Quantifying vertical wind shear effects in shallow cumulus clouds over Amazonia

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Second review:

I thank the authors for their revised version and explanation of the changes made. In their revision, they have addressed the comments of reviewer 1 that concerned updating the tracking algorithm to ensure a larger statistical sample and providing more evidence of general results such as cloud profiles and time series of LWP/precip. This has surely improved the presentation of the simulation case itself. With respect to my comments, admittedly the authors have not implemented many changes and dismissed the opportunity to derive some more conclusions from the cloud tracking to address the influence of cloud sizes on the evolution of the boundary layer and its distribution of water. I accept their decision. However, I would ask the authors to still address the concerns regarding the evolution of wind and specification of wind forcing (which are also raised by reviewer 1), which they have not address appropriately.

- In the first review I asked whether the authors can clarify how the wind speed profiles evolve during the simulation, because this influences how we interpret the differences in clouds that are presented as a result of differences in shear in the cloud layer. The authors have shown in their response how the profiles change within the boundary layer, and comment that the HS(R) simulations develop about 1 m/s more wind in the boundary layer, which would lead to a more rapid development of the boundary layer – they also included this statement in section 2.2. I appreciate the authors showing me these profiles and I would be fine if the profiles themselves are not added due to the (already large number of) figures.

  However, the consequences and description of what they show should be correct and I don't think this is the case yet: First: the additional 1 m/s cannot lead to a more rapid BL development, because the surface fluxes are prescribed: then how (e.g., through a few deeper clouds), but are you sure this is significant? Looking at the boundary layer that has developed in the simulations (in the wind speed profiles in the reply to reviewers), they boundary layer height is approximately the same.

  More importantly though: the issue is not so much whether the wind speed is different, but what differences in shear develop. As the figures clearly show, at midday and in the afternoon, most shear is concentrated in the surface layer (up to 200 m) and at the inversion, while in the well-mixed boundary layer, between 200 – 1200 m, there is little shear in the HS(R) simulation. This is where the majority of your cloud fraction is sitting. In other words, while the simulations initially differ much in the amount of shear prescribed, as the boundary layer develops the amount of shear in a large part of the cloud layer has already been mixed away. I think this requires more thought or clarification in the text, or even a discussion! especially because the evolution of LWP and cloud top height are overall rather similar across the simulations and seemingly independent of the shear. It should be discussed that this might be in part because the shear is (already, quickly) mixed away.

  - The authors have not sufficiently clarified the large-scale geostrophic wind forcing that is applied – I note this was also requested in major comment 1 of reviewer 1, and seemingly not addressed / edited in the text. Perhaps I missed it, but I don't see any description of large-scale wind forcing. Figure 2 only reads input profiles, which would mean initial profiles, it does not write large-scale wind profiles anywhere.

- NOTE: The profiles of the wind direction show that you apply a clockwise turning of the wind with height (wind veering from ~ 100 – 190 deg): the text still says that you have a counter-clockwise turning.

- The authors have not addressed the concern that it needs to be specified how the Galilean transform is done, because the differences in cloud diameters between the shear cases is only 1 or two grid cells, something which could be easily influenced by the differences in wind speed in the cloud layer (and such differences will be there even though a Galilean transform is applied, because the wind profiles are sheared) .

- In my first review I also asked the authors to clarify the core and margin selection and why the diameter of the core and margin sum up to be larger than the cloud diameter? The authors reply: "the reason is that the data sampling is slightly different for the cloud, core and margins" and explain how it can happen that some pixels will not be classified at all. But that would imply that the core and margin would sum up to be less than the cloud diameter, and my concern is that sometimes they add up to be more than the cloud. The answer is not entirely satisfying. The other answer provided - that different shapes of the cloud, core and margins play a role here - would be more satisfactory, and if the authors are really certain that this is the case, they should state it in the text. It would have been nice if the authors double checked that the sampling works as they expect.