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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Occurrence of ozone anomalies over cloudy areas in TOMS version-7 level-2 data" *by* X. Liu et al.

X. Liu et al.

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Response to Referee Comments

We thank the reviewer of our paper Occurrence of ozone anomalies over cloudy areas in TOMS level-2 data by X. Liu, M. J. Newchurch, and Jae H. Kim (acpd_2002_0091). We very appreciate their thoughtful comments, which have helped us improve this paper. Most of the comments are taken into account in our revised paper. Response to specific comments are addressed below:

1. Introduction, 1st paragraph, 2nd sentence: A statement or explanation should be considered regarding what the latitudinal dependence of cloud-top pressure (CTP) is and why it is considered oversimplified.

Done. The original sentence "This correlation is caused mainly by the oversimplified assumption of latitudinal-dependence of the Cloud-Top Pressure (CTP)" is changed to

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"This correlation is caused mainly by oversimplifying that Cloud-Top Pressure (CTP) is dependent only on latitude but not on cloud types, cloud thickness, and regions (Thompson et al., 1993): Pcloud(atm) = 0.3 + 0.15[1-cos(2xlatitude)]."

2. Section 2: A general comment about the resolution (spatial and temporal) of the level-2 TOMS data should be considered. I know that the data is getting binned into 5x5 boxes, but I think it would help the reader(s) to know the density of the data points.

OK. First sentence in section 2 "We use the TOMS data from Nimbus-7 (N7) TOMS during 1979-1992 and from Earth-Probe (EP) TOMS during 1997-1999." is changed to "We use the global daily high-resolution TOMS level-2 data from Nimbus-7 (N7) TOMS during 1979-1992 and from Earth-Probe (EP) TOMS during 1997-1999. The spatial resolution at the nadir view is about 50 x 50 km2 for a N7 TOMS pixel and is 25 x 25 km2/39 x 39 km2 for an EP TOMS pixel before/after December 1997 (McPeters et al., 1996, 1998), larger at larger view zenith angle. On average, there are about 70 pixels in a 5 x 5 area."

3. Section 2, 2nd paragraph: This section discusses figure 1 and refers to "anomalous ozone distributions", however in the figure it states "total ozone distribution". I believe it is total ozone distribution, can you please clarify.

Fig. 1 (not just Fig. 1a) refers to the high correlation (Fig. 1c) between total ozone (Fig. 1a) and cloud reflectivity (Fig. 1b). That is why I say "anomalous ozone distributions over cloudy areas" instead of total ozone distribution only.

4. Section 2, 2nd paragraph: One of the criteria that you are using for binning the level-2 data is reflectivity of (30%. What is this criteria based on?

The original "reflectivity range is <= 30%" should be "reflectivity range is >= 30%." And it means the difference between maximum and minimum reflectivity in a 5 x 5 area. We want to avoid extreme cases where there are high correlation coefficients between ozone and reflectivity but the reflectivity range for that area is very small (i.e.

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all pixels are clear or cloudy with similar cloud fractions). Removing the 30% restriction has very small effects on the presented results except slightly increasing fractions of POAs and NOAs. Using a larger threshold will reduce fractions of ozone anomalies but again will not affect much on the interpretation of ozone anomalies.

5. Section 2, 3rd paragraph: This section states "There is a significant fraction of large negative or positive correlation coefficients". What makes the amount significant and above what threshold are you considering it to be large? It then goes on to discuss the selection of a correlation coefficient of +/- 0.5, which defines whether it is a positive or negative ozone anomaly, respectively. What level of significance does a correlation coefficient of 0.5 represent? It would give the reader a yardstick to know what 0.5 means in terms of statistical significance.

On average, the fraction of correlation coefficients between -0.5 and +0.5 is about 32%, which means about 68% (1-sigma) of the correlation coefficients are excluded if for a normal distribution. Also, if we apply Full Width at Half Maximum (FWHM) concept to the distribution of correlation coefficients especially in Figs. 3a and 3d, values +/- 0.5 are approximately the locations where the line FWHM intersects the correlation distribution. So the sentence "we select the intermediate values of +/- 0.5 as the criteria" is modified to "the intermediate values +/-0.5, between which includes approximately 68% (1-sigma for normal distribution) of the correlation coefficients, are selected as criteria." Using criteria slightly greater than 0.5 or less than -0.5 will not change the analysis why cloudy ozone anomalies occur, but reduce the fraction of POAs and NOAs.

6. Section 3, 1st paragraph: The authors make the statement that a significant portion of ozone anomalies occur in coastal areas and attempts to explain why this may be occurring. Have you checked into some work done by Cuevas (Cuevas et al., 2001) that deals with the Ghost (global hidden ozone structures from TOMS) effects of TOMS. It may be contributing to some of the enhanced coastal anomalies that you are seeing.

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Yes, I have checked that paper by Cuevas et al. (2001). The sea-land total ozone difference is mainly explained by topography difference and probably by different sea-land distributions of UV-absorbing aerosols. Also consistent land-ocean difference in cloud features and ozone retrieval efficiency difference caused by different sea-land surface reflectivity may contribute to their analyzed sea-land total ozone difference. They analyzed the sea-land total ozone difference over all-sky conditions (monthly average), and we studied the cloudy/clear difference. So it is difficult to evaluate what how their mechanisms lead to occurrence of ozone anomalies. I agree with you that these mechanisms by Cuevas et al. (2001) will contribute to our observed ozone anomalies over coastal areas (i.e. increasing the possibility of ozone anomaly occurrence), when the cloud distributions are correlated with sea-land topography. The original sentence "A significant portion of OAs (˜22%) occurs in coastal areas, suggesting that differences between land and ocean such as cloud features and terrain heights might contribute to OAs in coastal regions" is changed to "A significant portion of OAs (˜22%) occurs in coastal areas. Cuevas et al. (2001) found persistent total ozone difference between continents and oceans mainly caused by truncation of lower tropospheric ozone due to topography and probably the presence of more UV-absorbing aerosols over land. The sea-land total ozone difference caused by these mechanisms and by sea-land ozone retrieval efficiency difference due to different sea and land surface reflectivity, when correlated with cloud distributions, will increase the possibility of ozone anomaly occurrence."

7. Section 3, 2nd paragraph: Here the authors mention the N7/EP TOMS bias. Is the bias, that is referred to, the 5 DU increase in the N7 data versus the EP data stated in the next sentence? You might want to include a statement that clearly states the bias.

Yes, it needs to be clarified. The bias refers to the N7/EP difference in ozone anomaly occurrence described before that sentence. So the sentence "Despite the N7/EP TOMS bias" is changed to "Despite the N7/EP TOMS difference in the distribution of POAs and NOAs"

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8. Section 3, 3rd paragraph: This section discusses Figure 5. I am not sure what this figure is trying to show other than that the ozone is more variable across the mid-latitudes than across the tropics. In the case of a positive anomaly during cloudy conditions (reflectivity of 100%), it appears that there is an overestimation of the amount of ozone by the amount shown. While in the case of the slope of a negative ozone anomaly, it appears that it shows an underestimation of that amount of ozone. Is this the correct interpretation or am I missing something? Also, the NOAs for both N7 and EP look very similar, while the ozone fraction of N7 and EP (Figure 4) do not look very similar. Can you please explain?

This figure shows that the magnitudes of linear regression slopes between ozone and reflectivity (i.e. cloudy/clear difference) are not small. It also shows the spatial distribution of ozone anomaly slope, which will help us understand the causes of ozone anomalies. If I do not show this figure, one will question what the magnitudes of the slopes of ozone anomalies is. If the magnitudes are too small, then why we need to worry about those ozone anomalies. You are right that a positive anomaly indicates that there is more ozone over cloudy areas relative to clear areas with an amount equal to the slope multiplied by the reflectivity difference between cloudy and clear areas. As you pointed out, the slope does not depend much on the sign of an ozone anomaly. Fig. 4 refers to the fraction of ozone anomalies while Fig. 5 refers to the average slope of those ozone anomalies not matter how large is the fraction. Both mid-latitude POAs and NOAs are mainly caused by the dynamic features and are related to the total ozone variability, maybe that is why the slopes are similar between NOAs and POAs. Although NOAs and POAs are caused by different mechanisms in the tropics, they happen to have similar magnitudes on average at most regions, but could have a large difference at a particular region and a particular time.

9. Section 3, 4th paragraph: Here the authors look at ozone anomalies relative to two different El Nino periods. In the text, you say that Fig 6 compares the average fraction of ozone anomalies during non-El Nino periods and El Nino periods. The figure only

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focuses on negative ozone anomalies. You should clarify, in the text, that you are only looking at negative ozone anomalies.

Done. The "OAs" in "Fig. 6 compares the average fractions of OAs during the non-El Nino periods" is changed to "NOAs" instead.

10. Section 4.1, 1st paragraph: In this section, the authors discuss the utilization of the THIR data t o help analyze errors in assumed monthly mean CTPs from the ISCCP cloud data.Some references should be considered that exhibit the utilization of the THIR data for such analyses and also discuss what the THIR data is and its limitations. As far as Fig 7, I understand what the figure is saying. However, I think a statement about what the 14 points represent (such as certain pressure levels or layers) should be considered.

The paper by Stowe et al. (1989), which discusses the THIR algorithm its results, is added as a reference. A sentence "THIR CTPs are derived from radiances measured at 11.5 ęìm along with the NCEP/NCAR (National Centers for Environmental Prediction/ National Center for Atmospheric Research) 2.5 x 2.5 6-hourly grids of temperature profiles (Stowe et al., 1989)" is added before "The six year (1979-1984) THIR CTPs are collocated with.....". A sentence is added after "The six year....": "The major sources of potential errors in the derived CTPs would be in the accuracy of NCEP/NCAR temperature profiles and the collocation of THIR and TOMS pixels over broken cloudy conditions (Newchurch et al., 2001)." "Each of the 14 point represents the average CTP pressure difference for clouds with CTPs within a 50-hPa layer ranging from 800 hPa to 100 hPa" is inserted before "On average, the assumed CTP is overestimated with....."

11. Section 4.1, 1st paragraph: Here the authors mention the (delat-P correction method from Newchurch et al. 2001. A statement that briefly describes what this correction is would help the reader to better assimilate what you are trying to show and why it applies to this investigation. I think it is an important piece of information that

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should be included in the text versus just referencing it.

Yes. We add "The delat-P correction method corrects three types of cloud-height-related errors using pre-calculated look up tables of these errors: radiation interpolation error, ozone retrieval error above clouds, and ozone retrieval error below clouds." before "Correcting incorrect cloud heights help us understand other causes of OA formation."

12. Section 4.1, 3rd paragraph: The last sentence talks about the use of the ISCCP cloud climatology for the 86-87 and 91-92 El Nino events, which is contributing to the smaller differences. However, doesn't only the cloud climatology only run through 1990? I think this should be clarified.

Yes. The sentence "The much smaller increase in the 1986-1987 and 1991-1992 El Nino events relative to non-El Nino periods is not only because of the weaker El Nino activities but also because the TOMS algorithm uses the ISCCP cloud climatology from these two years" is changed to "The much smaller increase in the fraction of NOAs for the 1986-1987 and 1991-1992 El Nino events relative to non-El Nino periods is because of the weaker El Nino activities during these periods. The used cloud climatology from 1986 and 1987 for the 1986-1987 El Nino event also reduces the increases in NOAs."

13. Section 4.1, 4th paragraph: In this paragraph, the authors discuss the use of tropospheric ozone climatologies. It is confusing since, up until now, you have been discussing total column ozone (TCO). In this discussion of tropospheric ozone climatology you have four references and state that there is an incorrect tropospheric ozone climatology being used. Which climatology are you referring to?

It refers to the tropospheric part of the TOMS standard climatology of ozone profiles. To clarify this, "Considering the incorrect tropospheric climatology used in TOMS data" is changed to "Considering the incorrect climatological tropospheric ozone added below clouds to complete the total ozone column."

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14. Section 4.3, 2nd paragraph: The 2nd sentence in this paragraph states that earlier surface observations indicate that the TOC at mid-latitudes is usually correlated with the surface pressure and upper tropospheric pressure. I think you need a reference here. Are you talking about earlier in the paper or the sentence before this one or what? The next sentence states that high ozone is usually associated with cyclones and low ozone with anticyclones. At the surface, high ozone is usually associated with anticyclones. I think the increase in ozone you 2 aąärre referring to is when an upper tropospheric trough digs south (in latitude) and allows the transport of ozone-rich stratospheric or high latitude air to mix into the upper troposphere or lower latitudes. This then allows the column of tropospheric ozone (in the midlatitudes) to increase, but not necessarily impacting the surface. The whole paragraph seems to move between the surface and total column. The increase in the ozone that you 2 aąärre referring to does occur in the mid-latitudes and is common in the winter and spring seasons, however it 2 aąärs more a function of increase in the total column ozone in the midlatitudes. Stating that high ozone is usually associated with surface lows in the mid-latitudes is not always the case and can sound confusing or contradictory. I understand the upper tropospheric trough connection to increased total ozone. However I think this paragraph needs to be reworked to clarify the difference between increase in total ozone and its connection with surface low and high-pressure systems, since surface systems can contribute differently to surface ozone.

"Earlier surface observations" refer to the measurements by Dobson and Harrison (1926) et al. (1926, 1928). For the rest of this paragraph, I did not talk about how surface ozone is affected by anticyclones and cyclones but total ozone, in which the largest contributions result from the lower stratosphere and upper troposphere. And I agree with you how the cyclones and anticyclones affect total ozone. The reason that I mention "surface" is because an anticyclone is usually associated with a surface high-pressure system and a cyclone associated with a surface low-pressure system. Here the surface low means surface low-pressure system instead of lower surface ozone. To clarify this paragraph, the following are modified. The second and third sen-

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tence in this paragraph is removed. "More recent studies have examined this ozone-meteorology relationship by analyzing satellite data (Vaughan and Price, 1991; Salby and Callaghan, 1993; Hudson and Frolov, 2000)" is changed to "Recent studies have examined the influence of middle latitude disturbance on total ozone distribution using satellite data (Vaughan and Price, 1991; Salby and Callaghan, 1993; Hudson and Frolov, 2000, Olsen and Stanford, 2001)." The sentence "The dynamic disturbance associated with synoptic weather systems and planetary-wave activities can change tropospheric ozone as well as lower stratospheric ozone with a magnitude up to 30% of the TOC in the mid- and high- latitudes (WMO, 1998)" is changed to "The dynamic disturbance associated with synoptic weather systems and planetary-wave activities can change lower stratospheric ozone with a magnitude up to 30% of the TOC in the mid- and high- latitudes (WMO, 1998, Olsen and Stanford, 2001)." The sentence "The much higher fraction of POAs than NOAs at mid-latitudes supports the fact that high ozone is usually associated with cyclones and low ozone is usually associated with anticyclones" is changed to "The much higher fraction of POAs than NOAs at mid-latitudes supports the fact that high TOC is usually associated with cyclones and low TOC is usually associated with anticyclones over pollution-free regions." "surface low" in the text is changed to "surface low-pressure system."

15. Section 4.4, 1st paragraph: The last two sentences make reference to Shadoz. I think you need to reference it here. Since you are using it to conjecture about the relationship between tropospheric ozone and marine stratocumulus clouds, a statement about this particular effort and what it found would go along way to allowing the reader to make this connection.

A reference (Thompson et al., 2003) is added after the "1998-2000 SHADOZ monthly mean tropospheric ozone at Ascension". Two sentences "SHADOZ project established a network of 10 southern hemisphere tropical and subtropical stations to perform regularly measurements of ozone profiles (Thompson et al., 2003). The station at Ascension is very close to WCSAF." is added at the beginning of the next paragraph.

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16. Section 4.4, 2nd paragraph: The explanation concerning Figure 12 (a) is a little confusing. You say that tropospheric ozone at Ascension ranges from 29.0 DU in April to 48.7 DU in October. However, the figure gives 50-hPa layer ozone amounts. So is the 29.0 in April an integral of each of the 50 hPa layers shown in the profile from 1000-100 hPa? If that is the case, I think a description that better explains how you interpret this figure should be considered in this paragraph.

OK. The sentence "The tropospheric ozone at Ascension ranges from 29.0 DU in April to 48.7 DU in October" is changed to "The tropospheric ozone column integrated from 1000 to 100 hPa profiles in Figure 12 (a) at Ascension ranges from 29.0 DU in April to 48.7 DU in October."

17. Section 4.4, 4th paragraph: The authors should consider using the term "a photolysis rate" instead of "a j-value" when talking about the production of ozone above low-altitude clouds, since it is a more readily identifiable term.

OK. The term "j-value" is changed to "photolysis rate coefficient"

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