

## ***Interactive comment on “On the link between the Amazonian forest properties and shallow cumulus cloud fields” by R. H. Heiblum et al.***

### **Anonymous Referee #2**

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This is a very interest study that examines dependence of the occurrence of shallow cumulus clouds on the enhanced vegetation index (EVI, a good indicator for the evapotranspiration as suggested in literature) over the Amazonian forest using satellite measurements (clouds, aerosol optical depth - AOD, and EVI from MODIS/Aqua) supplemented by meteorology from the reanalysis meteorology. The authors first developed an empirical method to classify the MODIS/Aqua cloud fields into three classes: shallow cumulus clouds (or FCu), deep convective clouds, and sparse or no clouds. Then they examined dependence of the probability of FCu (pFCu) during the dry season on meteorology (RH and geopotential height, among others), AOD, and EVI. They found that pFCu increases significantly with increasing EVI of forests (nearly linear relationship in deep forests), although this pFCu-EVI dependence is secondary to the effects

C10586

of meteorology and AOD. One important implication of this study is that changes of land cover in Amazon could lead to significant changes in cloud fields and climate. The topic is of great interest to readers of ACP and suitable for publication in ACP. The paper is generally well written. I recommend the paper be published in ACP after the following comments (minor in general) are adequately addressed.

General comments:

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?

Specific comments:

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. ..."

p.30014, l.24: which year?

p.30015, l.1-2: should "parallel to" be "perpendicular to"?

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.

p.30016, l.21: I believe smoke interaction with LW radiation is very weak.

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

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

C10587

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

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

p.30018, l.11: “where” should be “were”.

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

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”.

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

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

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

p.30021, l.24: delete 1st “the”.

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)?

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

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  $EVI > 0.585$ ”.

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

C10588

designated as “higher order effects”).

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

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

p.30033, last line: delete “on a given day”.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30013, 2013.

C10589