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Interactive Comment

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

Anonymous Referee #1

Received and published: 4 May 2009

This paper investigates tropospheric ozone impact on solar radiation through various satellite measurements from the A-train including ozone and cloud observations. The paper is well written and the methodology is sound. This paper provides new approach to compute daily ozone radiation effect at local and global scale, by using only satellite-based observations. It also gives important information on cloud effect in the short wave and long wave. In my opinion, this paper deserves publication in ACP journal. I have few comments and suggestions as described below.

General comments:

The Authors gives a detailed description of their methodology's limitation to calculate



radiation effect induced by various parameters (e.g. tropopause definition, homogeneous tropospheric ozone layer...). However the various uncertainties related to these parameters are not always clearly examined in terms of RE calculations' limitations. In my opinion this should be taken into account in Sec.4 or in a specific section, as well as cumulative uncertainties in order to strengthen the manuscript. A Table could summarize the different uncertainties as well as the ones discussed in Appendix B.

P 5507 lines 21-end; page 5508 lines 1-11 Limitations of the methodology. These are two main points that need more discussion in the next sections. The Authors should mentioned once again and discuss in the next sections (Sec.4 and Sec. 5) the effect of unadjusted calculations on radiative effect compared to other studies. Concerning the second point, ozone radiative effect is sensitive to the altitude of the ozone change, as mentioned in various studies (Fortser and Shine 1997, Forster et al., 2007) and by the Authors in Sec.3 lines 2-9. Even this is not information allowed by ozone satellite observations, the paper should better describe how this might specifically affect their result (and not just refer to another study from Kiehl et al. 1999), as they just account for total tropospheric ozone effect. One approach could be to make sensitivity studies in the Chou-Suarez radiative transfer model to ozone change in altitude (low, middle and upper troposphere), by latitude range, in order to give uncertainties range in their RE estimation. Indeed some areas are mostly affected by ozone anomalies in planetary boundary layer (continental downwind of emissions), whereas regions affected by global scale subsidence will be affected with ozone anomaly in the middle / upper troposphere (South Atlantic, USA, Middle East...).

P5512 lines 23-26 P 5513 lines 1-18. Tropopause effect discussion. It is not clear to me why the Authors mentioned they used two standard definitions of tropopause heights that will impact RE calculations. Indeed this aspect is no longer discussed in the study and briefly discussed in section B3. The Authors should better quantify the uncertainties in RE calculations coming from tropopause definition by showing global maps of RE calculated with each definition, or RE calculated for various latitudinal

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bands. Indeed they mention in section B3 high local differences (70-90%) that does not seem to exist in global average, but compensation effect could occur while averaging at global scale. Besides the Authors mentioned P 5517 lines 24-26 that they choose the lapse-rate tropopause definition without giving justification. This should be clarified.

P5515 Section 2.5 How RE calculated by the Authors are sensitive to a priori meteorological parameters. Indeed the Authors do not discuss the choice of meteorological analysis. I suspect a lot of work would be needed to evaluate this effect, but the Authors should at least mention how strong are their calculations with GEOS 5 compared to calculations using GEOS4 or NCEP or ECMWF met fields.

Specific comments

P 5507 lines 4-7. "In these model ... and climate". Please restate. Tropospheric ozone burden variations are both driven by natural and anthropogenic changes. Variations in stratosphere to troposphere intrusion are a natural one, not anthropogenic one.

P5508 lines 3-5 "Our results...as defined in Forster et al., ". O3 can be "naturally" produced (eg from lightning or soils NOx) and not only from anthropogenic emissions. Please modify.

P 5512 lines 14-22 If the improvement between satellite-derived and sonde-based tropospheric ozone column using OCCP is a specific result of this study, this should be better highlighted by showing for example the enhanced correlations using the Authors' approach.

P 5513 lines 10-19 Discussion of Figures 1 and 2. Even if the Authors discuss the methodology to discard data, some ozone enhancements which appear for a day or in monthly average remain unclear to me. For example should the ozone maxima over the Antarctic and the Arctic not be discarded? This is important as the Authors uses daily observations to calculate the RE.

P 5519 lines 20-23. The Authors show a strong SW RE over the Artic and the Antarctic.

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What is the confidence of their calculations over these regions where it is difficult to retrieve ozone columns because of high albedo?

P 5519 lines 24-26 "Contributions to the SW ... significant" How can the Authors assume the effect of biomass burning in cloudy regions. This is not demonstrated here. Please remove or make it clearer.

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

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