First of all, we would like to thank Reviewer#3 for the valuable suggestions and comments. We have addressed all the comments raised by the referee in the response point by point and introduced the corresponding modifications in the manuscript. Below, we repeat the Reviewers' comments in normal font. Our replies are in bold-face and changes in the original manuscript are in italic.

Overview:

This paper reports on an 'exploratory study of the potential alterations to the boundary-layer dynamics as calculated by large-scale models, when the roughness sublayer (RSL) is taken into account.' The authors conclude that (1) the RSL has a very limited effect on CBL dynamics (because the surface fluxes are affected only slightly), and that (2) when comparing simulated mean quantities and transfer coefficients near the canopy top with observations, it is important to account for the RSL. This is a relevant and useful conclusion. I have several remarks, though:

Specific Comments:

1) A major shortcoming is that no quantitative error statistics are used to underpin statements of model performance. One has to judge model performance by looking at figures (eg Fig 8,9) to visually inspect the deviation of the model result (lines) versus the observations (dots). It should be easy to add error statistics (RMSE, R2, bias, ...), and it will make the paper more rigorous.

Answer: We agree with the referee's comment and suggestion. We performed the model vs. observations mean absolute error (MAE) statistics and placed the results in Table 2. We then refer to this error statistics in Table 2 when discussing the results in Figs. 6a, 8a, and 9a.

New:

Table 2: Calculated mean absolute error (MAE) of MXL+MSAD and MXL+RSAD numerical runs with respect to observations. The values of the MAE are presented in units of the corresponding quantities; the values in brackets show the model percentage of the MAE values relative to the daily means (between 08:00 and 17:00 LT) of the observed quantities respectively.

	$\lvert U(z_r)\rvert$ [m s^{-1}]	$C_M(z_r)$ [-]	u_* [m s^{-1}]	$\theta(z_r)$ [K]	$q(z_r)$ [g kg^{-1}]	*SH* [W m^{-2}]	*LE* [W m^{-2}]	*h* [m]
Mean observed	***1.45***	***0.11***	***0.44***	***293.86***	***8.60***	***128.46***	***250.88***	***473.06***
MXL+MSAD								
Mean model	***1.00***	***0.20***	***0.32***	***294.37***	***8.49***	***222.53***	***313.72***	***463.84***
MAE	***0.50***	***0.10***	***0.13***	***0.47***	***0.22***	***87.18***	***59.22***	***34.09***
(%)	***(34.90)***	***(88.75)***	***(31.30)***	***(0.16)***	***(2.62)***	***(67.82)***	***(23.60)***	***(7.18)***
MXL+RSAD								
Mean model	***1.64***	***0.06***	***0.30***	***294.01***	***8.81***	***217.38***	***307.26***	***457.80***
MAE	***0.34***	***0.06***	***0.15***	***0.41***	***0.37***	***81.81***	***52.86***	***35.97***
(%)	***(24.06)***	***(41.53)***	***(34.20)***	***(0.24)***	***(4.41)***	***(63.68)***	***(21.07)***	***(7.60)***

Table 2 shows the overview of the performance of the two numerical experiments with and without RSL representation (MXL+RSAD and MXL+MSAD, respectively) with respect to observations, as quantified by the mean absolute error (MAE). The numerical experiment with RSL representation performs better than the numerical experiment that omits the RSL when representing the wind speed and the drag at canopy height. Both numerical experiments (MXL+RSAD and MXL+MSAD) however underestimate the observed friction velocity. The small difference in magnitude of the friction velocity between the experiments is due to use of different roughness length and displacement height formulation: as stability dependent variables in MXL+RSAD, and as fixed parameters estimated under neutral condition in MXL+MSAD. MXL+RSAD also

represents the potential temperature better than MXL+MSAD at the same level, but slightly overestimate the specific humidity. As expected, the largest MAEs are found for the surface fluxes (e.g. ~60 % MAE for SH with respect to the mean observed SH). Again, note that the observed SH and LE are not the 'true' surface fluxes since the energy balance is not closed (Fig. 3).

2) The authors take the 29-m level as representative for the mixed layer; I tend to disagree with this, so, unless the authors provide arguments for their claim, I would consider the 29-m as being much too low to represent the mixed layer.

Answer: 29 m is the highest measurement level. We agree with the referee that this height is still in the surface layer. However, it is the closed to the mixed-layer characteristics. A deviation will indeed still be present, but since the surface layer is approximately 50 m at its deepest and the logarithmic profiles within the surface layer result in weaker deviations (with respect to mixed-layer values) in the upper part of that layer, the observations won't show strong deviations compared to mixed-layer values. This assumption is supported by the observations of the quantities of the two upper-most levels (23m and 29m). For instance, the slope derived from the potential temperature or specific humidity at 23 and 29m is less than 1% with respect to the vertical coordinate.

The following text is added in the manuscript to better explain the assumption of selecting the 29 m as a representative mixed-layer height in this study:

(New): The level of 29 m is considered to be representative of the mixed-layer values, since it is either located within the mixed layer or in the upper part of the surface layer, where deviations compared to mixed-layer values are small. Therefore, we employ it as the most representative of the mixed-layer characteristics.

3) On the days considered in this study, CBL dynamics appears to be dominated by large-scale effects (advection, subsidence, ...). (See also p.10: "The analysis presented in Fig.4 shows that the complex boundary-layer structure at the CHATS site is highly dependent on the large-scale effects, including subsidence, advective cooling and moistening, as well as entrainment of dry air from the free troposphere.") Hence, I am wondering whether this case is the most appropriate for studying the impact of the RSL on the CBL.

Answer: As mentioned in the manuscript, in selecting the most appropriate days to carry out our research we define the following criteria: well-mixed boundary layer cloudless conditions, well-developed RSL (southerly winds during the entire day to maximize the effect of the footprint). In the entire period during the observations, mesoscale effects (e.g. horizontal fronts) were relevant, having a large impact on the diurnal variability of the measured quantities (Mayor 2011), similar as in our case studies (e.g. potential temperature drop of 1-2 K at around noon). These mesoscale effects have been previously studied and analyzed over the California Valley region where very active advection and topography driven flows where found (e.g. Zaremba; Carroll 1999; Bianco et al. 2011). We therefore took this opportunity to study the canopy effects on the CBL dynamics by also taking the large-scale effects into account in a systematic way.

Placed in more general context, there are several reasons why we chose the CHATS dataset as the main observational evidence to study the effects of RSL on the CBL-dynamics. High-quality measurements of the thermodynamics (and chemistry, used in our current work) is the first reason. Another reason is related to the canopy homogeneity in combination with the observed, relatively constant- wind direction, which allows a well-developed roughness sublayer above the canopy. This is convenient for studying canopy-atmosphere interaction in an 'idealized' way, since an irregular shape and distribution of the canopy would bring additional uncertainty in the turbulence structure within and above the canopy (Raupach et al. 1996; Finnigan et al. 2009).

4) The authors say on p9l1-2 that "modelled SH & LE are likely to be the more correct values" (as compared to the observed values). I agree with that statement, but then I don't understand why they use data that are clearly not correct (i.e., the energy balance isn't closed) to validate their model. In fact, now you have a situation where the authors say, 'OK, the data aren't entirely correct, but we conclude that the model is performing fine anyway'. Hence I also question the statement "The comparison presented here confirms that our modelling system is capable of reproducing the diurnal variations in radiation and surface energy balance with sufficient accuracy" (p9l4-6).

Answer: Here we quote Foken (2008) with respect to energy balance closure: "The comparison of observational data and model output remains problematic". As discussed in Foken (2008), the reasons for the energy balance non-closer are related to the large scale turbulent structures, which the measurements in the surface layer are not able to capture. Due to this reasons, some studies even suggested that the energy balance (EB) closures should not be used as a quality criteria for turbulent fluxes (Aubinet et al. 1999). Nevertheless, we still use the sensible and the latent heat here, since we would like to compare the surface fluxes calculated with and without RSL parameterization, as shown in Table 2. We agree however with the referee's question about the statement "The comparison presented here confirms that our modelling system is capable of reproducing the diurnal variations in radiation and surface energy balance with sufficient accuracy". To make it more precise, we therefore have modified this statement into:

New: "The comparison presented here confirms that our modelling system is capable of reproducing the diurnal variations in radiation with sufficient accuracy. As in many other studies (see Foken 2008), the observed surface energy balance remains not closed, but with the deviations of similar magnitude as observed in other studies above high canopy.

Minor remarks:

5) p1l28: "turbulent exchange of energy, momentum and matter between the Earth's surface and the free troposphere" - in this description you short-circuit the atmospheric boundary layer, perhaps better to replace 'free troposphere' by 'lower atmosphere'?

Answer: we agree with the referee's suggestion and replaced 'free troposphere' by 'lower atmosphere'. We consider this term more robust in the context of the statement.

6) p2l29: I presume 'potential' ought to be 'potential temperature'

Answer: we corrected to 'potential temperature'.

7) p3l20: It would be useful to include a figure (map) showing the measurement site and surroundings

Answer: we agree with the referee that it would be useful to include a figure (map) with the measurement site and surroundings. However, those figures and maps are already presented in the cited literature (Patton et al. 2011; Dupont; Patton 2012). Thus, in order not to overload the manuscript with figures, we have decided just to refer to the figures in these papers.

8) p4l14: sublayers => sublayer

Answer: 'sublayers' corrected to 'sublayer'.

9) p5Eq6: the slash in Eq 6 is not OK (should be slant and not vertical)

Answer: The referred vertical bar is one of the two vertical bars around $U(z_r)$ to denote that the modulus is used, similar to Eq. (3). For clarify a whitespace is inserted between the variables in Eq. (6).

10) p5l20: ’heightd’ => ’height d’

Answer: ‘heightd’ has been corrected to ‘height *d* ’ .

11) p6l8 and l11-12: ’strong unstable’ => ’strongly unstable’

Answer: ’strong unstable’ has been corrected to ’strongly unstable’

12) p7l3: what is ’toggled large-scale forcing’?

Answer: the ‘toggled large scale forcing’ refers to including or omitting subsidence, advection, free tropospheric drying at certain moment based on observations. We will delete this term however, since the sentence is clearer without it.

13) Fig.2: Observed G (soil heat flux) appears small (especially given the sparse canopy)- is this the value at the ground surface or at 5 cm depth? This could make a big difference, and explain the model-vs-observation discrepancy (and partly explain energy balance non-closure).

Answer: the soil heat flux (G_m) is measured at z = 5 cm depth. Then, the soil heat flux at the surface *G* includes the heat storage in the soil, and is calculated as (Oliphant et al. 2004):

$$G = G_m(z) + C_s \frac{\Delta T_s}{\Delta t} z,$$

where T_s is average soil temperature above the heat flux plate, *t* is time and C_s is soil heat capacity (see Oliphant et al. (2004) for details about the method for estimating C_s)

To be clearer, we have added the following sentence in the text:

New: “Note that presented G accounts for the heat storage in the soil, as calculated following Oliphant et al. (2004).”

14) p10l14: ’on time’ => ’with time’ (?)

Answer: ’on time’ has been corrected to ’with time’

15) p.10: On page 10 you make a lot of assumptions: ’probably related to the sea breeze’, ’probably related to drying associated with entrainment’ etc..., using these to (try to) explain the simulated profiles’ tendencies. All these ’probablies’, are not very re-assuring and highly speculative. Maybe reconsider how you present all this in a more convincing way.

Answer: We have deleted the “probably “terms in our statements. We have also added relevant previous literature to support our hypothesis instead.

New: “We hypothesize that the rapid temperature drop before noon is related to the advection of cold air, due to a sea-breeze front, which is frequently observed around noon at the CHATS site (Mayor 2011).”

New: “After this increase, q remains steady until the end of the day (17:00 LT). We related this behavior of q after noon to the drying associated with the entrainment of free tropospheric (drier) air into the boundary layer, which can be driven by returned flow over the complex topography (Bianco et al. 2011). ”

16) Table A1.1: Mentions ’lateral’ wind speed component several times, shouldn’t this be ’latitudinal’ instead (to be consisten with the ’longitudinal’ component)? Also: for the quantity CGsat in Table A1.1, the units seem odd, please check.

Answer: although the coordinate are presented in latitude and longitude, the term "lateral" is often used in the literature to define winds "from the side". We therefore prefer to use this term. As for the second part of the comment, we thank the reviewer for this specific comment about the units of the quantity of the saturated soil conductivity of heat is in units [J m^{-3} K^{-1}**], as stated in the table. This variable, modified for the soil moisture content, is multiplied by the soil heat flux to yield the soil temperature tendency.**

17) Fig 5(c) shows the u component of the wind speed twice, I guess the labelling should be changed to include both u and v

Answer: The referee is correct. We have made new figure and corrected the typo.

18) p15l6: "By applying the roughness sublayer formulations within the surface scheme of the model, the representation of the diurnal evolution of the boundary layer state variables and the corresponding drag coefficients at the canopy height is improved." => this isn't so clear, e.g. in the case of specific humidity rather the contrary would appear to be true (Fig.9a). Again, such statements should be underpinned by quantitative error statistics (see remark above).

Answer: We agree with the referee that we should be more precise in our statements. In that respect, we modify the statement:

New: "In our modelling framework, and in general in the coupled land-atmosphere models, the representation of the surface fluxes is locked and controlled by the boundary conditions. The sensible and latent heat fluxes are bounded by the surface available energy, and the momentum flux is constrained by the pressure gradient and the entrainment of momentum, the latter dependent on the boundary-layer growth. In consequence, adding a roughness-sublayer representation in the surface scheme of the model, alters the partitioning of the surface fluxes (e.g. sensible and latent heat) through the altered roughness length and displacement height. Specifically for our case studies, the canopy's impact on convective boundary-layer dynamics is relatively minor, due to its small effect on modelled surface fluxes and the bulk boundary-layer properties well above the canopy ($z > 2h_c$). The tall canopy however strongly affects the mean gradients and transfer coefficients within the roughness sublayer. Thus, considering the roughness sublayer parameterization is important when comparing observations and large-scale model outputs of the mean quantities near and just above the canopy."

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