Reply to Reviewer # 3:

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: 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.

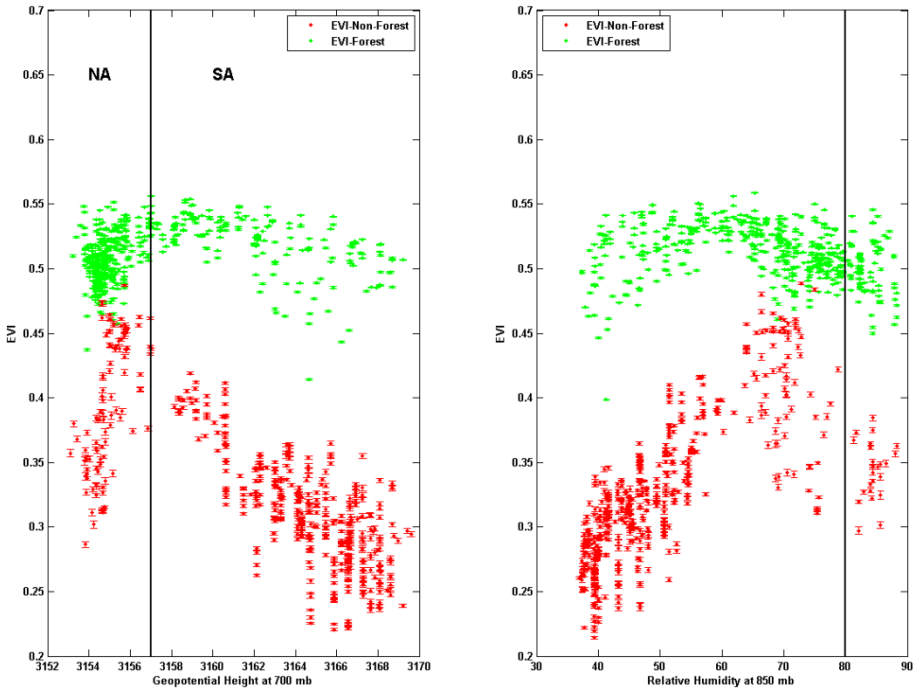

Figure R1. Total region of interest (Fig. 1, main text) EVI dependence on selected meteorological parameters, for forest (green dots) and non-forest (red dots) landcovers. Left: Geopotential height at 700 hPa. Right: Relative Humidity at 850 hPa. It can be seen that forest landcover EVI is relatively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

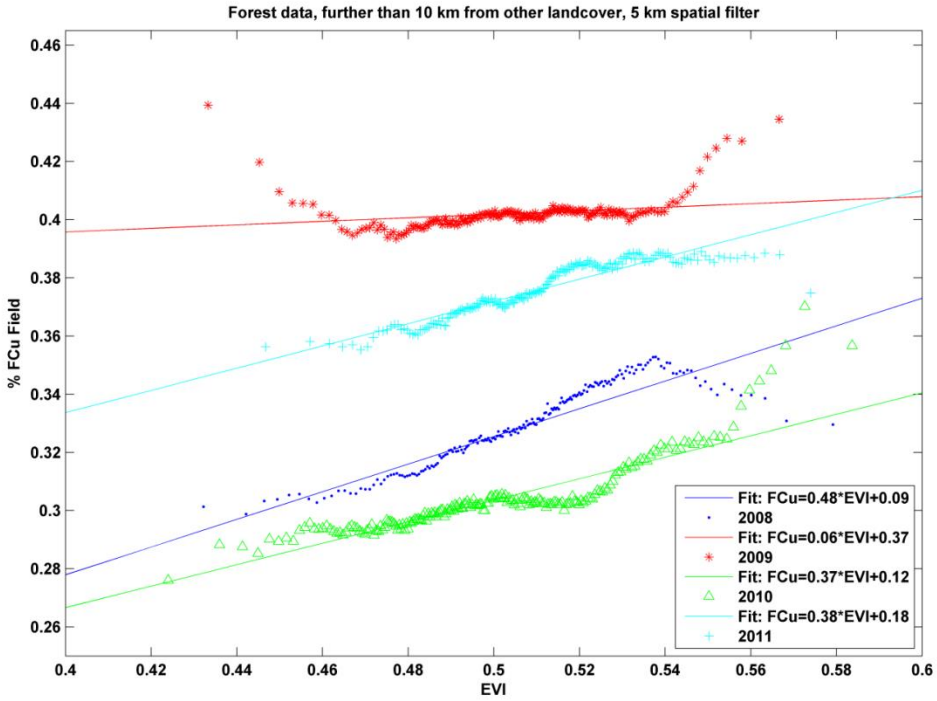

Figure R2. J-A-S pFCu as a function of EVI, for years 2008-2011 (see legend), above forest landcover. Data is confined to the NA region (with RH<threshold), further than 10 km from other landcover types. A 5 km disk shaped spatial filter was applied to the EVI data for each year. Linear fits for all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

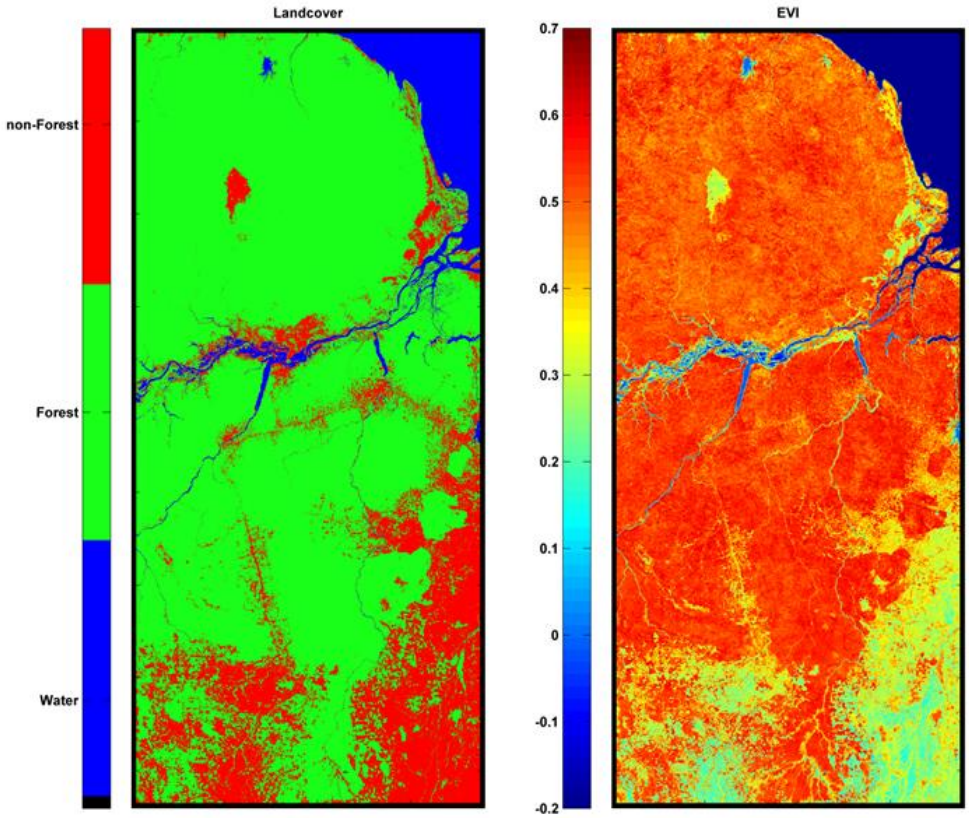

Figure R3. Landcover classification (left) and mean EVI (right) for J-A-S months, 2011.

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".**

References

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