

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

Anonymous Referee #2

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The article is definitely useful and relevant. In order to assess the reliability of modeled atmospheric concentration data, comparisons with measurements are definitely needed. Since biomass burning (BB) is a major contributor to combustion carbonaceous aerosols (organic carbon (OC) and black carbon (BC)) in Africa, it is relevant to examine the modeling of the BB component for those aerosols. As appropriately indicated in the paper, there is a large spatial, seasonal, and annual variability in the African BB emissions. Therefore, atmospheric concentration data should be compared with modeled data that apply to the same month and year as the measurements were done. And here is a serious weakness of the article. This requirement was clearly not (always) fulfilled. The two sets of modeled BB emission data (L96 and L06) do not only differ with regard to the methodology used to arrive at them, but also with regard

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to the years to which they apply. The authors state that the L96 emissions data are representative of the 1990s. Do they mean the entire decade of the 1990s or only the first half of this decade? Then the L06 emission data are apparently for the year 2000 and if I understand lines 11-12 of page 6662 correctly, there are for L06 separate modeled emission data for each month. Thus, it seems to me that the authors often compare apples and oranges. The only correct comparisons seem to be those between measured and modeled data for the year 2000, whereby use is made of the L06 model. With regard to the difference between the two sets of modeled BB data, very little information is given, i.e., only in Table 1 with the yearly BC emissions from Forest and Savanna fires (FSF) for the entire African continent. The paper would benefit from a figure, which shows the spatial distribution of the FSF BC emissions for L96 and L06, with for L06 the annually averaged data.

It is stated in the Abstract that comparisons between model and experiment are made for both BC and OC. Clearly, the paper does not live up to this promise. Comparisons are only made for BC and AOT. Incidentally, the heading of Table 1 indicates that there are data for OC in it, but this is not the case.

The article uses experimental BC data from various authors (and methods). It is known that especially for biomass burning aerosols very substantial differences in BC data can be obtained by different techniques and by different temperature protocols within the same technique. A cautionary note about this should be made.

The discussion in section 4 (BC budget analysis) could be improved. For example, with regard to the positive easterly and negative westerly advection terms for West and Central Africa in both L96 and L06, to which extent are these due to the prevailing winds (which are presumably from the east) or to spatial differences in emission data?

Page 6663, lines 1-2: Some explanation why the simulated and observed AOT fields are expected to be different for west Africa in all seasons and in July over the Red Sea would be welcome. No literature reference is given here, so that the reader may

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wonder what is so special about these areas, in particular about the Red Sea in July. Is mineral dust or rather sea salt strongly enhanced over the Red Sea in this month? And if so, why?

Technical corrections:

p. 6655, l. 26: There is no "Solmon et al., 2005" in the list of References.

p. 6657, l. 16: There is no "Junker and Liousse (2007)" in the list of References. On the other hand, there is a "Junker and Liousse (2006)" there, which is not referred to in the text.

p. 6657, l. 25: "are based" should be replaced by "is based".

p. 6663, l. 13: The explanation of the acronym SAFARI 2000 was already given on p. 6656, lines 8-9. It should not be repeated here.

p. 6663, l. 14, and p. 6664, l. 2: A reference is needed for the "special issue on SAFARI-2000, 2003" and the "2003 JGR special issue". In the list of References there is "Swap et al., 2003", which is not referred to in the text. Do the authors perhaps have this reference in mind when thinking of the 2003 JGR special issue?

p. 6680, Fig. 7: The caption states that layer-averaged extinction coefficients are in the abscissa. This may be correct for (a) and (b), but definitely not for (c) and (d). The units here are in $\mu\text{g}/\text{m}^3$. In case these units are correct, are the m^3 then for ambient conditions or at standard temperature and pressure (STP). There is a large difference between both when going up to 500 hPa.

List of References, pp. 6666-6668: The references to most 2003 JGR papers are incorrect. Only the one to Swap et al. (2003) seems to be OK.

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