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Interactive comment on "Modelling the radiative effects of smoke aerosols on carbon fluxes in Amazon" *by* Demerval S. Moreira et al.

Anonymous Referee #1

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The paper describes an interesting modelling exercise and seems to be sufficiently backed up by measurements to warrant the output to be within reasonable limits. The results demonstrate an interesting and to some degree surprising effect of the high aerosol load over the Amazon basin. The topic is obviously suitable for publication in ACP and in my opinion has the potential to attract an interested readership.

Unfortunately, the wording is often quite particular (shouldn't the title read '...in the Amazon region' or similar), despite the English language being overall comprehensible. Examples of such a particular wording which provides wrong spelling, twisted logic as well as unusual usage of words are P5L9 "...mixing ratios are diagnosed from the prognostic variables using the saturation mixing ratio with respect to liquid water", P12L6 "...fire emissions were not expected to contribute only minorly to CO2 mixing ratios...", or P14L22 "... a high GPP for C4 plants, but not high enough to compromise

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Discussion paper

their photosynthesis process". A considerable language editing should therefore be carried out. This can be accompanied with extensive shortenings, particularly towards the end of the manuscript (i.e. P16-20).

Furthermore, the paper needs more emphasize on the biosphere model. The reader only learns that the JULES model has been used and that it had been evaluated for sites in the Amazon before. What has not been explained in the methodology section is how the model considers direct and diffuse radiation for photosynthesis or how this response depends on plant functional type. It is also important to know how radiation and temperature changes influence simulated respiration (calculating a fixed or variable fraction of photosynthesis being lost as 'growth respiration', exponential temperature dependence on maintenance respiration, allocation shifts regarding exudation or fine root turnover changes the effect decomposition,. . .?). The depicted model properties (simplifications) should be used in the discussion to point out the appropriateness of the processes or the need for improvements.

One of the reasons why the sensitivity of the model is important is that the importance of the direct and indirect aerosol effects might actually been less important than it looks like. I refer to chapter 3.2 where it is mentioned that the direct aerosol effect (by shading) reaches -100 Wm-2 (Tapajos 80-123 Wm-2), which comes along with a certain amount of cooling. This corresponds to about 460 umol m-2 s-1 global radiation or roughly speaking 230 umol m-2 s- PAR reduction. On the other hand, Fig. 12 shows that the increase of diffuse PAR due to the indirect aerosol effect is from app. 250 to 800 = 550 umol m2 s-1. If direct and diffuse radiation are similarly effective in the model (please explore), the aerosol effect by shading should thus be about half the magnitude of the increase in diffuse radiation. Since it seems to be smaller, the cooling effect (part of direct aerosol effect) seems to compensate for the greater part of the shading. In my opinion this should be discussed in greater detail, using the sensitivity of the model against temperature changes for argumentation.

Some more specific notes:

P7L4: I don't understand what is meant by 'spin up artifacts'. Usually spin-ups are used to avoid artifacts originating from uncertain initial conditions.

P7L7ff: From Fig. 12 it is apparent that diffuse PAR is about 250 umol m2-s-1 under conditions of AOD = 0 (clear sky conditions). I guess that this is about 5 percent of the total radiation even if AOD is actually 0. It seems likely that some clouds are even increasing this fraction. On the other hand the DIR-AER scenario seems to exclude this part of the radiation, which causes a bias that underestimates radiation and thus photosynthesis. Can you comment on this?

P8L6ff: Equations 5 and 6 seem superfluous to me. A short description in the text should suffice.

P8L26/28: Why are there two different algorithm numbers (3B42 and as 3B43)?

P10L22: What is meant by 'several precipitation systems'?

P10L27ff: The description of the soil moisture is a bit confusing. I would like to know how the soil is considered and initialized in the model (soil depth, number of layers, stratification of potential water content).

P11L10: The number of fire needs a reference. It seems to be considerable higher what is given in Chen et al. 2013 (Biogeosciences, Vol. 118, P495ff).

P11L19ff: I don't see any connection between the CO concentration and the biosphere model (but I may be wrong), which would mean that the DIR-AER and DIR+DIF scenarios should result in very similar concentration distributions. Is this correct? The simulation of CO concentrations seems to serve primarily for showing that the physical processes involved are correctly represented in the atmospheric model. This should be highlighted.

P12L8ff: I think it should be clearly articulated that the model fails to represent the CO2 concentrations. Model results are clearly not 'in an acceptable range' for most of the sites and periods. The reasons seem not to be clear but I am sure that some of the most likely ones can be depicted instead of blaming a 'complex myriad of physical processes'. I would differentiate between uncertainties in transport and biosphere exchange processes. If the authors render air chemical reactions as important despite the relative small reactivity, they might include them too. It should be noted, however, that blaming biosphere process uncertainties (including uncertainties in soil drought determination) means to question GPP and NEE results. Overall, I would suggest clearly arguing that the model is not sensitive to the CO2 concentration within the given range (app. 385-395) and that therefore the model problems should not have a major effect on final results.

P15L14: Here it is firstly indicated that the investigated year might not be representative for the general conditions ('a relatively drier and smokier year'). This should be discussed further. To which degree differs the year from others? Which effect might this have on the overall results?

P15L21-24: I have the impression that for this analysis, it is decisive to evaluate the difference of respiration (and other fluxes) between scenarios. The variability in soil conditions that is certainly influencing the absolute magnitude seems to be less important.

P18L15ff: I recommend refraining from an additional summary like it is done with the 'Final remarks' section and instead create a 'conclusions' section that points out what has been learned from the analysis and what should be considered in future research.

Figure 7: Note that observations are generally depicted on the x-axis while simulation results are shown on the y-axis.

Figure 9: I am missing the effect of the scenarios on total/direct radiation.

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