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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.

M. J. Newchurch et al.

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Response to Reviewer #1 Comments

Question 1 Introduction, last sentence of first paragraph: DonŠt you mean that the method works best in the tropics where stratospheric ozone variability is generally small?

Answer: Yes, you are right.

Corresponding modification to the original paper: Introduction, first para, last sentence, change Ş Ě stratosphere is relatively benign.Ť To ŞĚ stratospheric variability is generally small.Ť

Question 2 Introduction, second paragraph: Generally 0.2 reflectivity threshold probably wonŠt make much difference in calculating clear-sky total ozone Ű 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.

Answer: Yes, you are right.

Corresponding modification to the original paper: Introduction, 2nd para: add Şof a couple of DUŤ after Şwill be included and may cause errorsŤ

Question 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. [2001a] 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 Şwave-3Ť 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 wonŠt show much variability from these tropical waves.

Answer: As you say, the cloud anomaly may be partially responsible for the wave-1 structure of the stratospheric ozone discussed in Newchurch et al., [2001b], but it can not explain the entire stratospheric ozone wave-1 4-DU amplitude (peak-to-peak difference is 8DU). This manuscript applies the cloud anomaly correction to the CCP tropospheric ozone which results in an amplitude of about 2DU (peak-to-peak differ-

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ence of 4 DU).

It is true that Newchurch et al., [2001b] is the only paper quantifying a climatology of a significant stratospheric ozone wave one. We have investigated all possible errors we know about and still can not explain the entire wave-one amplitude in stratospheric ozone except to ascribe the observed variations to be stratospheric ozone wave one. In that particular paper, we use cloud points at all tropical longitudes (including Pacific and Atlantic) to calculate stratospheric ozone. As you suggested, we also saw more evidence of wave activity at weekly time scales than at the monthly time scales used by other researchers.

Corresponding modification to the original paper: Intro, 3rd para, after 1st sentence, insert: ŞA wave-1 fit represents a first-order approximation to a phenomenon that probably contains higher order wave numbers. Part of the discrepancy between the stratospheric wave-1 pattern found by [Newchurch et al., 2001] using 5-day means and other researchers using monthly averages of stratospheric measurements may be explained by the potential effect of tropical Kelvin waves or equatorial Rossby waves that will not be apparent in monthly averages.Ť

Question 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 (6DU?) refer to the EP TOMS time period only (using archived data without subtracting 5 DU)?

Answer: Yes, the 9 DU refers to the EP/TOMS time period only.

Corresponding modification to the original paper: Introduction, last para. add Ş(using archived data without subtracting 5 DU)Ť at the end of this paragraph.

Question 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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Answer: Yes, thanks

Corresponding modification to the original paper: In abstract and Section 2, after 1st occurrence of ŚTOMSŠ, add Śversion 7Š.

Question 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 stratospheric ozone (e.g., comment 3 above) the only significant distinction between the two methods is that the CCP technique measures high reflectivity scenes thorough 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 zone because of detected ozone inside cloud tops. The CCP algorithm attempts to correct for this multiple scattering cloud-error effect.

Answer: The CCP technique uses both THIR and reflectivity to identify high-altitude clouds if THIR data are available. However, we found that using reflectivity for deriving tropospheric ozone produces similar results, so we extend the CCP technique to period after 1984..

Corresponding modification to the original paper: none

Question 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?, 8DU?)

Answer: The best estimate of the accuracy of the CCP comes from comparisons to the only independent, in situ data available, the SHADOZ ozonesondes. These results, shown in Table 1 and described in section 7, indicate a mean bias of 3 DU, 1 S. D. of 5 DU, and 1 Standard Error of the Mean over the comparison dataset of 1 DU.

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Corresponding modification to the original paper: Change title of section 7 to:Ť Accuracy assessment of CCPŤ

Question 8 Section 5.1: This section discussed 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.

Answer: Thanks

Corresponding modification to the original paper: Section 5.1, end of 1st para, add Ş This efficiency correction applies to version 7. The next TOMS data release, version 8, will incorporate an internal efficiency correction.Ť

Question 9 Section 5.3, line 6: ŞThis offset variesĚŤ The reason (perhaps contact Richard McPeters or Charlie Wellemeyer for more accurate discussion) is largely caused by a Raman scattering effect (molecular oxygen dimmer 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.

Answer: Thanks

Corresponding modification to the original paper: Section 5.3, para 3, replace Śoffset is not clearŠ with Śoffset is not currently clear; however, release of version 8 may show that Raman scattering by the O2 dimer at 360 nm is responsible for this offset.Š

Question 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 here are no measurements) looks a bit fishy in the low-pass filtered time series Ű ISd remove the latter points in these time series. If you look carefully you will

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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 times series Şend effectŤ present.

Answer: Thanks for pointing the Şend effectŤ problem.

Corresponding modification to the original paper: Section 6, 2nd para, before last sentence: ŞOne limitation of this filtering technique is the presence of end effects in the first and last ij cycles (1st and last 3 months) that will either increase or decrease the derived tropospheric ozone, perhaps by as much as 15 DU at some locations.Ť

Question 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.

Answer: It is THIR minus No-THIR

Corresponding modification to the original paper: Section 6, 3rd para, 2nd sentence, replace Ş Ě between THIR CCP andĚŤ with Ş(THIR CCP minus No-THIR CCP)Ť Caption for Figure 5, add Ş(THIR CCP minus No-THIR CCP)Ť after Ştropospheric ozoneŤ

Question 12 Section 7, first paragraph: Please clarify how tropopause pressure was determined. In the tropics, tropopause pressure should be nearly identical whether itŠs 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?

Answer: With regard to Table 1, CCD data has no 5 DU subtraction

Corresponding modification to the original paper: Section 7, after 1st sentence, add Şthe tropopause is determined using WMO tropopause definition when integrating sonde tropospheric ozoneŤ.

Table 1 caption, insert Ş The CCD data are from

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http://code916.gsfc.nasa.gov/People/Ziemke/ without any subtraction for NIMBUS-7/EP TOMS offset.Ť Before ŞCCDŠ refers to ĚŞ. Add to the end of the caption: Ş not the 5 DU subtraction from EP TOMS based on a clear-sky total ozone column offset between NIMBUS-7 and EP TOMS.Ť

Question 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).

Answer: We agree with you.

Corresponding modification to the original paper: Section 7, end of last para add: ŞBecause upper tropospheric ozone over the Pacific ocean is relatively low, the omission of these cloud effects in the CCD method is fortuitously less significant than it would have been over the Atlantic ocean.Ť

Question 14 Section 8, first sentence: I wasnŠt able to obtain data, etc. From the given web site (I may not have done this correctly).

Answer: It has been solved by our webmaster.

Corresponding modification to the original paper: None

Question 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? IŠm sure that it will but are there any significant differences between the two measurements for internannual variability? Can you add anything new(not yet published) regarding the 19982-1983 El

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Nino?

Answer: We agree that those are good science questions, but this paper is already long, so we plan to address them in our next paper.

Corresponding modification to the original paper: None

Question 16 Section 9, line 6: Ş1984Ť

Answer: Right.

Corresponding modification to the original paper: Change 1994 to 1984 in Section 9.

Newchurch, M.J., D. Sun, and J.H. Kim, Zonal wave-1 structure in TOMS tropical stratospheric ozone, Geophys. Res. Lett., 28, 3151-3154, 2001.

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