**Reply to Reviewer # 1:**

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#:

**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.

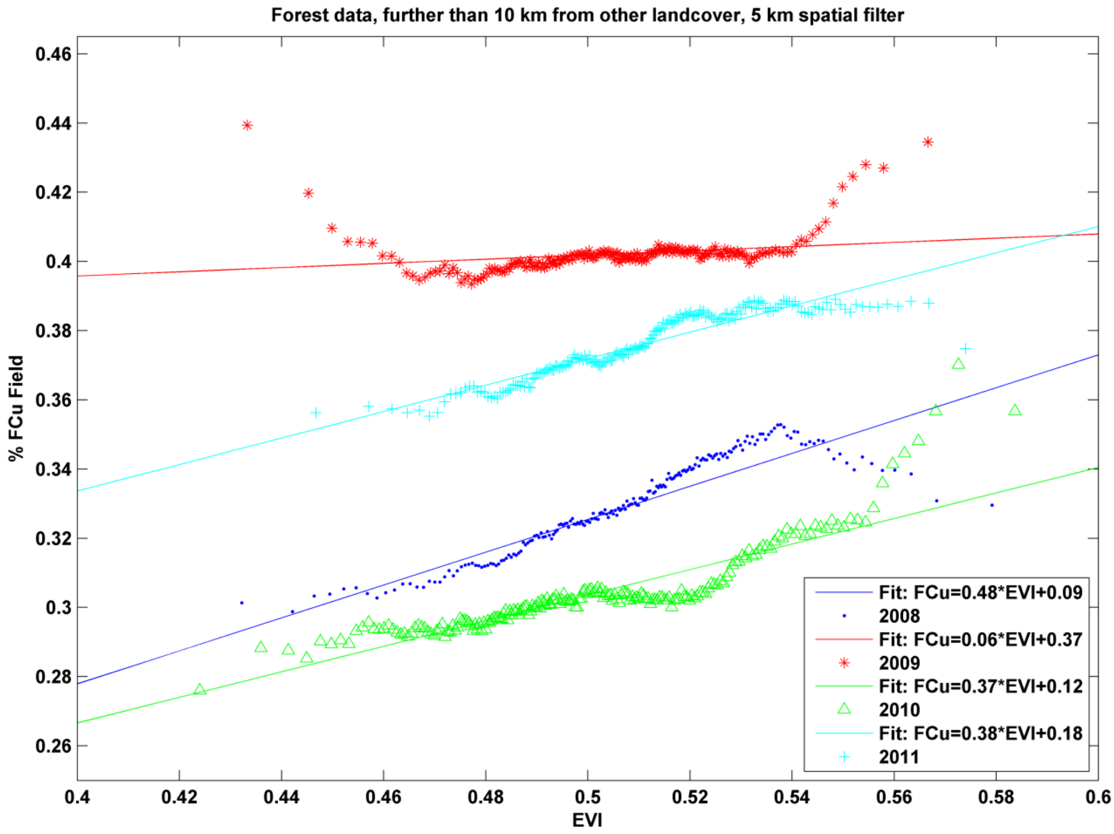


Figure R1. J-A-S pFCu as a function of EVI, for years 2008-2011 (see legend), above forest landcover. Data is confined to the NA region (defined for each year separately), with RH<threshold, further than 10 km from other landcover types. A 5 km disk shaped spatial filter was applied to the EVI data. 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 [%] | $\bar{A}$ [km$^2$] | $\sigma_A$ [km$^2$] | $\bar{D}$ [km] | $\sigma_D$ [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.

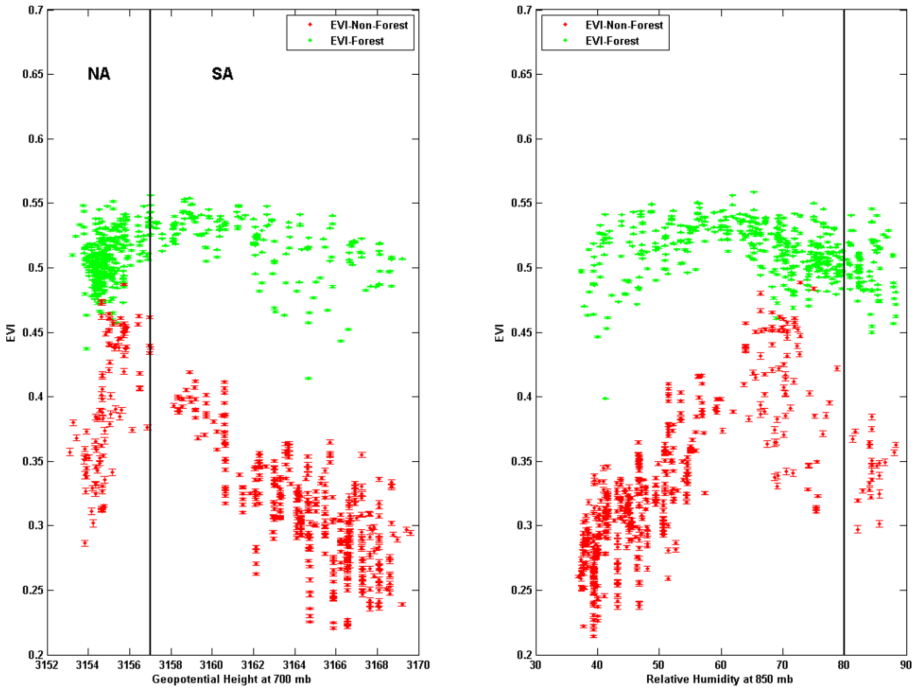


Figure R3. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively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


Davidi, A., Koren, I., and Remer, L.: Direct measurements of the effect of biomass burning over the Amazon on the atmospheric temperature profile, Atmos Chem Phys, 9, 8211-8221, doi:10.5194/acp-9-8211-2009, 2009.

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