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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.

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The lead author posted his response on 30 July to my review comments on 10 July. This is, unfortunately, hardly satisfactory by responding only to two issues, neglecting all the others that I raised. The actual responses on these issues are hardly satisfactory, either.

• Concerning their assumption of a constancy of the sensible heat flux with altitude, the lead author somehow decides to invoke the surface heat budget for justifying it. However, this argument is hardly satisfactory: it essentially claims that since the net radiative heating, R_{net}, approximately remains constant with altitude, all the three terms in the right hand side of the budget given by Eq. (1) must also individually remain the same order. Of course, in reality, these three terms would respond to the net radiative heating differently in different situations, thus such a conclusion does not follow. It is also important to realize that though the solar radiative flux reaching the surface may approximately be constant with altitude, the longwave radiation would change with altitude. Thus the net radiative heating would not be close to constant with altitude in any obvious manner. Clearly, an observational data analysis would be required to make any of such claims. That is totally missing in the present manuscript.

I do not understand why the lead author refers *again* to an observational study by Wang *et al* in the response: such a study does not explain why the vertical eddy heat flux increases with altitude, and more specifically, whether an increase is due to the density effect or not, as the present authors claim.

Strangely, the lead author does not comment on my original argument based on the bulk formulation of the heat fluxes: if their claim is correct, my argument must be disputed.

• Please show an explicit demonstration/derivation that (A11) is actually a solution of (A10), because I strongly doubt it. Recall that, as I stated in the original review comments, Eq. (5) with $\alpha = 2$ is indeed a solution of (A11). It looks like to me, the authors are simply dismissing this simple fact, and trying to "invent" a new solution.

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