (Fig. S5) is only marginally useful and only with caveats"

Response: We feel this discussion is important to include since it highlights how a nonstationary time series in observations may affect the interpretation of some model diagnostics. In addition, the discussion encourages the use of the most detailed emission data available in future model simulations, especially when testing the skill of a model over time periods with large trends in ozone precursor emissions.

Minor comments:

"4) Page 11376, line 3: How is the regridding performed? Bilinear? Would it make

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more sense to have a climatology resolution closer to the original model resolution?"

Response: The regridding is performed using a first-order conservative mapping technique. Essentially the technique determines the ozone abundance at a 1°x1° grid cell (i.e., the target grid) as the weighted average of the abundances in the native model grid cells which overlap with the target grid cell. The weights are equal to the fraction of overlapping area in the target grid. In terms of the regridding resolution, we decided to use the 1° x 1° grid for the most commensurate inter-model and model-measurement comparison. Since the models have varying grid structures, in terms of both resolution and grid placement, we would have essentially needed to create an observational dataset for each model. This would have led to uncertainty in inter-model comparisons since we would then be comparing each of the models to a different observational dataset. Also, the observational dataset was optimized for this scale in a previous analysis. In addition, the developers/contacts of the models used here did not express concern at the choice to regrid the models to a higher resolution. In any case, the results are focused mainly on regional averages and thus should not be greatly affected by regridding due to its conservative nature.

"5) Section 3.1: unlike the annual seasonal cycle, there is no discussion of range (in the observations and in the model) in this diagnostic? It seems that it would be useful to include such additional information."

Response: We did not feel this discussion was relevant since unlike the annual cycles, the interannual variability of the diurnal cycles in Sect. 3.1 is quite small, especially since they are averaged over a 3-month season.

"8) Page 11393, line 5: this is an interesting statement (on the importance of diurnal cycle in emissions), although it is not clear whether such datasets are available for global (or at least North America/Europe) simulations. It would be good to point the reader to such datasets if they are available."

Response: We are unaware of the existence of such a dataset, at least one that is

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commonly used. Our intention was only to highlight this as a possible reason for the overall underestimates of the amplitude of the diurnal cycle rather than to point out or encourage the use of a particular emission dataset.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11369, 2015.

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