Atmos. Chem. Phys. Discuss., 11, C15995–C16002, 2012
www.atmos-chem-phys-discuss.net/11/C15995/2012/

Atmospheric Chemistry and Physics Discussions

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.

C. S. Zender et al.

zender@uci.edu

Received and published: 11 March 2012

Reviewer 2 (Anonymous):

We are grateful for the Reviewer's thoughtful comments. We revised the manuscript to address most of their suggestions. These suggestions have considerably strengthened the manuscript.

Detailed response (in plain text**) interspersed with Reviewer 1 Comments (***in italics***):**

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 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.

Duncan et al. 2003 and Tosca et al. 2010 include modeling results that simulate the climate and chemical response to radiative and chemical forcing by smoke. Tosca et al. 2011 was originally cited because it shows the relation of fire counts to precipitation from 2001-2009 which illustrates the ENSO signal in fire (and thus smoke). However, equivalent (though annually averaged information) is present in Tosca et al., 2010 and Tosca et al. 2011 shows no causal evidence of smoke effects on climate, so the revised manuscript omits this citation.

And Duncan et al. 2003 and Tosca et al. 2010 use atmospheric models with resolutions of 2x2.5deg and 2.5x2.deg, respectively. Therefore, they cannot possible 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).

We agree that the imprecise wording in the original manuscript could give the impression that smoke plumes, and plumes alone, cause all the effects mentioned (heating, precipitation changes, SST changes). The introduction has been clarified to explain that 1. smoke (plumes and clouds) have the significant effects mentioned in Indonesia, 2. the plumes are an important source of the smoke clouds, and 3. that this manuscript is concerned with the plumes because they have discernible shapes and more easily calculable wind velocities:

“The condensed portions of fire-generated emissions often appear as visible plumes emanating and carried downwind from the burn region, sometimes merging into larger scale smoke clouds of indistinct origin. In regions like Indonesia, smoke plumes and clouds are frequent and extensive enough to significantly alter the energy budget by shadowing the ground and heating the atmosphere aloft (Duncan et al., 2003, Tosca et al., 2010). Moreover, our model results (Tosca et al., 2010) suggest that smoke may reduce regional SST and dry-season precipitation, causing a potential feedback that increases drought-stress and air quality problems during El Niño years.”

“Here we study smoke plumes rather than smoke clouds because plumes have a discernible shape which can be characterized, and a transport direction that allow heights and wind speeds (and thus plume ages) to be more accurately estimated from MISR imagery.”

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.

Agreed.

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 not described in sufficient detail and what is described does not appear to be completely appropriate for the purpose of describing the smoke plumes: âĂć 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 quantifyable.

Section 2.1 of the revised manuscript elaborates on the plume selection criteria and the differences between a plume and a cloud:

“Plumes were accepted for inclusion if they exhibited substantial opacity, had a clearly defined transport direction, and were not fully obscured by water clouds. Furthermore, all plumes were visually associated with a point source, in contrast to larger “smoke clouds” which were masses of smoke with no discernible surface origin.”

This definition is significant because it allows us to compute wind speeds using MINX, which employs a superior modeling procedure to that used in the MISR products but relies on a user-supplied transport direction, which is only possible if the smoke area has a clearly defined transport direction.

âĂć 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.

Agreed. The description of MINX in Section 2.1 in the original manuscript was inadequate and has been replaced with a brief summary of the “Minx Algorithms” section, pgs. 87-102 of the “MISR Users Guide” prepared by Nelson et al. (2009). For more details of the algorithm, readers and the reviewer are encouraged to read the guide.

âĂć Section 3 "Results" properties states that the uncertainty associated with the mean and median plume properties [...] is reported as a standard errro SE= sigma/sqrt(N). SE 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 by of interest results but the spread around the mean that occurs in reality. The division by sqrt(N) results in extremely low error measurements that made me suspicious already when reading the abstract the very first time.

Agreed. All mentions of standard error in the paper have been replaced by the standard

deviation, which is a better representation of the spread about the mean. The one exception occurs in the intercomparison of plume age. Here we are interested in the standard error, since we are assessing whether the mean plume age in Borneo is significantly different from the mean plume age in Central America.[a]

âĂć 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?

This is an interesting idea. It is in principle possible to convert each pixel of each plume into an age based on the mean upstream wind field. Then pixels from different plumes could be binned by age, and characterized that way. There are, however, some conceptual and practical issues with this idea. First, the mean plume is based on a conceptual model of plumes being intrinsically isomorphic, and "stretchable" to the common, normalized length scale on which we characterize the properties. Essentially the wind field stretches plumes that would otherwise be similar into various lengths depending on time-since-emission. Our length renormalization procedure "undoes" the effects of wind-field stretching (and thus time-since-emission) to uncover the mean plume characteristics. Binning pixels by estimated age would reduce the results from two-dimensional (down-plume and cross-plume) to one (age) in which the spatial information was lost. In other words, information about which pixel were on the plume edge, and thus susceptible to additional optical retrieval errors, would be lost. Thus a time-based analysis brings its own problems. It would be a complemenary (a no less valid) conceptual model of plumes to the one we have analyzed. There are also practical issues: ransforming from a spatial to a temporal analysis would dramatically alter the paper, while simply adding a temporal analysis would increase its (already long) length. As a compromise, the revised manuscript clarifies the correspondence between the spatial location in the mean plume and the mean age of the associated

pixels, e.g.:

"The 0.03 SSA increase we observe mid-plume occurs over an estimated elapsed time of 1.5 hours, a brightening rate consistent with Abel's results."

âĂć 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.

Agreed. The revised abstract mentions that length, width and width-to-length ratio follow lognormal distributions. Additionally, the abstract now reports the 25th and 75th percentiles for each quantity rather than the standard error, since the spread between the percentiles better captures the asymmetrical nature of the distribution than does the standard deviation.

"50% of these plumes have length between 24 and 50 km, height between 523 and 993 m and width between 18% and 30% of the plume length. Length and cross-plume width are lognormally distributed, while height follows a normal distribution."

"Plume area (median 169 km2, with 25th and 75th percentiles at 99 km2 and 304 km2, respectively) varies exponentially with length, though for most plumes a linear relation provides a good approximation."

The results section likewise now emphasizes that length, width and width-to-length ratios follow lognormal distributions, and standard deviations are only mentioned to quantify the observed spread in the data.

âĂć In Fig. 3 the values given for σ are the standard deviations, i.e. square root of the variance, while I am used to σ denoting the scale parameter when talking about a lognormal distribution. The symbol σ is not defined in the manuscript and the authors need to be much more careful in the presentation of their result.

The symbol σ in Figures 3 and 4 has been replaced with “Std. Deviation” to clarify that this value is the standard deviation rather than the scale parameter of a lognormal distribution.

The last sentence of the Discussion section states that the presented parameteriztions 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.

The wording in the initial manuscript was overly ambitious and the word “sufficient” has been replaced with “plausible” Additionally, we now caution potential users that the shape parameteriztaion only to fires from regions with characteristics similar to those of Borneo:

“Modeling studies that require a realistic representation of plume density can scale our empirically based density parameterization Eq. (11) by an assumed plume length. While the distribution of length is affected by the overflight-time sampling bias described above, plume shape and density should not suffer the same magnitude of bias. Therefore, this parameterization is a plausible, empirically accurate shape for a prescribed stochastic distribution of tropical biomass burning smoke plumes. Plumes have associated radiative and air quality effects whose area of impact can now be included mesoscale meteorological models, without requiring full knowledge of fire ignition, physics and behavior. As discussed in Section 5 below, the PDFs and parameterizations derived here are based on Borneo, and modelers should make appropriate adjustments before applying them to regions with different fuel types, interannual and diurnal fire emission cycles, and meteorological circulations.”

In summary, the manuscript is in my opinion deeply flawed because (1) the presentation is severly 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 propperly assessed.

The revised manuscript more clearly represents the prior literature, and more accurately and usefully describes the results of our analysis of MISR retrievals. Our methods have been clarified to improve reproducibility. Despite all these changes, the sizes and shapes of Borneo plumes have not changed and we think that the revised manuscript contributes useful and original findings that will be welcomed by the researchers in the biomass burning community.

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