Dear Editor,

We would first of all like to thank the reviewers for their efforts and for their important comments that helped us present a clearer and more complete paper. We tried to our best efforts to address all of the reviewers' comments while maintaining a concise and coherent story to the paper.
Three main issues were raised by the reviewers:

1. The need for additional analysis of other years except 2011, with the option of cutting the domain to smaller subsets.
2. The choice of meteorological parameter used to "decouple" meteorology effects from EVI effects.
3. A request for a more elaborate description of the physical and dynamical processes at play linking surface EVI with cloud field statistics.

We addressed the first issue by adding the results of analyses for the years 2008-2010 to the paper, showing similar results to 2011 and strengthening the main conclusions. Further division of the domain to smaller subsets would increase the noise and greatly weaken the signal (see for example GA1, reply to reviewer #1). Regarding the second main issue, we acknowledge that full meteorological decoupling can never be achieved for such scale, and hence omit the word "decouple" from the text. Additionally, we give physical basis for the two specific meteorological parameters chosen in the text, and show that for forest landcover, EVI is not correlated with meteorology and hence EVI effects are unlikely to be meteorology related (see GA1, reply to reviewer #2). The third main issue raises important questions regarding the actual physical mechanism at play creating Forest Cumulus (FCu) fields. However (as explained in reply to reviewer #3, GA1, GA3), we feel that an analysis of such sort is out of the scope of this paper and requires a whole study by itself, using numerical modeling methods. Nevertheless, a short discussion of the possible physical mechanisms and future work possibilities is added to the discussion section.

We hope you we find the revised manuscript suitable for publication in ACP.

Sincerely,

Reuven H. Heiblum

Reply to Reviewer # 1:

Our answers to the comments will be presented point by point (first answering the general comments marked by GC# and answer by GA#:

GC1: This is an interesting paper… .I suggest the inclusion of multiple years and subdivision of the analyzed domain to demonstrate the reproducibility of the results
GA1: Thank you for the comment. We agree that including more years would strengthen the paper. Therefore, using the same methodology, we added the EVI vs. pFCu analysis for the years 2008-2010 in addition to 2011, showing consistency for all years except 2009 (possible reasons for the weaker EVI dependence during 2009 are discussed in the new manuscript). Moreover, statistical parameters of the linear trends (slope, correlation coefficient) were added to the text (in Table 3) for indication of statistical significance of all trends. The results are shown in figure 9 in the revised main text, and are shown here below as well.

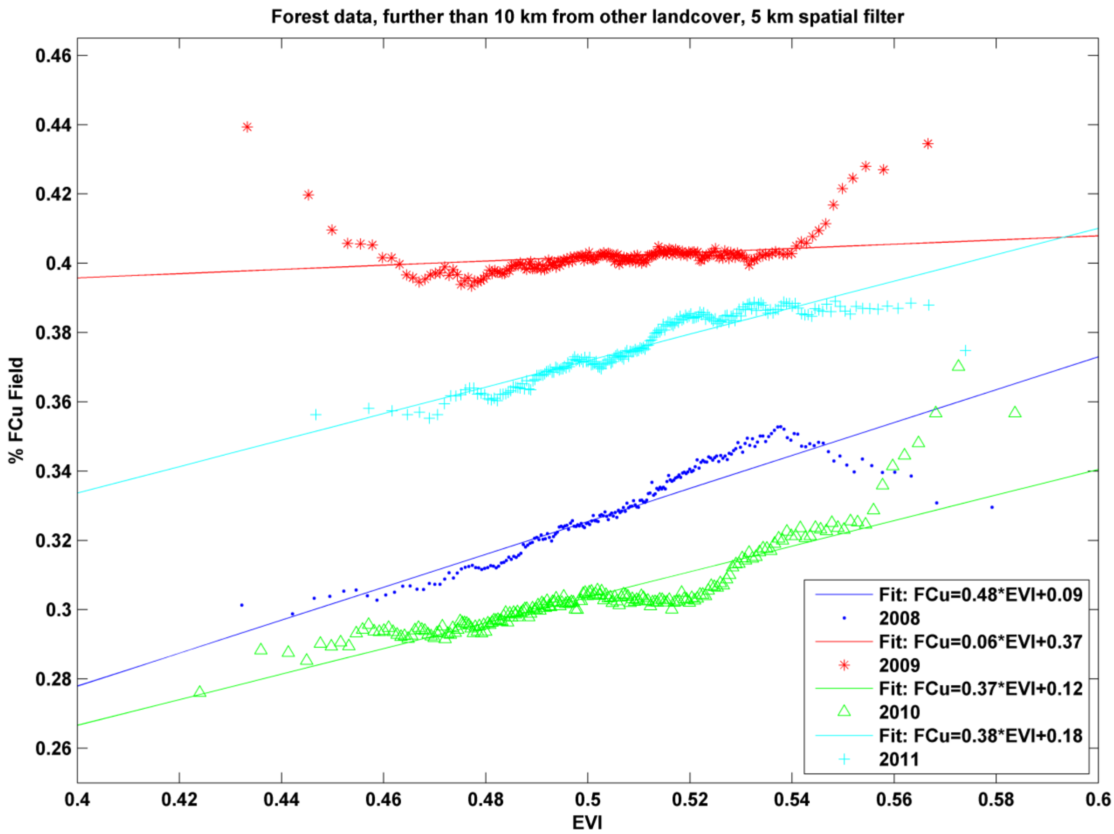

Figure R1. J-A-S pFCu as a function of EVI, for years 2008-2011 (see legend), above forest landcover. Data is confined to the NA region (defined for each year separately), with RH<threshold, further than 10 km from other landcover types. A 5 km disk shaped spatial filter was applied to the EVI data. Linear fits for all cases added in figure legend.

Concerning the subdivision of the analyzed domain to further sub-domains, the main EVI vs. pFCu analysis is already performed on a heavily cropped domain; i.e. Only northern Amazon region (NA in Fig. 4a), only regions with RH<80%, and only forest data farther than 10km away from water or non-forest landcover (see Fig. 6 in the new manuscript). Since we are looking at cloud field properties, there is a natural scale below which we cannot consider changes in cloud field properties. Our cloud fields are defined using a 25 km moving window, hence we need regions on the order of 100s of km to get significant trends, which is about the order of our total cropped region. Therefore, as expected, we find that smaller scales do not produce consistent trends and are much noisier. Only when large enough areas are chosen does the positive EVI vs. pFCu dependence appear. This shows that the large scale EVI variance has a much stronger effect than the small scale variance. The low-pass filter analysis used in Fig. 7d shows exactly that, as we get a stronger signal when smoothing EVI data over larger areas.

GC2: In the bigger picture I'm left wondering what implications this work might have on cloudiness, precipitation, and climate given the clear trend towards ever expanding deforestation in the Amazon. This is beyond the scope of the work but some comment in the conclusions would add relevance to the paper.

GA2: We thank the reviewer for this important comment. As a first attempt in this direction, our main focus was on a specific subset of clouds in the amazon (FCu), which are likely to be less frequent with increasing deforestation. But it is still a challenge to predict which cloud types and by how much are we to expect over deforested land in areas of meteorological conditions favorable for FCu formation, since both an increase or decrease in total cloud cover can occur. As advised, we added a short comment on this issue in the paper discussion: **"As for climatic trends**

in Amazon cloud fields, the effect of large scale biomass burning is more straightforward, with high aerosol loading tending to suppress cloud formation. It is hard to conclude how largescale deforestation would affect total cloud cover since meteorological and landcover gradients roughly coincide in our study region. We can predict a reduction in dry season FCu fields as forest landcover undergoes transition to non-forest or as forest wellbeing decreases (reduction in EVI), however more extensive studies are needed to understand the total effect on the radiation budget and water cycle in the Amazon due to such changes".

SC1: line 10, pg 30018: 'Five basic characteristics where shown to contain most of the information: cloud fraction, mean and standard deviation of distances between cloud centroids, and mean and standard deviation of cloud areas.' This seems unsurprising but the authors should demonstrate that this is indeed the case. For example how do they quantify information? Why don't they think that other properties like cloud water path (or reflectance) contain useful information? This seems rather arbitrary as it stands.

SA1: From the outset, the purpose of this work is to explore a connection between forest characteristics (EVI) and the cloud fields above using only *morphological characteristics* to define the fields. Of course others methods of cloud field classification could be used which incorporate spectral information (visible or IR) as well, however, we found that the spatial characteristics of the FCu fields are consistent and sufficient for classification based on subjectively testing 100s of cloud fields that appeared to be FCu-like.

At first we considered using additional morphological characteristics such as cloud perimeter, number of clouds within the moving window, entropy, autocorrelation, etc., but these all turned out to be mostly redundant to the five basic characteristics, hardly changing the classification results. We do acknowledge that using more parameters (such as suggested by the reviewer) could potentially narrow the classification, but we feel that adding such complexity would not add further value to the paper.

SC2: Lines 14-22, pg 30018: I suggest a table demonstrating the statistics for the three subjectively defined regimes and their frequency of occurrence.

SA2: Thank you for the helpful suggestion. A table demonstrating the mean cloud field statistics for years 2008-2011 (instead of Lines 14-22, pg 30018) was added to the text and shown here:

"Table 2. Average statistics for the cloud fields (and their spatial parameters) as defined in Table 1, years 2008-2011. Missing data represents irrelevant statistics (e.g. distance between clouds is meaningless for sparse and deep convective fields since we commonly observe only one cloud within the 25 km moving window)."

Parameter → **Field Type↓**	**Year**	**CF [%]**	$\bar{A}$ **[km²]**	σ_A **[km²]**	$\bar{D}$ **[km]**	σ_D **[km]**
Forest Cumulus (FCu)	2008	0.23±0.02	2.93±0.37	5.79±1.08	2.21±0.12	0.91±0.07
	2009	0.23±0.02	3.00±0.31	5.64±0.95	2.26±0.10	0.90±0.06
	2010	0.24±0.02	3.03±0.41	5.80±1.13	2.25±0.13	0.88±0.07
	2011	0.24±0.02	3.13±0.37	5.65±1.00	2.31±0.13	0.87±0.07
Sparse	2008	0.03±0.02	1.31±0.43	1.33±0.43	-	-
	2009	0.04±0.01	1.24±0.38	1.24±0.42	-	-
	2010	0.03±0.02	1.55±0.62	1.54±0.63	-	-
	2011	0.03±0.02	1.39±0.47	1.33±0.50	-	-
Deep Convective	2008	0.83±0.06	144.0±58.2	-	-	-
	2009	0.82±0.06	139.7±56.7	-	-	-
	2010	0.83±0.06	143.5±56.4	-	-	-
	2011	0.83±0.06	143.9±59.8	-	-	-

The frequency of occurrence of each of the fields is shown in Fig. 5 of the main text for years 2010 and 2011. We added the years 2008 and 2009 in the revised manuscript for a more complete analysis.

SC3: Line 26, pg 30018: Again we need more information to see this for ourselves.
SA3: Using the information of the proposed Table 2 (above), one can see that the three cloud types converge around specific mean cloud field values with very little variance between the years. This strengthens the statement in the text that: **"The narrow distributions and interannual consistency of these key cloud properties allowed for a robust detection of the fields".**

SC4: Line 22, pg 30021: superfluous ’the’
SA4: Thank you, 'the' was omitted.

SC5: Line 2, pg 30022: ’possibly indicating invigoration of convective clouds by biomass burning aerosol’. Is the difference statistically significant?
SA5: Thanks for the comment. The reviewer is right and since invigoration is out of the scope of this paper we decided to omit those sentences (and l. 23-25 p. 30024 in the discussion as well) from the text.

SC6: Line 25, Pg 30021: Can the published Koren et al. model for cloud fraction versus AOD in this region explain the 2010-2011 differences?
SA6: Yes, we think that these results strengthen previous findings (Koren et al., 2004;Koren et al., 2008) that smoke inhibits clouds cloud formation via absorption of SW radiation and profile stability. We added the following line 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".**

SC7: Line 3, pg 30022: Yes there is lower AOD but that does not necessarily imply that the results from 2011 are less likely to be influenced by aerosol effects. In fact wouldn't you expect larger aerosol sensitivity at low AOD values than at high AOD values. You are essentially arguing that a partial derivative with respect to AOD is small using the magnitude of AOD. Why? Does the 2011 data not support your conclusions. If so then tell us. For that matter why not look at other years as well. Limiting this study to 2010 leaves me wondering how real the conclusions are.
SA7: As already mentioned in GA1, we accepted the reviewer's advice to include additional years for the main EVI vs. FCu analysis (figure 9 in new manuscript). We choose the year 2011 as a good representative year not just because the AOD was lower, but more so because the variance of AOD without the whole region is minor, hence limiting the possibility for spurious spatial correlations induced by AOD gradients rather than EVI gradients. It is true that the sensitivity of clouds to aerosols is more sensitive for lower AOD, but we were mainly looking to avoid the case of very high AOD (>0.5) due to absorbing aerosols that stabilize the atmosphere, as described in SA6.

SC8: Line 7, pg 30022: These are regional correlations of the seasonal mean maps, correct? More explicit explanation of the time and space scales would be appreciated.
SA8: The relevant paragraph was modified as follows: **"To minimize influences of AOD and meteorology on the data, we limit the current analysis of EVI effects on FCu fields to the NA region, excluding RH>80% areas, during 2011 (area enclosed by dashed black contour, Fig. 6), taken as a representative example. The J-A-S pFCu data (Fig. 3d) was sorted as a function of the mean J-A-S EVI data (Fig. 4b)…".**

SC9: Line 17, pg 30022: It seems silly to describe the data as parabolic (a very specific function) without testing the fit of a parabola or without the guidance of some physical model that would predict a parabolic dependence.
SA9: We agree with the comment, without physical basis it would be meaningless to suggest a parabola fit. Therefore, we changed the sentence to: "**For low EVI values (EVI < 0.48), there is a strong positive dependence (similar to that seen in forest**

landcover), but for higher values of EVI > 0.48 there is a clear decrease of pFCu with EVI". Moreover, the third conclusion in the discussion (p. 30025, l. 5-6) was changed 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. 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".**

SC10: Line 20, pg 30022: I disagree that the data can be considered decoupled from meteorology or AOD. In what sense do you mean this? meteorology and AOD certainly have regional variations which were not controlled for in any proper statistical or physical manner here. correlations with other variables within the study boundary with other variables (i.e. AOD, geopotential height, RH, etc...) should be shown.

SA10: Thank you for the comment. We acknowledge that full decoupling of the data from meteorology or AOD is an impossible task, since many meteorological parameters (which will always vary to some degree within our confined region of interest) can be chosen. Hence, we restated the whole sentence to: "**Until now, we have focused on limiting the effects of meteorological and aerosol variance on pFCu, but have yet to consider the effects of mesoscale circulations that may form at the boundaries and transition areas between landcover types**". Our specific choices of two meteorological varaibles (Geopotential Height (HGT) at 700 hPa and Relative Humidity (RH) at 850 hPa) are based on both the fact that they best describe the spatial variance of pFCu and the following physical reasoning (added to the 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 (Figueroa 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."**

In Fig. 4 of the main text one can see that we focus our analysis on the area where HGT<3157 and 60<RH<80, an area where there doesn't seem to be any correlation between these two meteorological variables and pFCu. Additionally, we checked the correlation between EVI and theses two meteorological variables as seen in Fig. R3 below. It can be seen that for Forest landcover, the meteorology hardly has any effect on EVI, and EVI is rather an inherent property of the forest. For non-forest landcover however, the meteorology is highly correlated with EVI. The EVI increases with RH and HGT until a threshold RH=70, HGT=3155, where the dependencies shift sign and decrease. This can possibly explain why for the non-forest landcover (Fig. 7a), we get a decrease in pFCu for high EVI values.

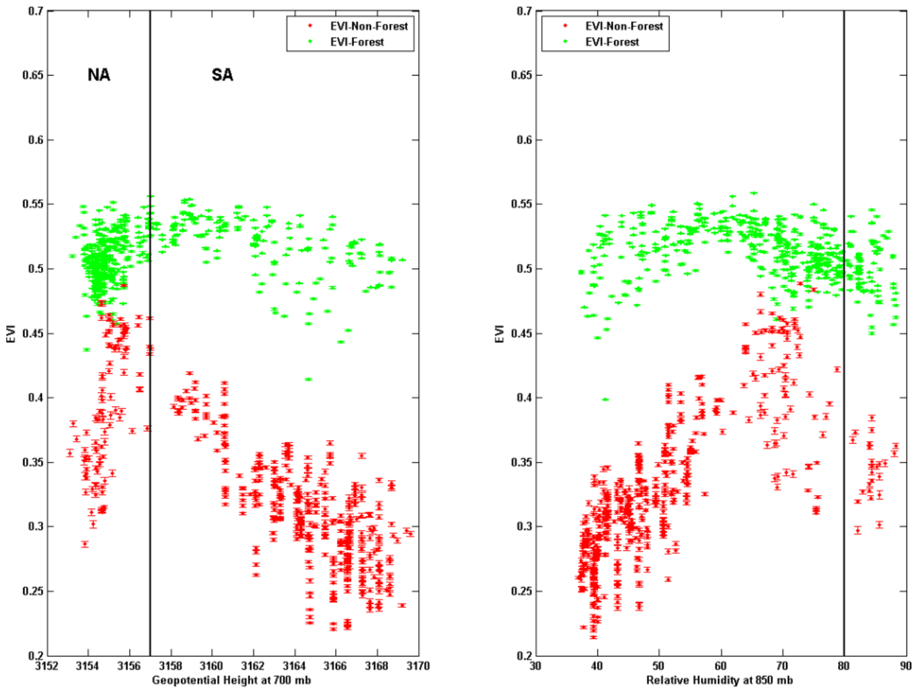

Figure R3. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively "immune" to meteorological changes, as opposed to non-forest EVI which is much more sensitive.

SC11: Line 21, pg 30023: ’To test the significance of the linear trend above’. You are not testing significance, which would involve the calculation of some statistical confidence interval. Instead you are demonstrating the scale dependence of the relationship. A better test of the significance might follow from sub-division of your domain into smaller domains or the additional examination of other years (see above comments). Are results reproducible from year to year and as the domain is chopped up?

SA11: Thank you for the comment. Indeed the word "significance" might be misplaced here. We switched the word to "**Robustness**" instead. See GA1 above.

References

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Koren, I., Kaufman, Y. J., Remer, L. A., and Martins, J. V.: Measurement of the effect of Amazon smoke on inhibition of cloud formation, Science, 303, 1342-1345, doi:10.1126/science.1089424, 2004.

Koren, I., Martins, J. V., Remer, L. A., and Afargan, H.: Smoke invigoration versus inhibition of clouds over the Amazon, Science, 321, 946-949, doi:10.1126/science.1159185, 2008.

Reply to Reviewer # 2:

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".**

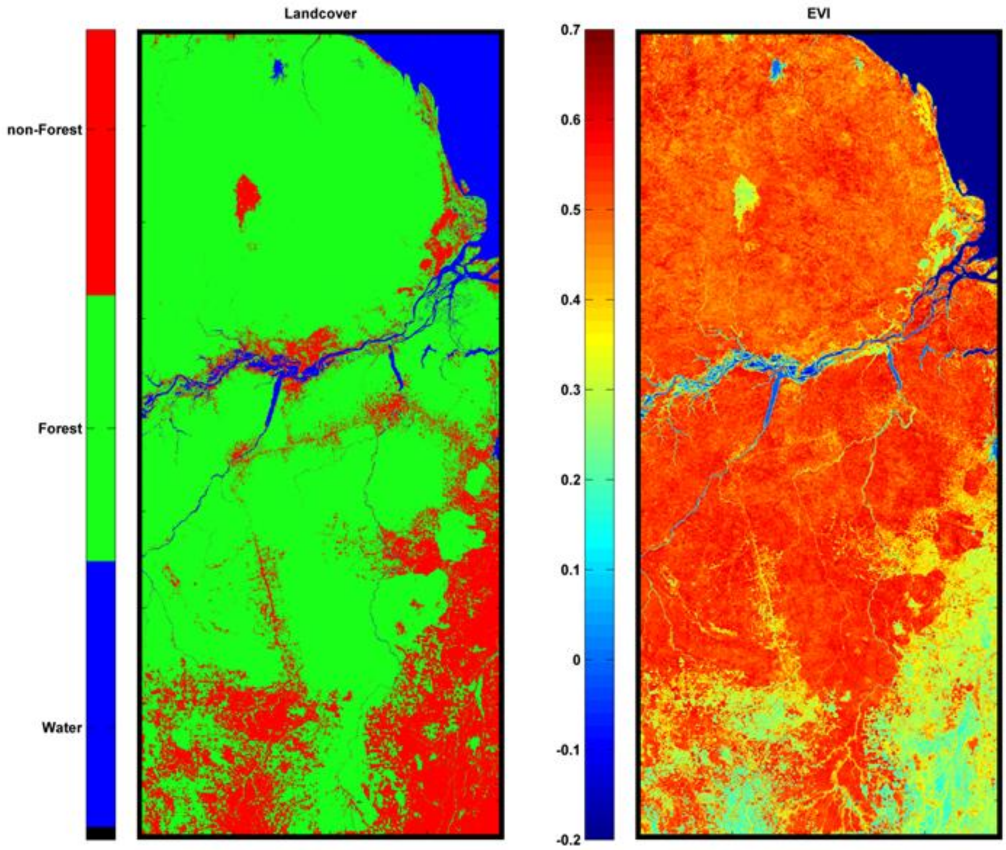

Figure R1. Landcover classification (left) and mean EVI (right) for J-A-S months, 2011.

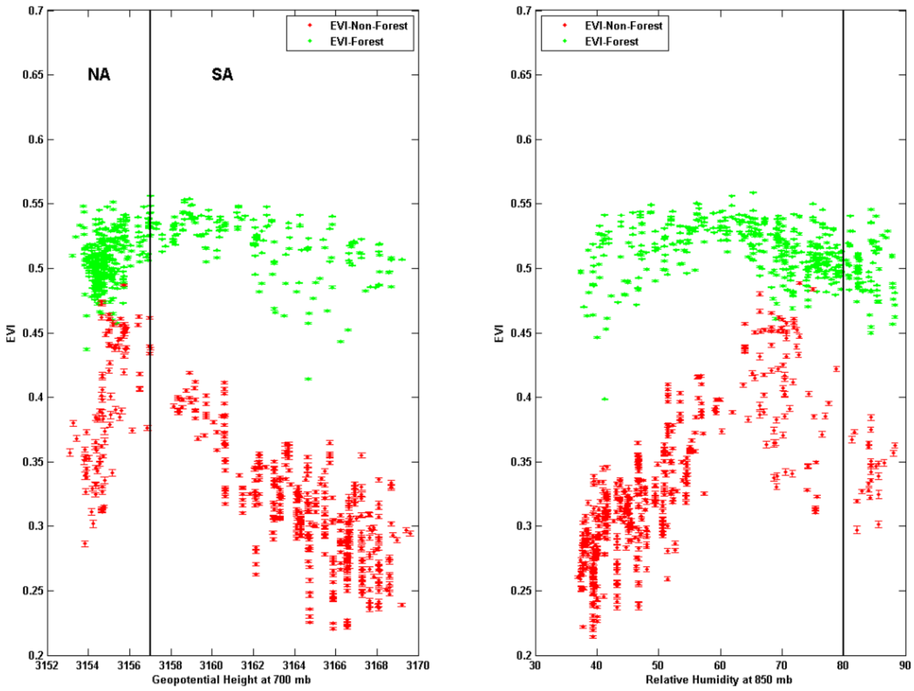

Figure R2. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively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

SC1: p.30014, l.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^{o}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, l.24: which year?

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

SC3: p.30015, l.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, l.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 (Figueroa 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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Reply to Reviewer # 3:

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: As it stands, the paper presents a correlation between two variables with little discussion of why we should expect them to be correlated or any supporting evidence to show that the physical connection is plausible. With some further attention to the actual physics at work relating surface fluxes to cloud formation, the paper may be suitable for publication.

GA1: We agree with the reviewer that this topic warrants in-depth analysis of the physical mechanisms at work when relating EVI with cumulus cloud organization. However, we feel that such an analysis is of a much larger scope than one paper, and this work should be considered as an initial step towards the goal of fully understanding the FCu clouds.

As a matter of fact, when looking at other common cloud fields (such as cloud streets, closed/open cells), the main physical mechanisms behind their formation is still widely debated today. We think that lots of potential lies in treating the FCu fields as a case of deviations from the dry convection case (often approximated as a version of Rayleigh-Benard convection) driven by the cloud and precipitation feedbacks on the system. Here as a first approximation we consider the EVI levels as a marker for appropriate fluxes for typical FCu organization. To fully understand the physical mechanism presented here, an appropriate cloud resolving model that manages to reproduce these cloud fields is essential. We are perusing this direction but getting the spatial and temporal organization similar to the observations is far from being a trivial task.

Nevertheless, we do agree with the reviewer that these ideas should be addressed (or at least introduced) in the paper, and therefore we added a paragraph to the introduction: **"… these changes influence the diurnal evolution of the atmospheric boundary layer (Betts, 2000). The latter study showed how vegetation resistance controls the boundary layer depth (with lowest resistances corresponding to the oceanic limit) and the partition between latent and sensible het fluxes. Hence, the**

evapotranspiration properties of the landcover vegetation are tightly linked to the dynamics of the boundary layer and the shallow Cu clouds which commonly cap the boundary layer", and discussion: **"elucidating the dynamical processes which are responsible for the formation of FCu field require future work. We can speculate that the FCu fields correspond to a specific solution of Rayleigh-Benard thermal convection over land (or specifically cloud streets, as discussed in section 1), since the basic physical settings are similar over the Amazon and ocean surfaces, namely: a homogeneous warm surface, and a moist boundary layer with a well defined inversion layer. Hexagonal open convection cells have already been simulated over tropical land in the western pacific (Saito et al., 2001). The fact that vegetation properties control to a large degree both surface fluxes and boundary layer depth (h), and that the Rayleigh number (R_a) is highly dependent on that depth (proportional to h^4), suggests a physical link between forest and the cloud fields formed above".**

GC2: The term “meteorology” is used vaguely throughout this paper, and frankly throughout most aerosol/cloud interactions literature. Presumably “decoupling meteorology from higher-order effects on clouds” (e.g. P.30024, l.12-13) means separating geopotential height and humidity from higher-order effects (presumably the EVI). But perhaps a more specific description of the analysis presented in fig. 4 and section 3.1 is an understanding of the regional distribution of shallow cumulus based on the regional distribution of humidity and geopotential height, rather than a full “decoupling”.

GA2: Thank you for the comment. The issue of meteorology "decoupling" is a main comment address by all the reviewers. We have omitted the word "**decoupling**" from the text since a full decoupling of meteorology is an impossible task, and replace it with "**distinguishing**". Moreover, we have put more emphasis on the reasoning for the meteorological parameters chosen, and show that for forest landcover (unlike non-forest landcover), EVI and the chosen parameters can be considered uncorrelated (see Fig. R1 below). In depth answers to the issue of meteorology can be found in SA10 in the reply to reviewer #1 and GA1 in the reply to reviewer #2.

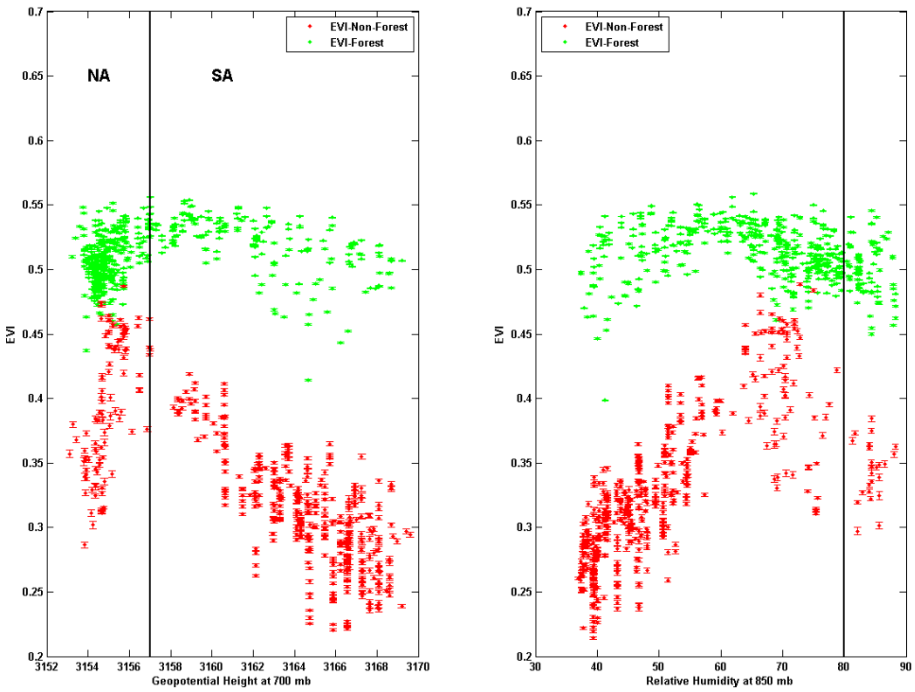

Figure R1. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively "immune" to meteorological changes, as opposed to non-forest EVI, which is much more sensitive.

GC3: Furthermore, if the higher-order EVI effect is real, then there must be some residual correlation between temperature, relative humidity, and EVI. If increasing EVI influences clouds because it is a proxy for an increase in the latent heat flux, then presumably the difference in heat flux corresponds to a difference in the low-level profiles of temperature and humidity, and that is why there is greater abundance of shallow cumulus.

In the manuscript there is much more attention paid to the physics of mesoscale organization of cumulus clouds above the underlying land surface type patterns than there is the basic physics that would explain how increasing EVI should lead to more cumulus clouds. I think more attention to this underlying physics is warranted. Are there relationships between the EVI and the meteorology observations that can be presented that support the causal link between EVI and clouds?

GA3: As written in GA1, we agree that more in-depth analyses of temperature, relative humidity, and heat fluxes profile are required for full understanding of FCu fields, but we consider this paper as a first attempt to report on such a link. It should be noted that for the most part, high quality data of relevant meteorological parameters in the domain we chose is lacking. For example, no radiosonde data from the past decade during the dry season is available for our region of interest. This problem is more severe with large scale analyses of boundary layer meteorological parameters, because those usually rely on satellite data which have many limitations themselves. Therefore, high resolution large eddy simulation will probably be the best tool to simulate and understand FCu cloud formation in a remote densely forested region. As we wrote above we are perusing this direction but setting up the Amazon FCu conditions in large eddy simulation is a large project on its own.

We emphasize that higher EVI doesn't necessarily mean more cloudiness. Our main conclusion refers to occurrence of FCu clouds, a specific subset of clouds commonly observed in the Amazon.

GC4: Is the correlation between cumulus clouds and EVI an expression of colocated spatial gradients in the two quantities, or mainly an expression of temporal variability in the two at specific locations? This can be addressed in two ways: (1) the authors could show the spatial pattern of EVI, just as they have done for geopotential height and relative humidity; (2) the authors could subdivide their region into smaller boxes and include more years for statistical robustness as suggested by another reviewer.
GA4: Thanks for the comment. This was one of the main comments repeated by all the reviewers. We claim in this work that the correlation between pFCu and EVI is due to collocated spatial gradients of the two quantities. The temporal variability for specific location isn't considered in this work as EVI data is taken as the mean for J-A-S months. To further strengthen this point, we have performed both of the suggestions listed above: 1) Adding an EVI and Landcover map for 2011 (see Fig. R2 below), and: 2) Added analyses for years 2008-2010 as well (Fig. R3 below). As explained in the reply for reviewer #1, further subdivisions of the already heavily cropped domain increase the noise to a point where we don't find any consistent correlation. This is due to the fact that we are looking at cloud field statistics, and

such analysis is scale limited so that only areas much larger than the cloud field scale (>100s of km, similar to the scale of the cropped domain in Fig. 6 of new manuscript) are appropriate for our analysis.

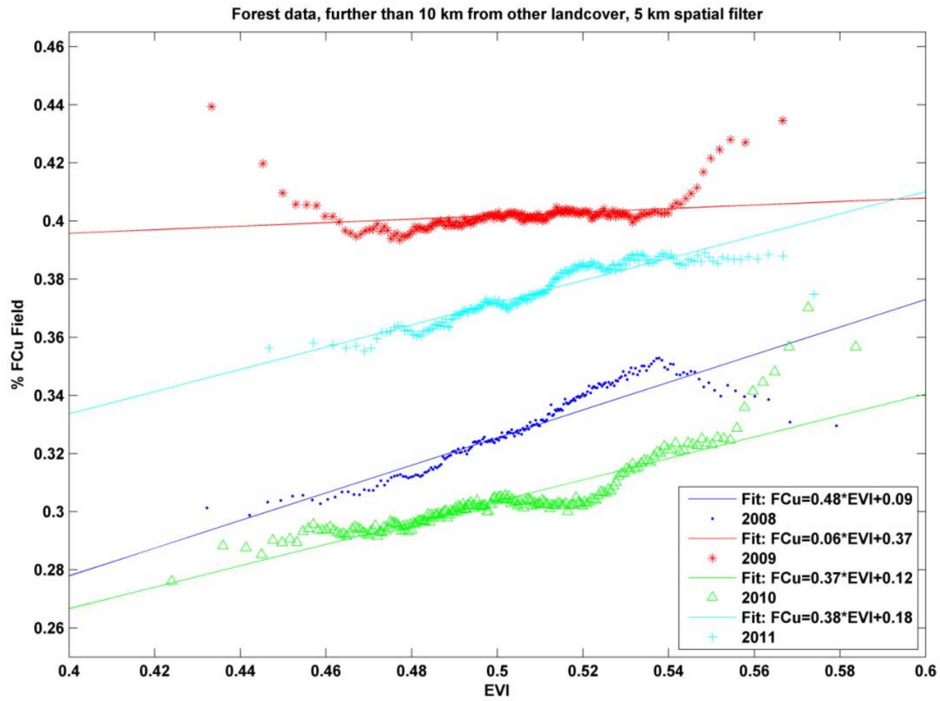

Figure R2. J-A-S pFCu as a function of EVI, for years 2008-2011 (see legend), above forest landcover. Data is confined to the NA region (with RH<threshold), further than 10 km from other landcover types. A 5 km disk shaped spatial filter was applied to the EVI data for each year. Linear fits for all cases added in figure legend.

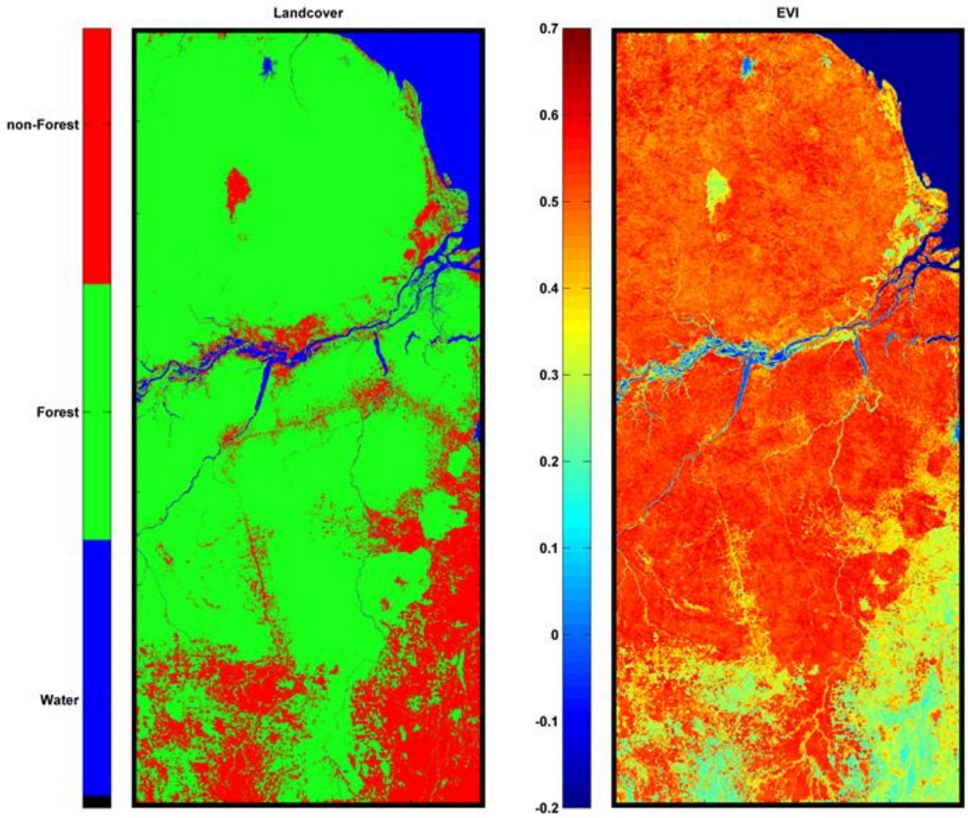

Figure R3. Landcover classification (left) and mean EVI (right) for J-A-S months, 2011.

GC5: Is it possible that there is an artifact whereby the retrieval of EVI may be impacted by variations in AOD? Since the two are purported to impact clouds, this potential should be dismissed.

SA5: In fact this is an important reason for why we chose to use EVI. We think it is highly unlikely for there to be an artifact for which EVI is impacted by AOD. First of all, we checked correlations between AOD and EVI for 2008-2011 and found no correlations. As seen in Fig. R2 above, it is clear that EVI isn't dependent on AOD. Last, studies have shown EVI to be less affected by aerosols in comparison with NDVI, therefore we added the following (in italic) to the text: **"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".**

GC6: Regarding the decrease in cumulus cloud frequency with increasing EVI at high EVI values in non-forested cases: Are we to presume that the decrease in cumulus cloud occurrence corresponds to an increase in the frequency of cloud-free conditions? Or could it be that the cumulus are giving way to deeper clouds that do not match your cumulus criteria?

GA6: It is hard to point out a single reason for the decrease in non-forest pFCu for high EVI. However, it is likely to be due to mesoscale effects nearby water bodies and thus more cloud-free/less organized cumulus conditions, and not deep convective conditions. As from Fig. R2 it can be seen that most of the northern Amazon non-forest data is located nearby rivers, especially for high EVI data. This still doesn't explain why non-forest pFCu increases for lower EVI data located near the same rivers. The distribution of the non-forest data is so that it is impossible to spatially separate high EVI non-forest from low EVI non-forest.

Other reviewers have commented as well on the non-forest dependencies. Since the non-forest data is much more dependent on meteorology and located closer to rivers, we downgraded our conclusions regarding this landcover type as written in the revised manuscript: **"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".**

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