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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.

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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 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. 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. 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? 4) A statement should be made regarding the vertical and horizontal resolution of the back trajectory model, and if the insentropic 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? 5) 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? 6) 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. 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.

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