

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.

Anonymous Referee #1

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This paper develops a new biomass burning inventory relevant for the AMMA period (AMMABB). The results may well prove useful: implementation of the biomass burning inventory in models other than that presented here will presumably allow for a more rigorous assessment of how well the emission inventory performs. My initial thoughts are that it will lead to a better comparison as discussed below.

This paper is marred by the quality of the English and some extremely poor and sloppy presentation. It reads like a first draft of a paper rather than one that is publishable in its present form. Unless significant efforts are made, I would not support final publication in ACP. The errors and inconsistencies are so numerous that I've only been able to

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address some of them. In several instances, references are made to relatively old literature. References to new literature including the substantial amount of literature that has already appeared from the AMMA program is hardly made at all. This must be rectified if the paper is to make any credible contribution to the current literature. Unless all of these suggestions for improvement are performed, I fear that the impact of the paper will be very low if it does indeed make it to ACP at all.

I have tried to sort out the major grammatical problems where I can, but even when these corrections are made, I would suggest that a native English speaker read through the paper to ensure clarity.

Abstract:

remove AMMABB from the abstract

‘This paper discusses comparisons between’ -> ‘This paper compares’

‘Major aerosol’ -> ‘Aerosol’

The sentence ‘It is the first time to our knowledge’ is poorly written and is not true. Jacobson (2001) treated OC with a BC core in global modelling studies. He has published several updates since then using spherical shell models. The sentence should be removed.

Introduction:

I was alarmed by the neglect of references to other measurements in the AMMA program.

Specifically:-

Page 7349 Line 21. ‘...Sahara.’ -> ‘.... Sahara (e.g. Haywood et al. (2008).’

Line 27. ‘... wet season.’ -> ‘... wet season (e.g. Capes et al, 2009).’

Line 26. Remove the line ‘Both’ And Replace it with ‘Subsequent to emission,

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anthropogenic biomass burning particles and natural mineral dust particles become internally mixed to varying degrees.'

Page 7350 Line 2. properties result -> properties may result

Line 7-8. You can't have 'few' field studies followed by 'Many'. Delete 'Many'.

Line 13. 'First' -> 'Initial'

Line 17. European programs -> European programs (e.g the Saharan Dust Experiment, SHADE; Tanré et al, 2003).

Line 18. Remove 'including HNO₃ and dust interactions', and add at the end of the sentence 'and during the AMMA campaign by Crumeyrolle et al., (2009) and Matsuki et al., (2010)'

Line 21. 'is offering' -> 'offers'

Line 22. the different participants to -> the participants in

Line 24. combustion aerosols -> combustion aerosols (e.g. Johnson et al, 2008; Osborne et al, 2008).

Line 24-Line 27. A reference is vital here. I suggest Lebel et al, 2009.

Line 29. Suggest 'on combustion aerosols' -> 'on combustion aerosols and the development of the AMMA Biomass Burning emission inventory (AMMABB).

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1st paragraph. The order is muddled. You should state that the first attempts used land use statistics, an alternative approach used satellite observations and Michel (2005) and Stroppiana et al (2010) used combinations of the two approaches. The authors should also note by means of a caveat the more recent developments that relate the fire intensity to the biomass burned via Fire Radiative Power (e.g. papers by Wooster).

Line 25 study about -> study of

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Line 5. Refer to Jacobson (2001).

Line 13-18. Why are no comparisons made against the aerosol size distributions, and vertical profiles determined from AMMA-SOP0? There are a host of papers that cover the in-situ measurements and the vertical profiles (Haywood et al, Johnson et al, Osborne et al, Johnson et al, 2008b to name just three). There are also relevant modelling studies detailed – Haywood et al provides an overview of these. The paper as written shows a very Francocentric view of operations, which does not do the wide international collaboration of AMMA any justice at all.

Section 2.1:

EM and EF_v are not included in equation 1. I don't think that the notation is consistent between equations (1) and equations (2). Something is wrong or there needs to be more explanation.

Section 2.2:

Figure 1 – the text associated with the Figures is illegible, as are the color bar scales. I'm not happy with any of the other figures – the text is illegible in ALL of them.

Table 2. The numbers are interesting, but once again the authors have not referred to relevant AMMA SOP-0 papers: a second Johnson et al paper (Johnson et al, 2008b) shows that the Dentner et al/van der Werf emissions need to be scaled up by a factor of 2.4 to achieve agreement between the observations and the modelling using HADGEM2. It appears that the use of the AMMABB emissions (a factor of 1.85 higher) would lead to better agreement than use of the Van der Werf emissions. This would be worth quoting as it supports the idea that the Van der Werf emissions are too low at least in the N Africa regions.

Section 3.1:

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P7357, line 3. paragraph -> section.

Section 3.2

ss3.2.1.

I 12. Measured BC concentrations accurate to 10%. This is a very low estimate of the uncertainty. Aethelometers measure the particle absorption. In dusty conditions both dust and BC absorb, but dust the dust absorption is of a high uncertainty whether or not it is corrected for. Additional corrections for instrumental artifacts need to be considered (e.g. Bond et al. (1999)). Then you've got the problem that you don't know what the effective density of BC is given all of this uncertainty 10% is a very, very conservative estimate. 25% might be more appropriate especially given that you don't provide any information from the Galy et al study.

Figure 4 is so difficult to read that I can't say whether I believe the analysis or not.

Summary and Major concerns:

The references provided by the authors is extremely limited and Francocentric. It does not do justice to the international nature of the AMMA project.

The data analysis is very limited in many aspects:

(1) AMMA measurements went to considerable lengths to measure the vertical profile of both dust and biomass burning aerosol (e.g. Johnson et al, 2008b). Why is the model not tested in this regard? It really should be. In addition to the aircraft measurements, there were a host of lidar measurements made as well.

(2) Comparison against the PARASOL data suggests that the model still under-predicts the AOD in N Africa – while the emissions might help move towards the correct answer, it appears that it's still not that well represented. Even the spatial pattern appears incorrect in S Africa. The authors should be very careful not to overstate the results from their model.

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(3) You've got a model that you say represents the size distributions – however, the bins have been chosen really to represent mineral dust – adaptation to try and model mineral dust and biomass burning in your model is really quite limited owing to the dust being present in the coarse mode and the biomass burning aerosol being present in the accumulation mode. Indeed you only seem to get mixing in a single bin (0.4-1.3microns). You are not making size resolved measurements of the chemical composition except very coarsely (PM2.5 to PM10). Therefore the modelling remains poorly constrained.

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

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