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Interactive comment on “A global climatology of tropospheric and stratospheric ozone derived from Aura OMI and MLS measurements” by J. R. Ziemke et al.

Anonymous Referee #1

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The paper describes a climatology of tropospheric and stratospheric column ozone computed from 6 years of Aura OMI and MLS data and its methodology builds on Ziemke et al (2006). Such a data set can be very useful to scientific community, to modelers in particular. Examples of applications are given in Section 5. The manuscript meets the standards and scope of ACP.

I have a few initial comments and suggestions to kick off a discussion

1) Introduction L10. "it remains to be shown that invoking more sophisticated methods beyond simple interpolation [...]". Agreed. However, and I'm very close to citing some work that I've been involved in, there exist recent studies exploring this topic to some

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degree (e.g. Doughty et al (2011)). They demonstrate that there are advantages to data assimilation. Simple methods can work very well for coarse-grid climatologies. On the other hand the strength of assimilation is its ability to provide global fields at synoptic times so those two approaches aim at a bit different goals. I realize that this is a matter of subjective opinions.

2) Section 3. L25 "The left panel in Fig. 3 represents station latitudes 25S–50N and the right panel is the same but includes stations poleward of 50N." This does not match Figure 3. The title over the right panel and the caption say 25S - 90N not poleward of 50N.

3) Section 3. In addition to the discussion of the climatology minus sondes differences in the final paragraph it would be helpful to have an estimate of errors in sonde ozone. Don't some locations exhibit large differences between ozone derived from sondes and from Dobson and lidar measurements? If such estimates are known this could help explain of some of those very large RMS difference values in Table 1

4) I find Figures 1 and 2 very interesting. They show a nice agreement of the new climatology with ozone sondes. There appear to be some systematic biases though which are not mentioned in the text. Specifically, in the Tropics. the high values are too high compared to the sondes. This is also clearly seen in the scatter plot whose slope appears to be greater than 1 (is that statistically significant?). In the extratropics (Figure 2) the low values seem too low and, consistently, the slope of the scatter plot is slightly less than 1. Are those two biases related? Is it possible to trace them back to their sources (the use of MLS below recommended levels, bias in sonde measurements?). I would like to see a little more discussion here.

5) Both, tropospheric and stratospheric ozone exhibit a great deal of interannual variability due to dynamics. For example, stratospheric values were unusually high in the northern high latitudes in 2010 (e.g. Steinbrecht et al. 2011 GRL). The authors state it clearly that their product is specific to years 2005-2010. Would it be possible to include

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plots of, say standard deviations along with the annual mean (Figures 7 and 9)? This would be helpful to users. Alternatively, could tropospheric ozone for El Nino and La Nina years be shown separately?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17879, 2011.

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