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Interactive comment on “Long term measurements of sulfur dioxide, nitrogen dioxide, ammonia, nitric acid and ozone in Africa using passive samplers” by M. Adon et al.

Anonymous Referee #2

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The manuscript “Long term measurements of sulfur dioxide, nitrogen dioxide, ammonia, nitric acid and ozone in Africa using passive samplers” by M. Adon, C. Galy-Lacaux, V. Yoboué, C. Delon, J. P. Lacaux, P. Castera, E. Gerdvat, J. Pienaar, H. Al Ourabi, D. Laouali, B. Diop, L. Sigha-Nkamdjou, A. Akpo, J. P. Tathy, F. Lavenu, and E. Mougin reports interesting data of atmospheric trace gas concentrations over a wide spread area of Africa. This data set spanning the years 1998 to 2007 certainly warrants publication. Employing passive samplers can be seen as an adequate method for measuring atmospheric trace gases for the given local situations. So this contribution helps to fill our gaps of knowledge about African trace gas concentrations, a wide field of ignorance.

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By and large the manuscript is well prepared. Nevertheless, some questions, remarks, thoughts may be appropriate.

One part covers the samplers. One would like to learn about the sampling efficiency in the extremely humid conditions possible in the tropics. Side by side measurements of ozone with a continuous instrument, for instance, in dry and extremely humid seasons (of Lamto ?) may shed some light. Another question related to humid and warm conditions is the stability of the samples versus microbial activity. Are especially the nitrogen containing compounds 'not digestible' when trapped on the impregnated paper disks ? Own experience tells that gaseous ammonia and nitric acid mixing ratios are unstable when enclosed in stainless steel walls. So contact to the stainless steel mesh at the sampler is somewhat irritating. The coating solution reported for assessing ozone presumably is not NO_2 (named in Table 3). Samplers may suffer interferences as said in the manuscript, what kind of interferences were encountered ? Break-through (saturation) obviously was never experienced after a month of collection ? Calling a correlation of $R^2=0.76$ good, may be a bit daring. What would be of more interest were the environmental conditions during sampling of this data set. Is advecting wind velocity critical, as is often mentioned in papers dealing with passive samplers ? Which sampling height was really chosen, as it is only stated above 1.5 m ? MEGATEC continuous analyzer(s) may not be familiar to the audience. Most important would be to know the physical (or physico-chemical) principles used in these instruments employed for comparison. It seems that the passive samplers were somewhat overestimating. Was that not covered by the L_c factor ?

Another part concerns the presence of gaseous HNO_3 in presence of much higher ammonia concentrations. A multi-phase atmospheric equilibrium (presumably together with additional compounds) has to be assumed. Can it be shown that the sampling result represents a static view of such an equilibrium ? Whether the GDP is an appropriate measure for the found ammonia concentrations may be doubtful. In the here presented form this part of the discussion does not add much assistance. Using equa-

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tion 4 is only a weak support, as it is a rather crude estimate. Would not such an approach require that all participating processes in the deposition flux have a linear behavior. Could that be shown ? I am afraid that from the experimental side this can be done only if the probing time scale is distinctly shorter than that of any participating (multi-phase) process of the whole system.

In general: It is important, that the authors publish these results. But they may be cautious not to extend the information of the data beyond their limits.

Additional points:

The mentioned database at <http://www.obs-mip.fr/idaf/> is not accessible (page=4409/line=29). Please explain gourma (4414/10). Please remain consistent in naming: Zoétélé (4414/25 and following). please add L, A, t, and D in the explanation (4416/5) and reference could be made to Table 4 as well (4416/11). Please use W in Whatman as it is a name (4416/17). Did Rondón et al. (1993) use passive samplers (4421/15)? Trichardt (4426/26) and Dentener et al. (1996) report modeling work. So they cannot have shown that dust particles They can have deduced it (4427/28). Are corrosion studies relevant here (4428/16) ? Katibougou (4429/25) A reference is missing for ozone measurements in French Guayana (4431/20). What follows after However (4438/25) is discussion and should not appear in the conclusions section. Put Williams et al. (1992) into the correct alphabetic order. Omit the b of 2003 in the reference of Zhang et al. (4448/2).

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