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Atmospheric
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Discussions

Interactive comment on "A multi-decadal history of biomass burning plume heights identified using aerosol index measurements" *by* H. Guan et al.

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Authors Reply to Referee's comments for MS Number acp-2009-751

Title: A multi-decadal history of biomass burning plume heights identified using aerosol index measurements

by H. Guan et al.

General authors comment: We thank the anonymous referees for their efforts in reading the manuscript and offering comments to help us improve it. We also thank Dr. M. Penning DeVeries for her computations that she has attached to her comments.

Our specific Responses follow each referee comment below.

Anonymous Referee #1 (Comments):

This is a useful study, in which the authors have applied a technique based on the TOMS/OMI aerosol index (AI) to determine the heights of smoke plumes. They calibrate the method using coincident CALIPSO data. They then analyze 31 years of AI data to identify smoke plumes more than 5 km above the surface, and estimate their heights. The results extend the CALIPSO smoke plume height climatology in both space and time, and can be used to help validate the vertical placement of smoke plumes in chemical transport models. In my opinion, several points should be addressed before the paper is accepted.

1. Abstract, and Introduction Paragraphs 3, 4, and 6. The term "injection height" is used here to refer to the heights of plumes up to several days old. Subsequent to initial injection, plume height can change significantly due to advection, turbulent mixing, self-lofting, and other processes. Many references cited in the introduction, both modeling studies and observations, use "injection height" to mean only the initial injection of smoke associated with the fire-generated buoyancy. As such, the definition of "injection height" might be clarified, and the characterization of these references might be revised to reflect this distinction.

Our reply: Injection height is clarified in the Introduction Paragraph 3 and the characterization of the related references has been revised.

2. Section 3, page 7, line 23. Why do you select the highest CALIPSO height in the plume area? Most of the smoke will reside below this level, and in addition, the highest pixels could be outliers that can occur for a variety of reasons. Statistics obtained this way are likely to be biased high relative to the effective heights of the plumes themselves.

Our reply: We use the highest value for the height from the CALIPSO measurements because the median value is difficult to determine for the optically dense smoke layers discussed in this paper. The lowest altitude where CALIPSO lidar signal is observed is reported as the base however in actuality, this point may lie well above the true base. Therefore we are not able to accurately retrieve the plume bases and calculate the effective heights. An inspection of CALIPSO images for all plume cases shows that plume-top heights are relatively uniform with a maximum variation within 2 km, closed to 1-sigma uncertainty of AI-derived height. The reason of the use of the highest CALIPSO height is now clarified in the text.

3. Section 3, page 8, line13. How did you determine the height uncertainty? More specifically, what is the actual height uncertainty of the AI technique?

Our reply: We are not exactly sure what the reviewer means by “the actual height uncertainty of the AI technique.” But there at least are three sources of uncertainty in the AI technique. The first is from the measurement of height by the CALIPSO instrument and visual identification of plume top as given in the paper (∼+/-500m). The second uncertainty is the uncertainty in the AI value computed by OMI. That uncertainty is difficult to assess (see De Graf et al 2005 a and b) and the OMI team does not give an AI uncertainty. The third uncertainty is the uncertainty in the linear correlation. The correlation coefficient for the correlation is 0.81. As stated in the text to Figure 1 +/- one standard deviation is about 1.9 km. We have revised our discussion in the text somewhat to hopefully make the sources of uncertainty clearer. DeGraf et al (2005a and b) show the sensitivity of the AI to many quantities. We are working on a follow-up paper to discuss these uncertainties further.

de Graaf, M., P. Stammes, O. Torres, and R. B. A. Koelemeijer (2005a), Absorbing Aerosol Index: Sensitivity analysis, application to GOME and comparison with TOMS, J. Geophys. Res., 110, D01201, doi:10.1029/2004JD005178

de Graaf,M. and P. Stammes (2005b), SCIAMACHY Absorbing Aerosol Index – calibration issues and global results from 2002–2004, Atmos. Chem. Phys., 5, 2385–2394.

4. Section 4, page 9, lines 5 - 14. “. . . suggests that for all plumes. . .” There are not really very many plumes in the climatology. Over 31 years, 181 plumes averages

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to about 6 plumes per year over the entire globe. What are the statistical uncertainties in Equation 2? Also, what factors contribute to the uncertainty in the retrieved height itself? The histogram in Figure 7 seems to suggest confidence in the height determination of about 2 km. An uncertainty analysis is needed, and the degree of applicability of the results needs to be assessed based on the result. In my view, this is important.

Our reply: As mentioned above we have added a discussion for the uncertainty in the text. What the reviewer appears be asking is a discussion of the uncertainties in the dependent variables (AOT, SSA, Surface albedo, viewing angles) in an AI calculation. That is, what is the implication of the linear correlation? We agree that it is an important question (see De Graf et al 2005a and b) and we are working on a follow-up paper to discuss it.

5. Section 4, page 9, lines 20-23. How much does the asymptote depend on particle single-scattering albedo? What contribution does this make to the overall uncertainty of the method?

Our reply: Radiative transfer calculations (see comments from M. Penning de Vries) show the dependence of AI on AOT asymptotes at an AOT of approximately 4-5. The value of AOT that the asymptote occurs seems to be independent of the value of SSA. The value of the AI asymptote is of course a function of the SSA value. M. Penning de Vries's results are also consistent with those of Wong and Li (2001).

Wong, J. and Li, Z.: Retrieval of optical depth for heavy smoke aerosol plumes: Uncertainties and sensitivities to the optical properties, J., Atmos. Sci., 59, 250–261, 2002.

6. Section 4, page 10, lines 3-4. Might dissipation and particle aging both contribute to changing the AI value as plumes evolve?

Our reply: Aging indeed has influence on aerosol optical properties. For example, study in Steven et al. (2003) suggests that an increase in the single scattering albedo

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as the aerosol ages, from 0.84 at source to 0.90 in the aged regional haze only in a few hours (5-hour) period. This suggests the effect of aging process on SSA mainly appears in initial period of plume formation (hours order). The plume's ages in our study, in general, are older than a few hours, often one-day order. Therefore, aging effect on SSA may be minor. We also examine MODIS/AOT and OMI/AI images day-by-day. Both maximum AI values and large-AI regions decrease with plume aging. The decreasing trend with time is also very obvious for MODIS AOD, suggesting the dissipation contributes significantly to decreasing AI value as plumes evolve. The possible minor effect of aging to AI is now added in the paper.

7. Section 4, page 10, line 12. Why would you assume that plume heights do not change with aging? Any forward trajectory model will illustrate how much change can be expected.

Our reply: As suggested, we now use the Goddard trajectory model to estimate the plume heights when they were young (1-day old). Figure 4 is also replotted with the estimated heights.

8. Section 5, page 11, Figure 5. There is not one plume over Africa in the entire climatology. Could this be a sampling issue associated with the AI technique, e.g., due to particle property or optical thickness requirements for plumes to be detected with this method? What proper ties must a plume have be to be detected by the AI method?

Our reply: Fires in Africa burn biomass like savanna and grassland. Previous literatures suggest that savanna (Freitas at al., 2007) and grassland (Freitas at al., 2007; Martin-Val, 2010) fires produce lower injection height compared to forest fires. Lower fire intensity for both types of fire has been well recognized as a reason for their lower injection height. AI is not sensitive to low-altitude plumes. Only dense high-altitude absorbing plumes can be detected by the AI method.

Freitas, S. R., Longo, K. M., Chatfield, R., Latham, D., Silva Dias, M. A. F., Andreae, M.

O., Prins, E., Santos, J. C., Gielow, R., and Carvalho Jr., J. A.: Including the sub-grid scale plume rise of vegetation fires in low resolution atmospheric transport models, Atmos. Chem. Phys., 7, 3385–3398, 2007.

Val Martin, M., Logan, J. A., Kahn, R. A., Leung, F.-Y., Nelson, D. L., and Diner, D. J.: Smoke injection heights from fires in North America: analysis of 5 years of satellite observations, Atmos. Chem. Phys., 10, 1491-1510, 2010.

9. Conclusions, page 12, lines 14-15. The authors mention that detecting low-elevation plumes would not be expected with the AI method. In addition, the sampling is very limited, and there are likely other systematic biases inherent in the data (e.g., Point 8 above). As such, the percents reported here might not be all that meaningful.

Our reply: Please see reply 8 for point “there are likely other systematic biases inherent in the data”. We replace the percents by the number as suggested now.

In summary – I think the key result is that there are 181 smoke plumes, in various stages of their evolution, that could be used to test the vertical distribution of smoke calculated in CTMs. An uncertainty estimate on the heights is needed, and generalizations about global plume behavior need to be tempered by the sampling limitations of this data set, and biases inherent in the technique.

Our reply: We discuss the uncertainty estimate issue above. We are not quite sure what the reviewer means by “tempering” our generalizations. However, we have revised the conclusions to make sure the reader understands the limitations of our analysis.

Anonymous Referee #2 (Comments):

General comments. The manuscript entitled, “A multi-decadal history of biomass burning plume heights identified using aerosol index measurements” provides a simple, seemingly robust relationship between aerosol index (AI) and plume height (from CALIPSO) that allows plume height to be estimated without CALIPSO measurements.

This reduces the restrictions on plume height information placed by satellite over pass time/location and provides an additional constraint on chemistry transport models. The manuscript is clear and well written. One concern I have is the dependence of AI on plume age, discussed below. I recommend this article for publication with the following changes.

Specific comments Abstract To be more direct, the abstract to should say what aerosol property (optical properties or absorption, etc.) is being used to estimate plume height, rather than the measurement (AI). For example, “We have quantified the relationship between aerosol backscattering (Aerosol Index, AI) measurements and plume height for young biomass burning plumes . . .”

Our reply: We are not quite sure what the reviewer is getting at here. The AI quantity is essentially the ratio of a measurement (the radiance at 340 nm) to a calculation of the radiance for a Raleigh-only atmosphere) as such it is a function of a number of other variables (AOT, SSA, solar angle, viewing angles, surface reflectance). A remarkable finding of this study is that the AI for young plumes that reach above 5 km is strongly correlated with the height. This implies that the height is the only important variable (that is, for these plumes the other variables are relatively constant). We are exploring this implication in the follow-on paper. As such, we think our wording of the sentence is the best.

Introduction Paragraph 5: The authors should provide the value corresponding to "relatively good agreement" from the Jeong and Hsu 2008 study.

Our reply: “(correlation coefficient = 0.86)” has been added into the text.

Aerosol Index (AI) should be explained with 1-2 sentences somewhere in the introduction before the last paragraph. Not all readers will be familiar with it enough to understand why it is ideal for this analysis.

Our reply: The related sentence has been added into the beginning of the last para-

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graph.

Section 3, Identification methodology for high-altitude plumes What, if any, are the effects of using a very short calibration period (3 yrs) to estimate plume heights over more than 30 years? Have the authors checked the AI set for drifting values? These issues should be addressed explicitly in the manuscript to add confidence to the observed (positive) relationship between AI and plume height.

Our reply: We are now addressing explicitly the consistency issue of the while AI record. The paper (Li et al., 2009), which demonstrates a consistency between the AI products from TOMS and OMI, has been cited. The confirmed consistency offers a basis for combined studies using both TOMS and OMI data. Another related paper by Gautam et al. (2009) has been also cited. The AI data during 2000-2004 drifting period is not included in this study.

Li, J., B. E. Carlson, and A. A. Lacis (2009), A study on the temporal and spatial variability of absorbing aerosols using Total Ozone Mapping Spectrometer and Ozone Monitoring Instrument Aerosol Index data, J. Geophys. Res., 114, D09213, doi:10.1029/2008JD011278.

Gautam, R., Hsu, N. C., Lau, K.-M., and Kafatos, M.: Aerosol and rainfall variability over the Indian monsoon region: distributions, trends and coupling, Ann. Geophys., 27, 3691-3703, 2009.

Results Paragraph 4: Could chemistry (in addition to dilution) be responsible for the difference in AI between young and old plumes at the same altitude? Can the authors distinguish between the effects of chemistry (e.g. oxidation, increasing hygroscopicity) and dilution? Either way this issue should be addressed explicitly since there have been documented observations of chemical changes coincident with aging smoke particles (Capes et al., 2008) that will certainly affect the AI.

Capes, G., Johnson, B., McFiggans, G., Williams, P. I., Haywood, J., Coe, H., 2008.

Aging of biomass burning aerosols over West Africa: Aircraft measurements of chemical composition, microphysical proper ties, and emission ratios. Journal of Geophysical Research–Atmospheres 113, doi:10.1029/2008JD009845.

Our reply: Possible minor effect of chemistry on AI as plume aging is now added in the text.

Section 5. The authors show that plume age affects the relationship between AI and plume height. However, in this section historical plume height is determined from the demonstrated relationship with addressing how the authors know that these are all young plumes. If they are not young plumes, then the plume height is likely underestimated. Perhaps this has been accounted for but it is not clear to me and should be addressed explicitly. The authors should also caution users of the AI/plume height relationship to confirm that only young plumes are being characterized, in order to preserve the integrity of the calibration.

Our reply: We agree with the reviewer's suggestion. In the absence of MODIS fire count data (prier to 2000) the accurate estimate of plume age is impossible. Figure 7 may include some aged plumes. We are now excluding Figure 7 and associated discussion.

Figure 5. This figure would provide more information if instead of showing only high-altitude plumes, points were colored by AI and all plumes where shown. This would reinforce the authors' point that most of the high altitude plumes come from North America but would provide more information than is currently available from this figure. An alternative would be to pair this map with a map of the same points colored by plume height (using the relationship in Fig. 4).

Our reply: We prefer our representation because we want to show the distribution of both aged and young plumes. The reviewer's suggestion would add more detailed and meaningful height distribution only if all plumes in Figure 5 were in young age.

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Figure 6. It seems there is room on the x-axis to provide region name instead of ID (A-D) for simplicity. If available, it would be beneficial to add the number of low-altitude plume observations for each region to this figure.

Our reply: ID (A-C) has been replaced by corresponding region name as suggested. It is not possible to find the number of low-altitude plume observations based on AI since optical-dense low-altitude plume and optical-thin high-altitude plume may have a similar AI value.

Technical Corrections Future submissions should include line numbers to aid reviewers.

Our reply: Good suggestion. We are including line numbers in this revised version.

Abstract Consider defining OMI in the abstract. Rephrase "injection heights $\geq$ 8 km but below12 km" as "injection heights between 8 and 12 km" for clarity.

Our reply: Changed as suggested.

Introduction Sort lists of citations by year, from earliest to most recent.

Our reply: Changed as suggested.

Section 5 Last paragraph: In "may have a most important impact on the radiative budget," the word "a" should be replaced with "the"

Our reply: Changed as suggested.

Anonymous Referee #3 (Comments):

The results of this study will be important because they provide a climatology database to validate biomass burning plume injection heights in chemical transport models. The authors first use CALIPSO and concurrent AI data to arrive at a simple empirical relationship for injection height, which was then applied to a wide range of data in time and space. This method is an improvement to previous ones in that it applies the same

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simple algorithm to a longer time period of observations, yielding a more objective climatology to use to compare with models. The following requires improvement.

1) In section 3, it is explained how and why Gobi/Taklimakan desert signals are removed. In Figure 6, the bar value for Russia/N.E. Asia is smaller than N. America. Is it possible that those removed from the analysis in this region could make its value higher than N. America? This is relevant as it is well known that plumes (possibly mixed with dust and smoke) from Russia/N.E. Asia influence the Arctic and N. America, presumably because they were able to inject higher and allow long-range transport.

Our reply: The plume number for Russia/N.E. Asia is still less than N. America when adding potential dust cases (a few). The number of dust plumes screened out is now added to the Russian bar in a different color. In addition, the intercontinental transport of wildfire is not only limited to the transport from Russia/N.E Asia to the Arctic and N. America. The transport of fire plume from North America to Europe has also frequently been noted (Stohl et al., 2003; McMillan et al., 2008).

Stohl, A., C. Forster, S. Eckhardt, N. Spichtinger, H. Huntrieser, J. Heland, H. Schlager, H. Aufmhoff, F. Arnold and O. Cooper, A backward modeling study of intercontinental pollution transport using aircraft measurements, J. Geophys. Res., 108(D12), 4370, 10.1029/2002JD002862, 2003.

McMillan, W. W., J. X. Warner, M. M. Comer, E. Maddy, A. Chu, L. Sparling, E. W. Eloranta, R. M. Hoff, G. Sachse, C. Barnet, I. A. Razenkov, and W. Wolf, 2008: AIRS views of transport from 12-22 July 2004 Alaskan/Canadian fires: Correlation of AIRS CO and MODIS AOD with forward trajectories and comparison of AIRS CO retrievals with DC-8 in situ measurements during INTEX-A/ICARTT, J. Geophys. Res., doi:10.1029/2007JD009711.

2) Injection height seems loosely defined, with not many remarks about the measured thermodynamic structure of the atmosphere for each case. Regional and diurnal temperature profiles could change greatly from case to case, and a plume could appear

to be injected while actually still confined to the lower troposphere. Is it possible that height- varying temperature inversions, at the surface and/or tropopause, had an influence on the identification of injection height? It may be possible to use NCEP reanalysis to plot temperature profiles for each case, and then determine the actual tropopause height to see if the linear model still predicts injections.

Our reply: The definition of smoke plume injection height (see below) is now included in the paper. The factors affecting injection height include the thermodynamic stability of atmospheric environment; the surface heat flux released by fire; and environmental wind et al. factors (Freitas et al., 2010). The studies in Khan et al. (2007) and Val-Martin (2010) suggest that plume height is related to temperature inversion height. However, some plumes are also present in the absence of inversion layer (Freitas et al., 2010 and Val Martin et al., 2010). Freitas et al. (2010) shows that plume in a windy environment is much lower than a calm environment. Therefore, the height of stability level is not only influence factor on injection height. “Smoke plume injection height, the altitude at which the smoke particles are injected to the atmosphere before transport (Kahn et al. 2008)”

Freitas, S. R., Longo, K. M., Trentmann, J., and Latham, D.: Technical Note: Sensitivity of 1-D smoke plume rise models to the inclusion of environmental wind drag, Atmos. Chem. Phys., 10, 585-594, 2010.

Kahn, R. A., Li, W.-H., Moroney, C., Diner, D. J., Martonchik, J. V., and Fishbein, E.: Aerosol source plume physical characteris-tics from space-based multiangle imaging, J. Geophys. Res., 112, D11205, doi:10.1029/2006JD007647, 2007.

Kahn, R. A., Chen, Y., Nelson, D. L., Leung, F.-Y., Li, Q., Diner, D. J., and Logan, J. A.: Wildfire smoke injection heights: Two perspectives from space, Geophys. Res. Lett., 35, L04809, doi:10.1029/2007GL032165, 2008.

Val Martin, M., Logan, J. A., Kahn, R. A., Leung, F.-Y., Nelson, D. L., and Diner, D. J.: Smoke injection heights from fires in North America: analysis of 5 years of satellite

observations, Atmos. Chem. Phys., 10, 1491-1510, 2010.

3) Several grid spacings were mentioned in this paper, but none seem to be at the same resolution. Is it safe to draw conclusions based on the finer resolution CALIPSO, coarser AI data, and even coarser back trajectory grids?

Our reply: Plumes focused here, In general, have a larger horizontal size. The plume top does not indicate a large variation over an OMI/AI grid (1.25°x1°). Trajectory model has been run at each OMI/AI grid point within the defined AI plume.

4) A statement should be made regarding the vertical and horizontal resolution of the back trajectory model, and if the isentropic setting was valid as opposed to isobaric. Do all the back trajectories eventually fall back to the surface at the time when the fires were located? How sensitive are the results in this study to the initial height/latitude/longitude of the back trajectories for each plume? Does the arbitrary 'young' vs. 'old' definition change based on running the model in a matrix setting (perhaps a 3x3x3 array in height, latitude and longitude around the identified start location), rather than one starting point?

Our reply: The spatial resolution of the back trajectory model is now added in the paper. The Godard trajectory model for both isentropic and isobaric settings does not include sub-scale plume injection process. This process controls buoyancy-driven initial injection height. Therefore back trajectory is less possible to fall back to the surface at the time when the fires were located. 'young' vs 'old' definition is based on running the trajectory model starting at each grid point within the biomass plumes, which cover a large area (generally much larger than 3x3 horizontal grids), not a single starting point. The trajectory model was also run in different starting heights (0.5 km resolution). The age classes for each plume are not changed.

7) After mention of the source region differences in plume height, is it possible that source regions also control the aerosol size distributions and chemistry, both of which will affect the radiative transfer and thus could cause bias in the AI measurements and

derived injection heights? What appears to be a difference in injection height due to regional fire characteristics could possibly be instead biases in optical depth due to aerosol composition.

Our reply: The radiative transfer calculation (see Penning de Vries's comments) shows that aerosol index is not influenced very much by aerosol size distribution. Therefore, a minor effect of aerosol size distribution on the derived relationship is expected. The chemistry will affect the absorption and thus the AI value (see Penning de Vries's comment Fig.2). However, we believe that the variability of the SSA is smaller in the plumes we have chosen than the range chosen by Penning since the uncertainty in the height is only $\sim$2 km.

In summary, this paper offers a promising and simple approach to uniform plume identification that has wide applications. It was surprising to see the differences between N. America and Russia/N.E. Asia, where one might expect the latter to have more plumes.

Anonymous Referee #4 (Comments):

The manuscript "A multi-decadal history of biomass burning plume heights identified using aerosol index measurements" presents an interesting method to identify the plume height of fresh biomass burning fires using the TOMS and OMI aerosol index (AI) data, which is available for a long time period. Historical AI data are then used to examine the plumes reaching at high altitudes, and their geographical occurrences. The results could have important implications for understanding the aerosol emissions from fires and improving the chemical transport model simulations. It should be suitable for publication in the Atmospheric Physics and Chemistry, but the following issues need to be addressed.

1. First the injection height of plumes in the paper was not clearly defined. It represents different meanings in different context but was interpreted the same. For example, the selected CALIPSO measurements, which are used to correlate with the AI, indicate only the maximum heights reached by the young plumes (<2 days), while the injection

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heights in the chemical transport models generally refer to the vertical distribution of plume at fire sources. They are not always equivalent. The authors thus need to clarify the model applications of the maximum plume heights inferred from the AI data with this method. The present discussions in Conclusions about using them as direct input for injection heights in chemical transport models seem to be far too stretched, given the large variability in the vertical distribution of plumes near the fire sources. And what is the rational of using 2-day aging from fire sources to separate young fresh fires from old aged fires, not taking local meteorology and burning conditions in count?

Our reply: The definition of smoke injection height by Kahn et al. (2008) is now added in the paper. The reviewer is correct that plume height that we use is different than the injection height used in chemical transport models. We have tried to make that difference clear in the text. The rationale for using 2-day aging is simplicity. It would be very difficult to describe the local meteorology and burning conditions for each fire.

2. Moreover, on page 9, lines 23-25, ". . .plume height will determine the value of AI." This is used to support the linear regression in Fig. 3, so presumably, the plume height here is the maximum height reached by plumes. But in the cited reference next, Jeong and Hsu, 2008, the plume height (aerosol layer height) refers to the radiatively effective height of aerosol layer, assuming a Gaussian vertical distribution with a constant width of 1km. Jeong and Hsu 2008 also suggested that the inferred plume height from AI and aerosol optical depth "may not be interpreted as a geophysical vertical profile". Therefore, what are the assumptions needed here in order to link AI to the maximum plume height, when the effects from aerosol optical depth and SSA are insignificant? Does the obtained linear relationship between AI and the plume height apply to multi-layer plumes, or thick (a few kilometers) plumes with a large gradient of intensity?

Our reply: The reviewer makes a good point. For the plumes we studied the CALIPSO data showed that they were relatively thin so that there is little difference between the maximum plume height and the radiatively effective height. However a very thick plume, say several kilometers, this may not be the case and the plume may not fit on

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our linear correlation. More work is necessary to understand if AI values are linearly correlated to the maximum height of very thick plumes.

3. It refers to the Wong and Li's paper for the asymptote behavior of irradiance varying with aerosol optical depth at wavelength of 0.65. However it should not be difficult to perform similar radiative transfer calculations at the two wavelengths of AI, 0.331 and 0.36 micro meter. These radiation calculations could yield the threshold values of aerosol optical depth for a typical smoke SSA, 0.85âĹij0.9. Comparison with the satellite i.e. MODIS, aerosol optical depth could provide some support to the statement that the large-AI young plumes sampled are "sufficiently optically thick".

Our reply: The radiative transfer calculation (see Penning de Vries's comments) at 0.336 and 0.377 micrometer also shows similar behavior. In the meantime, we are also working on a follow-up paper. A series of radiative transfer calculations at the two wavelengths of AI, 0.331 and 0.36 micrometer will be presented. The asymptote behavior of irradiance varying with aerosol optical depth at these wavelengths will be also verified. Although the conservative nature of the MODIS cloud masking eliminates a great deal of high optical depth aerosols as "clouds" (Hoff et al., 2005; Soviev et al., 2009), we agree that it can still be a qualitative tool to check the denseness of a plume for our next paper.

Hoff, R. M., S. P. Palm, J. A. Engel -Cox, and J. Spinhirne (2005), GLAS l ong-range transport observation of the 2003 California forest fire plumes to the northeastern US, Geophys. Res. Lett., 32, L22S08, doi:10.1029/2005GL023723.

Sofiev, M., Vankevich, R., Lotjonen, M., Prank, M., Petukhov, V., Ermakova, T., Koskinen, J., and Kukkonen, J.: An operational system for the assimilation of the satellite information on wild-land fires for the needs of air quality modelling and forecasting, Atmos. Chem. Phys., 9, 6833-6847, 2009.

Minor corrections:

1. Page 4, line 20, should be “. . .CALIOP instrument. . .”

Our reply: Changed as suggested.

2. Page 5, in the footnote, define τ , ω, and z.

Our reply: The definitions of τ, ω, and z are added in the footnote now.

3. Page 6, section 2.2, specify the over-passing time of CALIPSO, since it mentioned that the MISR instrument on Terra may detect fires before they reach their maximum intensity.

Our reply: The over-passing time of CALIPSO is now specified.

4. Page 10, line 12, how valid is this assumption that “the plume heights do not change during aging”? Is it very likely for those 4 aged fires shown in Fig. 4 based on local meteorology?

Our reply: We now estimate the heights of those 4 plumes when they were young using forward trajectory model suggested by the reviewer 1.

5. Fig. 5, there is no single fire with large AIs (>9) in Central Africa and only one in S. America? Could this be a bias due to the cloud or dust screening?

Our reply: African dust is mainly emitted from northern Africa. Therefore the dust-screening process is not applied to central Africa. The cloud screening process does not affect the number of plume over S. America and Africa very much. Fires in Central Africa mainly burn biomass like savanna and grassland, which produce broader and lower injection layers due to lower fire intensity (Freitas et al., 2007). Compared to middle/high-latitude boreal fire, the tropical forest fire in S. America is also found to have a lower fuel consumption rates, hence, lower intensity of burning and smaller quantities of combustion products (Nicole and Foster, 2004). These factors may contribute to lack of large-AI plumes in Africa and S. America.

Freitas, S. R., Longo, K. M., Chatfield, R., Latham, D., Silva Dias, M. A. F., Andreae, M.

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O., Prins, E., Santos, J. C., Gielow, R., and Car valho Jr., J. A.: Including the sub-grid scale plume rise of vegetation fires in low resolution atmospheric transpor t models, Atmos. Chem.Phys., 7, 3385–3398, 2007.

Nicole Spichtinger-Rakowsky and Caroline Forster (2004): Intercontinental Transport of Trace Substances from Boreal Forest Fires.The Handbook of Environmental Chemistry Vol. 4, Part G (2004): 255–276. DOI 10.1007/b94530.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1, 2010.

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