

Interactive comment on “Ozone budget in the West African lower troposphere during the AMMA (African Monsoon Multidisciplinary Analysis) campaign” by M. Saunois et al.

Anonymous Referee #1

Received and published: 11 May 2009

The new simulation (NOCH4) performed by Saunois et al. has helped to shed some light on the ozone budget in their model by revealing the sensitivity of ozone production in the northern part of their model to the availability of photochemical “fuel” from the background air, as opposed to fuel advected northwards from the vegetated southern part of their model. Based on this new simulation, as well as their NODEP and ID-SOL simulations, it seems to me that the authors can now make some basic claims about what is happening in their model; their modelled ozone gradient is due to a combination of enhanced deposition over vegetation in the south, and enhanced photochemical production in the north due to the enhanced NO_x emissions. The peroxy radicals required for this enhanced ozone production in the north are generated from

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a combination of the oxidation of background CH₄ (and likely also background CO) as well as from partially oxidised VOC produced in the vegetated south from oxidation of isoprene.

The new Table 1 (from the response to my earlier interactive comment) is an especially welcome contribution to the quantification of some of the sensitivities in the modelled system. This new table should be included in the revised manuscript, and should be used as a basis for a quantitative discussion of the influences on the ozone budget in the model. In my opinion, this will strengthen the manuscript, which is unfortunately rather vague in its present state.

The authors must also improve the quality of the scientific language used in the manuscript. In general, the quality of the English used is poor. The text is usually quite hard to follow, and in many places the meaning is not clear.

Below is a list of some specific comments on the manuscript. This list is by no means exhaustive.

p 6981, l 25: Be consistent with the use of parentheses: ...carbon monoxide (CO)...hydroxyl radical (OH)...

p 6982, l 4: Delete “the” before “atmospheric chemistry”.

p 6982, l 25: Delete “the” before “ozone formation”.

p 6983, l 24: No parentheses around “Aghedo et al. 2007”.

p 6984, l 20: ...north *of* them... , not “north *to* them”.

Section 4: All measurements are presented as zonal averages. It would be useful for the reader to see some indication of the zonal variability in the measurements in order to assess the applicability of the 2D modelling approach used in this study.

p 6990, l 2: includes, with an “s”.

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p 6990, l 4: Sensitivity studies *are* discussed (not “is discussed”).

p 6990, l 4: Refer to Figures with consistency, using either “Fig.” or “Figure”, not both. This applies to the whole manuscript.

p 6990, l 5: How have the measurements been analysed onto the grid?

p 6991, l 21: Which part of the domain are you referring to here? Only the PBL? Most of the model underestimation of CO I can see from Fig. 2 looks like it can be explained by transport of CO from elsewhere.

p 6991, l 24: *explain*, not “explained”.

p 6992, l 7: Replace “tree” with “vegetation”.

p 6992, l 14: If loss of CO from the PBL in the model due to convective updraughts is so important, then perhaps the high CO mixing ratios in the southern part of the domain could be due to transport from the free troposphere in convective downdraughts. You could test this with a sensitivity study in which you impose CO mixing ratios in the model’s free troposphere similar to those observed.

p 6994, l 9: Which “given day”?

p 6996, l 27: Replace “and hence” with “which is”.

p 6996, l 28: In this paragraph you appear to be saying that the modelled NO_x is simultaneously within the range of the observations and also overestimated by the model.

p 6997, l 19–24: It is incorrect to assume that northward transport of any kind is *required*. It is also possible that ozone in the north is produced locally from increased local NO_x emissions and background CH₄ and CO. In fact, the smallest N-S ozone gradients in your model come from both the IDSOL and NOCH4 simulations, suggesting strongly that enhanced production from local NO_x emissions and “fuel” from background air is a significant contributing mechanism to the ozone maximum in the

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north. Northward transport of ozone precursors must be shown to be happening (as you in fact do show later in the same subsection), not just assumed. It would make a lot more sense to show first that there is northward transport happening in the model, and *then* to suggest that this could be contributing to enhanced ozone production in the north.

p 6997, l 20: Do you mean that the mixing ratios of these reactive VOCs are lower?

p 6997, l 24–27: This sentence does not make sense.

p 6998, l 4–7: This sentence is extremely convoluted.

p 6998, l 6: What does northward transport have to do with the simulated zonal wind?

p 6999, l 20: Why do you have this discussion of OH reactivity? Since this section is about the ozone budget, it seems to me that the manuscript can do without Figure 10, and the size of this paragraph could be cut in half, beginning around line 8 of the following page, in which you begin the discussion of NO to NO₂ conversion via peroxy radicals.

p 7003, l 21–22: An increase of ozone with increasing isoprene is not evidence for a NO_x limited regime, if anything the opposite.

p 7005, final paragraph: This paragraph reads more like a conclusions paragraph, and should be incorporated into the conclusions (with the appropriate modifications as necessitated by modifications based on all other referee comments).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6979, 2009.

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