**Atmospheric
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Interactive comment on "Emission sources contributing to tropospheric ozone over equatorial Africa during the summer monsoon" *by* I. Bouarar et al.

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We thank the referee for their comments which are addressed as follows:

a) In Introduction, at the first paragraph referring to the role of ozone and ozone production, although this is a well established knowledge a few relevant references should be added.

The following references have been added: Forster et al. (2007), Brasseur et al. (1999), Jacob et al.(1999), Olsen et al. (2002), Crutzen (1974) and Atkinson (2000).

b) The authors should clarify what is the added value of this work against previous

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similar global modelling studies focusing on sensitivity of African tropospheric ozone to different emission sources. Is it basically due to the approach of percentage reduction in emissions while the other studies used the on/off approach of emissions?

A part of the introduction has been updated as follows to show the added value compared to previous studies:

"….... Most previous modeling studies on the influence of African emissions were conducted either by focusing on a specific emission category or by switching off emissions one at a time. However, when turning off an emission source it is difficult to estimate its contribution to O3 due to non-linear effects (Wu et al., 2009; Grewe et al., 2010). In order to take into account such effects, our approach is based on the estimation of the influence of percentage reductions (20%) in different African emissions on O3 within equatorial Africa. In this study we also examine the impact of Asian emissions (20% reduction) on O3 over Africa which has largely been ignored in previous studies. Using emission estimates for 2030, we also estimate O3 changes due to growth in African and Asian anthropogenic emissions in the future. Results presented here are also compared to previous studies, where appropriate…..."

c) The authors refer to the RETRO anthropogenic emissions database but they do not give any description of the species emitted.

Some details on the RETRO anthropogenic emissions used were added as follows to section 2: "Concerning surface emissions, RETRO anthropogenic emissions of CO, NOx and VOCs for the year 2000 were used. Emissions are provided for different source sectors (e.g. residential and industrial combustion, road transport, solvent use and agriculture and landuse change). Details about the chemical compounds for which emissions data sets were generated as well as categorization of source sectors can be found on the RETRO website (http://retro.enes.org)".

d) The authors find that the corresponding modelled CO profiles show little difference between the convective and non-convective cases. These cases were selected follow-

ing data disaggregation according to the proximity to mesoscale convective systems. The reason for no differences between convective and non-convective cases seems to be related to the low resolution of the model which then fails to represent mesoscale convective systems. I am actually rather astonished why the authors followed so much data analysis to prove something that is self-explanatory; that a low resolution global model cannot resolve mesoscale phenomena which they need much higher resolution. Can the authors justify what is the reason of this comparison? If not, I would rather suggest removing this part. Generally, I do not understand the whole analysis of convective and non convective cases to evaluate the model performance when this model cannot resolve such mesoscale phenomena. They authors should clearly justify the essence of this approach.

The overall purpose of this section is to evaluate model performance over the convective region of West Africa during the summer monsoon. As explained in section 4.1, the methodology used to separate the convective and non-convective cases is not based only on proximity to mesoscale convective systems. Backward trajectories were also calculated in order to distinguish air masses impacted by convection during the recent hours or days. The analysis allows us make the distinction in the data collected in air masses influenced by convection in the last few days and more aged air masses. The results show that for longer-lived species like CO and ozone there are less marked differences than for a NO which has a strong lightning influence. The model results do show that LMDz-INCA is capable of capturing the general shape of the vertical profile. Only small differences are seen in the modelled CO and ozone between convective and non-convective air masses. This was also observed showing that the whole region was influenced by the regular convection occurring over the region. The CO observations do however show more variability in the convective cases than the model results. As noted correctly by the referee, global models run at coarse resolution with convective parameterization schemes, cannot be expected to reproduce the convective uplift associated with a particular MCS. However, since they are the tools used in IPPC for chemistry-climate predictions etc. it is important to at least try to evaluate them in a

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more rigorous fashion with the aim of improving them in the future. We could have just compared to mean profile of all the data but this would have masked subtleties such as the increased variability in the CO data in the convective air masses which global modelers should strive to reproduce in their simulations. This section is also important because it allows an evaluation of the LiNOx scheme in the model and shows the underestimation of these emissions with our current scheme. For these reasons, we prefer to leave this section in the paper. The text has been improved to make the rationale for this comparison clearer in the text (introduction to section 4 and section 4.1).

It is also worth noting that new work on the dynamical part of the model has shown that convection in the model occurs at midday while the intensity of deep convection over West Africa occurs generally in the late afternoon. The diurnal cycle of convection is being improved using results of recent studies on the Emanuel's convection scheme (e.g. Grandpeix et al., 2010) and will be incorporated in the chemical transport of tracers.

e) There is 10 times along the text the wording "not shown". I find it not common to refer so many times to figures that are not shown. The authors should try to reduce this effect by removing not necessary references to non-existing figures or by adding some supplementary figures.

Following the referee recommendation, some unnecessary references were removed.

f) In page 13788 (lines 5-6) the authors conclude that the overestimation of UT CO in KE AMMA could be due to an overestimation of African BB emissions in the L3JRC inventory. Sometimes a better comparison may be for the wrong reason. Could it be also related to the convective scheme? For example it has been clearly shown that Emmanuel scheme leads to more efficient convection than other convective schemes.

Our first conclusion regarding the overestimation of UT CO in KE_AMMA compared to MOZAIC data was that it is due to the efficient convection in Emmanuel scheme. How-

ever, as showed by Barret et al. (2010), global models based on the Tiedtke scheme (TM4 and p_TOMCAT) also overestimate CO compared to MOZAIC. When comparing the vertical distributions over West Africa, we showed that KE_AMMA results show reasonable agreement with the observations in the UT. On the other hand, reducing the BB emissions improves the results with MOZAIC. That's how we concluded that overestimation of BB emissions could explain the overestimation of UT CO in KE_AMMA compared to MOZAIC.

g) In page 13793 (lines 7-8) the authors state that significant changes up to 1 ppbv are simulated over central and north Africa. Are these changes of 1 ppbv statistically significant at 95%? Please specify the level of significance.

The text was unclear and has been changed. We did not apply a statistical method to determine whether an O3 change was significant or not rather we compared it with the maximum calculated changes. The text in section 5.3 has been updated as follows: "The impact of the BIO_red run on O3 in the UT also extends to central and north Africa (up to 1 ppbv O3 changes) due to redistribution of air masses within the UT branches of the Hadley cells".

h) In page 13793 (lines 25-26) the authors write that "These changes can be compared to 16 ppbv in LMDz INCA." What are these 16 ppbv? Please clarify. Earlier the authors refer to 8 ppbv maximum ozone changes in the LT and MT when switching off biogenic VOC emissions.

The text in section 5.3 has been changed as follows: "Aghedo et al. (2007) calculated O3 changes of 10 to 30 ppbv in the LT when both biogenic VOCs and soil NOx emissions were excluded from their model. These changes can be compared to 16 ppbv in LMDz_INCA when switching off both biogenic VOCs and soil NOx in the model. Indeed, as was also found in BIO_red, the SNOx_red test produced up to 8 ppbv O3 change in the LT."

g) Technical comments 1) Page 13772, line 6: "driven" should rather exchanged with

"controlled".

The sentence has been changed.

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