**Atmospheric
Chemistry
and Physics
Discussions**

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

Anonymous Referee #4

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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.

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 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?

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?

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, 0.85~0.9. 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".

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Minor corrections:

1. page 4, line 20, should be "...CALIOP instrument..."

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

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.

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?

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?

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