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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical tropospheric ozone derived using Clear-Cloudy Pairs (CCP) of TOMS measurements" *by* M. J. Newchurch et al.

Anonymous Referee #1

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I've listed below several (mostly minor) comments for improving the manuscript. The more important comments are probably providing some estimation of errors of final derived CCP measurements (e.g., two-sigma=?), the stratospheric wave 1, N7/EP TOMS offset difference, and possibly providing more in the Results section given that lengthy 1979-2000 time series data were derived (the current Results section is small). Recommendation: I rate the paper as potentially very good and recommend publication after the authors consider the comments below.

REVIEWER COMMENTS:

1. Introduction, last sentence of first paragraph: Don't you mean that the method works best in the tropics where stratospheric ozone variability is generally small?

2. Introduction, second paragraph: Generally 0.2 reflectivity threshold probably won't

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make much difference in calculating clear-sky total ozone - the version 7 TOMS algorithm places at most a couple of DU below the assumed cloud height at the 0.2 reflectivity level. You might mention this when discussing this threshold value.

3. Introduction, third paragraph: The stratospheric wave 1 may be a weak part of the algorithm. I think that maybe the only study to indicate zonal wave structure variability in stratospheric ozone climatology was Newchurch et al. [2001b] (this should probably be mentioned in the paper). It is possible that the measurements may have been partially caused by ozone lying inside cloud tops over the Atlantic region where the upper levels in clouds may have considerably more ozone than over the broad Pacific region. The positive anomalies in Newchurch et al. [2001b] appear most predominant over Africa and South America. The wave 1 in Newchurch et al. [2001b] really looks more like three positive anomalies with influence (i.e., a "wave-3" with maxima over Africa/central Atlantic, South America and Indonesia), with relative minima in between. The paper should mention that a wave 1 fit in the algorithm represents a first-order approximation to perceived zonal variability in stratospheric ozone. In any case whether correct or not, it may not affect the final derived CCP tropospheric ozone significantly, given that this anomaly is only around 4 DU peak-to-peak on average. A stratospheric wave 1 variability your 5-day means could nevertheless be real and caused by tropical Kelvin waves or equatorial Rossby waves in the stratosphere. Monthly means won't show much variability from these tropical waves.

4. Introduction, last sentences of third paragraph: There is some confusion regarding the 9 DU and the archived values for CCD. Does the 9 DU refer to the EP TOMS time period only (using archived data without subtracting 5 DU)?

5. Section 2: Should state version 7 TOMS data were used (in Abstract too). Should also mention the footprint size for the level-2 data somewhere. In the future, smaller footprint size from new instruments will likely improve the CCP and several other retrieval methods, perhaps significantly.

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6. Section 3, line 11 (and elsewhere in paper): The archived CCD data estimated stratospheric ozone by averaging over both the eastern and western Pacific. Just as a note, the CCP and CCD methods are actually fundamentally very similar in that the main algorithm component for estimating tropospheric ozone with both methods is to use only high-reflectivity footprint scenes. Aside from an assumed wave 1 in the stratosphere (e.g., comment 3 above) the only significant distinction between the two methods is that the CCP technique measures high reflectivity scenes throughout the tropics and not just the eastern and western Pacific. This includes the broad Atlantic region where high reflectivity scenes may significantly over-estimate stratospheric ozone because of detected ozone inside cloud tops. The CCP algorithm attempts to correct for this multiple scattering cloud-error effect.

7. Section 5: Somewhere in this section there should be given some estimate of total combined error in the final CCP measurements (e.g., two-sigma of 5 DU?, 8 DU?)

8. Section 5.1: This section discusses the importance of retrieval efficiency in TOMS data. Soon in year 2003 the TOMS version 8 data will be released and these new data have an internal efficiency correction. Especially with reference to Martin et al. [2001], something should be mentioned that these efficiency corrections apply to version 7.

9. Section 5.3, line 6: "This offset varies..." The reason (perhaps contact Richard McPeters or Charlie Wellemeyer for more accurate discussion) is largely caused by a Raman scattering effect (molecular oxygen dimer O2-O2) in low reflectivity scenes (no clouds or low clouds, and a long scattering path in the troposphere) at 360 nm in EP TOMS. There may be other (smaller) contributions to this offset as well. It was found that the difference of version 7 minus version 8 throughout the tropics where you are measuring is about +6 DU.

10. Section 6, second paragraph: In your Figure 4 you must have applied interpolation to fill in missing data prior to low-pass filtering. Also, the latter half of year 2000 (where there are no measurements) looks a bit fishy in the low-pass filtered time series - I'd

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remove the latter points in these time series. If you look carefully you will find in each of the four plots that the beginning and ending time series values in the low-pass filtered data are equal. There appears to be some type of time series "end effect" present.

11. Section 6, third paragraph (and in Figure 5 caption): Please clarify if the difference is THIR minus no-THIR, rather than no-THIR minus THIR.

12. Section 7, first paragraph: Please clarify how tropopause pressure was determined. In the tropics, tropopause pressure should be nearly identical whether it's defined by cold-point, 2K per km, potential vorticity threshold, etc., but the definition used should be mentioned in any case. Also for Table 1 was the archived CCD data (without the 5 DU subtraction) used for these EP TOMS time-period comparisons?

13. Section 7, last paragraph: It is true that the CCD method makes no correction for cloud error. It should be mentioned that ozone in the upper troposphere is less over the Pacific compared to the Atlantic. Because of this, the cloud error problem will likely be small over the Pacific compared to the Atlantic (i.e., the CCD method is somewhat fortuitous by measuring only over high reflecting clouds in the Pacific).

14. Section 8, first sentence: I wasn't able to obtain data, etc. from the given web site (I may not have done this correctly).

15. Section 8: This section discusses results but seems very short (three short paragraphs and three figures). It would strengthen your paper if you could show some more results. Since you have already generated a lengthy 1979-2000 data set, why not plot some interannual variability time series, perhaps comparing CCP with CCD? Does the CCP data also show an interannual dipole structure about the dateline in the Pacific relative to the 1997 El Nino like CCD and 3D models? I'm sure that it will, but are there any significant differences between the two measurements for interannual variability? Can you add anything new (not yet published) regarding the 1982-1983 El Nino?

16. Section 9, line 6: "1984"

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 225, 2003.