Reviewer #1

Overall comments

The revised manuscript is in good shape. I wonder if the authors could add more analysis for the effect of the directional shear, since this is one of the main findings from this study. Does directional shear tilt clouds as much as without it? I do not feel this analysis is not necessary but including it makes the manuscript widely read. After the following minor corrections, I think the manuscript is ready for publication.

Response

We are thankful for the pertinent comments from Reviewer #1 and for coming back for the second round of review. We agree that a deeper analysis of the directional shear would be interesting. However, we found it hard to highlight the directional shear results based on what we wanted to analyze in this manuscript. Our primary goal is to quantify the effects of vertical wind shear on the physical dimensions of the clouds and on their core/margins properties. Our results show that the directional shear did not present significantly different results than the wind speed shear, which is why we do not focus much on the directional shear. There is sure to be other interesting aspects of directional shear to look into, but for the purposes of this manuscript we did not feel the need to highlight it as much.

Minor Comments

Responses are italicized and in red.

- line 24: Delete "(w)" and "(LWC)". *Done.*
- line 46: Since "ACRIDICON-CHUVA" is highly likely an acronym and its first appearance, it should be expanded. Check ACP's formatting rule for acronyms. *Done. Expanded the GoAmazon2014/5 name as well.*
- line 52: Define "VOCs" and "UT". *Done.*
- line 56: "...other sources of aerosol particles." I wonder the discussion (from line 45) for this topic is necessary? It is certainly interesting, though. The discussion could be made simpler. *We have removed two sentences to reduce the discussion of this topic. We consider the topic itself to be important because it is one of the motivations behind the study of VWS effects in cumulus clouds – i.e., whether VWS affects or not their capability of growing into deep convective clouds. Our results suggest that VWS may indeed help the formation of deeper clouds.*
- line 61: Define "VWS" here because of its first appearance in the text. *Done.*
- line 107: With what time stepping scheme? *The time stepping is adaptive, with a maximum of 1 second (mentioned in Section 2.2).*
- line 108: "radiation" => "radiative". *Done.*
- line 121 and below: Use italic face for variables such as "dS", "T", etc. in both text and captions. *Done.*

- Equation (2): The round bracket in front of the dS_macro in the right hand side is not closed yet. *Good eye. Closed it.*
- line 126: "The supersaturation sink coefficient (gamma)" should be introduced immediately after its first usages in Equation (1). *Done.*
- line 163: I still do not know how u and v wind profiles are maintained over the simulations. *We have clarified that the wind speed/direction increments are applied to the initial and large-scale forcings.*
- line 199: The moving domain with the horizontal wind speed at 1 km height should be mentioned in the experimental setup. This treatment is also beneficial to reduce numerical diffusion. *We appreciate the suggestion but feel like this information is a good link to discuss the tracking algorithm. In that sense, we would prefer to keep it in Section 2.3.1.*
- line 252: Is the cloud top over 4 km (fig. 4) too close to the domain top of 5 km? Which level is the damping/sponge layer applied? The configuration should be mentioned in the experimental setup. *The dumping layer starts at 4 km, so indeed the cloud top is within it. We now mention it in the experiment description as well as in this first paragraph of Section 3.1.*
- line 287: Should "the statistics of the ... bias towards smaller clouds..." be "... bias towards larger clouds..."? For smaller clouds, statistics well match up. *The meaning here is that the statistics of the <u>tracked</u> clouds will bias towards smaller ones because they are more numerous. The largest clouds are not tracked since they are way more likely to have pixels crossing over the boundaries. The domain-wide statistics are indeed biased towards the largest clouds, however.*
- line 411: "continuous lines" should be "contour lines" Correct in text as well as captions. *We use "continuous lines" here to distinguish from the dashed lines present in the same figure.*
- line 426: Should "...latent release..." be "...latent heat release..."? *Absolutely, thank you.*
- Figure 1: "Prescribed turbulent fluxes..." => "Prescribed surface turbulent fluxes...". *Done.*

Reviewer #2

Overall comments

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.

Response

We are thankful for the insightful comments from Reviewer #2 and for the continued efforts in this second round of revisions. The comments from Reviewer #2 are very relevant and we detail our responses below.

Major Comments

(Major Comment #1)

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).

(Response to Major Comment #1)

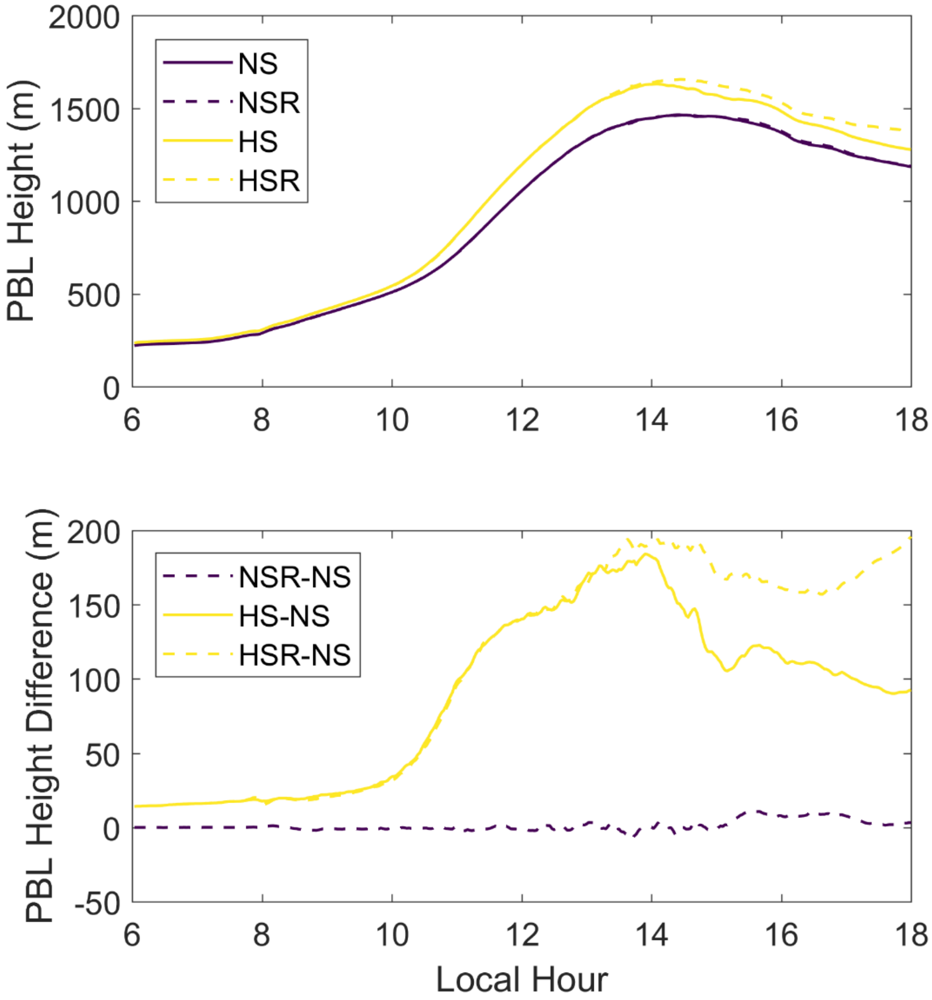
We thank Reviewer #2 for this comment, it helped us have more clarity on the processes we are discussing in this manuscript. For clarity in our response, we will split it into topics, as follows:

- The extra 1 m s^{-1} wind speed in the HS(R) runs and the differences in boundary layer (BL) height

We believe we may be discussing two different things at the same time here. In one hand, we have the added ~1 m s^{-1} of wind speed in the BL in the HS(R) runs as compared to NS. This was just an observation made to the Reviewers in the response material and is not discussed much in the manuscript aside from the comment in Section 2.2. On the other hand, we have the addition of wind sped <u>shear</u> throughout the whole vertical domain, which naturally also adds wind shear within the BL itself. We do not claim that a 1 m s^{-1} wind speed addition by itself causes the growth of the boundary layer. Instead, we mention in Section 4 that this is likely related to the wind shear. We discuss this within the context of the Henkes et al. (2021) work because it is a nice observational reference for the same region. We have added comments to Section 4 to clarify this point, with the following reasoning: with added wind speed shear, the entrainment rates at the top of the BL will be increased (Pino et al., 2003), which causes it to grow faster. This is consistent with the Henkes et al. (2021) findings that the low-level jet is stronger on days with deep convection – i.e., in that case, the increase in turbulence within the BL would be due to stronger low-level jet. In our case, the results capture such mechanism in a different way because our wind shears are linear. Nonetheless, our sheared runs have added wind shear within the BL, as well as in the entrainment layer, which produces the extra BL growth even though the surface fluxes are prescribed.

To make sure our BLs in the HS(R) are indeed higher than in NS, we have calculated the BL height using the bulk Richardson method with a threshold of 0.25. We show the results in the figure below. On the upper panel, we show the time series of BL height for the NS(R) and HS(R) runs, while the bottom panel shows the differences. This figure shows that prior to the deepest cloud at 14:00 in the HS run there is a

~150 m difference between the BL height in HS and NS. This is consistent with the mechanism we described earlier where the added wind shear induces more entrainment and accelerates BL growth. We have added a comment on this point in Section 4 to improve the discussion of this important topic brought forth by the Reviewer.



- The wind shear mixing in the layer between 200 m and 1200 m

We agree with the Reviewer that the wind shear is low between 200 m and 1200 m. This is due to the mixing within the BL, but it should not affect clouds as much. We note that the BL height is usually close to cloud base: in our calculations using the bulk Richardson number approach with 0.25 threshold, the cloud base height is within ~150 m of the BL height. Of course, this would change depending on the threshold used or even if other methods to estimate BL height are used. Nevertheless, the clouds should develop mostly above the BL, where the wind shear is stronger. For example, at 12:00 local time, the figure above shows that the BL height is approximately 1000 m. From our previous response to the reviewers (vertical profiles of wind speed), this level is where the wind shear starts growing again. Noting that our clouds are at least ~100 m deep (Figure 9 in the manuscript), they should indeed develop under wind shear conditions and not under the mixed wind regime as mentioned by the Reviewer. Note also that in Figure 5 of the manuscript the maximum cloud cover is slightly above 1000 m, which should be close to cloud base height – this also indirectly indicates a deeper BL in the HS run because of the higher height of cloud cover maximum.

- Mention of large-scale wind forcing

We now mention the large-scale forcing in Section 2.2 and in the caption of Figure 2. The large-scale winds are the same as the input wind profiles and are kept constant throughout the simulations. We thank the Reviewer for noting this lack of information in the manuscript.

- Clockwise vs counterclockwise wind rotation with height

The Reviewer is correct, the winds have a clockwise rotation and not a counterclockwise rotation as we stated in the manuscript. Thank you for the correction.

- The Galilean transform

The Reviewer raises a good question – i.e., whether the different wind speeds mentioned in Section 2.2. would affect the cloud sizing. Here we note that we are using a tracking algorithm that follows the clouds regardless of their displacement velocity. Therefore, the Galilean transform is implicitly taken care of by the tracking algorithm and the cloud size calculations are independent of their displacement velocity.

(Major Comment #2)

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.

(Response to Major Comment #2)

We have given this issue much more thought and went back into the data to analyze in detail. What we find is that, indeed, the sum of the core and margins lengths is almost always slightly larger than the cloud dimension. Upon further inspection, we found it hard to confirm the effects of different shapes – i.e., the response that would be more satisfactory to the Reviewer. Given the large number of clouds, it is quite hard to quantify the effects without a complex algorithm that detects the shapes and then compares them between the cloud, cores and margins.

What we can confirm, however, is that there are indeed some sampling differences between the cloud, core and margins pixels used in the calculations shown in Figure 10. Note that the core and margins classifications are quite restrictive because they require all pixels to have all positive (or negative for the margins) updraft speed, buoyancy and supersaturation. So, at a given height in a tracked cloud, it is possible to have pixels classified as "cloud", but none classified as "core" or "margins". Or we can have situations where there is only "cloud" and "core" at a given height, for instance (and the same for "cloud" and "margin"). In the end, this produces a non-trivial comparison between the cloud, core and margins lengths because they have different samples. Overall, we found that the cross sections that are fully classified as "cloud" (with no "core" or "margins") usually have smaller dimensions than cross sections that contain either "core" or "margin" classifications. Therefore, the cloud dimensions in Figure 10 have a bias towards smaller sizes and the opposite happens with the core and margins dimensions. We have added this explanation to the text.