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***Interactive comment on* “The impact of
deforestation in the Amazonian atmospheric
radiative balance: a remote sensing assessment”
by E. T. Sena et al.**

Anonymous Referee #1

Received and published: 27 August 2012

This paper estimates from remote sensing measurements the radiative forcing related to deforestation, including emitted smoke particles from biomass burning, changes in surface albedo and column water vapor amount associated with deforestation. Ten years of observations from CERES, MODIS, and AERONET are used, in conjunction with SBDART model that extends the satellite instantaneous observations to daily mean. The study is original and offers important insights into interactions of smoke, surface, and radiation. It is also nice to see that efforts have been made to estimate uncertainties in estimated aerosol direct forcing. The paper is well written. I recommend that the paper be published in ACP after the following minor issues are adequately addressed.

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In the abstract and throughout, it should be clarified that you are dealing with solar radiation and in clear sky only. abstract, line 12: better to say the biomass burning season considered in this study is August–September. Abstract, line 15: “the imbalance in the radiative forcing”. I don’t quite understand this term. To me it is just maximum aerosol direct forcing. page 14839, line 22–24: there are some other studies that suggest biomass burning smoke may delay the wet season onset, such as Zhang et al. (2009), Impact of biomass burning aerosol on the monsoon circulation transition over Amazonia Geophys. Res. Lett., L10814, , 36, 10.1029/2009GL037180. page 14840, line 6–7: Haywood et al., 2000, and Forster et al., 2007. I would suggest you cite more original references in the 60s and 70s. page 14842, line 8–10: readers would prefer to see here how good the MODIS AOD is. You may give a summary based on evaluation studies in the literature. page 14843, line 26: “overpass” may be better than “timepass” page 14846, line 21: what is “blue-sky albedo” page 14846, line 26: “Dubovik et al., 2000” should be “Dubovik and King, 2000” page 14847, line 1–10: here you need to say how many aernet sites are used and how you deal with aerosol properties at wavelength <440 nm and >1064 nm. page 14847, section 2.5.3: when converting instantaneous forcing to 24-hr mean with SBDART simulation, it is assumed that AOD is constant throughout a day. Several studies have shown that AOD in Amazon shows large diurnal variations based on AERONET measurements, such as Kaufman et al., GRL, 2000; Smirnov et al., GRL, 2002; Zhang et al., JGR, 2012. Please discuss how your assumption of constant AOD would affect your results. page 14848, line 21–24: do you have any insight why 2004 is so different than other years in cloud-screening? page 14849, line 15–21: without showing the correlation between AOD and SWARF (scatter plots with correlation coefficient), it is hard to argue that 2005 has stronger correlation than 2008. page 14850, line 25: “analysed” should be “analyzed”. page 14851, line 10: here you have instantaneous LYRF of -23.7 W/m^2 . Based on Table 4, I can get -13 W/m^2 . What causes this discrepancy. Maybe I have missed something. I am also curious what you can get from SBDART simulations by using surface albedo of 0.140 for forest and 0.155 for cerrado. How does such SBDART estimate compare

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with satellite measurements? page 14854, line 9-14: can you give your 2000-2005 average for a fairer comparison with Patadia et al. (2008)? page 14855, line 14-23: you found that mean SWARF was 35% higher over forest than over cerrado. You also found that the mean daily forcing efficiency is about 68% higher over forest than over cerrado. As shown in Table 5, AOD over forest is much higher (nearly 100%) than that over cerrado. It makes me puzzled how to reconcile these percentages. Table 2: what does number in parenthesis represent? Table 3, caption: “daily for the Amazon region SWARF” should be “daily SWARF for the Amazon region” Table 4, caption: add “aerosol” before “forcing efficiency”. Also what does the standard deviation suggest? Table 6: what does AF and AH stand for respectively? Figure 1: “high decrease” may be replaced with “sharp decrease” Figure 2: it is too crowded. Can you separate the figure into three, one for each site? Figure 4: please add correlation coefficients to the figure. It seems to me that the linear correlation at the cerrado site is better to be categorized into two regimes, one for $AOD < 0.2$ and one for $AOD > 0.2$. Why does this occur? How will your single linear correlation affect your determination of clean TOA fluxes and SWARF? Figure 7: please include names of AERONET sites in the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14837, 2012.

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