Interactive comment on "A novel methodology using MODIS and CERES for assessing the daily radiative forcing of smoke aerosols in large scale over the Amazonia"

by E. T. Sena and P. Artaxo submitted to Atmos. Chem. Phys.

Answers to Anonymous Referee #2:

We would like to thank referee #2 for the very careful review. We had really appreciated the several suggestions and comments that helped us improving the final manuscript. In bold type, the issues raised by the reviewer, followed for our detailed action in each issue raised.

The manuscript introduced a novel method for defining smoke aerosol radiative forcing over Amazonia using satellite data. The key improvement in this method is that the aerosol forcing can be defined on a daily basis. In addition, the satellite-based results compare well with the AERONET inversions, which is promising. I recommend the publication of this article after some revision of the current version of the manuscript. One of the major issues relates to the terminology and the exact definitions of the key parameters (e.g. aerosol forcing, clear vs. aerosol-free environment). The authors should be more clear in which kind of satellite-based studies their method could improve the temporal resolution, and also discuss a bit about the limitations of this method. From this current version of the manuscript the reader might get the impression that this specific method could be used to improve all the previous studies where coincident CERES and MODIS satellite observations were used to estimate the direct aerosol radiative effect (forcing), which, to my understanding, is not true.

General comments:

1) The abstract is too long and in some parts too detailed. The authors should rewrite the abstract in a more concise way, focusing on the key findings.

We fully agree with the reviewer comments. The abstract was rewritten in a more concise way, as suggested.

2) The authors should define more clearly already in the introduction what they mean by DARF, i.e. that it considers the direct aerosol radiative forcing of smoke aerosols only. Also, they should be more specific when discussing about ”previous studies” that also used coincident CERES and MODIS observations to define the direct aerosol radiative forcing (/effect), especially whether those studies considered the radiative forcing of all aerosols or only of some specific aerosol type. The difference is that in this kind of specific forcing study both polluted (smoke) and background (clean) (AOD<0.1) SW TOA fluxes can be observed. On the other hand, when considering the total aerosol forcing, the aerosol-free flux (i.e. AOD=0) can not be observed. Therefore, at least in some of the ”previous studies” referred in the current manuscript, coincident AOD- TOA flux observations over longer time period (months) were needed in order to get an estimate for the mean aerosol-free flux, which also set the boudaries to the temporal resolution in which the total aerosol radiative forcing could be defined. I.e. if understood correctly, your method can be used to define F_clean – F_pollution at high temporal resolution but not F_aerosol-free – F_all aerosol. For example in Sect. 3.3 authors could emphasize already earlier in the section that in the ”previous studies”, i.e. Patadia 2008 and Sena et al. 2013, the aim for using coincident AOD-TOA flux satellite observations was to find the mean TOA flux for aerosol-free conditions (AOD=0) but in this study the ”clean” environment is defined as AOD<0.1. I.e the ”previous studies” actually defined the total aerosol forcing. In the case of Amazonia the total aerosol forcing is most probably nearly the same as

smoke aerosol forcing, but generalizations to other kind of environments do not necessarily work similar way.

We've really appreciated the referee's suggestions on how to improve the introduction and clarify the differences between the methodology introduced in this work and previous studies. Both, the introduction and section 3.3, were modified in the revised manuscript in order to explain the differences between the total DARF and smoke DARF. However, as aerosol-free conditions (AOD=0) don't exist, considering a more realistic clean condition (that is, defining Fclean in the presence of background aerosols) could be regarded as an improvement over previous methodologies. Since background aerosols are always present in the atmosphere, the contribution of background aerosols to the radiative balance should not be considered as forcing in the strict sense. In fact, as pointed out above by the referee, some authors define the contribution of background + polluted aerosols as the direct radiative effect instead of direct radiative forcing (eg., Yu et al., 2006).

3) The water vapour content variation had been taken into account in the radiative transfer simulations but how large effect these variations could have when defining the instantaneous satellite-based forcing?

That is an interesting and relevant question. CERES does not take into account water vapor variations to define its angular distribution models (ADMs), used to convert radiances to flux. Since CERES ADMs are defined based on monthly averages, there could be a bias at the flux at the TOA when water vapor is higher or lower than the monthly average. However, since the forcing is the subtraction of fluxes in background and polluted conditions, we expect those biases in water vapor to play a smaller role in the DARF. To accurately answer this question, it would be necessary to define empirical ADMs from CERES radiance measurements, using a similar approach to the one used by Patadia et al., 2011 paper to account for aerosol anisotropy. Instead of taking aerosol variations into account, as Patadia et al., 2011 did, one could study how water vapor content modifies radiances and build flux retrievals from the new empirical ADMs. Although this could be studied in the future, we believe this approach is beyond the scope of this work.

4) In Sect. 3.3. the discussion about Figure 5 could be more concise, the different explanations could be e.g. listed and then discussed in more detail.

We thank the referee for this suggestion. The discussion about Figure 5 was reformulated, as well as section 3.3, in order to make the whole section more clear and concise.

5) Since this method defines only cloud-free smoke aerosol forcing, the authors could give a rough estimate of how large proportion of all the possible satellite overpasses are cloud-free (and cloudy) during the forest fire season over Amazonia.

Thank you for raising this important point. We've included an estimation of the percentage of cloud-free and cloudy areas over the Amazonia during the studied period, based on MODIS Level 3 cloud fraction retrievals. This product indicates that during the studied period (August to September) the cloud fraction over Amazonia is on average about 47%, during Terra morning passage (about 10:30 AM LT), increasing to about 56%, during the afternoon (Aqua passage time is about 1:30 PM LT).

Specific comments:

Sect. 4: ”Validation of aerosol forcing” (also later in the Section); I would suggest to use ”comparison” instead of ”validation” since both the AERONET inversions and the model simulations are not ”direct” measurements.

Yes, this suggestion was implemented. We have used the word "comparison" instead of "validation" throughout the text, including Section 4 title.

Figures:

Figure 1 and 4 captions: Which is the time period when these observations were collected?

In Figure 1, 4 months worth of data, from July to October, 2005, over the grid cell were used in this example. This information was included in Figure 1 caption. Figure 4 was removed from the manuscript following reviewer's #1 suggestion, but 2 months of data were used in this figure, from August to September of 2005.

Figure 6: From which data are these lines defined? (Many grid cells or one grid cell, one year or multiple years...)

This figure is just a schematic illustration of the possible issues caused by adjusting linear fits between CERES flux at the TOA and MODIS AOD from collection 4 and collection 5, used in previous studies to calculate the DARF. It is meant to explain why using MODIS collection 4 AOD could overestimate the DARF, using a previous methodology. No real data was used in this figure. We've added this information in the figure caption, in the revised manuscript, in order to make this point clear.