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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.

S. Henne et al.

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Reply to Referee 1

We would like to thank the anonymous referee for his/her comments that will improve the presentation of our results.

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, 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.*

Most of these more general statements are discussed in more detail below. A second manuscript mentioned by the referee is currently under revision and should be published soon in the Journal of Applied Meteorology and Climate. It contains a detailed discussion of the local meteorology and local flow systems influencing the measurements at the Mt. Kenya GAW site. Further discussion of the diurnal cycle of O3, CO and other parameters (such as black carbon) might follow with the ongoing extension of the measurements.

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 two 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.

As correctly stated, biomass burning only indirectly affects the MKN measurements by changing concentrations in the southern hemispheric background. There is rare direct transport of biomass burning emissions towards the site. However, the transport of background air masses from southern Africa and the southern Indian Ocean towards MKN undergoes certain variability that influences the measured mixing ratios. This aspect of our results was better highlighted in the revised version of the manuscript.

Introduction: 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

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important.

The surroundings of the site were discussed in more detail in Henne et al. 2007, Mt. Kenya GAW Station (MKN): Installation and Meteorological Characterization, submitted to Journal of Applied Meteorology and Climate. We added a brief discussion of the main vegetation features in the revised manuscript. Below the station grassland (*Festuca pilgrim, Carex spp.*) and shrubs (*Artemisia afra, Protea, Helichrysum*) dominate the vegetation. Above the station grassland and sparse lobelia (*Lobelia telekii, Lobelia keniensis*) are the only vegetation. The timberline is situated about 5 km to the northwest of and 500 m below the site. Therefore, the influence of the forest on O3 deposition is considered minor especially during night-time when the prevailing winds are down-slope (from the south-east). However, the role of dry deposition in the grassland environment is discussed in more detail below.

Introduction: 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.

We agree that the correlation was not very good and also see a main reason in the rather small sample size. Furthermore, the comparison is exacerbated by the fact that samples are not taken from the exact same volume of air. CO fluctuations at the time of the day when canister samples are usually taken are strong because the up-slope wind just starts to lift up air masses with larger CO concentrations. Canister samples would better be taken at night. However, this is not feasible because regular overnight stay or access to the site during night is not possible. We added a statement mentioning the difficulties of the inter-comparison and we give the time frame of the comparison.

Introduction: The figures in this manuscript are plotted in UTC, but state what the local

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time difference is for MKN to facilitate the readers interpretation if your results.

We unfortunately forgot to mention the time difference of Kenyan local time compared to UTC, which is +3 hours. This information was added to the site description.

Introduction: Trajectories - This section now reads well.

We are happy to hear this.

Introduction: 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".

We agree and follow the suggestion of the referee and have changed our statement to "O3 export".

Introduction: O3-CO Correlations - 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.

Since we have no own experience with the simulation of O3-CO relationships we have to trust the judgement of the referee. We made this statement after reading an earlier study by Chin et al. (1994, JGR, 99, D7) who suggest that O3-CO relationships would be a better test for the ability of a model to accurately compute the actual O3 production. However, we followed the referees comment and removed the respective sentence from the manuscript.

Results and Discussion: Line 26: This sentence should be re-written.

We rewrote the sentence.

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.)

We regret that we did not clearly explain the assumptions underlying the categorization of individual days. The main idea was to identify days that do not show a diurnal cycle in CO and therefore no ABL influence without using the CO data itself. Two independent criteria - the first based on the most likely transport process (thermally induced wind) and the second based on specific humidity as an indicator of ABL influence - were combined. All of these categories show a diurnal cycle, which means that the transport process that we thought would cause the diurnal cycle can not be the only reason for the diurnal amplitude. A second process (convective mixing in the ABL and the ABL top reaching above station altitude) was identified. With these two processes in mind it is also possible to explain the different timing of the CO maxima for the categories therm and syn. The first maximum is reached by direct ABL air transport in the slope wind layer (category therm), while the observed second maximum (category syn) is caused by the growth of the ABL top reaching its maximum extent later in the afternoon.

We extended the discussion of the underlying concept and of the results in the revised manuscript.

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.

This intriguing offset can already be noted in Fig. 1. There we argue that ABL influence and photochemical destruction of O3 under low NOx conditions explain the O3 diurnal cycle. O3 destruction reaches its cumulative maximum just at sunset, corresponding to the observed O3 minimum. The strong increase of O3 after sunset can then only be understood if O3 destruction was smaller for air masses at higher altitudes. This is likely since O3 destruction will depend on the availability of H2O which strongly decreases above the ABL.

p. 17785, Line 13: Should re-word this sentence to something like, "below 4 km and in the marine boundary layer".

We changed this.

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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?

We agree that an r^2 of 0.3 is not large on an absolute scale, but in the context of the correlations observed at Mt. Kenya it is. However, we agree with the statement of the reviewer that we need to change our interpretation and confirm that little O3 production occurred within these air masses. The possible reason given by the referee, little NOx emissions from smoldering fires, can also be corroborated by the fact that the high CO observed during these periods most likely originated from regional (Kenyan) sources. The vegetation in the Kenyan highlands is relatively dense and therefore the fraction of smoldering fires might be larger than for savannah type fires that dominate further south. We changed the respective passage in the revised manuscript.

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

That's correct and was changed in the revised manuscript.

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.

The values of the slopes are indicative only since the correlations were not highly significant. The regression method used in this study tends to yield large slopes for conditions with little correlation. We added a statement to the manuscript that explains these concerns.

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

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only values higher than the month median values or some other appropriate parameter would yield a better result for photochemically active air masses.

This is exactly what we intended by analyzing the CO-O3 relationship for the individual air mass origins identified by the cluster analysis. Since this also did not yield more significant results we don't expect any better results from selecting only values above the mean or median. From the individual scatter plots of CO and O3 it was not possible to detect any more obvious correlations for the higher range of both parameters.

p. 17792, Line 27: Add "be" before 0.75.

We did this.

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.

Since also referee $\#2$ criticized this section because of the large uncertainties involved in the trajectory statistics, we decided to remove it from the revised manuscript. However, we are considering to replace it with a MOPITT inter-comparison at the location of MKN.

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

We did this.

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.

This subject is also emphasised by referee 2. The differences between sounding and mountain measurements might not stem from dry deposition in the vicinity of the station

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alone but might result from a systematic bias of the soundings at low altitudes. The flow towards the site during night-time is in general in down-slope direction. Above the site mainly grassland dominates the slopes of the mountain. Typical values for O3 dry deposition values for night-time grassland are about 0.1 cm/s. In order to deposit 10 ppb O3 from an initial concentration of 40 ppb it would than be necessary to advect air in very shallow layers (< 20 m) along the slope for more than 1 hour. Average down-slope wind speeds at MKN are in the order of 5 m/s. Within 1 hour air traveling at this speed would be out of region of influence of the mountain. Therefore, we are not convinced that dry deposition is the main cause for the observed differences between sounding and mountain measurements. We added an extended discussion of this comparison and the potential of O3 deposition to the revised manuscript.

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

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