Responses to the interactive comment on "The effect of low density over the "roof of the world" Tibetan Plateau on the triggering of convection" by Jun-Ichi Yano (Referee)

We would like to thank the referee for his comments on the manuscript. The following is a point-by-point reply to the comments.

1. Surface heat flux

We assumed that the sensible heat flux H is invariant with the density (or elevation) in LES simulations and appendix A. Based on this premise, we discussed the effect of low air density over the Tibetan Plateau (TP) on the formation and development of cumuli. This hypothesis was based on our observational data analysis of the third Tibetan Plateau Experiment (TIPEX III). The analysis showed that for unstable stratification the buoyancy term increases with increasing elevation (see Fig.13 c, .Wang Y. J. et. al., *J. Geophys. Res. Atmos.*, 121, 9540-9560, doi: 10.1002/2016JD025401, 2016.). This implies that the surface heat flux basically does not vary with elevation in unstable stratification.

The bulk transfer formula mentioned by the referee is

$$H = \rho C_p C_h U \left(T_g - T_a \right) \tag{1}$$

Where ρ is the air density, C_p is the specific heat of air at constant pressure, C_h is the bulk transfer coefficient for heat, U is the mean wind speed at 10 m, and T_g and T_a are the surface skin temperature and air temperature at 2 m, respectively. Equation (1) is a semi–empirical formula for calculating H. In our case, U increases with increasing elevation over the TP. C_h depends on many factors (e.g. underlying surface conditions, wind speed, stratification, etc); thus C_h is a parameterization for use in numerical models rather than a constant, and H data can be used to estimate C_h from equation (1). We do not discuss variations of C_h and U for varying elevation.

The referee asserts that ρ (or elevation) has nothing to do with C_h, U and T_g-T_a. If all these variables (C_h, U and T_g-T_a) are fixed, $\overline{w'\theta'}$ would not change with air density and H would then decrease with decreasing ρ. However, observations show that T_g-T_a significantly increases with increasing elevation over Asia, Australia and South America (Pepin et al., 2005). Xu et al., 2010 observed that if elevation increases from 1000 m to 4000 m over China, T_g-T_a increases by an average of about 50% in spring and summer. Wu et al., 2017 came to a similar conclusion by using 73 meteorological stations data over the TP. Although both T_a and T_g decrease with increasing elevation over the TP, T_a decreases faster than T_g, so that T_g-T_a increases with elevation. It should be pointed out that all the above results derived from daily mean values, the phenomena that T_g-T_a increases with an increasing elevation will be more obviously in daytime in summer for the case of our study. It’s the surface energy budget that determines the surface heat flux. The temperature difference T_g-T_a is a response to the forcing expressed by the surface energy budget.

2. Logical coherence and geographical focuses

Thanks for the referee’s suggestion. In order to make the Figure 1 (a) more readable and to fully demonstrate our point, we changed the transparency of the dots (as suggested by referee 3). As we described in our manuscript, Figure 1 (a) shows that more low cloud exists over the high altitude area of the TP with low ρ_{2m} and low RH_{2m} ($50\% < RH_{2m} < 70\%$), and figure 1 (b) indicates that high LCC (low cloud cover) occurs mainly over the mid-eastern TP rather than eastern China monsoon region

(ECMR) during summer. From ERA-Interim reanalysis data, Figure 1 (c) indicates that *LCC* might be greater than 35% north of 30 °N over the mid-eastern TP for RH_{2m} < 70%, and this is not the case at low altitude. These are only statistical results, so we use large eddy simulation (LES) to partly explain the reason for this phenomenon. In order to clearly state the questions to be resolved by further analyses, and improve the coherence of our manuscript, the corresponding parts of the sentences will be revised. Here we explain the reason why we only map the values over China rather than global. The title of the manuscript is "The effect of low density over the "roof of the world" Tibetan Plateau on the triggering of convection"; thus we mainly focus on the TP and surrounding region. TP has a uniquely large area with an average altitude above 4000 m, a land surface that absorbs a large fraction of incoming solar radiation, and a relative humidity over the mid-eastern TP that is typically larger than 50% in summer. Furthermore, we have extensive data over the TP. However, the results should have general application to regions with similar conditions. Therefore, we plot Figure. S1 and S2 from ERA-interim reanalysis data in East Asia in summer, here the samples include all the grids on land within the latitude and longitude range (70 °E-140 °E, 0 °N-60 °N). Figure S1 (a) shows that *H* has no obvious correlation with elevation. Both T_g-T_a and $\overline{\boldsymbol{w'\theta'}}$ significantly increase with increasing elevation as shown in Figure S1 (b) and (c). The results from ERA-interim data are consistent with our previous data analysis.

The referee asked why we choose density rather than other physical quantities (.e.g. pressure, potential temperature, etc). In fact, all the LES sensitivity tests in our manuscript have taken into account the variations of pressure and potential temperature for varying air density (or elevation). The LES sensitivity tests (1.2ρ_{CON}, 1.4ρ_{CON}, and 1.7ρ_{CON}) have the same initial profiles of temperature T and relative humidity RH from the surface to 6 km above the ground level (Sorry, there is a mistake in line 504, it should be temperature T rather than virtual potential temperature θ_v). This manuscript mainly discusses the growth rate of the convective boundary layer and the effect of thermal turbulence on the formation and evolution of cumulus in daytime, so we mainly focus on the effect of surface sensible heat flux especially for varying air density. This is also the reason why we set the same initial profiles of temperature T, relative humidity RH and large scale forcing for LES sensitivity tests (1.2ρ_{CON}, 1.4ρ_{CON}, and 1.7ρ_{CON}). In order to illustrate our point further, the relationships among monthly means of LCC, $\overline{\boldsymbol{w'\theta'}}$ and RH_{2m} (RH_{2m} > 50%) from ERA-interim data are also plotted as shown in Figure S2. With the same $\overline{\boldsymbol{w'\theta'}}$, LCC increases with an increasing RH_{2m}. On the other hand, with the same LCC (e.g. LCC = 40%), RH_{2m} decreases with an increase $\overline{\boldsymbol{w'\theta'}}$. Larger $\overline{\boldsymbol{w'\theta'}}$ at high elevation (or low density) region increases the moisture transport from the subcloud layer into the cloud layer. With the same LCC for different air densities, the average relative humidity with higher air density will be greater than that with low air density.

Appendix A:

In the main text, the role of environmental descent, $w_e \neq 0$, is explicitly taken into account (cf., Eq. 2). However, this effect is simply neglected (cf., Eq. A1) without any justifications.

The purpose of Appendix A is to demonstrate via a simple model the sole effect of density on the buoyancy flux and its impact on the growth rate of the CBL depth independent of other factors. The results of w_s for all the LES experiments in this manuscript are identical from the surface to 6 km, which are derived from ERA-interim reanalysis data (Figure B2). As shown in Figure B2(c), from 10:00 LST to 13:00 LST, maximum $w_s \approx 3$ hPa hour$^{-1} \approx 10^{-2}$ m s^{-1}. The rate of change of h dh/dt and the entrainment velocity w_e are about one order of magnitude larger than $w_{s;}$ thus dh/dt mainly depends on w_e rather than w_s. In order to make the LES simulated profiles as close as possible to the observed profiles from radiosondes (Please refer to Figure B3), we take into account w_s in LES experiments. However, if the effect of w_s is neglected in LES experiments, the main conclusion in this manuscript does not change.

Throughout the Appendix, the prime sign is missing everywhere for indicating the eddy values. The temperature, T, must be replaced by the potential temperature, θ, throughout.

We will add prime sign. T is replaced by θ, but this modification will not affect the conclusion.

Eq. (A9) does not make any sense.

Eq. (A9) is an intermediate step in the derivation.

Eq. (A11) is clearly not a solution of Eq. (A10), thus all the subsequent discussions are irrelevant. In fact, a solution for Eq. (A10) is given by Eq. (5) with $\alpha = 2$, as explicitly stated in Zhu et al (2005). [It is obvious that the authors are citing this paper without reading it. Also note that this solution (5) is earlier derived by Betts (1973, see his Eq. 42). However, the authors simply fail to give such a simple credit.]

In fact, the solution given by Eq.(A11) is equivalent to Eq.(5) with $\alpha = 2$ (i.e., $w_s = 0$), multiplied by $\boldsymbol{h}^{((1+\beta_1)/\beta_1)}$ with an added constant of integration C.

Specific Comments

Line 88, EDDYPRO: It is a totally non–essential matter for readers what software the authors have used. However, it is crucial to present how turbulent fluxes are actually computed based on which theory, formula, etc.

Thanks for your suggestion. EddyPro Software is used for processing raw eddy covariance (EC) data from the sonic anemometers and gas analyzer to compute atmospheric fluxes of trace gases such as CO_2, H_2O, CH_4, as well as energy, which is widely used in atmospheric sciences, ecology, and related topics. In the manuscript, we pointed out that turbulent fluxes were computed using the eddy covariance technique, and we have provided the website address of EddyPro. We can add a detailed description of theory, formula and method, etc. if that is deemed necessary.

Line 107, LCC: Please spell this out.

We have spelled LCC in line 72.

Sec. 3 (Lines 106–120): Please re–write the text to the point. One has to read it dozen times to understand this rather conjugated text to understand what the authors really want to say. As it stands for now, the remaining part of the main text could be read equally well even by totally removing Sec. 3.

Thanks. We will revise the relevant content, and improve the text, so that the readers will have a clear understanding of the main idea of the text.

Lines 137–146: This paragraph is not linked to any other part of the text. One can simply remove it.

As discussed in section 3 and 4, one of the important variables for a thermal that reaches the lifting condensation level (LCL) is the relative humidity at the top of the mixed layer (ML) RH_h. The paragraph between lines 137–146 is mainly to illustrate the relationship among the height of the mixed layer h, RH_h, and the relative humidity at the top of the surface layer RH_0, so we think it important to leave it in place.

Line 169: Sullivan et al (1998) is just one of many papers quoting an earlier claim with a constant 0.2. This is just an example of arbitrary quotation system that the authors use. The earliest claim of this value that I could dig is Ball (1960). However, the authors must search carefully.

Here we quote Sullivan et al (1998) because this includes a fairly recent summary of extensive research over the years that have reinforced the validity of this value, as well as quantifying its dependency on other parameters, not because it is the earliest claim of this value. We do not find the content that Ball (1960) used this value, please tell us the specific position.

Eq. (5): Indeed this is a valid solution for Eq. (A10) with $\alpha = 2$ when an environmental descent is absent, i.e., $w_e = 0$. However, Zhu et al. (2002) did not derive (cf., Line 169) this expression as a general solution for, say, the system with Eq. (2). They merely suggest it as a phenomenological generalization of the solution with $\alpha = 2$. This is an example of the present authors' system of arbitrary quotations of earlier results out of context.

Based on Eq.(5) of Zhu et al. (2002), α is a subsidence-dependent parameter whose likely maximum range is between 1 and 2. For $w_s = 0$, $\alpha = 2$; and for $dh/dt = 0$ (i.e. $w_e + w_s = 0$), $\alpha = 1$.

Lines 179–181: This premise is hardly justified, which also makes the present manuscript invalid. A work based on such an ill–posed premise should not be published.

We have previously answered this question.

Eq. (7): This is just another example of the present authors' system of arbitrary quotations. This formula is just a curve fit out of an LES, and there is no reason to believe its universality.

Here we compare the parameterization results with LES, not only to describe the differences between them, but also point out that smaller density favors the formation and development of cumulus.

Line 311, "Water vapor is relatively abundant over ECMR in summer": This is a very odd manner to start a final section of a paper considering the role of the low density with a high altitude.

Thanks. The relevant content will be revised. The purpose of the text (Line 311-313) is to illustrate the fact that the relative humidity over ECMR is not less than that over the mid-eastern TP, but high *LCC* occurs mainly over the mid-eastern TP rather than ECMR during summer.

Lines 316–317, "This density effect is demonstrated with a simple mixed–layer model in Appendix A": NO

We have previously answered this question.

Lines 321–322, "Stronger ascending motions": This is nothing to do with a low density

The stronger ascending motions within the thermal are mainly caused by larger $\overline{(w'\theta_v')}_s$ for low density.

Lines 452–454, "a simple microphysics scheme (Grabowski et al 1998) that considers the impact of the relatively low temperature over the TP": NO

Thanks for your suggestion. Of course, there are shortcomings in this microphysics scheme, but the selection of microphysics scheme does not affect the main conclusion in this manuscript.

Line 477, "corrected": Please describe what kind of corrections are made

As described in Appendix B, in order to make sure the results of the LES simulation are close to the observations, we change the original *H* and *LE* results calculated by the eddy covariance method.

Fig. 4: plots are so scattered that I do not think that we can draw any sensible conclusions out of them.

Due to the complexity of the problem itself, the plots are scattered, but the fitting coefficients in the LES sensitivity tests have an obvious trend for varying thermal turbulence and relative humidity. The coefficient of determination is important, which indicates the need for more work in the future.

References

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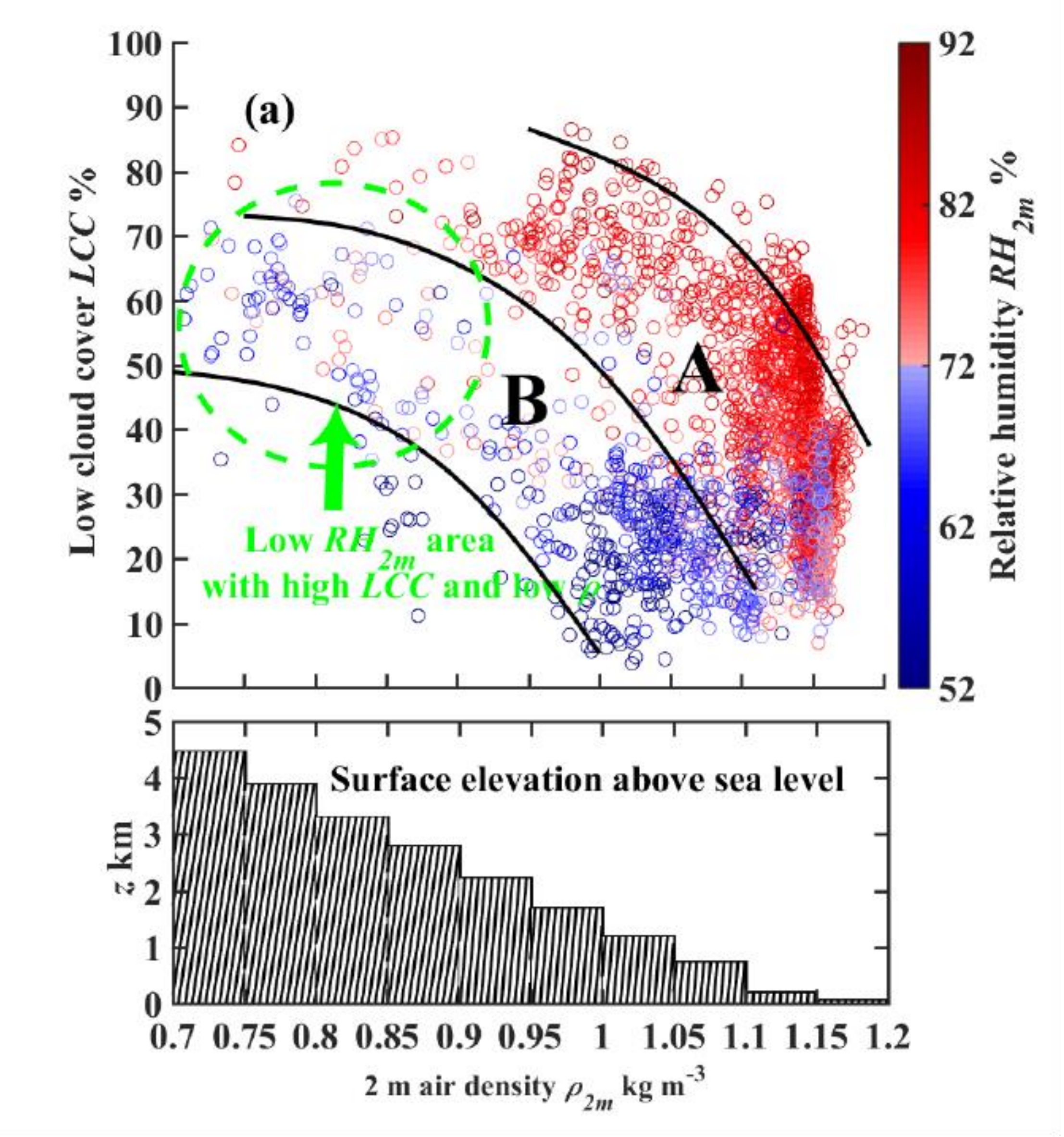

Fig. 1 (a) The relationships among monthly means of LCC, ρ_{2m} and RH_{2m} observed by the AWS in summer. The samples are divided into two groups: RH_{2m} > 72% (red dots) and RH_{2m} < 72% (blue dots). Region A and region B generally correspond to RH_{2m} greater than and less than 72%, respectively. The histogram shows an approximate relationship between ρ_{2m} and surface elevation above sea level z at the bottom of

Figure 1 (a).

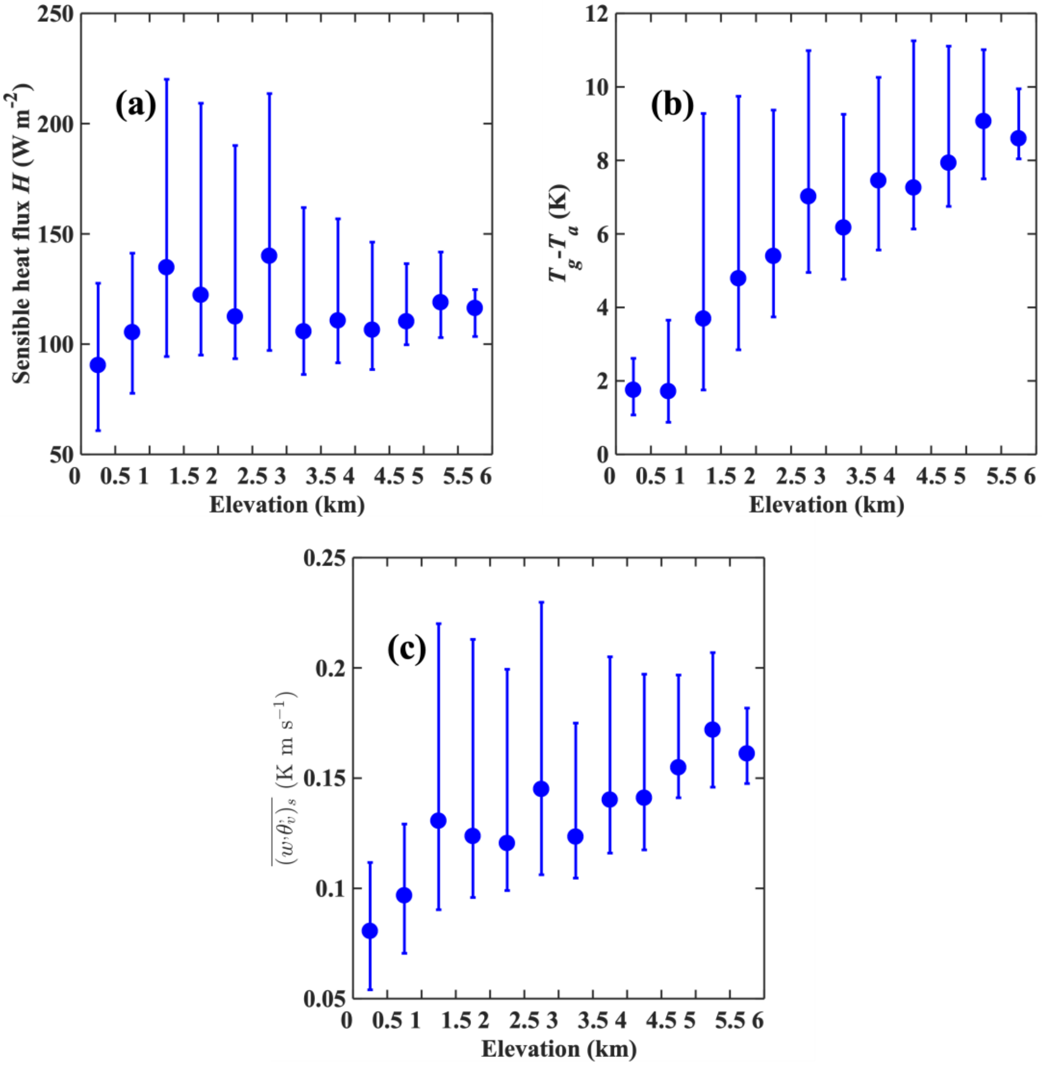

Fig. S1 The relationship between elevation and (a) H, (b) T_g-T_a, (c) $\overline{w'\theta'}$ from ERA-interim data from about 9:00 LST to 15:00 LST (3:00 UTC to 9:00 UTC) in East Asia in summer. The solid circles denote median values. The error bars represent 25% and 75% probability values respectively.

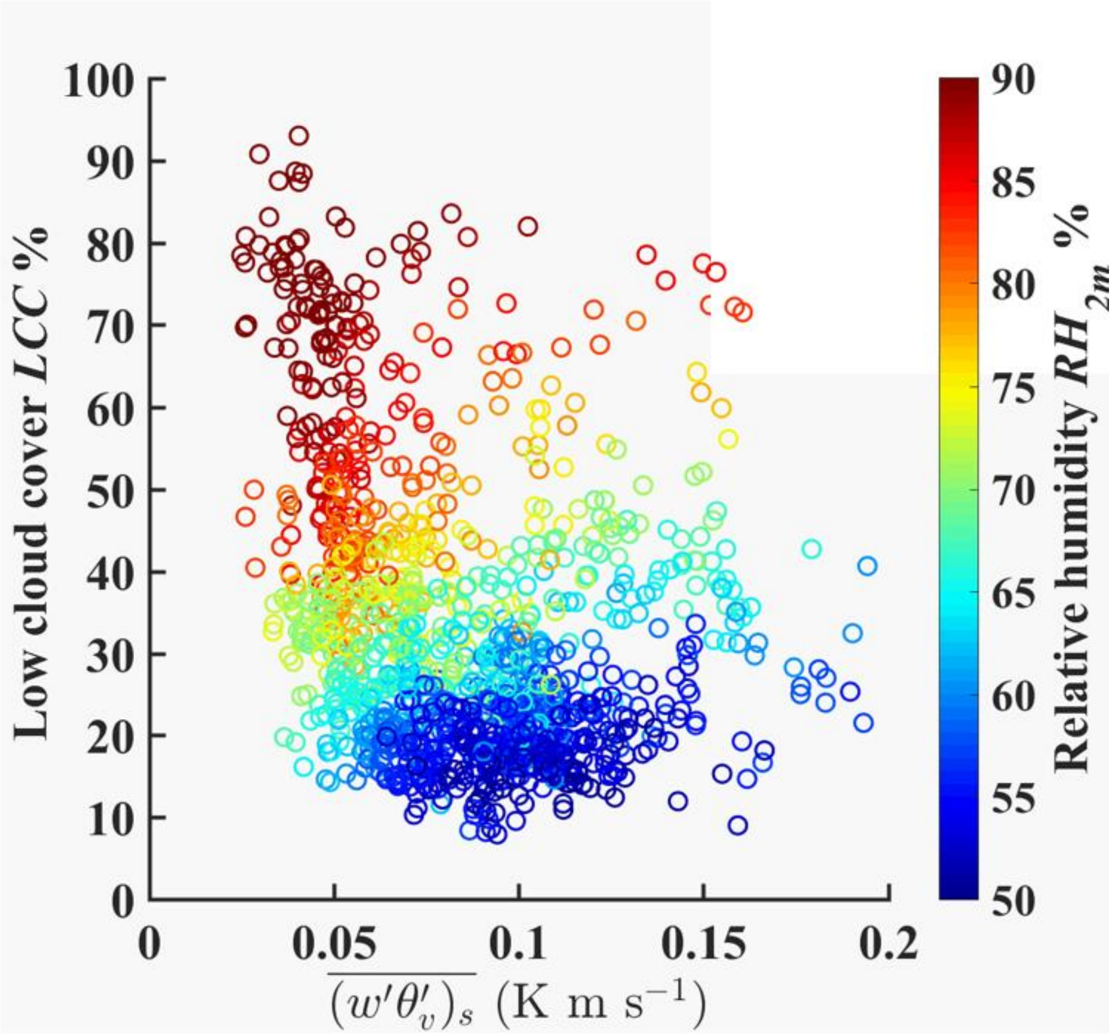

Fig. S2 The relationships among monthly means of *LCC*, $\overline{w'\theta'}$ and RH_{2m} from ERA-interim data from about 9:00 LST to 15:00 LST (3:00 UTC to 9:00 UTC) in summer.