Atmos. Chem. Phys. Discuss., 9, S2367–S2370, 2009
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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.

J. Joiner et al.

Received and published: 29 May 2009

Response to Reviewer 2

The authors thank the reviewer for the careful review and for providing constructive comments on the paper. We have revised the paper accordingly as stated below.

1 Major comments

1. Regarding the sensitivity of the ozone radiative effect to the ozone profile, the reviewer is correct that the vertical distribution of ozone is relevant for the retrieval from OMI, the RE, and cloud effect. We have now clarified at the beginning of

the results section that the ozone is assumed to be homogeneous for the OMI retrieval in the UV. To be consistent, the short-wave calculations also assume the ozone to be distributed uniformly. The short-wave RE is significant only when the reflecting surface is bright such as over clouds or desert. In these cases, the SW RE does not depend significantly on the vertical profile of ozone. The LW calculation, however, is significantly dependent upon the ozone vertical profile. To address this point we have recomputed the radiative effect using a model profile scaled to give the measured column. The difference is significant ($\sim$30% globally averaged). We now use the model profile shape for all LW calculations. We compare this with the radiative effect previously computed with a uniform profile (in a new appendix subsection). This is now described in the beginning of the results section. We changed the wording of the sentence that included “build up of ozone in the upper troposphere" to “large column amounts of ozone".

2. Regarding the wavelength dependence of the light path: The reviewer is correct that the light path is wavelength dependent. However, we have found that in the presence of clouds, the wavelength dependence appears to be relatively small. Sneep et al. (2008) show with radiative transfer calculations that cloud pressures derived from Raman scattering in the UV, O_2-O_2 absorption in the visible (477 nm) and O_2 A band (further into the red) should be very similar. When compared (in the A-train), they are very similar in most cases. The remaining differences are believed to be due instrumental or algorithmic effects such as the treatment of the surface in thin and broken cloud conditions. Discussion is now provided on this point.

3. Regarding the assumption of constant clouds throughout the day, we have not attempted to estimate the possible bias introduced by this assumption. We have examined differences between cloud fractions derived from the Aqua and Terra satellites (3 hours apart). Averaged globally, the daytime differences are not significant. Comparing daytime and nighttime cloud fractions is more problematic

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as the algorithm differs slightly. We added the text on this point as well as the statement to the effect that in order to perform a quantitative comparison of RE with present-day models, the model could be sampled during the Aqua daytime overpass to minimize the effects of the diurnal cloud variability. For example, the Global Modeling Initiative (GMI) provides output at both the Aqua and Terra overpass times (day and night).

2 Minor comments

1. abstract, last sentence: changed as suggested.

2. P 5507 2nd para: We have added more description on the qualitative effect of clouds. We split this paragraph into 2 separate paragraphs, one for the LW and one for the SW. Each paragraph now describes scenarios where clouds can either increase or decrease the tropospheric ozone radiative forcing.

3. P 5516: Yes, we assume a Lambertian albedo. This is now clearly stated in the text. The albedo for the RE calculation is not consistent with that used in the OMI ozone retrieval. A paragraph describing the OMI-TOMS algorithm surface treatment has been added.

4. P 5517 regarding the averaging of the LW radiative effect at 1:30, 13:30 (note that we are not averaging the skin temperature): We added the following text "This averaging may produce local biases in the computed RE over areas such as subtropical deserts where the diurnal skin temperature variation is large and asymmetric. Over ocean, the diurnal variation in sea surface temperature is insignificant. We calculated the global 1:30-13:30 difference in the RE for January 2005 (0.12W/m^2). This is an upper limit for the error in RE that would result from assuming a constant skin temperature equal to either the 1:30 or 13:30 value.

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The actual error produced by averaging the 1:30 and 13:30 RE will be much smaller because it will result only from the diurnal asymmetry in the skin temperature. Note that locally the 1:30 and 13:30 RE can vary by more than 1.5W/m^2 over arid regions such as the Sahara and portions of Australia and South Africa."

5. Fig 2: We agree that the caption (for Fig. 1 and 2) should mention that the column mean tropospheric O_3 mixing ratio is down to (approximately) the (mean) effective pressure P_{eff}, not the surface, and have added it as suggested.

6. Figs 6 and 8: We had cut off the polar night regions from the plots as there are no OMI/MODIS UV/VIS measurements. However, to make the comparison more easy by eye, we have replotted these from 90S-90N as suggested.

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