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***Interactive comment on* “Development of a climate record of tropospheric and stratospheric ozone from satellite remote sensing: evidence of an early recovery of global stratospheric ozone” by J. R. Ziemke and S. Chandra**

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**COMMENTS BY ANONYMOUS REFEREE#2:**

The authors first evaluated the convective-cloud differential (CCD) method with Aura OMI ozone measurements. Tropospheric and stratospheric column ozone data using the CCD method are compared with the Aura OMI/MLS residual measurements. The authors showed the two datasets are within a few Dobson Units in the Pacific as well as the zonal means from the tropics to high latitudes. The authors then extended the

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CCD method to earlier UV instruments and developed a 32-year long dataset of tropospheric and stratospheric column ozone. Quasi-Biennial Oscillation (QBO) signals in tropospheric column ozone and trends in stratospheric column ozone were examined with this long time record. Overall, the study is well conducted and written. The 32-year long dataset of column ozone measurements from satellite instruments provides valuable information to assess the ozone trends. I recommend publish on ACP after addressing the following minor comments.

Specific comments:

1. Page 3178, Line 3: Suggest delete "unrelated". It is not clear to me what it relates to.

This paragraph has been re-worded in the revision.

2. Page 3179, Line 2-3: In Figure 4, there are persistent negative values of cloud ozone over the northern Pacific except in summer. Is it a possible factor that MLS SCO are biased high?

It is possible that MLS is biased high rather than OMI being biased low in the higher latitudes during winter which could yield negative differences of OMI minus MLS, but we cannot determine the exact cause. On this point we mention in our paper that there exist unresolved offset errors in these regions/times between OMI and MLS measurements associated with the thick clouds.

3. Page 3180, Line 5-13: I do not understand why the authors compare OCCP cloud pressures with IR cloud-top pressures here. Would it be more direct to compare OCCP cloud pressures with tropopause pressures? In that way, we may have some hints whether the large positive cloud ozone values in the extra-tropics are associated with large differences between OCCP cloud pressures and tropopause.

This is a good point mentioned also by Referee #1 (the 8th comment). Please read our response to the Referee #1 comment. We discuss this issue in the revision.

4. Page 3181, Line 13-15: Please describe the implications of these large cloud ozone over the extra-tropical regions. Does it reflect that TCO data using the CCD method over these regions can be underestimated by up to 50%?

In the paper we have re-worded and clarified the sentences.

5. Page 3182, Line 24: The word "shown" is redundant.

Word has been deleted.

6. Page 3184, Line 11-12: Should the offsets in TCO be the same as the offsets in SCO shown in Fig. 10, because the two methods use the same OMI total column ozone measurements?

We mention this in the revision for Figure 11 – we had noted this for the RMS amplitudes in Figure 13 but we hadn't noted this for Figure 11.

7. Page 3184, Figure 13: Can you please explain why the RMS values for zonal means are smaller than those for the Pacific means where the CCD method are robust? Does it reflect there are compensating errors when averaging zonally or reflect effects from deseasonalization?

We have added discussion in the revision describing likely reasons including having more gridded measurements for zonal means (better signal to noise and statistics), and also that offsets between CCD and MLS SCO appear everywhere as largely recurring annual cycle variations which are greatly reduced/eliminated following deseasonalization.

8. Page 3186, Line 27-28: Please explain the underlying physical process that causes the negative correlation between zonal winds and tropospheric column ozone.

In the tropics the QBO signal detected in TCO in Figure 14 is anti-correlated with 50 hPa zonal winds and also anti-correlated with SCO. The negative correlation between TCO and SCO in the tropics was discussed in earlier studies referenced in the paper,

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but as of yet this relationship is not fully explained. Lee et al. [2010] using SHADOZ sonde measurements along with Ziemke and Chandra [1999] and Chandra et al. [2002] from TOMS measurements all concluded dynamical forcing rather than a photochemical effect as the most likely source for the QBO signal in TCO. Understanding the tropospheric QBO signal from a modeling perspective is a task that we are currently working on but which is beyond the scope of our current paper.

9. Page 3188, Line 26-29: In Figure 16, are the column ozone averaged over the Pacific or all longitude range between 60S and 60N? Are the SCO data record with the CCD method only averaged over the Pacific?

This is noted in the revision.

10. Page 3190, Line 6-12: I also suggest point out other possible factors contributing to the observed recovery of stratospheric ozone layer in addition to the Montreal Protocol, such as changes in transport and temperature. A review is given by Weatherhead, E. C., Andersen, S. B.: The search for signs of recovery of stratospheric ozone layer, *Nature*, 441, 39-45, 2006.

We have re-written the final section on issue of stratosphere ozone recovery detection involving dynamical factors and the Montreal Protocol. We include several new references in the revision including Weatherhead and Anderson [2006].

11. Page 3199, Fig. 4: Suggest add a color bar. The contour values are difficult to read.

We have re-done the figure to make the line contour numbers easier to read. Part of the previous problem may have involved the PDF conversion.

12. Figure 3204, Fig 9: Please describe in the caption the contour values and increment.

Done.

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