

***Interactive comment on* “Total column ozone variations over oceanic region around Indiansub-continent during pre-monsoon of 2006” by M. C. R. Kalapureddy et al.**

Anonymous Referee #1

Received and published: 30 April 2008

The paper reports on total ozone measurements obtained by a Microtops-II instrument on a ship cruise surrounding the Indian subcontinent. The paper particularly claims to document large systematic diurnal variation in total ozone. The authors attribute this large systematic diurnal variation to tropospheric ozone. To my knowledge, large systematic variation in total ozone has not been observed at any other place in the world except from measurements of the same type of instrument performed by the same group in Pune, India. In case of total ozone measurements of Dobson and Brewer instruments (the standard instrument used for total ozone measurements) systematic diurnal variations (at least when peaking at noon) are according to my knowledge usually the consequence of instrumental calibration errors.

Interactive
Comment

General comments: I strongly doubt the suitability of this type of instrument to measure systematic diurnal ozone variation. Microtops-II instruments have been used to trace day to day variability of total ozone with reasonable accuracy. Some similarities of day to day total ozone variability between satellite and the Microtops-II data (with a large unexplained offset of 23 Dobson units) have been found in the submitted study (see Fig. 2 in the paper). However, the systematic diurnal variation is basically a different feature than day to day variation and therefore the similarity in day to day variability between Microtops-II and satellite column ozone measurements hardly can be used to support the systematic diurnal variation. A systematic diurnal variation would change dramatically our present textbook knowledge, because no systematic diurnal variation is known from stratospheric ozone and 10% in total ozone variability would make a tremendous diurnal change in tropospheric ozone. Ozone in the troposphere systematically varies on a diurnal scale in the strongly polluted planetary boundary layer (namely as consequence of titration of ozone by NO during night and diurnal changes in inversion layer structure), but such a change is too low to explain a systematic variation of the magnitude claimed in the paper. At some distance away from strong emission sources (more appropriate for the measurements on the ship cruise) I don't see any possibility for explanation of a large systematic diurnal variability and particularly the decrease in ozone concentration in the late afternoon seems very suspicious to me. Indeed, ozone in the troposphere can change substantially as consequence of changes in transport of polluted air masses but this would not appear in any systematic diurnal way. The authors speculate about the effect of water vapor, but I don't see any plausible explanation for a dominant water vapor influence diminishing ozone in the afternoon when solar radiation becomes weaker. Because of my strong doubts concerning the suitability of the used instrument I don't support the publication of the paper.

Other critics: 1. Line 145: "If the three measured values of any parameter are not close in magnitude, the data set is rejected from further analysis": This sentence implies that the instrument produces sometimes numbers which obviously are erroneous.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

The authors don't make any statement, how they defined "parameters not close in magnitude" and how often such measurements occurred: did they exclude 5%, 10%, 20% or even more of the measured values ? 2. Line 147: "An inter calibration/comparison of several similar ozonometers operated aboard the ship by different organizations was undertaken to ensure the reliability of measurements": How many instruments were on the ship ? What means "similar" ? In what respect were the instruments different ? What were the results of the comparisons ? Please clarify. 3. Line 149: "Data recorded around cloud passage on or near field-of-view have not been considered for the analysis": I don't understand this sentence. How did you identify possible interferences by sub-visible cirrus clouds ? 4. Line 166-168: "Significant diurnal variation with well defined maximum during noon time": Particularly the coincidence of ozone maxima with local noon is suspicious to me (see general comments). 5. Line 170-171: Comparison with Ernest Ray et al., 2004 (measurements at Pune): If I understood the paper of Ernest Ray et al. correctly, a Dobson spectrophotometer was operated at Pune, where also the Microtops II data showed a systematic diurnal variation: Were the systematical diurnal variations in total ozone measurements at Pune supported by Dobson data (the Dobson spectrophotometer is the standard instrument for total ozone measurements) ? Was the Dobson spectrophotometer at Pune well calibrated and maintained (how looked the results of the intercomparisons with standard Dobson instruments ?) ? 6. Line 177-181: Significant differences between satellite and Microtops II measurements: I believe, that at least for the measurements coinciding in time with the overpass satellite measurements the reported bias between satellite and Microtops II measurements is much too large to be attributable to differences in slant path. 7. Line 178: When looking at Fig 2, top panel I would not say, that the day to day variations are "very much in phase with each other" 8. Fig. 4: Is this Figure obtained from the measurements interpolated along the ship track ? I don't believe, that the ship measurements reached 50 S in the Western part of the measurements 9. The similarities of spatial correlations between OMI-TOMS and Microtops-II measurements are not obvious when looking at Fig. 4, 5a and 5b (at least the high values in the Western

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

part of Fig. 4 are not visible in Fig. 5b). 10. The discussion of the paper is not well structured: the paper of Lelieveld et al.(2004) discusses long-term changes of ozone measured from ships over the Atlantic, which is a different item than the systematic diurnal variation claimed in the paper. I don't remember, that Lelieveld et al. reported about diurnal variability in surface ozone measurements of the Atlantic.

Therefore I unfortunately don't support the publication of the study

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3143, 2008.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

