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Interactive Comment

Interactive comment on "Improving the seasonal cycle and interannual variations of biomass burning aerosol sources" *by* S. Generoso et al.

Anonymous Referee #1

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This paper adds little to the existing body of literature on the subject of estimating emissions from biomass burning. The authors developed an approach for scaling an existing inventory of emissions by use of ATSR nighttime fire observations. The approach is similar to that of Schultz (2002), however, in contrast to that study, it relies on the assumption of homogeneity over large regions. Differences to the work of Schultz (2002), or to the more comprehensive approach by Duncan et al. (2003) (applied to carbonaceous aerosol in Chin et al. (2002)) are not discussed, so that the merit of developing this algorithm is not clear.

The authors have implemented their new inventory into a general circulation model, which includes an aerosol module, and compare results for 3 years with those from a reference simulation using the "static" inventory of Lavoue et al. (2000). They describe some improvements in the seasonality of the aerosol optical thickness for selected stations from the AERONET network (later onset of burning compared to the standard inventory), and they claim further improvements for the interannual variability. However, close inspection of figure 2 reveals, that some of the changes introduced by the new emissions seem rather spurious (e.g. the enhancements over southern hemispheric Africa in 2001), whereas in other areas (Ilorin) hardly any change is seen at all, and the agreement remains poor. Duncan is right in his comment to point out that a much more extensive comparison between a model and observations has been performed in the Chin et al. (2002) study, and indeed, it seems to yield better agreement with both, TOMS and AERONET data. [Note: the better agreement with TOMS aerosol OD may however be a circular argument, because this data set was used to construct the Duncan et al. (2003) inventory].

Finally, the "case study" for the Indonesian fires during 1997/1998 remains qualitative and inconclusive. There is a substantial body of articles available, which discuss this specific fire episode and give carefully derived estimates of the likely emissions from those fires. The Generoso et al. paper does not provide one number, what their algorithm predicts for the scaled emissions, so that quantitative comparisons are impossible.

In summary, I cannot recommend the publication of this article in its present form. In order to become a valuable piece of the scientific literature, the analysis of model results must go far beyond the present text, and the differences to the other methods using satellite data for scaling emissions from vegetation fires must be discussed. If no clear reasons are provided, why this variant of such an algorithm would be superior e.g. to the Schultz (2002) approach, then there is little point in introducing it.

References:

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Duncan, B.N., et al., J. Geophys. Res., 108(D2), doi:10.1029/2002JD002378, 2003.

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Schultz, M., Atmos. Chem. Phys., 2, 387-395, 2002.

PS: correction of the comment of Duncan - Schultz, M. is not a coauthor of this paper. There has been a confusion with Schulz, M.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1973, 2003.

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