Review of:
"**Study of a prototypical convective boundary layer observed during BLLAST: contributions by large-scale forcings",**
by H. Pietersen, J. Vilà-Guerau de Arellano, P. Augustin, O. de Coster, H. Delbarre, P. Durand, M. Fourmentin, B. Gioli, O. Hartogensis, M. Lothon, F. Lohou, D. Pino, H. G. Ouwersloot, J. Reuder, and A. van de Boer**.**

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General comments.
Why did the authors not examine a longer time series of the advective terms? I have no great love of 'golden day science.'

This is an extremely cautious paper—the authors do not try to demonstrate whether or not their findings are new or useful. They set up a small challenge for their models, meet their low expectations and then come up the excessively general conclusions.

The abstract ends with: "*We conclude that the prototypical CBL can still be used as a valid representation of the boundary-layer dynamics near regions characterized by complex topography and small-scale surface heterogeneity, provided that surface- and large-scale forcings are well characterized."*

Why do they select a single day from their field project and proudly label it "*representative*"? How can a day without clouds be called representative? The authors have to provide some context. Is it never cloudy in Spain? This is relevant, because folks are not going to be surprised that a box model works fine when, well.., when the box model works!

What is the 'news' here? Why are the many authors surprised by the fact that "*…this pattern suggests that not only synoptic scales exert their influence on the boundary layer, but also mesoscale circulations*." The authors should be more forceful in pointing out their *new and surprising* results. If everything to say is in accord with what we already expected, cannot we say that in a sentence. They key here is determining what situations are amenable to this modeling approach now often these occur, and whether or not this effort may sometime lead to improved forecasts of surface conditions, pollutant dispersal or local climate.

However, I am pleased to see the authors demonstrating (again) the wisdom of exploring relatively simple models of the convective boundary layer (CBL).

Specific comments.

1. "*…the values for turbulent kinetic are lower…*" ←include **energy** here.

2. In Figure 3, show in the wind direction scale the orientation of the valley.

3. Figure 12: "*However, inside the boundary layer, the modeled fluxes are roughly twice as high as the observed fluxes. Both model and observations do show latent heat flux profiles that are almost constant with height indicating that the evaporation at the surface is compensated by the drying at the entrainment zone*." In Figure 8 q doesn't look too 'constant' to me over the day. Please elaborate your argument.

3. Figure 14: "*For TKE, we do find a fast decay rate around the time the sensible heat flux becomes zero*." Why do the authors use the surface TKE to represent the entire PBL? We know the surface decouples first, but the models, presumably meant to represent the entire PBL die off first. Please elaborate you explanation.

4. "*As shown in Fig. 6a, there is a large amount of scatter between different estimates. In analyzing the observations in more detail, we find that, even if we do not take outliers into account, the differences in boundary-layer height can be in the order of 100 m*." How does this point come into the discussion of 'good agreement' between model prediction and observations of CBL thickness?