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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Are there urban signatures in the tropospheric ozone column products derived from satellite measurements?" *by* J. Kar et al.

Anonymous Referee #2

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Review of 'Are there urban signatures in the tropospheric ozone column products derived from satellite measurements?' by J. Kar et al.

The paper combines satellite observations from different instruments and species to demonstrate the capability of TOMS/SBUV and OMI/MLS instruments to observe tropospheric ozone enhancements. Two methods are compared to demonstrate that the observed enhancements are consistent and not an artefact of the respective method. The observation of megacity ozone plumes is complemented by NO2 observations from SCIAMACHY over the same cities which further gives a strong indication that the observations are caused by tropospheric photochemical ozone production within the

plumes. To circumvent terrain induced modifications of the results climatological volume mixing ratios are shown for the same areas giving consistent results. The latter also leads to some observations of ozone plumes over forrested relatively pristine regions, which they attribute to pollution transport from adjacent cities. Overall the paper adresses an important question of the capability of satellites to observe ozone plumes in the troposphere. The results are consistent and well presented and the paper. I recommend it for publication, but like to suggest the consideration of the following points.

It is clear that the observations give different results using different methods given the individual uncertainties of each method and all the constraints given from the retrievals. It is really a great opportunity to have these patterns, which can be associated to pollution. However, I missed a bit a statement on the uncertainty of the absolute amount of ozone or the reliability of the results. Since the data as described in the paper might be of particular value for model evaluation or air quality standards and might motivate future satellite missions some statements on the uncertainties should be included. Are these data appropriate for quantitative analyses or can this be expected for the future?

The results would gain much more evidence, if the authors would include some ground based observations (e.g. climatological diurnal cycles from monitoring sites or air pollution net works) for at least one of the cases in and outside the plumes, for example in st. Joaquin valley, Yosemite).

The observations of high ozone over Sequoia and Yosemite is very interesting, but the reason for this remains speculative, since no further evidence is given (e.g. NO2 observations, ground based measurements) or other ozone sources are discussed.

Further the acronyms should be explained.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3807, 2010.