Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2016-1147-AC1, 2017

Atmospheric Chemistry and Physics
Discussions

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

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Received and published: 1 July 2017

We thank the referee for his(er) insightful and very helpful comments, which contributed to improve the paper. The answers to his(er) questions and comments are below:

Legend: RC: Referee's Comment
AR: Author's response
AC: Author's changes in manuscript

Specific comments

1. **RC: p 4, l 15: not clear what you mean by the two-way mode coupling, could you please describe in more detail how this coupling is implemented and how it works**

   AR: The coupling is considered two-way in the sense that, for each model time step, the atmospheric component provides to JULES the current near-surface wind speed, air temperature, pressure, condensed water and downward radiation fluxes, as well as water vapor and trace gas (e.g carbon dioxide and monoxide, methane, and volatile organic compounds) mixing ratios. After its processing, JULES advances its state variables over the time step and feeds back the atmospheric component with the sensible and latent heat and momentum surface fluxes, upward shortwave and longwave radiation fluxes, and a set of trace gas fluxes . Further details on the JULES x BRAMS coupling is described in Moreira et al., 2013, nevertheless we included a sentence about it in the current manuscript.

   AC: ...has been coupled in a two-way mode with the Joint UK Land Environment Simulator v3.0 (JULES), the land surface scheme of the UK Hadley Centre Earth System model, as described in Moreira et al. (2013). The coupling is two-way in the sense that, for each model time step, the atmospheric component provides to JULES the current near-surface wind speed, air temperature, pressure, condensed water and downward radiation fluxes, as well as water vapor and carbon dioxide mixing ratios. After its processing, JULES advances its state variables over the time step and feeds back the atmospheric component with sensible and latent heat and momentum surface fluxes, upward shortwave and longwave radiation fluxes, and a set of trace gas fluxes.

2. **RC: p 5, l 1-3: did I understand correctly that all other aerosol emissions, except biomass burning, were ignored in the model? If this is the case,**

I would like the authors to add a few words here on why this assumption is needed from a technical point of view and what inaccuracies is likely to introduce (e.g. neglecting masking effects and interactions from other aerosol types etc.). I think that rather than doing a “no aerosol" vs. “biomass burning only" comparison, it would be preferable to do a “all aerosol" vs. “no biomass burning" comparison.

AR: Correct, the model was run with only biomass burning aerosols. We do agree that it would be ideal to do “all aerosol” vs. “no biomass burning”. However, natural sources of aerosols (biogenic and soil dust) are not yet functional in the model and we don’t have a good inventory for urban emission sources for the Amazon region and northern of Brazil. Because of this issue, the urban emission was turned off. Nevertheless, regarding the aerosol loading across the Amazon basin and neighborhood, in the absence of biomass burning, AOD in the visible spectrum hardly overcomes 0.15, which would translate in a very low radiative impact compared with that observed under massives AOD values (very often above 1.0) that occur during biomass burning influence. Therefore, from the point of view of the radiative effect impacts, we would not expect to see substantial changes in the current results doing “all aerosol” vs “no biomass burning”, but, yes, it would be preferable and more consistent.

3. **RC: p 5, l 28: please explain in a bit more detail how the cloud filtering was done. Also need to say how ignoring the effect from clouds is likely to affect results presented in this study, preferably also attempting the quantify this.**

   AR: The cloud filter was used only in the interpretation of the model results, i.e. we considered only the model gridboxes where the total column integrated condensed water was equal zero.

   AC: However, as discussed below, the analysis presented here focuses only on areas, and during hours, without cloud cover, i.e. the results were obtained by filtering out the points with cloudiness, considering only the model gridboxes where

the total column integrated condensed water was equal to zero. The aim of this work is only to compute the aerosol effect; therefore this filter was essentially used to exclude the effects of the clouds in the CO_2 fluxes.

4. **RC: p 7, l 8-9: I struggle to understand why you did not run all 3 simulations for the whole 2-year period (as you did for the DIR+DIF experiment) and I would strongly recommend to do so. The ability to make annual estimations would substantially increase the significance of the paper.**

   AR: As the paper focus is on the evaluation of the biomass burning aerosols radiative effect impacts, which are only relevant during the dry season, we decided to focus on the biomass burning period. We agree that run all the experiment for the whole 2 year would add value to the paper. However, that would be interesting if we were able to evaluate accurately the clouds impact. The model current version does not have a robust and well tested parameterization to compute the diffuse radiation from clouds. As the reviewer may know, in the wet season the atmosphere in the Amazon region is dominated by clouds and it is very clean, as biomass burning aerosols or from any other source are almost absent. Therefore, since we are not able to consider the cloud effect with the current model version, the results of the three simulations would be very similar for the wet season. Therefore, not justifying to run the three for a full year . We are currently working on the inclusion of the diffuse radiation effect from clouds in our model, and we plan to extend the analysis of present study for a broader period in the near future.

5. **RC: p 10, l 11-12: Fig 3b shows in fact that the model values are outside the standard deviation range of the observed temperatures for all night and late afternoon hours**

   AR: That is correct, the model is slightly outside the standard deviation of the observation during the night and the afternoon. We have made the correction in the article.

AC: Changed "... was typically cooler (∼2.5 C) during the night period but still within the standard deviation of the mean temperature observed." to "... was typically cooler (∼2.5 C) during the night period and late afternoon hours, and was not far from the standard deviation of the observed mean temperature"

6. **RC: p 11, l 31: here and in other parts throughout the paper where you compare modelled vs. observed values, please quantify these comparisons by giving some relevant stats (e.g. mean bias, correlation etc.)**

   AR: We included, in Figure 7, the parameters of a linear fitting to the scatter plot and the respective R-Squared (see Figure below).

   AC: "Model results tend to underestimate CO and CO_2 observations, especially at low levels, in locations mainly affected by fire emissions both locally (*Alta Floresta*, *Rio Branco* and *Santarém*) and by long range transport (*Tabatinga*). The black line on each scatter plot in Figure 7 shows the linear fit and the correspondent R-Squared values. The largest CO underestimation occurred in *Alta Floresta*, with a slope of coefficient equal 0.58, but the highest dispersion occurred in *Santarém*, with $R^2 = 0.58$."

7. **RC: p 12, l 20: can you add a reference to these sensitivity studies?**

   AR: We included in the supplementary document figures showing the JULES sensitivity to some variables. Please, see Figures S.2, S.3 and S.4 below.

   AC: We performed sensitivity tests to assess JULES response to several atmospheric variables. We ran JULES offline (version 3.0) for September 2010 using as input BRAMS results for the NO-AER experiment considering the nearest gridbox to the Tower km-67. Figure S.1 (in the supplementary material) shows monthly variation of shortwave radiation (Rshort), longwave radiation (Rlong), air temperature near surface, specific humidity near surface, all used as input for

the sensitivity test. The soil carbon in this gridcell is 10 $\mathrm{kgCm^{-2}}$ and constant during all the month. Besides the BRAMS model results for each parameter, we also varied each parameter, reducing and increasing its original value to cover the standard deviation of the monthly mean. In addition, we varied the diffuse fraction of the shortwave radiation, which was originally zero (NO-AER scenery), from 0 to 0.8 of the total radiation (Rshort). Therefore, we ran 567 simulations for the month of September 2010. For each simulation, we calculated the monthly mean fluxes. Figures S.2, S.3 and S.4 show the results for these sensitivity tests. JULES results for soil respiration, and consequently NEE, are quite sensitive to the prescribed soil carbon content (Figure S.2). In addition, the GPP increases with the increase of soil moisture for all biomes (Figure S.3). However, R_H and R_P also increases with the soil moisture (Figure S.3a and S.3m). Therefore, for the forest and *cerrado* biomes, the NEE decreases until a certain value, after then increases again with the increasing of soil moisture (Figure S.3s). In summary, the sensitivity analyses show that i) for a 7% decrease in shortwave radiation there are minimal changes in GPP (Figure S.4a); ii) a change in temperature of one degree Celsius (from current midday conditions) also did not imply in major changes in the simulated GPP (Figure S.4b); and iii) a 40% increase in the diffuse fraction of shortwave radiation increased the GPP by 39% , 71%, 4%, and 72% in forest, C3, C4 grasses, and *cerrado* (shrubs) vegetation , respectively (Fig S.4c).

8. **RC: p 12, l 31,34: why are these results not shown as they seem to be important here?**

   AR: A figure describing the results was included in the supplementary document (see Figure S.5 below). Now we are mentioning this within the text:

   AC: Changed "A scatter plot of AOD values from the model and MODIS retrieval (not shown here) had a slope of 0.71 (with $R^2 = 0.73$)" to "The scatter plot of AOD values from the model and from MODIS retrievals (Figure S.5 in supplementary material) presents a slope of 0.71 (with $R^2 = 0.73$) "

9. **RC: p 13, l 18: "CO_2 mixing ratio peaking about 1 hour later" – this is actually not apparent from Fig 9b**

   AR: You are right, the CO_2 peak for the three simulations occurred at 10 UTC. We removed this phrase.

10. **RC: p 14, l 18-20: here you should discuss in more detail what these columns b-c actually show and what it means. Also, can you evaluate the results presented in Fig 13 against some observed values?**

    AR: We included a more detailed description about columns b and c. In addition, we showed the evaluation of model results against observed values in table 3 and Figure 9a. And, as described in the text in lines 23-26 on page 4, the evaluation of simulated diffuse radiation effects on GPP using JULES under primarily high or primarily low diffuse radiation conditions has been done for two flux sites in the Amazon rainforest (*Tapajós* and French Guyana) by Rap et al. (2015) and for temperate ecosystems in Mercado et al. (2009).

    AC: In Column b of the Figure 13 we show the difference between monthly mean GPP as simulated for the DIR+DIF and NO-AER experiments, i.e. the relative impact of the total effect of aerosols on simulated GPP for the 4 studied biome types: forest (b.1), C3G (b.2), C4G (b.3) and *cerrado*(b.4). In column c, we show the difference between monthly mean GPP of the simulation without the aerosol effect on the diffuse radiation (DIR-AER) and the simulation without any aerosol effects (NO-AER), i.e. we evaluate the relative impact on the direct solar radiation effect.

11. **RC: p 15, l 4: here and throughout the manuscript, please revise the way you calculated all percentage changes. If GPP increased by 293 Tg C month-1, from 913 to 1206, this means an increase of 32%, not 24%.**

    AR: You are right, there were some miscalculation of the percentages, which has been corrected.

    AC: p15, l 4: "...an increase of about 32% of the GPP..."

    AC: p15, l 7: "... about 20% and 30%, respectively."

    AC: p19, l 19-20: "...increase the GPP of about 32%, 30%, 9% and 20% for forest, C3, and C4 type grasses, and *cerrado*, respectively."

12. **RC: p 15, l 9-10: I don't quite understand how you derived the 13% increase in NPP. If A=B-C and B increases by 22% and C increases by 9%, this does not imply that A increases by 13%. Please clarify.**

    AR: You are right, the NPP estimate was wrong, we made the correction.

    AC: P15L7-10 ... about 20% and 30%, respectively. We estimated an average increase of 27% in GPP for the month of September 2010 in the LBAR region, associated to the aerosol effect in Amazonia (Table 4). However, Rap et al. (2015), using JULES model forced with aerosol field from another model, estimated an average increase in GPP of only 2.8% for August, considering the period of 1998-2007. Also, our estimative of net primary production ($NPP = GPP - R_P$) for the simulation DIR+DIF was 553 $\mathrm{TgC/month}$ (1,113 - 560) and for the simulation NO-AER was 363 $\mathrm{TgC/month}$ ((1,113-240)-(560-50)). Therefore, we estimated an increase of 52% in NPP for September 2010, due to the presence of biomass burning aerosol in LBAR region, while Rap et al. (2015) estimated an increase in NPP of only 5.4% in August. Our results for the aerosol impact over the Amazonia is higher than the Rap et al. (2015) estimation. However, one must keep in mind that Rap et al. estimation was based on 9 years (1998 – 2007) and for a month (August) that typically has much lower aerosol loading than September, while our work was based on September, the peak for the biomass burning season, and for 2010, a drier and smokier year.

13. **RC: p 15, l 10-12: I don't think you can make such extrapolations (one peak season month is by no means representative of the entire season). Also, here you say that the biomass burning season lasts for 3 months (and thus divide by a factor of 4), while later (p 20, l 6) you say that it lasts 4 months (and use that for other estimates). These comparisons really needs to be addressed properly and in addition to correcting the current mistakes, it is very apparent that the paper would benefit a lot from performing annual simulations for all 3 experiments.**

    AR: Yes, in the beginning we estimated the duration of the fire season as 4 months. However, after analyzing the monthly fire count data in Brazil from 1999 to 2016 we realized that 3 months was a better estimation (Figure S.7a). So, we decided to maintain the extrapolation with 3 months and kept it consistent all over the text. However, it is important to point out that 2010 is the second top year in terms of fire detection, only surpassed by 2004, and it is out of the standard deviation of the mean from 1999-2015 (Figure S.7b). So, the extrapolation is valid for the years when the fire activity is most intense but it is not representative of an average year.

    We are currently working on a longer term model simulation that will allow us to explore the individual and combined aerosol and cloud effects during a full seasonal cycle results.

14. **RC: p 19, l 28-30: can you include a direct comparison of your model results with these Yamasoe et al. conclusions, i.e. do you also see the same behaviour for these AOD intervals?**

    AR: We removed the phrase in the final version of the manuscript. Although the figure below illustrates that our modeling results are similar to Yamasoe et al. (2006) observations for low AOD values, in 2010 maximum AOD values were lower than in 2002 making it difficult to extrapolate the model results for the higher AOD interval. Moreover, observational results present higher variability and can

be attributed to the difficulty in controlling all the variables affecting the estimation, such as wind speed, air temperature and humidity, soil moisture, and in removing respiration from the CO_2 fluxes measurements to estimate NEE.

AC: Our model results indicated that the impact of the aerosol on the NEE changes is mainly related to the aerosol increasing the diffuse fraction of radiation, as suggested by Yamasoe et al. (2006).

15. **RC: p 39, Fig 9a: should explain in the text why is the effect (difference between the red and pink line) stronger during the night?**

    AR: This is a really interesting observation. These observations belong to the fetch of a flux tower (20Km x 20km) located near the Tapajos River, with 43% Forest cover, 24% water and 32% C3G coverage. A possible explanation may be related to neighboring influences. Observe in Figure S.6 (below) that the mean wind at 00 UTC is from East (forest region) bringing air mass that has carbon fluxes affected by aerosol during the previous daytime. Meaning that this grid point receives air mass from a region where the effect of the aerosol was more pronounced, leading to a greater difference between the simulation without aerosol and with aerosol.

    AC: A curious fact is that at night the difference between NO-AER and DIR+DIF is greater than in the daytime period. One possible explanation for this is the influence of the neighborhood. Note in Figure S.6 of the supplementary material that the average wind at 00 UTC is from East, a forest region, and has differences between aerosol and non-aerosol simulations. However, the wind at 10 UTC is coming from NorthEast, crossing the river, where the influence of the aerosol in the carbon fluxes is low.

16. **RC: In addition, I think the readability of the paper could be substantially improved by getting editing help from someone with full professional proficiency in English.**

AR: The document has been reviewed by a co-author with English as his first language.

Technical corrections

1. **RC: title: I suggest a slight change of title, replacing “Amazon" with “Amazonia" or “the Amazon region"**

   AR: We agree.

   AC: We changed the title to: "Modelling the radiative effects of biomass burning aerosols on carbon fluxes in the Amazon region"

2. **RC: use the present tense in the abstract when presenting your results, e.g. “our results indicate: : :" etc**

   AR: Thank you.

3. **RC: p 2, l 4: “to be a sink" → “to being a sink"**

   AR: Thank you.

4. **RC: p 2, l 8: “*cerrado*" → “*cerrado* areas"**

   AR: Thank you.

5. **RC: p 2, l 14: “areas of about several" → “areas of several"**

   AR: Thank you.

6. **RC: p 2, l 14: “out of the biomass burning season" → “outside the biomass burning season"**

   AR: Thank you.

7. **RC: p 2, l 18: “Angstrom exponent” → “The Angstrom exponent”**

   AR: Thank you.

8. **RC: p 2, l 23-26: please rephrase, possibly removing the first phrase which is unnecessary**

   AR: It was rephrased.

9. **RC: p 3, l 6: “deplete" → “achieve"**

   AR: Thank you.

10. **RC: p 3, l 10: not clear what you mean by “net radiation"; do you mean “total radiation"?**

    AR: You are right, the more adequate term is "total radiation". This has now been changed.

11. **RC: p 5, l 23: define D (from eq 1) somewhere in the text**

    AR: This is now defined.

    AC: "D represents the diffuse fraction and the values of the fitting parameters a, b, c, and d, of..."

12. **RC: p 7, l 21: “:" → “." (or rephrase using small letter after the colon, as it implies that a list of things is following)**

    AR: Changed “:” to “.”

13. **RC: p 8, l 6-7: not clear if you want ratios or percentages here**

    AR: The equations 5 and 6 were removed.

14. **RC: p 8, l 21 & p11, l 22: ’observation’ → ’observations’**

    AR: Thank you.

15. **RC: p 9, l 35: best to use consistently throughout the manuscript either "biomass burning" or "smoke"**

    AR: "smoke" was changed to "biomass burning" in all over the text.

16. **RC: p 10, l 12: please rephrase "diurnal cycle early in 1-hour"**

    AR: It was rephrased.

    AC: "In addition, the model temperature has a diurnal cycle with a gap of one hour more early than the observation."

17. **RC: p 13, l 27: "oC" → degree C**

    AR: Thank you.

18. **RC: p 14, l 1: here and throughout the manuscript, PAR already includes "radiation", so no need to say "PAR radiation"**

    AR: We removed the word "radiation". Thank you.

19. **RC: p 14, l 35: "GPP jumps" – please rephrase, e.g. "GPP increases"**

    AR: We replaced "jumps" by "increases". Thank you.

20. **RC: p 15, l 1: "Table 2 resumes" → "Table 2 summarises"**

    AR: We replaced "resumes" by "summarizes". Thank you.

21. **RC: p 16, l 24: "punctual" → "point"**

    AR: We replaced "punctual" by "point". Thank you.

22. **RC: p 17, l 1: do you mean "lower fraction of diffuse radiation"?**

    AR: We changed "fraction" for "amount "

    AC: "higher amount of the diffuse radiation implies higher GPP."

23. **RC: p 17, l 18: "weighting" → "weighted"**

    AR: Thank you.

24. **RC: p 17, l 25-28: not clear what you mean here, please rephrase**

    AR: This has been modified

    AC: The original sentence: 'Nevertheless, it is interesting to notice that the relative contribution of the diffuse to the total (diffuse + direct) aerosol effect on the NEE (Equation 4) has a quite distinct behavior depending on the biome type and exponentially decay, or increase, with the AOD increase for all biomes, and C4 grass type, respectively.'

    has been now replaced with

    ' Nevertheless, it is interesting to note that the impact of the aerosol influence on the relative contribution of the diffuse to the total (diffuse + direct) on the NEE (Equation 4) has a different behavior depending on plant functional type, decaying exponentially as the AOD increases for all biomes, except for the C4 grass type.

25. **RC: p 18, l 2: please replace "it is fair to say" with a more scientific wording**

    AR: We replaced. Thank you.

    AC: "it is reasonable to say that the contribution..."

26. **RC: p 18, l 12-14: not sure what you mean here, please rephrase this last sentence of the paragraph**

    AR: We rephrased it.

    AC: The sentence: 'The difference between modeling and observational estimation for NEE is likely to be within the yearly, and spatial variability of forest ecosystem physiology, which also includes disturbed areas and secondary forest.' has been changed to P16L26 and replaced with 'Table 3 shows that the

NEE observed during the dry season at the Amazon forest and pasture biomes exhibit substantial site to site and interannual variability. Nevertheless, for each site, the 2010 model results are within the observed variability.'

27. **RC: p 19, l 19: remove "all" from "all together"**

    AR: This has been modified. Thank you.

28. **RC: p 27, Table 1 caption: "three-degree" → "third-degree"**

    AR: This has been changed. Thank you.

29. **RC: p 33, Fig 3b caption: please clarify what standard deviations are shown for the black and red lines (e.g. what values were used to derive them)**

    AR: The standard deviations were calculated using the mean of the 72 stations showed in Figure 3a with site locations represented in white asterisks.

    AC: The original sentence in Figure 3 caption :'The standard deviation of mean temperature from observation and from the model results are indicated by shaded gray and red bars, respectively.' has been replaced with:

    'The standard deviation (shaded gray) and the mean observed temperature values were calculated using measurements at the 72 observational stations. While the model standard deviation (red bars) and mean temperature were calculated using the model temperatures at the gridboxes corresponding to the locations of the 72 stations.

30. **RC: p 34, Fig 4: since this shows precipitation, I suggest to reverse the colour scale (as you did for Fig 5)**

    AR: This has been changed (see Figure 4 below)

31. **RC: p 36, Fig 6 caption: please clarify what exactly you mean by "fire product"**

    AR: We changed the phrase.

    AC: Changed: "Fire product derived from AVHRR measurements during September 2010"

    To: "Burning points observed by the AVHRR sensor during September 2010."

32. **RC: p 37, Fig 7: please include some stats (here or in the text) for the scatter plots on the right. Also, the standard deviations are missing from the top scatter plots. I would also suggest to use a better colour scale for altitude to help visualising the results (at the moment the purple and pink are too similar – a more intuitive transition from low to high altitudes is preferable)**

    AR: We changed (see Figure 7 below). The standard deviations that are not appearing is due to the fact that they are very small.

33. **RC: p 39, Fig 9: please use different colors for the model results (the light pink is almost invisible). Since you use UTC, it might help to show with dashed vertical lines where the local sunrise/sunset times are on the X-axis.**

    AR: We used a darker color to represent the "NO-AER", so it should be visible now. We also included a bar with short wave radiation to indicate sunset and sunrise. (see Figure 9 below). Additionally, we included the bar with short wave radiation also in Figure 3b (see below).

34. **RC: p 40, Fig 10: why not showing the effect of the best simulation (DIR+DIF)?**

    AR: The legend is wrong, this figure is really of DIR+DIF. Thank you.

35. **RC: p 44, Fig 14: please add a legend**

    AR: The legend was included (see Figure 14 below).

36. **RC: p 45, Fig 15: please clarify what values are shown here (spatially and temporally)**

    AR: It is now indicated in the legend that the value is spatial over LBAR and temporal on September 2010.

    AC: ...during September 2010 (temporal) in the LBAR (spatial).

37. **RC: p 47, Fig 17: please add a legend**

    AR: The legend is now included (see Figure 17 below).

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2016-1147/acp-2016-1147-AC1-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2016-1147, 2017.

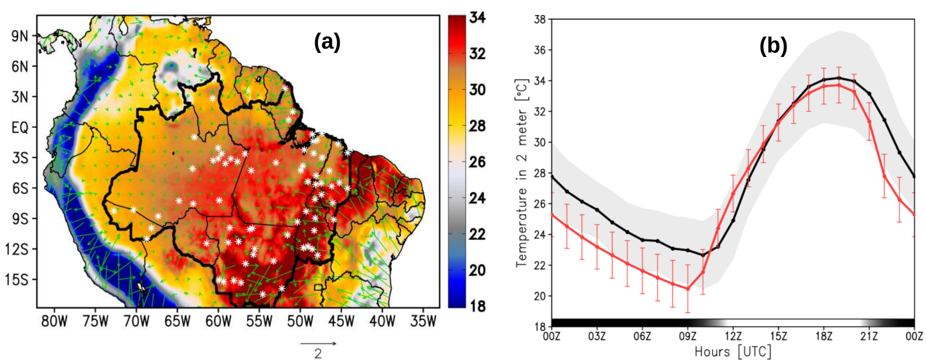

Fig. 1. Figure 3.

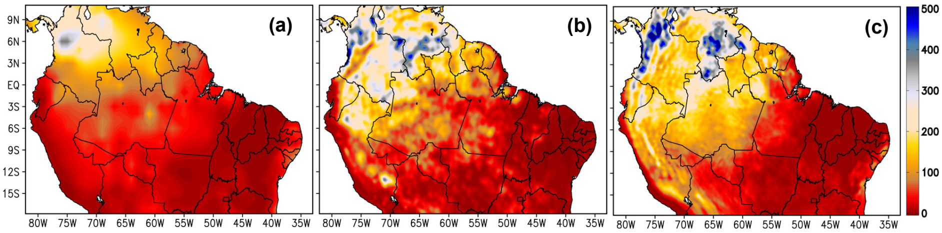

Fig. 2. Figure 4.

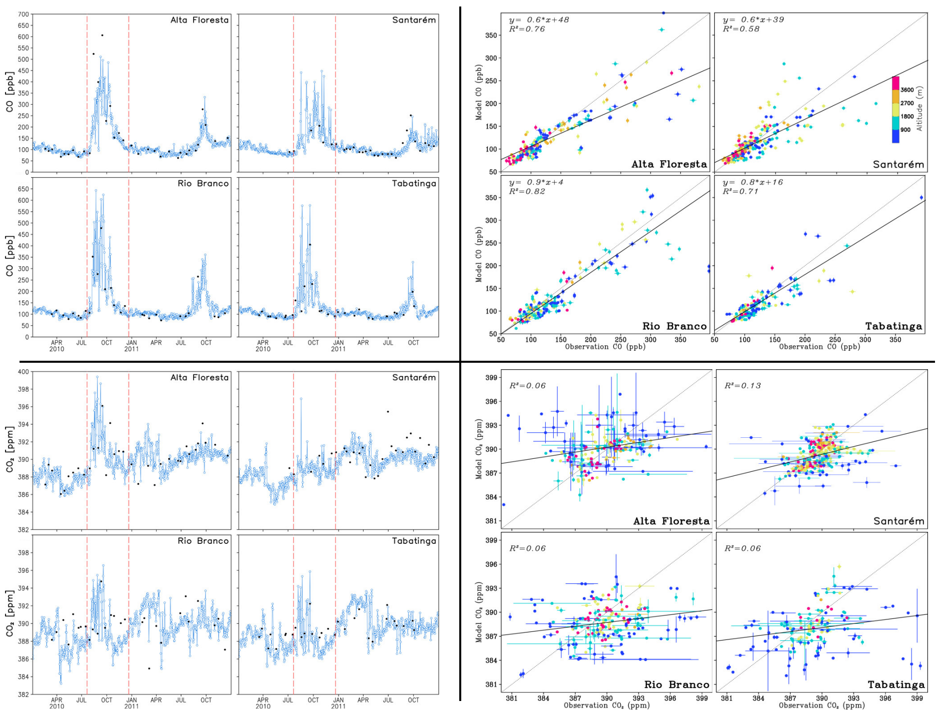

Fig. 3. Figure 7.

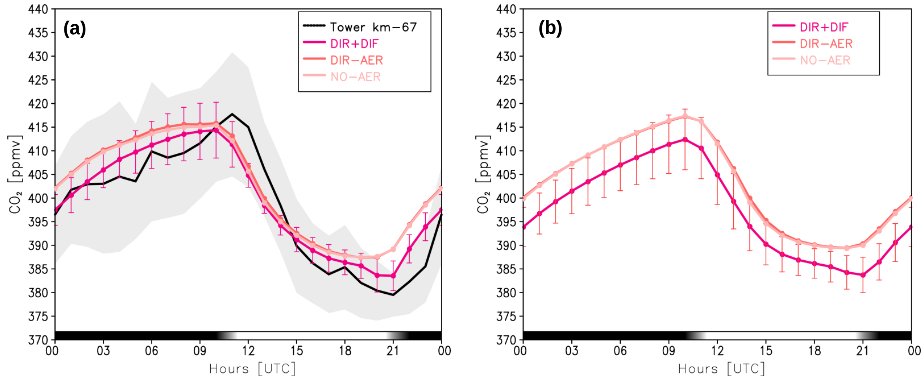

Fig. 4. Figure 9.

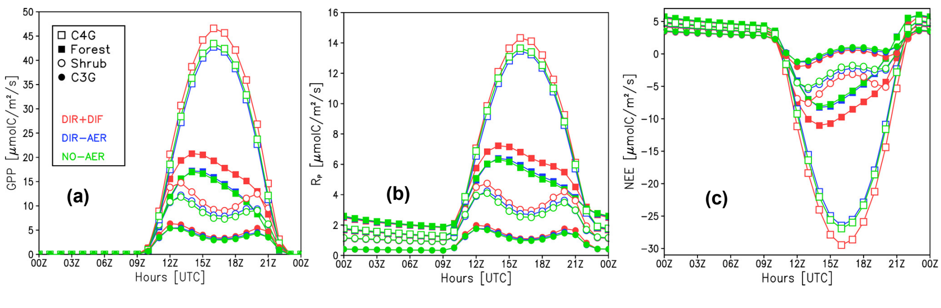

Fig. 5. Figure 14.

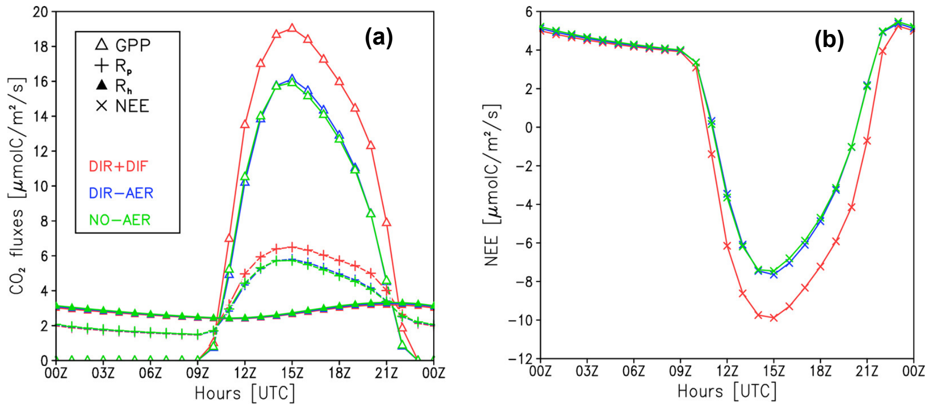

Fig. 6. Figure 17.