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 #2**

***MAJOR issues:***

***-place results in context with previous studies, both when describing the reference case as well as the aerosol sensitivity studies.***

This has been improved. Relevant previous findings including those from three additional references suggested by both reviewers have been discussed accordingly in the result sections. As an example, the Section 4 now opens with the following introduction:

"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".

***-Language-related issues, like grammatical errors and long and complex sentences make following the manuscript a difficult task. Please correct all grammatical mistakes (only some examples given along the line by line comments) and keep sentences short for the shake of clarity.***

We truly appreciate these massive line-to-line comments from both reviewers. Improving the readability of our manuscript has been done with our best effort as partially demonstrated from our responses.

***MINOR issues:***

***-Please clarify how exactly the tendency profiles are obtained from the radiosoundings. As it is now, I understand that the tendency applied to the LES domain over each hour is equivalent to the difference in T (and q) between two consecutive radiosoundings divided by the time passed between these. My current understanding is that such tendencies are used as proxies for large scale advection of moisture and temperature. If this is the case, I am afraid such tendencies may include not only the evolution of temperature and humidity due to large scale advection, but also the tendencies due to local thermodynamic effects such as radiative cloud top cooling or warming/moistening (after sunrise) due to surface fluxes.***

The reviewer's point is well received. Indeed, the procedure of creating the tendency profiles is largely as the reviewer mentioned above, and we agree that there is a chance that the local thermodynamic effect could affect could be included in the profiles. We have tried our best to minimize such an artifact by forcing the modeled clouds to follow as close as possible the observed quantities such as cloud top and base as well as surface incoming solar radiation. We have added the following sentences in the revised manuscript:
"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, among others, 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."

***-Section 3 goes through the results of the REF simulation in high detail. Readers would find it easier to understand, however, if instead of a description of each result, a more concise section with the most relevant results is presented. This would also allow, as suggested in the fist Major issue, some room to link the relevant results to previous studies.***
We appreciate the point of the reviewer. We have linked certain discussions with previous findings. Certain parts of the discussions have also been made more concisely.

***Line by line comments:***
***L44: Please introduce briefly the direct, semi-direct and indirect effects of aerosols, given they are recurrently mentioned along the manuscript.***
The following has been added after the opening sentence of the second paragraph of Introduction: "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".

***L67: remove dot.***
Done.

***L87: what effect?***
The sentence has been modified as: "…that sedimentation of cloud droplets, determined by droplet size, could affect liquid water path by removing droplets from the entrainment zone, or by lowering the cloud base and creating more heterogeneous cloud structure".

***L135-136: I see surface fluxes first, and sensible and latent heat fluxes later on. If it is referring to same measurements please delete one of the references. If they are different measurements, please clarify.***

The redundant "…as well as sensible and latent heat flux" has been removed.

***L141 analyzer instead of analyzed***
"analyzed" has been removed.

***L146; What is the approx spatiotemporal resolution of these measurements?***
These are airborne measurements so the resolutions are only relative to the path, and these should be indicated in the cited reference. The sentence has been revised to: "Meteorological variables such as temperature, humidity, pressure, and wind speed and direction were also measured by a suite of airborne instruments."

***L164: On top of the surface heating, I would expect also a weaker cloud top cooling due to solar radiation being absorbed at cloud top. If this is the case, please mention it. And if it is not, please explain why.***
"alongside a weak radiative cooling at cloud top (e.g., Ghonima et al., 2016)" has been added, thanks for the reminder!

***L186 remove comma after scales***
Done.

***L187 transport***
Done.

***L190 centered***
Done.

***L204: remove 'completing LIMA'.***
Done.

***L242: Stratus clouds are known to be very sensitive to the vertical resolution near cloud top (Stevens et al 2005). It would interesting to learn a bit more on the sensitivity of this case to the vertical level spacing (if previous numerical experiments with different vertical spacing were performed), and why 10m was decided eventually as the vertical spacing for the lower part of the domain.***
Thanks for the excellent point. We indeed tested using various vertical and horizontal resolutions in early fast runs. The selection of 10-m is based on the performance and most importantly, the diurnal convective PBL that might be difficult to use a generalized highlight zone for finer resolution. Nevertheless, this is an important point to indicate, the following sentences have been added in the revised manuscript:
"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 drastically 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 top during the simulation time and to provide the best economic computational performance for aerosol-cloud interaction with a fully coupled chemistry model".

***L255: Please explain further what is done regarding the tendencies for horizontal wind and the presence or not of a Nocturnal Low Level jet (since it was mentioned in L150 as being closely related to cloud formation).***
"and horizontal wind" has been added.

***-L256 Linked to minor issue n1. It would also be of interest to show the temperature and humidity profile obtained by the radiosoundings in (at least some) of the profiles in figures 7, 8, and 9.***
The point is well received. The figures are already quite messy, thus we are reluctant to add any more profiles to them. Fortunately, DACCIWA measurement data are publicly accessible, so the reader could obtain these profiles.

***Fig4. Adding the 4 phases introduced earlier and described below (stable, jet, stratus, convective) below the time axis would help the reader to locate the phases in this specific case and come back to it when needed along the manuscript.***
***L277: Please add an indicative UTC time for the onset of the convective phase to better guide the reader***
Done for adding the stages in Fig. 4 so the UTC time can be read directly from the figure.

***L318 'But, as the LLSCs…' It is difficult to understand the meaning of this sentence. Please rephrase.***
The sentence has been removed.

***L319 Any thoughts as to why the difference is reduced during the convective phase?***
The sentence has been removed since the difference is rather small.

***L372: I would substitute 'at the cloud' top by 'above the cloud top'. In fact, the strong longwave emission is a source for turbulence in the cloud layer as parcels near cloud top cool and sink along the cloud layer.***
The sentence has been modified to "above the cloud top, deepening the temperature inversion."

***L376: I am confused as the cloud layer is now called to be 'very stable', while one line above it was said to be well mixed.***
Agree that the sentence appears to be inaccurate, removed.

***L381 I am again confused by the use of 'stable' when the plot shows almost near-constant equivalent potential temperature.***
"stable" is revised to "well-mixed".

***L415: I find the concept of breakup confusing. In this line the breakup is said to happen at 16 00, but the lower Figures in Fig 5 at 16 00 suggest that, if the breakup is defined as the first***

***moment with LWP=0 somewhere in the domain, such breakup happenned earlier (even at 12 00, one could argue looking at FIgs 5 c,d).***

The paragraph discussing Fig. 5 (starts at original L296) has been revised to give a better description and to distinct break up from dissipation. This revised one reads as:

"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".

***L401 It is difficult to understand this sentence, please rephrase.***

It has been revised to: "However, the turbulent kinetic energy increases to 0.1 $m^2\ s^{-2}$ throughout the vertical layer from 50 meter above the ground to a level just below the cloud top. This enhancement of turbulence is expected to increase entrainment entering the cloud from above as well".

***L417: Now 16 00 seems to be the end of the breakup. Please clearly state how the breakup is defined and keep it consistent across the manuscript.***

The last sentence has been revised to "This increase coincides the dissipation of the LLSCs and indicates the arrival of the marine inflow".

***L420: I am confused as to at what altitude I should look for a 1.25 m2 s-2 TKE in in Fig 9. Please clarify.***

It should be "0.25", corrected.

***L421. Also in L387 vertical windspeed was assumed to be the driver fro TKE changes. I'd suggest therefore adding the profiles of windspeed, even in an appendix given the already full panel in Figures 7, 8, and 9.***

We very much appreciate the suggestion. However, as the reviewer knows that the small quantity of vertical velocity might not be a pleasure to read.

***L447: dividing, not deriving.***

Done.

***L465: It is very challenging to understand this sentence. Please rephrase and divide into shorter and clearer sentences.***

Revised to “This trend is reversed at 06:00 UTC when the droplets number concentration and radius are equal to 1208 *droplets* $cm^{-3}$ and 6.43 $\mu m$ for POL, 1305 *droplets* $cm^{-3}$ and 6.12 $\mu m$ for REF, respectively”.

***L494: I don’t think ‘inverse layer’ is the right term.***
Revised to “temperature inversion zone”.

***L495 increase instead of exceed***
Revised to “…evaporation rate of droplets would exceed that in CLEAN case”.

***L505 I cannot follow the last part of the sentence.***
It has been removed.

***L511 The explanation is interesting. To validate such hypothesis, it would not take too much effort to compute some metric for spatial SWRADSURF variability. If it turns out to be larger in POL and REF than in CLEAN, then the hypothesis of cloud-holes increasing the shortwave radiation reaching the surface would be reinforced. Further analysis of the cloud layer could also help, since all variables are present in a LES simulation.***
We appreciate the excellent suggestion. As the reviewer perhaps understands, this paper is a part of a Ph.D. thesis study that, as always, raises more questions than answers perhaps, and does leave quite a space to explore in future.

***L529 I would not call dispersion to cloud top and cloud base.***
Thanks! It has been revised as “cloud top and base”.

***L549: I dont see how Fig12c contributes to the sentence. Please clarify.***
Revised, “Fig. 13c and A3”.