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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.

Anonymous Referee #2

Received and published: 28 June 2011

This manuscript aims to examine the influence of different emissions on the distribution of tropospheric O₃ over West and central Africa as well as downwind over the central Atlantic Ocean during the summer monsoon in 2006. It merits publication to ACP and I would suggest acceptance of the paper after taking into consideration the following comments.

Comments

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.

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

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

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

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.

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

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Emmanuel scheme leads to more efficient convection than other convective schemes.

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.

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.

Technical comments 1) Page 13772, line 6: “driven” should rather exchanged with “controlled”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13769, 2011.

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