

## ***Interactive comment on “The effect of low density over the “roof of the world” Tibetan Plateau on the triggering of convection” by Yinjun Wang et al.***

**Anonymous Referee #2**

Received and published: 5 August 2019

The manuscript presents an interesting approach to looking at variations in convective activity over large orography, based on the corresponding variations in air density at the surface. The analysis is based on a combination of observational data, global reanalysis, and large-eddy simulation (LES). The results are informative and of sufficient relevance to merit publication in ACP, although I think there are a number of issues which should first be addressed:

C1

### **General comments**

1. Although the observation and reanalysis data used are briefly described in Section 2, there appears to be no section describing the LES model in the main paper. Without knowing which model is used, its domain size and resolution and major parameterisations etc., it is impossible to understand the likely impact of model errors in the results presented here. While it's fine to move further detail to an appendix, the basic information should be presented in the main text prior to the presentation of any results.
2. In various places, the variation of behaviour with air density is considered, but it is often not made clear to what extent this means the fixed variation of density due to the orography, or the synoptic variations which may occur at any given location.
3. There is considerable discussion of relationships between density  $\rho$  and CBL height  $h$ . However, it is not obvious that comparing a geometric height/thickness measure across large variations in density is appropriate. Consideration should be given to how the relationships would look with a mass-based measure of thickness (corresponding to a pressure-based rather than height-based vertical coordinate). The same applies to the question of variations in vertical velocity with density – these relationships may look very different between geometric vertical velocity and pressure velocity).

### **Specific comments**

**p.3, lines 75–76** Are “very small” horizontal scales of “tens of metres” adequately resolved by the LES configuration used in the study?

C2

**p.4, line 119** Kelvin are not degrees, i.e. the unit is K, not °K.

**p.454, lines 454–455** A resolution on the order of 6km is not an LES model, but in the realm of the highest-resolution global NWP models, or "cloud-system resolving models". Or should this read "A *domain size* of 6.4km × 6.4km × 6.0km..."? This would make the horizontal and vertical resolution 250m and 40m respectively, which seems more reasonable, but still unable to resolve well the "tens of metres" scale referred to on lines 75–76.

**Figures 2, 4** The panels should be formatted so that legends do not obscure the actual data points.

**Figure 3** The (a), (b), (c), (d) labels in white can barely be read against the patterns in the actual plots. These labels should probably be moved outside the plot area.

**p.17, lines 520–527** The original datasets used are stated to be available "upon request", rather than being deposited in a readily-accessible archive. I would draw attention to this, but leave it to the editor's discretion whether this is sufficient to meet the journal's data policy without further justification.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-273>, 2019.