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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Influence of satellite-derived photolysis rates and NO_x emissions on Texas ozone modeling" *by* W. Tang et al.

Anonymous Referee #2

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In this paper the authors take two approaches to improve CAMx modeled O3 over Texas. First, they use GEOS observations of cloud fractions to adjust photolysis rates. Second, they use OMI tropospheric NO2 observations in an inverse modeling framework to derive new estimates of NOx emissions. They tested these improvements against surface observations of O3 and NO2.

Implementing satellite-derived photolysis rates generally improved the model-measurement comparison of O3, albeit slightly. However, there was no change in the modeled NO2 bias. Implementing the OMI-adjusted NOx emissions had similar results. The authors also compared sector-based and region-based approaches in the

inversion method.

The manuscript is well written and presents results that would be interesting to the regional air quality community. It therefore should be published after addressing the following comments.

My concern with using GOES cloud fractions to adjust photolysis rates in the model is that it introduces an inconsistency with the modeled dynamics. Changing the cloud fraction directly affects the heat flux and therefore stability and the height of the boundary layer, both important drivers of ground level O3. I understand that it may take considerable effort to fully include satellite-observed cloud fractions in the chemistry and meteorological models. However, I think the authors should at least include a broader discussion of this topic and frame this analysis as a sensitivity study.

The last sentence of the introduction states that the manuscript will also present inverse modeling of VOC emissions, but there is no mention of this in the methodology. Some results of VOC inversions are presented in the Conclusions and the reader is directed to supplementary information. If this analysis is to be presented as one of the main aims of the manuscript, I think that the methodology and results should appear earlier in the manuscript.

The last sentence of the 2.5.1 states that the "the OMI averaging kernels are not applied here." I think this is misleading because it implies that the vertical sensitivity of the retrieval and dependence on the a-priori profile are ignored. This is in fact not the case, as is shown in the supplement, and I would urge the authors to reword this.

Specific comments: Page 24478 Line 13: The term 'ozone design values' is not common outside of U.S. air quality policy circles. Thus a typical reader may not understand the implications of ozone design values above the NAAQS standard. It might be good here to give a brief definition of the term, or phrase this in a different way.

Page 24483: I think it's misleading to say that GOES measures cloud fraction. The

12 km cloud fraction is derived from the fraction of GOES subpixels that are deemed cloudy. This should at least be made more clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24475, 2014.