

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-370

Anonymous Referee #2

Referee comment on "Estimation of the terms acting on local 1□h surface temperature variations in Paris region: the specific contribution of clouds" by Oscar Javier Rojas Muñoz et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-370-RC2>, 2021

In the ACP manuscript entitled "Estimation of the terms acting on local surface one-hour temperature variations in Paris region: the specific contribution of clouds", authors have used an analytical approach to derive temperature and compared with observed variability at SIRTAs sites on different time scale (diurnal, monthly, seasonal) and their analyses in particular, cloud radiative effect on temperature variability was further assisted by lidar scattering ratio observations. The manuscript is well written and contains some valuable information on the ABL thermodynamic variability under CL and CS conditions. I only a large number of specific comments and authors should consider addressing those during revisions. 4 different PBL parameterization schemes and compared the findings using observations of temperature, wind and PBL height in the YRD region of China.

Abstract: First two lines fit well for an introduction. What is the overall major scientific problem regarding the goal of this manuscript should be first spelled out in the first part of the abstract, instead of generic information on surface temperature variability?

Intro: Authors should clearly mention the need for such a model which is based on a lot of observations. How can this model help develop and improve surface layer parameterization scheme. Etc.

Line 28: Azores?

Line 55: This is result and too early to spell out here. Please remove.

Line 65: Set an example for each with references for "climate variability and extreme local

events". I suggest 2006 drought in EU.

Line 72: Repeated. Delete please.

Line 105: Which lidar and what is the temporal and vertical resolution of lidar instrument here. Some details could be found in Koffi et al. (Evaluation of the boundary layer dynamics of the TM5 model over Europe) on different EU sites on this.

Line 118: Unless it has been established before, this is too early in a manuscript. Please remove.

Line 171: Please use the term "combined"

Line 182: "mixing with an atmosphere of higher levels...". If so, then how does it represent a high positive correlation coefficient found in other literature where the authors have performed regression analyses of MLD and surface temperature. See Seidel et al. 2010, 2012 (Climatology of the planetary boundary layer over the continental United States and Europe). This has an important implication. Please clarify. I think above statement need to justified and corrected.

Line 186: Please quantify (remains low).

Figs. 2a and 2b: x-axes scale limits need to be symmetric; otherwise, one cannot justify the statements made in this regard.

Line 189 and associated figure: Since observation is the reference here for the analytical model, please exchange the x and y axes of Fig. 2c.

Fig. 4: Units are missing on the color bar scale limits. Please use symmetric color bar scale limits as well like in Fig. 4f.

Section 4.2: It will be important so that the authors should focus on the analyses of temperature variability during morning and evening transition periods which are the two most complicated phases of the diurnal cycle of temperature over land and this is also important for trace gas variability as well since the ABL interacts with upper layers in phases (e.g., Lee et al. Meteorological controls on the diurnal variability of carbon

monoxide mixing ratio at a mountaintop monitoring site in the Appalachian Mountains).

For the above, I suggest rather than each hour temporal variability, author could build a key temperature growth rate (between sunrise and 14 UTC) and compare that single parameter in different seasons and years (model vs obs).