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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Accurate satellite-derived estimates of the tropospheric ozone impact on the global radiation budget" *by* J. Joiner et al.

Anonymous Referee #2

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Review of the paper "Accurate satellite-derived estimates of the tropospheric ozone impact on the global radiation budget" by J. Joiner et al.

In this manuscript, the authors report on estimates of the effect of tropospheric ozone on the radiation budget at the tropopause for January and July 2005. The estimates are based on a new tropospheric ozone product based on OMI and MLS satellite data with a specific OMI cloud treatment. The latter takes into account the effects of higher visibility of ozone in the upper part of clouds which is relevant for the short wave radiative effect. Surface albedo in the short wave and cloud optical properties are taken from MODIS data while emissivity in the long wave is from climatology. The results

show broad agreement with previous studies based on model data and other measurements as far as the numbers can be compared but in particular for the effect of clouds, differences are also found.

The paper is well organised, clearly written and provides a lot of details on the study. It fits well into the scope of ACP and reports on improved values for the radiative effect of tropospheric ozone which is an important climate gas. I therefore recommend publication in ACP after minor revisions as listed below.

Major comments

The main point of the article is an improved treatment of tropospheric ozone determination and the effect of clouds. While I believe that this has in fact been achieved, I have three questions to the methodology used:

1) Throughout the paper, it is assumed that ozone is distributed homogeneously in the troposphere, at least above the lower pressure limit. However, the vertical distribution of ozone is relevant for a) the retrieval of the OMI data, b) the radiative effect and c) the relative importance of clouds. After reading the manuscript, it is not entirely clear to me at which of these three points ozone mixing ratios have been assumed to be constant in particular as on page 5518 the "build up of ozone in the upper troposphere" is mentioned. Also, I can not assess how large the uncertainty introduced by this assumption is. Please clarify.

2) One nice point about using the optical centroid cloud pressure is the fact that the measurement is already accounting for the light path change which is relevant for the short wave radiative effect. However, the light path of course depends on wavelength, and ozone retrieval is done in the UV, Raman retrieval in the visible and the RE calculation using the Chappuis absorption towards the red part of the spectrum. How is this accounted for?

3) A basic assumption of the method is that ozone and cloud information has to be

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taken at the same time to get the ozone effect right. As mentioned in the text, clouds have therefore to be assumed to be constant throughout the day which in particular for convective clouds doesn't seem to be a very good approximation to me. Have you tried to estimate the possible bias introduced by this assumption?

Minor comments:

- abstract, last sentence: effect on *ozone* instantaneous forcing

- page 5507, 2nd para: I found this discussion of the qualitative effect of clouds too short although detailed descriptions can be found in the literature

- page 5516, do you assume a Lambertian albedo? Is the albedo for the RE calculations consistent with the albedo used in the OMI ozone retrieval?

- page 5517, time average of skin temperatures 8211; is that a good approximation? I'd guess that depending on latitude, the average value is biased towards night or day

- Fig 2: May be it should be mentioned in the caption that the column mean tropospheric O3 is not down to the surface but between surface or OCCP and tropopause

- Figs. 6 and 8: I'd prefer to have the two seasons plotted on a global (90S to 90N) display for better comparison

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5505, 2009.

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Interactive Comment