

Interactive comment on “Integrating canopy and large-scale atmospheric effects in convective boundary-layer dynamics during CHATS experiment” by Metodija M. Shapkalijevski et al.

Anonymous Referee #3

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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:

* 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

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the observations (dots). It should be easy to add error statistics (RMSE, R2, bias, ...), and it will make the paper more rigorous.

* 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.

* 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.

* The authors say on p911-2 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" (p914-6).

minor remarks:

p1128: "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'?

p2129: I presume 'potential' ought to be 'potential temperature'

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p3I20: It would be useful to include a figure (map) showing the measurement site and surroundings.

p4I14: sublayers => sublayers

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

p5I20: 'heightd' => 'height d'

p6I8 and I11-12: 'strong unstable' => 'strongly unstable'

p7I3: what is 'toggled large-scale forcing'?

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).

p10I14: 'on time' => 'with time' (?)

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 'probabilities', are not very re-assuring and highly speculative. Maybe reconsider how you present all this in a more convincing way.

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

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

p15I6: "By applying the roughness sublayer formulations within the surface scheme of the model, the representation of the diurnal evolution of the boundary layer state

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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).

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