

## ***Interactive comment on “Combustion particulate emissions in Africa: regional climate modeling and validation” by A. Konare et al.***

### **Anonymous Referee #4**

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### **General comments:**

This paper describes the result of the inclusion of recent and updates inventories of combustion particulate emissions in Africa in a regional climate model. To illustrate the benefit of such updates inventories, the results are compared to simulations performed with the same regional model but with an older averaged inventory. The simulated surface concentrations and aerosol optical depths are compared with measurements.

Such an approach appears as relevant, however, the way the comparison with measurements is performed raised serious issues, in particular regarding one of the argument developed by the authors in favour of updated and precise inventories.

Indeed, the authors argue that the temporal and spatial variability of the different

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sources of carbonaceous aerosol in Africa requires the use up-to-date and precise inventories. They indicate that anthropogenic sources are increasing due to an explosive demography, with a progressive concentration of pollution sources in megapoles. They also mention the fact that biomass burning is highly dependent on climate change and environmental pressure that induces a high variability from season to season and year to year. In their work, a specific inventory for the year 2000 is used with better definition in the timing, location of the burnt areas and the estimation of their surface, allowing a better spatial and temporal definition of this source. Based on these arguments, it does not sound totally coherent to compare the simulations performed with the 2000-inventory with an inventory developed to represent the emissions of the 90's. In addition, the two inventories differ not only in the period of concern but also on the methods used for their establishment. As a result, it is not possible to discriminate the input due to a better characterization of the source locations and intensity since part of the difference is due to the difference in the time period for which the inventory is relevant. To really assess the difference due to the methodology, a kind of “degraded” inventory, similar to the one developed for the 90's, should be developed for comparison with the “better” resolved inventory.

In this context, the comparison with ground observations is also questionable. Different data sets, obtained for different periods, are compared with the simulations performed for the year 2000 and once again, it is not possible to discriminate the relative performance of the two inventories from the bias due to different in the time period between the simulations and the observations.

Finally, the manuscript largely focuses on the differences induced by the use of a new inventory, while the title suggest that the objective is the validation of the regional model, including combustion particulate emissions. May be the title should be changed to better reflect the content of the manuscript.

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## Other comments:

- The two inventories are not sufficiently described. In fact, only the Biomass Burning source is different between the two inventories. The weight of this specific source in the total emissions should be further discussed. In particular, a map of the emissions for July and December would help understanding the differences between the two simulations.
- In the introduction, the author mention that the simulations will be compared with OC and BC measurements, but only results for BC are shown.
- A budget analysis is performed over three sub-domains. The authors should comment on the way they select these domains. How representative are they in terms of emissions, land use, etc. ?
- The authors should further comment on the differences between the two inventories and budget in southern Africa, especially in terms of ratio between hydrophilic and hydrophobic BC.
- The comparison between the simulations for 2000 and measurements for 1990-1992 does not appear as relevant. Since the IDAF network still perform aerosol concentrations and wet deposition measurements, it is not clear why measurements for the year 2000 are not used. Another option would be to use more climatological averages.
- The comparison with the AOT raised some questions. It is not mentioned in the manuscript that only the part of the AOD due to small particle is used for the comparison. In addition, the MODIS monthly mean AOD are compositions based on clear sky measurements. Are cloudy days and location discarded to compute the monthly mean simulated AOD ?

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- However, the results look quite promising and would require a deeper discussion on the differences and on their possible origin. In particular, since the simulations also includes other aerosol species, can they contribute to the differences between the observed and simulated AOD ?
- The comparison with the vertical profile measured during the SAFARI 2000 experiment could have been interesting, but only the simulations are shown. The author should ask for the data to the different authors they refer to or at least scan the figures.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6653, 2008.

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