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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Western african aerosols modelling with updated biomass burning emission inventories in the frame of the AMMA-IDAF program" *by* C. Liousse et al.

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We greatly acknowledge the remarks and recommendations made by the referees which have been largely accounted for, resulting, we hope, in useful and valuable improvements. The text has been largely modified, new figures added and, we hope, better argumented. In the following, we have answered comprehensively and in detail the general and specific questions of the three reviewers. We sincerely hope that this new version is seriously improved vs. the previously submitted one.

(R is for reviewer and A for Author)

R : The manuscript presents a new biomass burning inventory for gases and particles

for Africa with a very fine coverage, created by the use of satellite data and validated by a global model. The paper has great potential, since it focuses in an extremely poor studied area of the globe, but the presentation of results is inappropriate and incomplete, making it not publishable in its present form. Below there is a list of my major and minor concerns. General comments 1) The authors promise to present results for BC and OC, but in the discussion they heavily focus on BC, almost completely neglecting OC. The title of the paper is even more misleading, since aerosols in general are mentioned. The word "carbonaceous" has to appear in the paper title, and then equal weight to BC and OC should be applied. Some of the places that OC results should appear include: table 1, section 2.2, section 3.2.2, table 1 and figures 2, 3, 4a.

A : In the new version of the paper now largely modified, we specified more precisely the aim of the paper. BC was targeted as a tracer of combustion able to validate biomass burning emissions. Only OC budgets appear in the comparison with GFED2 inventory. Title of the paper has also been changed.

R : 2) A proper presentation of the model used is missing. The reader should be directed to the previously published detailed description of the model, but some major components must be also presented here. These include, but are not limited to: the core/shell treatment coexists with internally and externally mixed aerosols? How does core/shell treatment coexist with the e-folding time ageing of Cooke et al? Where is sea-salt in the 3-layer treatment (page 7356, last lines)? What is the spinup time of the model runs? How is hygroscopicity and removal being treated with the core/shell model? How is the size distribution affected by ageing (page 7360, line 20)?

A : All these modeling issues are now explained in the text.

R : 3) Sea-salt is consistently mentioned in the paper as secondary aerosol, but obviously it is not. This appears in p. 7352, line 12; p. 7356, line 12).

A : We have corrected this mistake. Sea salt is in the second shell of the 3 layer – structure used to calculate aerosol optical depths. However it is mentioned as a primary

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aerosol.

R : 4) It is unclear where some numbers in the discussion came from. The authors speak about differences of 11% and 35%, while by looking at the figures a factor of 2 appears. This is explained in my specific comments 12, 13. They also present other numbers (see my comment 18 below) that are not consistent with the corresponding figure. A : Presentation of these values are now clearer with corrections when needed.

R : 5) Contrary to what the authors state in page 7360, lines 17-18, there are a few models that include aerosol microphysics. ECHAM (Stier et al., 2005), TM5 (Vignati et al., 2008), GISS modelE (Bauer et al., 2008), are just some of them. A : Use of ORISAM-TM4 is now displayed differently with references on existing models with an aerosol module.

R: 6) The authors state that SOA are a minor aerosol component. This is far from expected and should be investigated and clearly demonstrated. Other models (see e.g. Kanakidou et al., 2005) calculate that SOA contribution to the total organic aerosol mass maximizes at tropical forests, which certainly applies to Africa. Having 3-7% of SOA in the model (page 7361, lines 1-2) is extremely low and should be defended. In addition, in figure 6, OC in PM2.5 appears to be overestimated by the model by a factor of 2, although BC is excellent. This directly points to very high OC fluxes, presumably by biomass burning which is expected to be the dominant source over Africa. If this is the case, the authors need to clearly present the reasons why this overestimation occurs and possibly alter (fix?) their emission factors prior publication. A : The referee is right. In the text such a discussion does not appear anymore since the figures dealing with size resolved chemistry aerosol are no more shown. This is not the scope of the paper and the experimental database is not yet published. However, the referee is right : with ORISAM TM4 parameterization of SOA, SOA formation is low in biomass burning areas. Most of OC is due to primary OC biomass burning emissions. Further investigations are needed for comparison with Kanakidou.

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R : Specific comments 1) page 7351, around line 15: GFED2 should be mentioned. A :OK

R : 2) page 7352, lines10-11: How is the internal mixture of dust with carbonaceous aerosols being calculated? To what extent this happens in the model? Is it supported by measurements in Africa? A : Now explained in the text. We have considered that internal mixtures occur in Africa. However our results, as confirmed by AMMA measurements during the dry season, seem to show that we have a mixture between internal and external mixing. Regional modeling is now on going to test such parameterization.

R : 3) The last sentence of page 7354 is confusing and has to be rephrased. A : OK

R : 4) Table 2 discussion in section 2.2 should include more tracers and more regions.

A : BC may be considered as a tracer of combustion. CO comparisons would be very similar. We decided to show the whole Africa and West and Central Africa because our main analysis are centered here. In Tummon et al., 2010, accepted JGR paper), more details are given for South Africa. We choose to add a new figure to enlarge the comparisons with all the existing inventories.

R : 5) page 7355, line 4: “As shown earlier”. Where? I couldn’t find it. A : This has been corrected.

R : 6) page 7355 first paragraph: “Interannual variations are less important in JJA than in the DJF period” is inconsistent with “interannual variations are mainly due to the Southern Hemisphere fire activity”. How can this be possible? JJA burning primarily occurs in the Southern Hemisphere. A : Yes. We have rephrased the text which was not clear enough.

R : 7) page 7356, line 13: sea-salt has gaseous precursors? A : No. We have rephrased the text to be clearer.

R : 8) page 7357, line 13: NOx come from EDGAR3.2, or from ANCAT (line 15)? A : This has been specified.

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R : 9) page 7358, line 17: “modeled values are underestimated”. Measurements are underestimated, not model values. A : We agree with the referee. This has been corrected.

R : 10) page 7359, lines 15-17: May also disagrees, not only June. In addition, why dust is blamed? Where any severe dust events this time of the year that affected the monthly mean concentrations? Are any other evidence to support this? If dust is the problem, why it does not appear to other months? 11) page 7359, line 20: Is there any evidence to support that local sources are the reason?

A : We have tried to be clearer when arguing for explaining the differences between modeled and observed results. We have presented all possible biaises. We know that the dust scheme used has problems for some months ; This is also shown with the RegCM3 model (Solmon et al., 2009).

R : 12) Jan, Aug 2005; Jan, Jul, Aug, Sep 2006: these months appear to have a factor of 2 difference in fine AOD, not 35%. In addition, Jun 2006 appears to be great, why they exclude it? Are we looking at the same plot? 13) page 7359, line 27: During winter and spring there is a factor of 2 difference, while summer and fall look better. Where does the 11% came from?

A : These values have been better presented and alsocorrected when needed.

R : 14) page 7360, line 1: I disagree with that. Coarse grids smooth out wind fields, resulting in lower dust emissions. A : That was modified. Here may be a dust scheme problem.

R : 15) page 7360, lines 4-6: SSA change from 0.9 to 0.7 means a factor of 3 more absorption, this cannot be well captured.

A : The referee is right. This disagreement also appears in Banizoumbou.

R : 16) page 7360, line 12: I would expect that BB carbonaceous aerosols are not core/shell structures, but rather homogeneously internally mixed. A : BC is mostly

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insoluble. So homegeneously internally mixed aerosols are not expected.

R : 17) page 7360, lines 14-15: Some numbers should be presented here, to compare with the rest of the discussion in section 3.2.2. A : These values have been better presented and corrected when needed.

R : 18) page 7361, lines 13-15: Where did these percentages came from? Figure 6 shows a very different picture. Please check. A : These values have been better presented and corrected when needed.

R : 19) page 7361, line 17: “globally in agreement between experiments and modeling”. What global experiments? Measurements outside Africa? Where? A : This text has been changed (english problem).

R : 20) page 7361, lines 25-26: “Regional patterns given by the model are in global agreement” what does this mean? A : This text has been changed (english problem).

R : 21) Figure 2 should be combined in one for easier comparison. In addition, it would be better to present monthly data instead of annual, in line with the discussion in section 2.

A : We have prefered to keep to the previous figure 2 because it clearly reveals the two burning season in Africa. In case, it can be easily changed.

R : Technical comments 1) page 7353, line 2: where is EM used in eq 1? 2) page 7353, line 26: 30m by 30m spatial resolution, it is hard to believe that this is correct. 3) page 7354, line 2: these factors should appear in table 1 4) page 7356, line 7: none of the listed references are TM4. 5) table 1 should have 17 classes of compounds.

A : These technical comments have been considered.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7347, 2010.