We very much appreciate the encouraging summaries alongside constructive and super detailed major, minor, and line-by-line comments on our paper from both reviewers. These comments (now has been appreciated in the Acknowledgments), have led to a significant improvement of our manuscript, reflected from many massively rewritten sectors and sentences with typo or grammatical corrections in the revised manuscript. We have certainly made many additional modifications to the text as well. The following are our point-by-point responses to the reviewers' comments (here the reviewers' comments are displayed first in bold *Italic* font).

Reviewer #1 Dr. Mónica Zamora Zapata
Major comments
• There are multiple writing mistakes along the document, along with confusing statements. Please revise the manuscript carefully. Some suggestions are given below, but still, readability is essential for a paper.
We admit that the previous manuscript could be better prepared. As indicated in our detailed responses below that we have tried our best to rewrite certain sentences or even sectors of the paper and correct typos to improve the readability of the manuscript.

• Cloud break-up is used to refer to a state where the cloud deck has a low cloud fraction, but there is no clear definition of it nor a discussion about it. Some sentences talk about earlier or faster break-ups but I'd suggest to treat the concept carefully to avoid confusions, specially since coastal clouds go through cloud dissipation and the process is a complex one already.
The reviewer's point is well received. We use the term break-up to describe the stage in cloud diurnal cycle when the dense stratus cloud layer starts to become cloud blocks separated by cloud-void spaces, though the cloud-void space would generally increase until dissipation, but break-up here is not equal to the dissipation. We admit that this needs to be made clearly in the manuscript. We have revised the paragraph in original 295 (and in other places) to clarify the difference between break up and dissipation or clear up: "At 06:00 UTC, cloud deck covers the entire domain as seen in both modeled result and in observations (note the distinct cloud rolls in model results). Between 10:00 and 13:00 UTC, the CPP in layers between mean CBH and CTH decreases from near 100% to 90%. Near the two averaged values, CPP decreases more to reach near 60% and 80% at CBH and CTH, respectively. This leads to a less inhomogeneous cloud deck confirmed by the LWP map and the observation of the sky camera at 12:00 UTC shown in the middle row of Figure 5. Indeed, more cloud-free pixels begin to appear between clouds and sunlight is seen through the cloud deck by the camera. Finally, the CPP continues to decrease until the end of the convection phase with a maximum barely reaching 80%, and a value around mean CBH and CTH as low as 20% and 40%, respectively. This demonstrates the break-up of the cloud deck during convection and the cloud thinning. The bottom panels of Figure 5 show clearly the dissipation of a large number of clouds alongside substantially thinning of the others at 16:00 UTC PM. The LWP map (Fig. 5b) shows numerous thin clouds corresponding to those seen by the camera of Savè".

• The case studied was said to be a late dissipation one. They hypothesized that precipitation evaporation could explain this. No further analysis was done using LES data related to this issue, I think it should be diagnosed. I wonder if studying a more usual dissipation time case would give the same results. A more critical comment could be appreciated.

The reviewer's point is well received. We have added the following sentence in Line 174 in the original manuscript that reads as: "Nevertheless, our focus of this study is on the diurnal cycle of LLSC as influenced by aerosols alongside planetary boundary layer dynamics rather than examining the above hypothesis appeared to be related to a process beyond the local scale. Therefore, our model setting is made to specifically eliminate the influence of mid-cloud layer for the purpose". Regarding the selected case, it is the best one we can have in terms of availability of observations that we can use to constraint the modeling and to make comparison of modeling results with observations".

Minor comments

• I'd suggest to simplify the Meso-NH model description, focus on the setup and how the data was used to create initial and boundary conditions, and leave the model details on Appendix if desired. In my opinion, later experiments on aerosol contributions can also be described in the model section. This is personal preference, as I've seen both around.

We have the same feeling about the model description. This is the reason that we have only included a few model components that are keys to our modeling effort.

• Also related to readability, the results section can be summarized greatly, focusing on the big takeouts instead of carefully describing every value presented in the figures. While a description of the time evolution of the different vertical profiles seems fitting for this type of work, I'd suggest to be concise, highlight the most significant processes or values, since everything else is available in the plots.

The point is well taken. The revised Results section now contains more comparisons to the previous findings. Certain parts of the discussions have also been made more concisely as suggested by the reviewer. Also, the Section 4 now opens with the following statements: "Previous studies have indicated that the life cycle of stratus or stratocumulus within planetary boundary layer depends on the subtle balance among several critical while interconnected forcings including surface heat fluxes, cloud top and base radiative profiles, and thus turbulent mixing (*e.g.*, Stevens *et al*., 2005; Dussen *et al*., 2014, Ghonima *et al*., 2016). Apparently, our simulation results of the REF case support previous findings particularly for cases over land with surface sensible heat playing a significant role. Nevertheless, the role of aerosols in such a life cycle have rarely explored in-depth. Given the critical role of aerosols in determining cloud macro- and microphysical features and thus radiation, this is a must-addressed issue to advance our understanding of the LLSC life cycle. A unique component of our study is the deployment of an interactive aerosol and atmospheric chemistry module in this observation-constrained modeling effort. In the following section we will discuss roles of aerosol variations in both number concentration and chemical composition in influencing the diurnal cycle of observed LLSCs".

• Cloud formation is said to occur around hour 2, which is probably already part of the LES spin-up time. Could this be an issue?

As mentioned in Line 246 in the original manuscript, the simulation starts from 11pm previous day to have a 1-hour spin-up before July 3. Hour 2 is thus already 3 hours later. Giving the stable and dense stratus before sun rise, we believe this setting has less influence on the diurnal cycle of modeled clouds after sun rise.

• As results are presented, there is no critical comparison with the references given in the introduction for similar processes. It is important when summarizing to compare and also state what is novel.
We appreciate this excellent point raised by both reviewers. This has been improved by adding comparisons to previous findings wherever applies.

• Time-series of cloud fraction and liquid water path, both said to be available from observations and the LES runs, could complement the analysis greatly. Please include them in one of your figures, and strengthen the physical description of the PBL processes as needed.
The cloud fraction was displayed in a different form in, e.g., Fig. 4. We realize that the term "cloud presence probability" might read odd to many readers without a clear description. This has been done by modifying then moving a sentence in original L289 to a better place for Fig. 4 discussion, original L278. It now reads as: "Note that to analyze the cloud cover profile over the domain, the Cloud Presence Probability (CPP) at each model layer, differing from cloud fraction that is often defined as a column metrics, is calculated as a percentage of all cloud pixels with a total condensed water mixing ratio exceeding 0.05 g kg^{-1} at the given model layer (Fig. 4)".

• The cloud presence probability, CPP, is not carefully defined, so I don't understand how to interpret the values.
Please see above response.

Line by line comments
• L10 Is it necessary to remark that there is no precipitation like this in the abstract? Maybe combine with previous sentence "...effect, with all cases having negligible precipitation".
We appreciate the suggestion. We would, however, keep the current sentences because 'all cases' might appear to be a stretching statement for DACCIWA, and the second sentence is to emphasize on the sensitivity to aerosol concentration.

• L15 Why use cloud-void instead of cloud fraction or cloud cover? This is true for all the document.
They are the two sides of the same coin. Though, cloud-void here would link closer to the cloud development associated with evaporation and thus offer a better direct reference to the process in discussion.

• L21 What do you mean by stabilize? A more steady evolution?
It means that heating near the cloud top would enhance thermodynamic stability of the cloud layer beneath and reduce the upward development. To avoid the confusion, we believe that "…lower the cloud top height" should be sufficient, therefore, "and stabilize the cloud layer" has been removed.

• L23 Break-up faster means an earlier breakup time?
Not necessarily, please consult the response to the corresponding major comment.
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• L64 Why would aerosol emissions impact the wind?

There are several ways that aerosol direct or indirect effects could modify the energy budget and thus wind within the planetary boundary layer. Nevertheless, since this is not the main agenda of our research, "wind" has been removed.

• L74 Are these 2 studies observational or model-based?
The sentence has been modified to "Based on observations and parcel modeling, Taylor et al. (2019) and Denjjean ewt al. (2020a)…"

• L80 It might be good to summarize what the direct, semi-direct, and indirect effects are, maybe in the second paragraph of the Introduction.
A sentence of "This is because that aerosol can directly scatter or absorb solar radiation (direct effect or aerosol-radiation effect), or by serving as cloud nuclei, influence cloud microphysical structure and thus reflectance or lifetime (indirect aerosol effects or radiative effect of aerosol-cloud interaction plus cloud adjustment) (Boucher et al., 2013). The heating associated with aerosol absorption would be able to perturb atmospheric thermodynamic stability and thus dynamical processes as well (semi-direct effect) (Hansen et al., 1998). All these effects can modify the energy budget and thus the status of the planetary boundary layer where the stratiform clouds form."

• L74 This paragraph has a lot of info but it's hard to follow as there is no clear story in it. The last sentence helps, but maybe it'd be good to have a similar introductory sentence and then highlight the knowledge gaps as info is presented. Just a suggestion. Another idea is to remove all unnecessary mentioned results if they won't be used for comparison or contrast.
We agree with the reviewer that this paragraph is a bit too long. However, we believe that it serves a good purpose to describe major findings from the previous studies including aerosol sources and chemical compositions that benefit our study in many ways. And, as the reviewer appreciated that it has also indicated the shortcomings of the previous works particularly regarding aerosol-cloud interaction. Therefore, we decide to keep it here but with certain modifications, including the opening sentence now reads as: "Based on observations and parcel modeling, Taylor et al. (2019) and Denjean et al. (2020a) showed…".

• Fig. 1 Maybe a proper reference is better than the link.
It is from the publicly accessible image base; thus, the link is provided.

• L98 What local scale and fine resolution are you talking about?
We believe this sentence is a general statement suitable to be here. The model description coming later are sufficient for the details.

• L116 Local time is also UTC for this location, right?
"…(local time of Benin is UTC+1)" has been added.

• L124 The info in this paragraph is a bit heterogeneous; some descriptions are very detailed and others not as much. Is the importance given to the details of the sky camera meaning that this data is more important than the rest? Also, a matter of preference, but it could be easier to follow with a consistent sentence structure like: first the instrument, then the data it generates (as done first with the ceilometer).

We have removed certain details particularly for sky camara to balance the descriptions for different instruments.

• L135 Aren't these fluxes included in the last sentence?
The redundant "…as well as sensible and latent heat flux" has been removed.

• L162 Turbulent mixing maintains cooling? You mean through downdrafts or are you referring to the cooling advection?
The sentence has been modified as: "Turbulent mixing beneath the NLLJ alongside strong radiative cooling at the cloud top leads to the persistence…". The original sentence only described the sensible heat effect.

• L168 What is the scenario C?
Scenario C was summarized by Zouzoua et al. (2021) as indicated in the text and is actual elaborated in the following sentences.

• L170 What does break-up mean here?
Please see the response to the major comments and related modifications.

• L192 Add reference instead of link.
The link is removed. The reference provided in the opening paragraph of the sector should be sufficient.

• L243 This resolution is not particularly high for stratocumulus LES
We agree that the adopted vertical resolution is not the highest though quite typical. We have added the following text in discussion: "Note that previous studies regarding nocturnal stratus-stratocumulus suggested that a vertical resolution as fine as 5 meters near the cloud top would be necessary for reproducing the cloud top entrainment and thus cloud macrophysical structures (Stevens et al., 2005). Since the nocturnal-diurnal life cycle in our case involves a dynamically evolving cloud top (particularly in the daytime), it makes it difficult to prescribe a highlight zone for finer resolution. Our fast-testing results did not suggest an alarming difference between the run with 10 m and 5 m vertical resolution (not shown). Therefore, the current vertical resolution and the time step are selected to well cover all possible cloud tops during the simulation time and to provide the best economic computational performance for aerosol-cloud interaction with a fully coupled chemistry model".

• L247 Are these values used to prescribed turbulence related to the local observations in any way? Is it okay for the profile to not change over time when modeling a coastal case?
High pressure system and associated substance has been well documented.

• L249 if the model was tuned for the site observations, it'd be good to include that in an appendix. Otherwise, what kind of parameters is this surface model taking in for this case?
The Code and data availability has provided sufficient information for the data sources.

• L251 Are these 2 s chosen for numerically advancing the LES? If so it's more than adequate for diurnal variations, and probably needed not for that reason but for keeping a good CFL number. It's not clear as is written.
"…particularly involving aerosol and cloud microphysics" has been added.

• L254 Do you mean using those observations as the initial condition? What is the nudging timescale for the tendency profiles?
The words of "hourly radiosondes" have been added.

• L258 How did you combine all that data to produce vertical profiles?
This is quite a normal process for deriving profiles to drive LES runs. The quantities mentioned for the period beyond the availability of hourly radiosonde are the ones to better constrain the modeling. We have added sentences (in respond to the comment from another reviewer) to elaborate it: "Note that, despite these best possible efforts in configuring a set of observation-constrained tendency profiles to reproduce observed cloud field, it is difficult to eliminate the possibility that such profiles could reflect certain local thermodynamic effects however small they are. In practice, our principal is to make the profiles to be able to force the modeled clouds reproduce observed quantities of major features such as cloud top, base, LWP, surface incoming solar radiation in the REF case. This would serve the best purpose for us to address the major issue of this study, i.e., the role of different aerosol profiles in the diurnal cycle of modeled LLSCs".

• Fig. 3 Improve the description: mass composition of what? Percentage text in a) is overlapping. Is the dot needed in the units of the b) y axis label (and all other labels)?
The caption reads now as: "Aerosol chemical mass compositions (a) and size distribution fitted into 2 modes described in Table 1 (b) used in REF."

• Table 1 I'd suggest putting the symbol for each parameter after mentioning them in the description.
Done.

• Fig. 4 and other figures are vertically shrinked. "Vertical colored bars" are actually dots. Do simulated values represent the horizontally averaged CBH? Why are they reported hourly when the LES has a smaller step size? What is the meaning of this probability?
The figure has been adjusted to have a proper aspect ratio. CBH and CTH were both described with the term of "mean" in text and figure caption. Hourly display is due to the limit of output data (storage). Layer-defined Cloud Presence Probability has been better defined in the revised manuscript (see the response to a similar comment).

• L273 Do you say "formation" because the initial state for the LES is cloud-free? If so, initial conditions should be stated carefully.
Yes, it started from cloud free. This is a common approach. The initialization at 23:00 UTC in the previous day (thus with 1 hour spin up) was clearly described in the 3rd paragraph of 2.4.

• L274 Is the mean CBH estimated as the horizontal average of points where the first cloudy grid point is located?

Yes. We also added “domain” before mean in certain places.

• L280 You can be more specific than “some period”
It was indicated in the following words, “particularly late morning and afternoon”.

• L284 Could the difference also be due to prescribing a weak subsidence profile or due to enhancing entrainment by not having a very fine vertical resolution?
“the vertical resolution of radar profiles” has been added.

• L289 This aids the previous paragraph in saying that for 10:00-14:00 UTC, observed CBH is below the 0 level probability. I’d move this and start a new paragraph with the spatial results.
• L289 What does a CPP of 50% mean? Is it the geometric midpoint?
For the above two comments: the opening sentence has been modified and moved to two paragraphs before. Yes.

• L294 By visible camera do you mean sky images in the visible range?
“sky camara (visible range)” has been added.

• L295 “distinct cloud rolls” instead of “clear cloud rolls”, so that clear is not confused with clear sky. It can also be noted that this feature is not observed in the sky image.
Done as suggested.

• L296 What is the CPP at the mid distance? At the mean in-cloud height? I still don’t fully get the meaning of CPP values, so I don’t understand what is useful of this description
Revised to “in layers between mean CBH and CTH…”.

• L301 Rather than demonstrating the break-up, it evidences the already broken field. This comment is related to what is the definition of break-up.
With the clarification of “break up” in place (see the relevant response previously), this sentence should be fine now. Though, “break-up” in the following sentence is inaccurate, it has been revised to “dissipation of a large number of clouds alongside substantially thinning of the others”.

• L303 Still, the camera at Sav`e shows a big portion of the sky completely clear. The LES does not reach that type of organization.
This comparison means for qualitative not exactly quantitative purpose, and the sky camara’s image and the model has different resolution as well, not to mention the tiny white blocks in camara’s image are hard to identify.

• L317 Do you mean that the difference is due to the tuning being done for more persistent cloud decks?
As described in the newly added description of the model design, the reason is likely to be a mid-layer cloud way above the model top, which has been ignored on purposely.

• L319 Why would that difference be reduced in the convective phase?
The sentence has been removed because the result is insignificant.

• ***L354 along what altitude?***
Added “above roughly 200 meters”.

• ***Fig. 7 θe has not been defined, and its units are just K, not ◦K. Put w s in equation mode. Why the discontinuous lines in f)? Why the choice of plotting T, RH, and θe instead of just θe and qt?***
Θe was defined in figure caption as equivalent potential temperature, which is a commonly used metrics. “w_s” has been corrected, thanks.

• ***L376 Does stable mean constant cloud thickness here? Be careful not to be confused with thermal stability.***
Yes, and, thanks for the reminder. With “cloud layers” here we assume it should be sufficiently clear.

• ***L383 is this a fixed threshold to find the inversion height?***
No, it is apparently determined by the profile.

• ***L416 Does more unstable mean signs of decoupling?***
Not necessarily.

• ***L417 What does “end of cloud break-up” mean? That the clouds cleared up or that it’s fully covered again?***
Revised to “coincides with the dissipation of the LLSCs and indicates”.

• ***Fig 9. Why report at 17 h and not 18 h to follow the 2 hours spacing? w s to formula. Why does TKE, RH and θe increase above the PBL at 17 h?***
Most simulations ended at 17h, only a few test runs went beyond. A that moment, clouds are almost all cleared and planetary boundary layer top leveraged. TKE is likely due to wind shear above. “w_s” corrected.

• ***L435 What biases are you talking about?***
As discussed in the reference case. “even though …biases” has been revised to “despite certain biases”.

• ***L440 Do these cases represent extreme situations in the set of observations?***
As described here that these runs are based on actual observations. We assume true extreme situations are normally hard to capture in field campaign with limited time frame.

• ***L456 What is cloud extent? Cloud thickness?***
Please consult with Fig. 4 and 5.

• ***L464 What does “this way” mean? I’m guessing not by the model activation but the text is confusing.***
Revised to “under this condition”.

• L460 Here and in other places, comparing numbers in a more descriptive way can help the reader. For example, "NPOLc reaches half of the droplet concentration of the REF case (333 vs 653 droplets per cm3), with a slightly higher radius (6.45 vs 5.1 μm)."
We have tried our best effort to make the comparison more readable.

• Fig 10 Why are there 2 cloud layers at hour 6? It might be useful to also have a plot of the standard deviation of surface SW irradiance to accompany the last panel.
Please note that these are layer-averaged quantities while cloud top might not be always flat. Nevertheless, based on the values of the quantities, the second cloud layer (if we understand the comment correctly) does not look physically stable or even sound.

• L473 I'd suggest to check if the difference in LWC is related to changes in cloud cover. Also, since the changes in CTH and CBH are equivalent to just a few grid points, dive into that discussion as well, what was the expected outcome? Are there both positive and negative cloud thickness feedbacks that may be canceling each other?
It is hardly to understand cloud cover change would lead to a different liquid water content easily – the latter is defined by profile for a relatively well-mixed planetary boundary layer in our case. As what we see from the modeled results (alongside those of many others), condensation could be another reason to cause the difference since a sufficiently higher cloud droplet number concentration generally lead to a higher liquid water content.

• Fig 11 It is hard to distinguish the clear portions in the last panel. Maybe you can set the zero values as NaN for plotting them in white, and mention that in the caption. I don't know if it's related to the wind at the time but POL seems to have less elongated structures at 14 UTC. What is ADEON?
The difference is visible from the figure though not as large as in REF vs. CLEAN. This figure is actually a result from numerous testings with different color schemes and skills including NaN or alike. ADEON is not necessary to be here and has been removed, thanks!

• L491 When saying "major" reason, what is the other reason why the trend would be different from what you expected?
The normally expected outcome is described clearly in the following sentences.

• L492,L500 The solar zenith angle should be lower near noon, right? Do you mean solar elevation angle?
Thanks, corrected.

• L498 I don't understand this sentence well. When you say "direct solar flux is relatively low" you don't mean the time of the day, right? Then you talk about cloud reflectance, which I'm guessing you interpret through LWP or LWC, saying it is a secondary factor. Does this mean that for clouds without full cloud cover (which would be the primary factor), then changes in reflectivity are also promoting the unexpected result for the POL case?
Generally, yes to the first part of question except for that short-wave cloud reflectivity is related to the cloud droplet number concentration rather than total condensed water. To avoid confusion, "When direct…" has been revised to "Thus, before noontime cloud reflectivity…". The second part, no, the primary factor during this stage is the solar radiation reaching the ground directly by

passing through cloud-void space. Otherwise, more reflective cloud, as expected in the so-called classic Twomey effect, would lead to a lower amount of solar radiation reaching the ground.

• L500 By "this", do you mean the reduced PBL heating due to a higher reflected SW at cloud top?
"This" is revised to "The lower SWRADSURF in CLEAN".

• L504 Though the images do suggest that some layers may dissipate earlier, it'd be better to include time-series plots of LWP and cloud fraction, maybe as panels b and c in Fig. 4.
The point is well received. However, since the time series of LWP and cloud fraction (though layer-defined) have already provided in Fig. 4 and 5, and additional information of microphysical features in Fig. 10, we feel that adding one more panel in Fig. 4 might not serve the best of purpose.

• L505 The last sentence is confusing. It seems to relate a low number of variable pixels with variations in surface SW irradiance.
It has been removed.

• Table 3 Is this LWP or LWC (units of LWP are typically g/m2)? Why don't you put the % symbol instead of writing percentage again? Last column title shouldn't be PRP, right?
It is LWP, g/kg m is commonly used for lower atmosphere without involving air density. Percentage in Table caption is necessary, this would save the space in the Table.

• L511 Do you think that a different type of variation could cause the more expected result?
Thanks for the thought but this is apparently an open question now, perhaps worthy another effort.

• L523 You should explain the REF NOBC case here (I'm guessing it has no BC, but were all the other aerosols kept?)
This is indeed a sentence skipped being removal before submission. It has been removed except for the first sentence, revised as: "Apparently, BC is the major species behind the semi-direct effect in our case study."

• L534 You could also include this mentioned difference in liquid water content in Fig. 12, because it's not shown.
Please see the previous relevant response.

• Fig. 12 It might be useful to include cloud thickness in panel a too. Why are the first hours skipped? If break-up time is important, it could be marked in these plots.
Good point, though we already have too many figures and panels. Stable cloud formed at hour 2.

• L541 I don't completely understand the last sentences. If the CTH is kept nearly constant, dissipation could still occur due to other factors, are all of them unchanged? This could be diagnosed using your difference approach on a sort of budget terms (see van der Dussen 10.1175/JAS-D-13-0114.1 and Ghonima 10.1175/JAS-D-15-0228.1 works on LWP and cloud thickness budget equations, I'm not sure if there's work relating them to aerosol effects).

We assume the comment is on the effect of BC in lowering the cloud top. Please note that we have simulated rather a long time period from nightly stable to daily convective planetary boundary layer. During the latter stage, cloud top has always been evolving as shown in figures. The point is well received. Findings from the two works suggested by the reviewer alongside others have been discussed when necessary. An example is the newly added opening paragraph of Section 4 as indicated in a previous response.

• L550 This being said, maybe the analysis could benefit from comparing not only the domain averaged SW fluxes but by separating the domain in cloud-void and cloudy portions, in order to quantify how much the low cloud fraction effect weights.
Thanks for the thought. This is a largely Ph.D. thesis work. There are more questions than answer as the reviewer perhaps understand well. They could all be addressed in future efforts.

• L535 This decrease in TKE is very interesting. I'd interpret it as limiting entrainment.
Added, thanks!

• L554 How do you know that clouds break up slower? If you mean a state with greater or lower cloud cover, I think that's different from a break-up speed. Still, a cloud cover vs time plot could hint towards that.
• L555 Note that clouds are also larger for the ADEOFF cases at 16 UTC, this is relevant for cloud organization and solar variability.
Response to above two comments: "Fig. A3" has been added as the reference.

• L559 This is a bit confusing. Are these the effect of having BC or of not having it?
It is referred to a realistic situation, so BC is certainly included and thus the root cause.

Typos/Writing suggestions
We very much appreciate a massive and time-consuming effort of this reviewer in providing a detailed list of suggestions to improve the readability of our manuscript. All suggested changes have been done. To save the space, the full list of responding changes is not shown here.