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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.

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The authors would like to thank Reviewer 2 for the thoughtful comments and description of this paper as well written and as interesting to the regional air quality community.

Following are our responses to each of the reviewer's general and specific comments (shown in italics):

General comments:

1. 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.

We agree with the reviewer on this point. The model dynamic and aqueous phase chemistry haven't been adjusted by the GOES cloud fractions, and thus are inconsistent with the GOES-based photolysis rates. This work represents a sensitivity study of the impact of satellite-based photolysis rates but not a complete assimilation of satellite-based clouds. We have more fully discussed this limitation in the conclusion (page 24495, lines 13-15) by the sentences: "The GOES-retrieved clouds applied here adjusted only the modeled photolysis rates, while modeled clouds continued to drive the dynamics and aqueous phase chemistry. This inconsistency in the placement of clouds is similar to the approach of a previous study (Pour-Biazar et al., 2007). Thus, this work demonstrates a sensitivity study of using satellite-derived photolysis rates on model performance rather than a full integration of satellite-observed clouds into all aspects of the model. Future work could extend the use of GOES-retrieved clouds to also correct model dynamics and aqueous phase chemistry and investigate their impacts on NOx and O3 modeling."

2. 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 reason we studied VOC is that we want to see if the uncertainties in VOC emissions will significantly affect our NOx inversion results. Since this is not the main aim of this paper and the findings are not significant, we have moved the description of VOC

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emissions part in the introduction section (page 24480, lines 7-20) into the supplementary material. We keep the last sentence regarding the VOC work in the introduction section (page 22481, lines 1-2) and point it directly to the supplement.

3. 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.

We have changed the sentence in page 24486, lines 12-14 to "Since applying OMI averaging kernels (Eskes and Boersma, 2003) may introduce more uncertainties to the CAMx-derived NO2 VCD in this case (Supplement, Sect. 1), the CAMx modeled NO2 are compared to the OMI NO2 directly." to avoid any confusion.

Specific comments:

1. 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.

We have removed the term "ozone design value" and rephrased the sentence in page 24478, lines 11-17 to "First and foremost, the Houston-Galveston-Brazoria (HGB) region and the Dallas-Fort Worth (DFW) region exceed the 2008 O3 National Ambient Air Quality Standard (NAAQS) of 75 ppb and thus are both classified by US Environmental Protection Agency (US EPA) as O3 non-attainment areas. Next, Beaumont-Port Arthur (BPA), Northeast Texas (NE Texas), and Austin and San Antonio regions require attention for closely approaching that standard (Gonzales and Williamson, 2011)."

2. 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.

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We agree with reviewer on this point. The cloud fraction in the 12km model grid was integrated from GOES sub-pixels. The terms we use in our paper are “GOES-retrieved clouds” and “GOES-derived photolysis rates”. We have changed the sentence in page 24483, lines 11-12 to “In this study, hourly GOES observations with integrated 12km cloud properties from sub-pixels have been used.” to avoid any confusion.

References

Eskes, H. J. and Boersma, K. F.: Averaging kernels for DOAS total column satellite retrievals. Atmos. Chem. Phys., 3, 1285–1291, 2003. Gonzales, M. and Williamson, W.: Updates on the National Ambient Air Quality Standards and the State Implementation Plans for Texas, presented in TCEQ Trade Fair, Austin, TX, 4 May, 2011. Pour-Biazar, A., McNider, R. T., Roselle, S. J., Suggs, R., Jedlovec, G., Byun, D. W., Kim, S., Lin, C. J., Ho, T. C., Haines, S., Dornblaser, B., and Cameron, R.: Correcting photolysis rates on the basis of satellite observed clouds. J. Geophys. Res., 112, D10302, doi: 10.1029/2006JD007422, 2007.

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