Interactive comment on "The impact of deforestation in the Amazonian atmospheric radiative balance: a remote sensing assessment"

by E. T. Sena et al. submitted to Atmos. Chem. Phys. Discuss., 12, 14837–14874, 2012.

We thank the editor for the careful review of our manuscript. We found the two referee reports very useful and they helped to improve the final version of the manuscript. It was nice to learn that referee 1 mention: "*I recommend that the paper be published in ACP after the following minor issues are adequately addressed*."

Specific answers to Anonymous Referee #1:

In bold type, the issues raised by the reviewer, followed for our action in each issue raised.

We would like to thank referee #1 for his/her careful review and for the several suggestions that helped us improving the final manuscript.

In the abstract and throughout, it should be clarified that you are dealing with solar radiation and in clear sky only.

Thanks to pointing out this issue. The final manuscript was modified in order to make clear that we are dealing with solar radiation and clear sky only. This now is stated in the abstract and also in 2 parts of the manuscript.

abstract, line 12: better to say the biomass burning season considered in this study is August-September.

The biomass burning season considered in this study (August-September) was specified earlier in the abstract, at line 8.

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.

This sentence was changed in the final manuscript exactly as suggested.

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.

This reference was included in the final manuscript, together with Butt and Bevan papers.

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.

Several other references were included in the final manuscript citing the direct (Atwater, 1970, Charlson and Pilat, 1969, Coakley et al., 1983, McCormick and Ludwig, 1967, Mitchell Jr., 1971) and indirect effects of aerosols (Albrecht, 1989, Rosenfeld and Lensky, 1998, Twomey, 1977).

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.

Good suggestion. In the revised version we added a summary of MODIS AOD validations based on 3 relevant papers (Chu et al., 2002, Remer et al., 2005, Levy et al., 2007).

page 14843, line 26: "overpass" may be better than "timepass"

This suggestion was accepted and incorporated to the revised manuscript.

page 14846, line 21: what is "blue-sky albedo"

Blue-sky albedo ($\alpha(\theta_0,\lambda)$) is a nomenclature used to refer to the actual albedo, as described in the papers from Lewis and Barnsley, 1994, Lucht et al., 2000 and Schaaf et al., 2002. It is obtained from the linear combination of the direct (black-sky, $\alpha_{bs}(\theta_0,\lambda)$) albedo and the diffuse (white-sky, $\alpha_{ws}(\lambda)$) albedo. To make this point more clear, we added equation (1) in the revised manuscript to clarify this point. It states that:

$$\alpha(\theta_0,\lambda) = \left[1 - S(\theta_0,\tau(\lambda))\right]\alpha_{bs}(\theta_0,\lambda) + S(\theta_0,\tau(\lambda))\alpha_{ws}(\lambda) \ ,$$

where $S(\theta_0,\tau(\lambda))$ is the diffuse fraction of radiation that reaches the surface.

page 14846, line 26: "Dubovik et al., 2000" should be "Dubovik and King, 2000"

This citation was corrected in the revised manuscript.

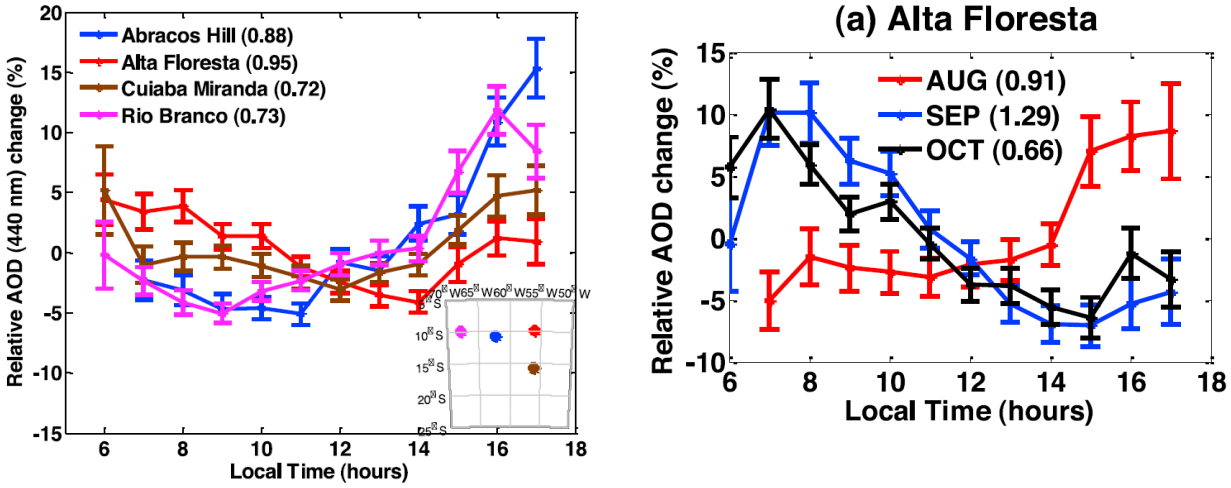
page 14847, line 1-10: here you need to say how many aeronet sites are used and how you deal with aerosol properties at wavelength <440 nm and >1064 nm.

We have used information from 7 AERONET sites (AF, AH, JP, RB, BA, BE and CM). For wavelengths < 440 nm and > 1064 nm, aerosol properties were extrapolated using the mean Angstrom exponent (between the wavelengths 440 and 870 nm) during the studied period. This information was now included in the revised manuscript.

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.

That is a very important point and we thought a lot about that. Zhang et al., 2012 (please see figures below) show that for Amazon sites the diurnal variability of aerosols was always smaller than 15%. This

diurnal variability is on the same order of the MODIS AOD uncertainty of ±0.05±0.15*AOD (Remer et al., 2005). Figure 7 from Zhang et al. shows that during Terra overpass (around 10:30 local time) the AOD is close to the mean diurnal AOD value. Furthermore, the highest AOD values are observed either early in the morning or late in the afternoon, when the solar zenith angle is low, and therefore aerosols have a smaller impact on the direct radiative forcing. Additionally, the effect of lower AOD before noon will be compensated by the effect of higher AOD in the afternoon (or vice-versa). For those reasons, the impact of aerosol diurnal variability on the mean daily aerosol forcing is expected to be small. We added a paragraph in the manuscript to make this point more clear.



From: Aerosol daytime variations over North and South America derived from multiyear AERONET measurements, from Yan Zhang et al., 2012.

page 14848, line 21-24: do you have any insight why 2004 is so different than other years in cloud-screening?

CERES-SSF product presents a large amount of missing values for MOD04 retrieved parameters, such as cloud fraction, aerosol optical depth, etc. for the year 2004 over the studied area. Up to 2004, CERES SSF database Edition 2 contains collection 4 MOD04 aerosol and cloud properties; after 2005, it contains MOD04 collection 5 information. However, only the year 2004 presents a high amount of MOD04 missing values for aerosol and cloud properties in CERES-SSF database. The percentage of missing MOD04 cloud and aerosol properties was around 45% for 2004, while for all the other years (2000-2003 and 2005-2009) missing values were on average only 10%. CERES science team has been formally contacted and informed about this problem and they are working on understanding it. They expect that this problem won't show up in CERES-SSF Edition 4, which is now being processed. In the revised manuscript, we included more information about this problem.

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.

Further analysis have shown that the correlation between SWARF and AOD is around -0.64 for both years, 2005 and 2008. Therefore, in the revised manuscript, we have removed this argument from the text.

page 14850, line 25: "analysed" should be "analyzed".

In this paper we chose to use the British spelling, therefore we have used "analysed" instead of "analyzed".

Page 14851, line 10: here you have instantaneous LURF of –23.7 W/m2. Based on Table 4, I can get –13 W/m2. 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 with satellite measurements?

The instantaneous LURF and the TOA flux in Table 4 are not really directly comparable, because cerrado and deforested areas have different surface albedo, and also different zenithal angles. The LURF was obtained from the difference of the TOA fluxes over forest and deforested areas in close-by areas, with same zenithal angle and water vapour content. Besides that, Table 4 shows the difference in the average instantaneous TOA fluxes measured over the whole forest and cerrado regions regardless of their latitude or water vapour content.

The comparison of SBDART and satellite measurements is complex. In a recent paper (Zhu et al., 2012, Effect of spectrally varying albedo of vegetation surfaces on shortwave radiation fluxes and direct aerosol forcing, Atmos. Meas. Tech. Discuss., 5, 4041–4076, doi:10.5194/amtd-5-4041-2012) the authors show how distinct surface models may lead to distinct SWARF results from SBDART. Furthermore, small variations in the single scattering albedo may lead to large variations in the SWARF obtained by SBDART. In a future collaboration with Zhu et al., we intend to compare satellite and SBDART SWARF estimates.

page 14854, line 9-14: can you give your 2000-2005 average for a fairer comparison with Patadia et al. (2008)?

In this work, the 24h SWARF average from 2000 to 2005 was -6.4 $\pm$ 1.0 W/m^2. This value is consistent with Patadia et al., 2008, that obtained -7.6 $\pm$ 1.9 W/m^2 for the 24h SWARF average for the same period. A new sentence was included in the revised manuscript with this comparison.

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.

The key to this point is that SWARF is not linear with AOD. We calculated the forcing efficiency taking into account AOD values smaller than 2, because of the non-linearity. Some authors limit this calculation to AOD less than 1. Fig. 6 and Table 5 show that forest areas present high AOD. But, the forcing efficiency should not be used to estimate the SWARF when AOD is higher than 2. Maybe if only low AOD

cases were considered, when SWARF x AOD linearity holds, it would be easier to reconcile these percentages. We added a text discussing the non-linearity of SWARF with AOD.

Table 2: what does number in parenthesis represent?

They represent the uncertainty in the last decimal place. An explanation has been included in the final manuscript.

Table 3, caption: "daily for the Amazon region SWARF" should be "daily SWARF for the Amazon region"

This mistake was corrected in the revised manuscript.

Table 4, caption: add "aerosol" before "forcing efficiency". Also what does the standard deviation suggest?

The word "aerosol" has been added before "forcing efficiency". The standard deviation suggests that biomass burning aerosol optical properties vary from year to year. It also suggests that the aerosol forcing efficiency over cerrado and over forest covered areas are different, due to differences in surface albedo.

Table 6: what does AF and AH stand for respectively?

AF and AH stand for Alta Floresta and Abracos Hill (Ji Paraná), respectively. In the final manuscript we have included the complete names in caption of Table 6.

Figure 1: "high decrease" may be replaced with "sharp decrease"

This suggestion was accepted and included in the final manuscript.

Figure 2: it is too crowded. Can you separate the figure into three, one for each site?

We have accepted the referee's suggestion. In the final manuscript the figure is separated 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?

The correlation coefficients were added to the figure. Surface albedo variation within a single 0.5 x 0.5 degree cell is much larger over cerrado than over forest. Therefore, cerrado sites show higher data dispersion. The apparent different regimes were probably due to this higher dispersion and are not usually observed. In the final manuscript we used a different example for the linear fit over the cerrado site in order to illustrate this higher data dispersion.

Figure 7: please include names of AERONET sites in the caption.

In the final manuscript the names of AERONET sites were included in the caption.

Answers for the points raised by Anonymous Referee #2

First I am not a specialist of aerosols, thus please read this review with this background information in mind. Overall I liked the paper and learned quite a bit. I have two main comments.

We would like to thank referee #2 for his/her careful review and for the good suggestions that helped us improving the revised manuscript and broaden the discussion to a larger audience.

Firstly for me a section which discusses what the results now mean in a broader context is missing, leaving the reader a little bit with the impression of 'so what'. Given the results I think it should be not very difficult to add such a section. Given the various effects will the biosphere (e.g. agriculture) now profit or not ? in the total are the effects a concern or not ? As an example you state at the end of the abstract 'The large radiative forcing values presented in this study point out that deforestation has strong implications in convection, cloud development and photosynthesis rate over the Amazon region.' Your study does not show that (e.g. 'strong implications on cloud development'). However your results could possibly lead to something like that if you broaden a bit the discussion.

Thanks for raising this important issue. You are right. We added one full paragraph in the conclusions about the broader context and implications on regional and global issues. Also several modifications were made throughout the text in order to clarify important points to the reader.

My second main comment is that for me too much technical material is in the main text. In my view the article would be more easy to read and thus would reach a wider audience if you would move some of the more technical materials in an appendix.

We agree that the manuscript is hard to read for non-specialists. We opted to leave the full technical material in the main text because there is new methods developed in this work that is crucial for the final result. Different methodologies may lead to different results, especially with so many confounding variables. Furthermore, we would like to guarantee the reproducibility of the work, because we believe there is room for improvement in the future and that this methodology if fully specified can be applied for other areas.

More detailed comments

abstract: line 17

(1) 'The surface reflectance plays a major role in the aerosol direct effect' - can you say in a few words why, what is the mechanism ?

The surface and aerosol particles scatter and absorb solar radiation. The presence of aerosols modifies the outgoing top of the atmosphere radiation flux over a surface. Biomass burning aerosols from Amazonia are usually bright (single scattering albedo around 0.9 at 550 nm). Therefore their impact is

larger over dark surfaces (such as forest) than over brighter surfaces (such as cerrado). We think that this is one of major points in our manuscript.

(2) p. 14839 first two lines -can you give per cent change of air column water vapour content ?

Yes, you are right. The 0.35 cm corresponds to about 10% of the total column water vapour content. This sentence was added to the final manuscript.

(3) line 3 - remove word 'impact'

This suggestion was accepted and incorporated to the revised manuscript.

(4) line 3/4 what do you mean by 'shortwave radiative effect' - do you mean 'shortwave radiation forcing' ? can you formulate more precisely ?

Yes. The phrase was modified in the revised manuscript.

(5) line 19 'controlling atmospheric composition' - in what sense ? can you be more precise?

The Amazonian wet season is dominated by low concentration of biogenic aerosols (about 300 particles /cm^3) . During the dry season, forest fires emit high concentration of gasses and biomass burning aerosols (up to 30,000 particles / cm^3). Therefore the atmospheric composition is modified due to the difference in the concentration and optical properties of aerosols and trace gases. The sentence was reformulated in the final manuscript to be easier to read.

(6) line 25 approximately 741365 km2 - seems a very precise number for being approximate ? - give standard errors ?

This sentence was reformulated in the revised manuscript. The actual number is provided by INPE remote sensing measurements, and in the revised version, the number was rounded.

(7) p. 14840 line 8 reformulate as follows: '...with consequences for the column water vapour content'

This suggestion was accepted and incorporated to the revised manuscript.

(8) Figure 2 - maybe split into three subfigures - for every site one subplot - it may be easier to 'read'

We have accepted the referee's suggestion. In the final manuscript the figure is separated into three, one for each site for easier reading.

(9) line 17 'This indicates that there was a shift in biomass burning activities, initially ...' what proofs that ? do you have more clean evidence ? otherwise formulate more cautiously

There are a few references that shows the shift in biomass burning activities. The following reference "J E Ten Hoeve et al., 2012, Recent shift from forest to savanna burning in the Amazon Basin observed by satellite , Environ. Res. Lett. 7 024020 doi:10.1088/1748-9326/7/2/024020" was included in the final manuscript.

(10) p. 14841 second paragraph - it would be good if you said what the role of AERONET data will be

In this work, AERONET data was used only to define the aerosol models used as input in SBDART simulations. A new phrase was added in section 2 (pg. 14841) to make the role of AERONET in this work clear. Additionally the role of AERONET was explained in more detail in section 2.5.2.

(11) The last sentence seems unnecessary / repetitive - instead the reader would probably expect a section discussing implications of the findings (as already mentioned in my general comments)

We have included discussions about the implication of the findings throughout the introduction and conclusion.

(12) p. 14843 line 17 '....cerrado covered areas' - based on what data / climatology ?

This sentence was removed from the revised manuscript.

(13) p. 14849 line 6 replace 'less' by 'least'

This suggestion was accepted and incorporated to the revised manuscript.

(14) lines 5 to 9 Impressive ! was 2008 particularly anomalous though ?

Amazonia suffered strong droughts in 2005 and 2010. We can't think of any reason to consider 2008 anomalous. Nevertheless, the year 2005 was anomalously dry contributing to the high AOD and therefore to the high SWARF values.

(15) line 16 replace 'intense' by 'strong'

This sentence was removed from the revised manuscript due to referee #1 comments.

(16) p. 14850 line 18 '...cerrado'. Can you explain better why this is - waht exactly is going on ?

Please see the comment in (1).

(17) p. 14851 first three lines - again why ? please explain in more detail

This sentence was reformulated in the final manuscript.

(18) p. 14852 line 20 replace 'considering' by 'assuming' line 22 - express in %

This suggestion was accepted and incorporated to the revised manuscript.

(19) p. 14853 line 12 define 'aerosol forcing efficiency' - e.g. what are the units ?

A definition of aerosol forcing efficiency was included in section 3.2 of the manuscript.

(20) line 18 to 22 - what do these changes mean ? interpret ?

This phrase is important to emphasize that forcing efficiency varies with wavelength. This is especially important when we compare forcing efficiency from different studies. In this part of the text, we were only trying to emphasize the need of comparing forcing efficiency values at the same wavelengths.

(21) p. 14854 line 26 - maybe replace 'understanding' by 'quantifying'

We agree. We have done more than "understanding". This work quantifies the direct aerosol forcing and the impact of surface albedo change due to the Amazonia deforestation. Nevertheless, the impact of aerosol is much broader, and aspects such as aerosol-cloud interactions are yet not well quantified and understood by the scientific community. We changed the phrase to make it clear the quantitative aspect of our manuscript.

(22) p. 14855 line 5 to 7 say this earlier on (see comment to p. 14841)

The text was modified and the role of AERONET is explained earlier in the revised manuscript.

(23) line 14 -15 again why ?

We changed the revised version to explain earlier the role of surface properties.

(24) line 21 (ii) explain a bit more

We changed the revised version to explain in more details the stronger forcing efficiency for forest versus cerrado regions.

(25) p. 14856 line 10-12 too simplistic - see also general comments

We changed the last paragraph to account more properly our main conclusions and future work.