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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Representativeness and climatology of carbon monoxide and ozone at the global GAW station Mt. Kenya in equatorial Africa" *by* S. Henne et al.

Anonymous Referee #1

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General Comments

This version of the manuscript is improved greatly, although it is still quite long. One suggestion is to cut the section on the Horizontal Distribution of CO (see below). The distinction between biomass burning impacts on the MKN site between plumes versus the general influence on background southern hemispheric air needs to be made more clearly to avoid confusion. The authors have done a nice job of distinguishing source regions and presenting it in an understandable manner. Overall, this manuscript presents a thorough discussion of large-scale processes affecting the MKN site. The O3 comparison with the SHADOZ profiles nicely puts a perspective on the data set,

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Interactive Discussion

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and makes an important point (see below). A second manuscript focused on the upslope/downslope flow and diel cycles of O3 and CO would be a nice follow up to this one.

Specific Comments

Abstract:

Lines 20 – 22: As written, this sentence seems contradictory to lines 9 - 10. If biomass burning rarely impacts the site, how can it explain inter-annual variations? I think what you mean is that biomass burning in the southern hemisphere in general increases the background mixing ratios. These to concepts need to be more clearly presented in the manuscript. Also, “in transport patterns” should be more clearly written. It’s not clear if the authors mean flow to the site, or if “in” should be deleted from the sentence.

Introduction:

This section is greatly improved with the addition of relevant field campaigns and other information.

Methods:

Site – I suggest adding a few sentences on the vegetation surrounding the site, the height of the tree line, etc., since surface deposition of O3 appears to be important.

Instruments - The correlation of your CO measurements with NOAA-GMD (Global Measurements Division) was not that good (r = 0.79), but comparing weekly canisters with continuous data is not an easy thing to do with a small sample size. I would revise the first line on p.17776 to reflect this (make it less blunt a statement). I would also include a statement of the time frame when these comparisons were conducted.

The figures in this manuscript are plotted in UTC, but state what the local time difference is for MKN to facilitate the readers interpretation if your results.

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Trajectories - This section now reads well.

O3-CO Correlations - Even though Parrish et al. (1993) used it to estimate O3 export from the northeastern U.S., the value of the ratio is of course time dependent and thus can not really be used “to estimate the total ozone potential”.

Lines 20 - 21: Our experience has been that models can not reliable simulate the O3-CO relationship for many reasons. There are just too many uncertainties involved.

Results and Discussion:

Line 26: This sentence should be re-written.

Figure 2 is a nice presentation of your results. Because the Syn-Var and Therm-Var have very similar diel trends, does this reflect a regional ABL influence? For the Syn-Const case, could this represent FT air? (Although it is similar to the Therm-Const. arguing against this interpretation.)

Only a very general discussion of results presented in Figure 2 is given, but the difference between the timing offset in diel CO maxima and O3 minima is intriguing.

p. 17785, Line 14: Should re-word this sentence to something like, “below 4 km and in the marine boundary layer”.

p. 17789, Line 21 - 23: I would not call an r2 value of 0.3 large. To me this is an indication of little to no correlation which is typical of aged air masses. Alternatively, high CO mixing ratios typically result from smoldering fires, with low NOx emissions. Thus, low O3 might be expected from this scenario. I believe that fire type, smoldering or flaming, might be a reasonable explanation. Did it happen to be wetter this year?

p. 17789, Line 28: I believe that there should be “Fig.” before the 10.

p. 17790, Lines 12 - 13: Add units to avoid any confusion; ppbv/ppbv. The values of these slopes seem rather high to me, especially looking at Figure 3. However, since the raw data is not shown in the manuscript, it’s only a guess.

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p. 17790, Lines 9 - 10: I am not surprised at this result. FT data at this location is a heterogeneous mixture of clean and biomass influenced air masses. When these are mixed together it results in low correlations. Perhaps sorting the data and using only values higher than the month median values or some other appropriate parameter would yield a better result for photochemically active air masses.

p. 17792, Line 27: Add “be” before 0.75.

p. 17793, Horizontal Distribution of CO: This section was a nice idea, but in reality I’m not sure it adds much to the manuscript due to the large discrepancies with the MOPPIT data. The aeral pictures can not be verified.

p. 17795, Line 19: Should add “of” or an “=” before 7 ppbv.

p. 17795, SHADOZ O3; This subsection should be retained since it gives a larger scale picture of the data collected at MKN - its more representative of altitudes well below the summit. To me, this is an interesting point, and perhaps a result that should be emphasized more in this manuscript. Overall, the data collected at summits with upslope/downslope flow patterns are more representative of lower elevations.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17769, 2007.