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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Harmonisation and trends of 20-years tropical tropospheric ozone data" *by* Elpida Leventidou et al.

Anonymous Referee #2

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Review of the paper by Leventidou et al.

The authors present in the paper a detailed trend analysis of tropospheric ozone over the tropics, using a long term homogenized data set based on satellite measurements using the Convective Clouds Differential method. This method and its application on individual satellite sensors has already been presented in various studies including a publication from the same group in AMT (Leventidou et al. 2016). In this paper they homogenize the data from three sensors and examine the variability and the trends over regions and mega cities within the tropics. The paper is well written and structured but there are many significant issues that should be considered before being accepted for publication in ACP.

The content of the paper has many similarities with the paper by Heue et al. 2016 in

AMT. Although it is clear that the Heue et al., paper uses a different version of total ozone data it is not clear from the current paper what are the differences between these two data sets concerning the application of the CCD method and the resulting tropospheric ozone estimates. The authors should elaborate more here.

Section 2.2 The authors attribute most of the differences between the TTCO mostly to the different cloud algorithms involved. Why they exclude eventual biases between the sensors also in the initial total columns? Is there any explanation for the different behavior if GOME-SCIA differences over 10oN shown in Figure 1? The authors should also provide an explanation for the GOME-2/SCIA drift. Does this originate from a potential drift in the total columns?

Section 2.3. The discussion of six scenarios in the paper is confusing, since they don't differ substantially concerning the outcome. I think the authors should just describe here the chosen approach of harmonization.

Section 3.3.1. The authors present regional trends in this sections. The choice of the regions to my understanding is based only on the significance of the trends and in a sense this looks like a random choice. Do these regions have some special characteristics that have to do either with prevailing dynamic features or emission sources? In general the discussion here should be improved.

Section 3.3.2. The authors attribute the positive trends to South Africa and South America to biomass burning. Is there any indication from another source that there is increased biomass burning over the years that can cause such a trend?

Section 3.3.3. The authors show trends over mega-cities in the tropics. The authors should provide a comment why they think a grid-point of 2x5degrees can represent the variability of tropospheric ozone caused by a mega city. The discussion against NO2 trends as shown in the paper is also not conclusive. Are there studies (modelling or in-situ ones) to support their findings? The authors also compare their results with Heue et al and although the approach is pretty similar they are differences. They should elaborate more here to explain this.

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