

## ***Interactive comment on “Tropical biomass burning smoke plume size, shape, reflectance, and age based on 2001–2009 MISR imagery of Borneo” by C. S. Zender et al.***

### **Anonymous Referee #2**

Received and published: 28 January 2012

This paper statistically analyses the properties of smoke plumes observed over Borneo by the MISR instrument. It builds heavily on the work published by the almost same group of authors in JGR (Tosca et al. 2010). Both papers focus on visibly discernible "smoke plumes" that are associated with a clearly observed fire source and both papers ignore more diffuse "smoke clouds".

The topic is certainly interesting and fits well within the scope of ACP. Unfortunately, the authors fail to discuss the relevance of such "smoke plumes" in any detail. Instead, the first paragraph of the introduction cites papers (Duncan et al. 2003, Tosca et al. 2010, 2011) that allegedly show significant effects of such plumes on the energy budget on

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the ground, the heating of the atmosphere aloft, sea surface temperature and precipitation. However, the conclusions of Tosca et al. 2011 do not mention any of these quantities at all. And Duncan et al. 2003 and Tosca et al. 2010 use atmospheric models with resolutions of  $2 \times 2.5$  deg and  $2.5 \times 2$  deg, respectively. Therefore, they cannot possibly represent "smoke plumes" that are shown in this manuscript to be narrower than 1 km (and contain smoke with an age of a few hours). In my opinion, this way of using citations is misleading and shows a lack in thoroughness by the authors that is not acceptable in the scientific literature.

The manuscript (1) derives statistical plume properties that quantify the geometrical properties of the mean plume that was already shown in Fig. 9 of Tosca et al. 2011 and (2) adds information on the mean optical properties. In so far, it presents new data. Unfortunately, the methodology is in my opinion not described in sufficient detail and what is described does not appear to be completely appropriate for the purpose of describing the smoke plumes:

- The entire work is based on a distinction of smoke "plumes" and "clouds". The definition of what constitutes a plume and the position of its perimeter is to a certain degree subjective. The criteria used in this manuscript need to be described in detail and be illustrated where they are not quantifiable.
- The creation of the plume database relies heavily on the "MINX" utility. Therefore, MINX either needs to be described in some detail or a reference to a detailed description needs to be given. The only reference for MINX in Sect. 2.1 "Creation of plume database" is Tosca et al. 2011, but the description in this paper is in my opinion not sufficient either, i.e. it would not allow an independent reproduction of the data processing. For example, the calculation method for wind vectors is completely unclear.
- Sect. 3 "Results" states that the uncertainty associated with the mean and median plume properties [...] is reported as a standard error  $SE = \sigma/\sqrt{N}$ .  $SE$

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is appropriate for repeated independent measurements of a fixed quantity. It is in my opinion irrelevant and inappropriate for the description of an ensemble of different objects like the smoke plumes. It is not the accuracy of the mean that is of interest but the spread around the mean that occurs in reality. The division by  $\sqrt{N}$  results in extremely low error estimates that made me suspicious already when reading the abstract the very first time.

- Fig. 6 shows the optical properties of all plumes and of the mean plume in relative spatial coordinates. However, the results are discussed on p. 31007, l. 21 – p. 31011, l. 4 in terms of the physical and chemical processes in the plumes, which depend on the age of the smoke rather than its position. Would it not be more appropriate to show the age-dependence of the mean optical properties?
- Figs. 3, 4, 10 show that the observed plume length, width-to-length ratio and area are follow lognormal distributions. However, the presentation of the results as mean  $\pm$  standard error, e.g. in the abstract, suggest that these quantities have Gaussian distributions. I find this inappropriate and misleading as it is not mentioned that the parameters follow a lognormal distribution.
- In Fig. 3 the values given for  $\sigma$  are the standard deviations, i.e. square root of the variance, while I am used to  $\sigma$  denoting the scale parameter when talking about a lognormal distribution. The symbol  $\sigma$  is not defined in the manuscript and the authors need to be much more careful in the presentation of their result.

The last sentence of the Discussion section states that the presented parameterizations would be "sufficient" for representing the smoke plumes in mesoscale meteorological models. This conclusion has not been proven in the manuscript. In fact, not even a criterion of being "sufficient" has been given.

In summary, the manuscript is in my opinion deeply flawed because (1) the presentation is severely misleading with respect to the already published literature and the results

and conclusions of this study and (2) the methodology is not documented in sufficient accuracy. I have the impression that the manuscript was not prepared with the required scientific thoroughness and believe that the authors will have to come up with major improvements throughout the entire manuscript in order to re-establish the credibility of this piece of work. Only then can the scientific significance and quality be properly assessed.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30989, 2011.

ACPD

11, C14798–C14801,  
2012

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