

Reply to Reviewer # 2:

We would like to thank the reviewers for their efforts and for their important comments that helped us present a clearer and more complete paper. We have addressed all of the reviewers' comments and we are confident that with the additional changes the paper is clearer.

Our answers to the comments will be presented point by point (first answering the general comments marked by GC# and answer by GA#, and then specific comments marked by SC#: and answer by SA#.)

GC1: To derive a more robust relationship between EVI and pFCu, some additional analyses could be done to minimize contamination of meteorology and AOD. As Fig 4(b) shows, RH at 850 mb has large spatial (in both zonal and meridional directions) variations in the study region. So readers would like to see maps showing distributions of EVI and land cover type (forest, non-forest, and water). Does EVI have any correlation with the RH? Similarly, is EVI correlated with AOD?

GA1: As advised by the reviewer and suggested by other reviewers as well, we added a figure of landcover type and EVI for 2011 to the text (Fig. R1 below). It is clear from that figure that forest EVI is much higher than non-forest EVI and that non-forest EVI is much more dependent on meteorological gradients. Moreover, we checked and found that EVI is not correlated with AOD, for both forest/non-forest landcovers. In Fig R2, the dependencies of forest/non-forest EVI on the selected meteorological variables are seen. For forest landcover, EVI is relatively constant for all RH and HGT values and hence (as a first approximation) can be taken as an inherent forest property. For non-forest, EVI is tightly linked to changes in meteorology, it increases with RH and HGT until a threshold RH=70, HGT=3155, where the dependencies shift sign and decrease. Therefore, EVI vs. non-forest pFCu results must be taken cautiously, as added to the paper discussion: "**The chance of observing FCu fields over non-forest landcover increases (decreases) for values lower (higher) than EVI=0.48, and is generally lower than over forest landcover. However, the scattered spatial distribution of non-forest landcover (see Fig. 4a) and the strong correlation between non-forest EVI and meteorology cast doubt on the significance of this finding**".

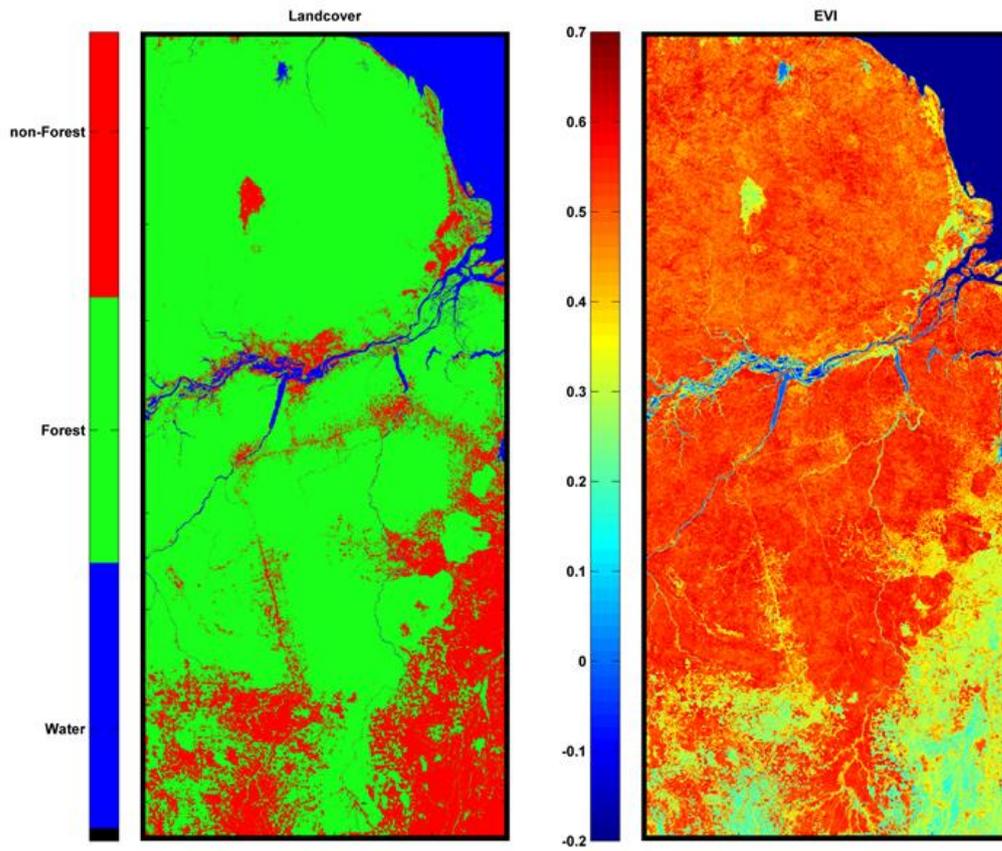


Figure R1. Landcover classification (left) and mean EVI (right) for J-A-S months, 2011.

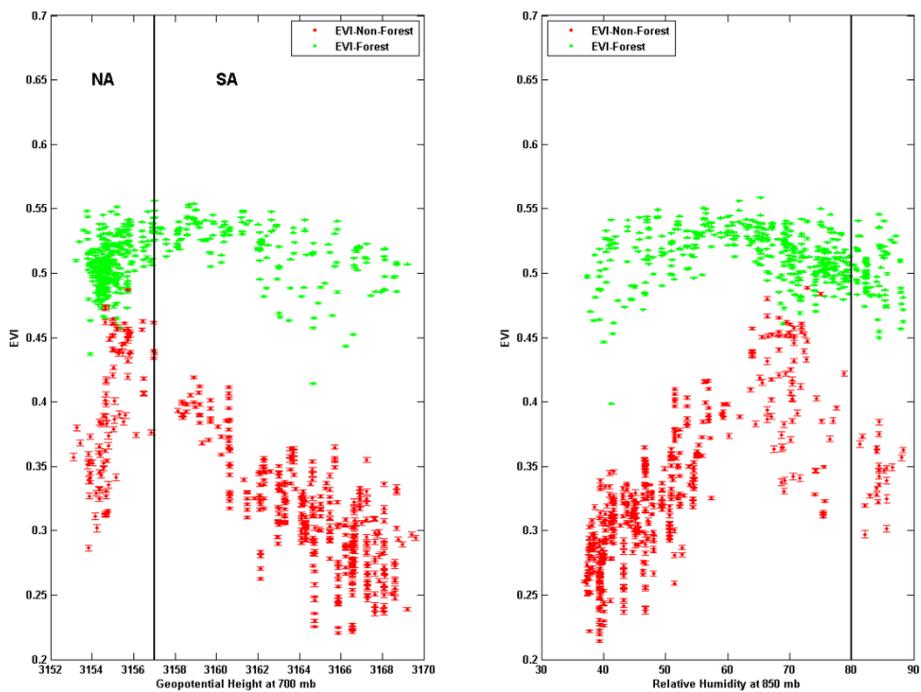


Figure R2. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively "immune" to meteorological changes, as opposed to non-forest EVI which is much more sensitive.

SC1: p.30014, 1.16-18: as written, the ITCZ is replaced by SASH during the dry season. It is better to rephrase it like “the ITCZ (need to spell it out) moves northward: “.. Large scale subsidence associated with SASH dominates the region..”

SA1: Thank you for the suggestion, the paragraph was rephrased to: "**During the Amazon dry season (austral winter months, June-September), the Inter-Tropical Convergence Zone (ITCZ) moves northward (reaches ~10°N at mid August), while large scale subsidence associated with the South Atlantic Subtropical High (SASH) dominates the region (Nobre et al., 1998) and relatively stable meteorological conditions prevail.**"

SC2: p.30014, 1.24: which year?

SA2: The year "2011" was added to the text.

SC3: p.30015, 1.1-2: should “parallel to” be “perpendicular to”?

SA3: Thanks for the comment, we did check the wind vector (at 900 ,850 hPa) for several cases and the tendency is for cloud formation parallel to the wind direction rather than perpendicular. However, this statement hasn't been sufficiently tested, therefore we decided to omit the last part of the sentence (i.e. "**parallel to the wind direction**").

SC4: p.30016, 1st & 2nd paragraph: how do you interpret “densely forested areas”, “deforested areas”, and “pasture”? Readers can get confused about the preference of shallow Cu over which land type.

SA4: To avoid confusion, "**savanna and pasture**" was replaced with "**deforested**".

SC5: p.30016, 1.21: I believe smoke interaction with LW radiation is very weak.

SA5: Thank you, sentence rephrased to: "(i.e. scatter and absorb shortwave radiation)"

SC6: p.30017, l.5: what do you mean by "raw" data? AOD is not "raw".

SA6: The word "raw" has been omitted from the text.

SC7: p.30017, l.6, change "land cover information" to "land cover type".

SA7: Suggested change was carried out.

SC8: p.30017, 17-18: change to "Analyses show that the FCu fields had no clear correlation with topography".

SA8: Thank you, the suggested change was carried out.

SC9: p.30017, l. 21-22: this sentence needs to be rephrased.

SA9: To clarify out statistical approach, the whole relevant paragraph was rephrased as follows: "**Our analyses of the FCu cloud field properties were focused on the statistical properties of the cloud distribution within the field. Measures like cloud area, average distances between cloud centers and level of organization were tested to optimize the classification. Unlike the case of a single cloud analysis when the sensitivity to the exact cloudy pixel is crucial and one need either to avoid cloud contamination of the cloud-free atmosphere (Martins et al., 2002), or in the case of cloud retrievals to make sure that the cloud mask is free of non cloudy pixels (Ackerman et al., 1998), our spatial-statistical measures (summarized in Fig. 3) exhibit less sensitivity to the exact method by which clouds are masked in the field**".

SC10: p.30018, l.11: "where" should be "were".

SA10: The suggested change was carried out.

SC11: p.30018, l.27: “where” should be “were”.

SA11: The suggested change was carried out.

SC12: p.30019, l.1-6: UMD land cover has 14 types. As written, it seems that there are 17 types. To avoid the confusion, you may want to say specifically which types are classified as “forest” and which types as “non-forest”.

SA12: Thanks for the comment. We wrote out the classifications more explicitly in the text: **"For the purposes of this study, we divided the UMD landcover classification into three types: i) Forest, classes 1 through 5 (i.e. all forest types including mixed), ii) Non-Forest, classes 6 through 10 (wood-lands, grasses, shrub-lands), 12 (crop-lands), 13 (urban), and 16 (barren), and iii) Water, class 17."**

SC13: p.30019, l.7: NDVI, spell it out. In comparison to NDVI, is EVI less affected by aerosols?

SA13: Both NDVI and EVI were spelled out in the relevant paragraph. The following line and reference (in italic) were added to the text as well: **"Since NDVI tends to saturate in areas of high biomass (*Huete et al., 2002*), and is more sensitive to atmospheric aerosol contamination (*Xiao et al., 2003*), EVI is preferred in our study."**

SC14: p.30019, l.22-25: it is better to give some physical explanations why RH and HGT are major factors controlling the FCu fields.

SA14: As recommended, and in addition to the original text, we added the following sentence to the main text: **"These parameters can also be seen as physically tightly linked to FCu formation. High geopotential height at 700 hPa (pressure levels 850 hPa – 500 hPa give similar results) indicates upper level subsidence, adiabatic warming and drying, and is associated with the SASH (Figuroa and Nobre, 1990). Relative humidity at 850 hPa corresponds to the mean cloud base height (based on ceilometer measurements), and is essential to cumulus formation."**

SC15: p.30020, l.20-22: without land cover map, it is hard to see.

SA15: Land cover map for 2011 was added to the main text for the readers' convenience (Fig. R1 above). Nevertheless, the claim that: ".pFCu dependence on meteorology is similar for both forest and non-forest landcover types" is based on the results seen in Figs. 4c,d in the text. We therefore rephrased the sentence to: "**As seen in Fig. 4c,d, the large scale pFCu dependence on the two meteorological parameters is similar for both forest and non-forest landcover types.**"

SC16: p.30021, l.24: delete 1st "the".

SA16: Thanks, deleted.

SC17: p.30021, l.27-28: better to specifically say why higher AOD caused lower pFCu based on previous studies (cloud burning as found in Koren et al., 2004?).

Given that 2010 is a drought year, would the combustion be more likely of a flaming phase and more absorbing (lower single-scattering albedo), as discussed in Yu et al. (Remote Sensing of Environment, 111, 435-449, 2007)?

SA17: Thanks for the comment. We added the following sentence to the text: "**These results are consistent with previous findings in the Amazon (Koren et al., 2004; Davidi et al., 2009). Shortwave radiation absorbed by biomass burning aerosols heats the mid-atmospheric levels, which results in stabilization of the atmospheric profile and reduction in cloud cover**". Considering the flaming phase of aerosols, although the theory is viable, it would be hard to prove. We looked at single scattering albedo (SSA) data from the Alta Floresta Aeronet station (located at the southwest corner of the study region), and found no significant differences between 2010 and 2011, with 2011 actually showing slightly lower (by ~ 0.01) SSA values. Therefore, we decided not to add any additional comments to the main text.

SC18: p.30022, l.1-5: can you cite previous studies that show the smoke invigoration effect in the region?

SA18: We could cite previous studies of that sort, however, since the statistical significance of the invigoration between 2010 and 2011 is questionable (as Reviewer

#1 pointed out), we decided to omit those sentences (and l. 23-25 p. 30024 in the discussion as well) from the text.

SC19: p.30022, l.14: it is hard to say based on Fig.7a that there is “an additional increase at the high end values of $EVI > 0.585$ ”.

SA19: We agree with the comment, the pFCu increase in Fig. 7a is very minor for the highest EVI values. The sentence was rephrased by omitting the "additional increase..." part.

SC20: p.30024, l.12-13: ‘higher order effect’ vs ‘lower order effects’. It seems that this sentence is contradictory to what in abstract (where meteorology and biomass burning are designated as “higher order effects”).

SA20: Given a second thought, the use of 'higher order effects' and 'lower order effects' is confusing. Instead, we replaced these with 'first order..' and 'second order..'. The abstract was corrected to: "**Although weaker than first order effects of meteorology and biomass burning,...**". And the sentence in the main text was changed to: "**Much of the analyses were concentrated on decoupling first order effects (such as meteorology and biomass burning) from the more subtle second order effect of landcover EVI**".

SC21: p.30025, l.3: “linearly”. Not exactly.

SA21: Thanks, we agree with the comment. The conclusion was modified to: "**The chance of observing FCu fields over forest landcover increases with EVI (excluding low EVI values for 2009), and can generally be represented by a linear fit**".

SC22: p.30025, l.5-6: “a negative parabolic dependence”, needs some explanation.

SA22: Reviewer #1 also commented on this issue. Therefore, we changed the sentence to: "**The chance of observing FCu fields over non-forest landcover**

increases (decreases) for values lower (higher) than EVI=0.48, and is generally lower than over forest landcover".

SC23: p.30033, last line: delete "on a given day".

SA23: Thank you, the suggested correction was performed.

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