

## ***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 #2**

Received and published: 7 May 2009

### **General comments**

The manuscript discusses factors influencing the ozone budget over 5 West African countries (Niger, Benin, Mali, Burkina Faso and Togo). The authors use a meridional-averaged latitude-height model with a reduced chemistry scheme. They also prescribe meridional-averaged emissions from various inventories calculated over the zonal strips of the regions considered to simulate the observed ozone concentrations. The manuscript would need additional work and revision to make it suitable for publication in Atmospheric Chemistry and Physics. In particular I would suggest a change of the title to "The factors influencing the lower tropospheric ozone budget over 5 West

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African countries during the AMMA (. . .) campaign”, because this is what is discussed in the manuscript. It is very misleading to use West Africa, when the research flight measurements and the simulations presented do not even cover the eastern half of West Africa (note that Nigeria, Ghana, and Cameroon are excluded). The manuscript should be modified to reflect this change of title, in places where West Africa is used. I also have a problem with the manuscript that says “ozone budget” but hardly gives or discusses any values of measured or simulated ozone concentrations (none in the abstract and the conclusions). Relative terms such as “increase”, “high”, “maximum”, “enhanced” are used pervasively throughout the manuscript. It is not clear how “high” high can be, or how “enhanced” enhanced can be. Please include some values in the abstract and the conclusions.

I cannot also but wonder why such an important scientific research flight would only consider few West African Francophone countries (Niger, Benin, Mali, Burkina Faso, and Togo), and completely ignore the most important and interesting mega-city in the region, which is Lagos, Nigeria.

On the whole, I am not impressed with the convoluted English language of the manuscript given there a quite a number of native English speakers as co-authors on the manuscript. See technical corrections for some examples. Please note that I did not made any attempt to suggest corrections for all the errors, which relate to language in the manuscript, since this will make this lengthy review even longer. Even a simple use of text editor (such as MS-Word) spelling and grammar features may reveal some of these errors. I would suggest using short simple sentences instead of long, and sometimes confusing ones. There is also a generally inadequate description of figures inside the text. As an example on these last points, in Section 5.3, it would be clearer and much more reader friendly if: “Figure 9 presents the 24 h average of the ozone tendencies in the layer 0–700m between 4°N and 19°N, plotted along with

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the meridional ozone concentration profile which exhibits the strong gradient at 13°N” is replaced with “Figure 9 presents the 24-hour average of the ozone tendencies in the layer 0–700m between 4° N and 19°N. It also shows the simulated meridional-average ozone concentration (black dash lines) whose values are as shown on the right-hand side bar of the figure.”. The whole discussion following these sentences explains the last phrase “which exhibits the strong gradient at 13°N”, so delete it here and start a new line or paragraph with the discussion of this main feature of the ozone concentration, which is also the main focus of this paper, as earlier pointed out by the anonymous referee 1.

### Specific comments

1. It would be good to include a detailed map of West Africa showing vegetation cover, geo-political boundaries, and highlight of the tracks of the FAAM BAe-146 aircraft. This would serve as a visual aid to the descriptions and discussions in the article, and would ultimately increase the readability of the manuscript.
2. Page 6981, the abstract: Generally the abstract provides explanation on “increase of ozone mixing ratio”, “ozone maximum”, “ozone minimum” e.t.c. The title of this manuscript includes the word “...ozone budget...” therefore, I suggest the authors should include some budget calculations in the abstract. I expect to see some values of ozone concentrations measured or simulated over the flight region. Relative terms such as “ozone minimum” and “ozone maximum” are difficult to assess. In addition the authors should discuss how the measured budget compare to what has been published earlier for the region (if any). Are the measured ozone volume mixing ratio typical for the region? or do they represent the seasonal expectation?

Page 6981, lines 15 – 17: The authors claim that “The model underestimates the observed OH mixing ratios, however this model discrepancy has slight effect

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on ozone budget and does not alter the conclusions.” Please substantiate this claim. Show the observed OH mixing ratios or quantify the low bias.

3. Page 6984: lines 1 – 11: This message of this paragraph is not clear, due to its lack of chronological presentation. The authors state that the “dry deposition of ozone is one of the most important sinks in the boundary layer” and that “majority of ozone flux experiments have been carried out during the dry season” and immediately jump off to report “wet season dry deposition”. The authors should arrange this paragraph in this order: first present the ozone flux and dry deposition measured in Congo and Amazonia during the dry season, and then compare these values to those measure elsewhere in the wet season. Also the dry deposition velocities should be given in the range of: night – day, i.e. reverse 1.8 – 0.26?

Page 6984, lines 15 – 18, and Page 6985, lines 9 - 27: these are examples of places where map in point 1 would be useful, and should be referenced.

4. Page 6987, line 21: do you mean “0.38 times that of methanol”? For which emission category is this weighted sum applicable?
5. Page 6988, line 9: what do you mean by “solar conditions corresponding to 15th July including diurnal variation”?
6. What would be the effect of using a reduced chemical scheme (ReLACS) on the overall calculation in this manuscript, given the fact that it assumed a linear relationship between the primary species and the lumped species?
7. Section 3.2: I cannot understand why vegetation emissions are split between 3 different sources (POET/GEIA, MEGAN/MOHYCAN and an approximate terpenes emissions of 0.1 times isoprene). This is even more confusing since the combination of isoprene and terpenes account for about 90% of all emissions from vegetation? Can you give reasons for your particular

- preference? Which  $\text{NO}_x$  emissions category is from POET/GEIA and the work of Jaegle et al, 2004?
- Section 4: What is the time frame for the plots in Figures 1 through 6 (monthly average or just one time stamp e.g. Aug 2, at 12 noon local time)? Figures 1 through 4, and Fig. 6: It is generally difficult to see the concentrations reported at the first 4 layers of these plots. This is quite unfortunate since somehow the surface processes are important within the boundary layer, which is the focus of the article. For example the authors state on page 6991, line 17, that "the model slightly overestimates HCHO near the surface", but the way the plots are made, it is difficult to confirm such statement as this. Is it possible to plot these figures with equal spaces to represent each grid-box, irrespective of their actual height (i.e. use non-uniform altitude in the vertical axis)?
  - Section 4.2: The section lacks coherence and persuasive ordering of arguments. The excuses given for the underestimation of CO concentration by the model is not convincing. The authors cite previous studies based on global climate-chemistry and chemical transport models. Are the authors suggesting that the regional model ReLACS deficiency is comparable to those of global models? What is the relevance of lines 24 – 26 on page 6991? Are you implying that direct CO emissions from vegetation used in the simulation is not as high as shown in the Jacob and Wofsy 1990's paper? Also it will help if the authors explain the composition of their anthropogenic emissions category (e.g. do they include fossil fuel burning, and gas flaring over Lagos, Nigeria?). The authors state in the introduction that "little biomass burning takes place in West Africa during the wet season", does "little" means "none"? If no, why are biomass-burning emissions not included in the simulation? What is the point of lines 4-6, page 6992 "However sensitivity tests detailed later in Sect. 6 show that isoprene oxidation accounts for around 10% of the total modelled CO below 700m"? Are you suggesting that approximately 10% contribution of isoprene

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oxidation to CO concentration justifies that the model chemical scheme is correct? The authors also stated on two different occasions within the section that the underestimation of CO concentrations suggests low CO emissions. It would have been more interesting to see a sensitivity simulation of increase CO emissions to support these claims. Since the southern hemispheric intrusion is included in the manuscript only to provide explanation for the features of measured CO concentration that is not covered in the paper, I suggest moving the statements on it to the very end of Section 4.2.

10. Is it correct to say that the ozone enhancement observed in the mid-troposphere south of 8°N is due to biomass burning intrusions of precursors transported from southern Africa, given the equally enhanced concentrations of CO, NO<sub>x</sub>, HCHO and acetone in the measurement data and the depletion of OH in the model? That is, are the ozone produced locally?
11. Section 4.3, page 6992, lines 26 and 27: these lines present an example of where being specific about the countries considered would help in the interpretation. Actually Lagos is on latitude 6.45°N, while Cotonou is on 6.37°N,
12. Section 4.4, page 6993, lines 12 and 13 state that "Mixing ratios higher than 40 ppbv are obtained in the boundary layer around 15°N–16°N in both the model and observations". The model ozone concentration is lower than 40ppbv for the region specified, and the ozone observed is higher than 40ppbv at 14°N – 16°N. Please revise the manuscript.

Page 6993, line 14, the latitude range should be 4°N – 8°N, and not 6°N-8°N.

Page 6993, lines 20 – 23, and page 6994, line 7: please clarify conflicting arguments on "photochemical production of ozone in the lowest levels of the model where the NO concentration is higher" and "below that level (that is 700m), ozone is photochemically destroyed".

13. Section 5.1, page 6995, line 21, the model captures the observation between 8 and 11 degrees N, and not 7 – 11 degrees N.
14. Page 6996, lines 1 and 2: "Also the MEGAN/MOHYCAN emissions shows a general agreement with the observed isoprene." What does this statement mean? Why would you compare emissions to measured concentrations? Please delete this statement, and start the next statement with "Our observations suggest...", that is, delete the "However the".

Page 6996, lines 4 and 5: "The same discrepancy is found for HCHO, CO and CARBO"? Do you mean that the assumption of constant latitudinal average emissions from vegetation in the 5 – 12 degrees N band also led to an overestimation of simulated HCHO, CO, and CARBO concentrations? Please be specific, because the words "the same discrepancy.." is too general, since you have discussed several discrepancies before this point in the manuscript.

Page 6996, line 8 and subsequent mentioning of Lagos: do you mean that Lagos pollution is transported to the location where the observation was made? Please state this clearly to avoid confusion, since the aircraft did not fly directly over Lagos?

Page 6996, line 7: With the exception of  $\text{NO}_x$  (which is also shown in Fig 3b), I cannot see any high concentrations of HCHO and CO at 13.5 degrees N.

Page 6996, lines 14 – 18, on Figure 7b: Where do the data in Fig. 7b come from, because Figure 3b shows missing or empty values in most of the grid boxes from surface to 700m, except at 5-7 degrees N and at 13 degrees N? The explanation provided in lines 18 – 27 is not clear. Define the detection limit, and its relationship with the averaging time (see point 11 of technical corrections). What is  $\text{NO}_{xy}$ ,  $\text{NO}_z$ ? What do you mean by "two instruments  $\text{NO}_{xy}$ " and

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"...about 16% higher than  $\text{NO}_{xy}$   $\text{NO}_x$ "? Again the tangled language makes this entire section difficult to read and comprehend.

15. Page 6997, lines 7, 9, 10, 19: please clarify all the "further north", "further south" and give a point of reference. The reader should not be left to assume your reference point.
16. Overlay Figure 8 with the wind fields.
17. Section 5.3, Figure 9: Replace this figure with a separate plot for night and day.
18. Section 6, change the title of this section to "Sensitivity of ozone distribution and gradient below 700m to emissions and dry deposition". The word "key factor" clearly connotes a different meaning. In section 6.2, page 7001, line 25 and page 7002, line 1, what do you mean by " $\text{NO}_x$  mixing ratios change essentially south of 14 degrees N with values higher by up to 50%"?
19. Section 6.4: what are the emissions value used in the equal soil  $\text{NO}_x$  emissions simulation (IDSOL)?
20. Section 6.5, Figure 7a, include the NODEP lines in the new revised plot.

### Technical corrections

1. Throughout the manuscript, follow the usual way by first writing out the meaning to acronyms before giving the acronyms in parenthesis. For example "A reduced chemical scheme for tropospheric chemistry, ReLACS (...)" should be replaced with "A reduced chemical scheme for tropospheric chemistry called Regional Lumped Atmospheric Chemical Scheme (RELACS, Crassier et al, 2000)..."



2. Page 6981, Line 2: Change "A bi-dimensional latitudinal-vertical meteorological..." to "A bi-dimensional (latitudinal versus vertical) meteorological..."
3. Page 6982, Line 3, change "...natural emissions (vegetation, soils), lightning NO<sub>x</sub>..." to "...natural emissions (e.g. vegetation, soils), lightning NO<sub>x</sub>..."
4. Page 6982, Lines 4 – 5, change "These trace gases can have a significant impact on the atmospheric chemistry and lead to the formation of ozone" to "These trace gases have a significant impact on the atmospheric chemistry and can lead to the formation of ozone".
5. Page 6982, line 23: delete the first "source". The sentence should read "and showed that this is the most important African emission source affecting the total..."
6. Page 6982, line 28: change "for" to "of", as in "Evidence of large..."
7. Page 6983, Lines 18 – 19: Insert comma and cite reference, since the lines are as published in Aghedo et al, 2007. That is change to "In Africa, anthropogenic emissions have a large spatial variability with highest impact on tropospheric ozone concentration over South Africa, Nigeria and Egypt (Aghedo et al, 2007)".
8. Page 6983, Line 23: change "severly" to "severely"
9. Page 6984, Line 4: give the full meaning of the acronym "LBA-EUSTACH"
10. Page 6984, line 24: change "Based on the zonal symmetry between 10 °W and 10 °E of (e.g.) vegetation cover, surface temperature and albedo, a 2-D..." to "Based on the zonal symmetry in surface features, such as vegetation, surface temperature and albedo over West Africa between 10 °W and 10 °E, a 2-D..."

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11. Page 6985, Table 1, lines 14 – 20 on Table 1: define the acronyms UV and VUV. What is a "detection limit"? please define here.
12. Page 6986, lines 11 – 12: change "The vertical domain extends to 20 km, with a stretched grid of 30m near the surface to 1 km in the upper troposphere" to "The vertical domain extends to 20 km, with a variable grid-box height, which is about 30m near the surface and 1km in the upper troposphere"
13. Please rephrase the wordings of Section 3.1, page 6986, lines 18 – end of the section. The native English speakers on the paper could help with this. For example, on page 6986, lines 23 – 26, page 6987 line 1: replace "Sea surface temperature (Atlantic Ocean and Mediterranean Sea) are taken from the Reynolds climatology of 1982–2003 ... using July profiles for the Gulf of Guinea and May profiles for the Mediterranean Sea" with "Atlantic ocean and Mediterranean sea surface temperature (SST) are taken from the Reynolds climatology of 1982 – 2003 (...). We use July SST profiles over the Atlantic ocean and May SST profiles over the Mediterranean sea". Especially the clauses containing "...including", "...and including", "...using" are not clear. Why do you choose different monthly SST for Atlantic Ocean and Mediterranean Sea?  
Page 6987: Instead of "real species" and "mechanism (model) species" use "primary species" and "lumped species". Also change the caption of Table 2 to reflect these changes. Note that BIOP is defined twice in Table 2.
14. Page 6988, lines 8 and 9, replace "is found negligible" with "is found to be negligible" or "is negligible"
15. Section 3.2, page 6988, line 17: avoid using the word "to" at the beginning of a new sentence.
16. Table 3: Include the word "MEGAN/MOHYCAN model" in the caption to this

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table, because in subsequent reference to isoprene and terpenes emissions, you use the word MEGAN/MOHYCAN.

17. Section 4, page 6990, lines 3-4: delete the short introduction to Section 6. It is not necessary.
18. Page 6990, line 8: The fraction of isoprene and terpenes in BIO is actually 91% and 9% (exactly 90.91% and 9.09%).
19. Page 6990, line 25: delete "as" at the beginning of this line.
20. Page 6991, line 15, change "(with 0.6ppbv at 2km)" to "(with concentrations of up to 0.6ppbv at 2km)"
21. Page 6992, line 18: change "...destruction of CO occurs elsewhere in the domain (not shown)" to "...destruction of CO occurs elsewhere (in the domain not shown)"
22. Page 6995, line 8: what does the acronym "a.s.l" stands for?
23. Figures 7: It is generally difficult for me to understand the caption to Figure 7. What does "The median is in red and the first and third quartile are in blue"? What are the red crosses? You may also want to match names in the caption to labels on the individual plots, e.g. "BIO" is mentioned in the caption, but "isoprene" is mentioned on plot title. Please arrange the figures to follow the label (a), (b), (c) ...and so on... to O<sub>3</sub>, NO<sub>x</sub>, isoprene e.t.c. What are the dash black lines on the box plots (are they the standard deviation of the measurement?) I do not see how RETRO alone can account for all emissions "RETRO (all emissions)", given the range of inventories mentioned in Table 3. Please change this name to something else. It would be useful to also state in the figure caption that model lumped species such as BIO, KET, CARBO, are being compared to their respective comparable observation species which are

- isoprene, acetone, and combination of MVK and MACR. Rearrange the pictures so that the numbering will match their appearance in the discussions in Section 5.
24. Section 5.1, page 6995, lines 22 – 25, and page 6996, lines 1 - 12, and lines 14 – 28: please correct the English language in the above lines. It is difficult to read as it is. For example, change "Discrepancies appear to both sides of this area where" to "We see discrepancies outside this range because". Also change "...from vegetation assumed in too a large latitudinal band", "...near Cotonou where the aircraft flew explaining..."; "...emissions drop off toward the coast and 12 °N..." and so on.
  25. Section 5.2, page 6998, lines 2 and 3: change "...has been followed by an later flight" to "was followed by a later flight". Also change "...has evolved" to "...evolved". Change "the nocturnal winds have redistributed the air" to "the nocturnal winds redistribute the air". On line 13, change "...distribution has discussed below" to "...distribution as discussed below"

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

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