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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.

Jun-Ichi Yano (Referee)

jiy.gfder@gmail.com

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General Remarks

First of all, I have to make it clear that I *did not* recommend to post the present manuscript to ACPD. The original submitted main file did not include the appendices referred in the main text, thus I simply suggested that this manuscript should not be considered for a publication until the appendices are included in the main file. Unfortunately, the Editor never returned to me, but simply posted the manuscript without consulting with me.

For the reason just stated above, I did not read through the original incomplete manuscript. In other words, this is the first time that I went through this manuscript.

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To be honest, I am not sure whether I would even have agreed to post the present manuscript to ACPD, not to mention a possibility of publishing it in ACP.

In my own reading, the manuscript is badly written: it essentially consists of a random collection of segments of results, which do not present any coherent story. For example, the Appendix A is simply wrong (see below). The paper also quotes various earlier results rather in random manner, by citing randomly chosen references, rather than tracing back original papers, that we are usually expected to do in our citation system.

The title suggests that the paper is considering the effects of low density at high altitudes in boundary-layer convection. An only result I found along this line is (as also explicitly stated in abstract) a rather trivial point that the vertical eddy transport of entropy increases with decreasing density when the surface heat flux (in unit of W/m^2) is fixed. However, this statement is meaningful only if the surface heat flux could be considered approximately constant under a change of the density. In fact, their LES simulations are performed by simply assuming this. On the other hand, a simple consideration based on a standard bulk formulation (see below) suggests that it is rather a vertical eddy flux which is invariant with the density. Thus, the whole premise of the work is simply invalidated.

Based on these considerations, I strongly suggest that the present manuscript should not be considered for a publication in ACP. I rather regret that this rather confusing manuscript has even appeared in ACPD.

General Evaluations of the Work

The weather of high altitudes is qualitatively different from that of a surface level at which majority of us live. This is a common knowledge for mountaineers, for example. Unfortunately, research of high–altitude weather is relatively scarce. Thus, by addressing this question, the present manuscript could be potentially a very welcome contribution.

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It is interesting to note that the present authors try to characterize high altitudes by low densities. Our standard approach takes either a geometrical height or a pressure level as a measure of the altitude. Alternative can be to take the potential temperature, or even the potential vorticity. However, the manuscript never explains well why the authors focus on the density rather than any other physical quantities.

Even worse, reading the abstract, the only part related to the density in their work is that “a decrease in air density enhances the buoyancy flux”. As I already stated, this statement is simply trivially true, but only if the surface flux itself is invariant with a change of density. The latter assumption likely fails. Thus, the present work simply loses any relevance.

Major Issues

In this section, I am going to discuss major issues with the present manuscript one by one.

Lack of Logical Coherence:

The manuscript lacks a logical coherence. The point is already clearly seen in the lead sentence of the abstract, which states that “We study the relationship between convective characteristics and air density over the Tibetan Plateau from the perspective of both climate statistics and large eddy simulations”.

Fine, probably, a clever design of LES will give an insight on this problem. Unfortunately, the authors lack with such an ingenuity (cf., Section *LES Experiment Design*). On the other hand, it is totally unclear how this question could even be addressed by any statistical analyses of climate.

The result of the climate analysis is summarized by Fig. 1(a). Here, the horizontal axis is the air density. However, it is clear that the air density can be replaced by any other quantities that characterize an altitude of the ground. Thus, Fig. 1(a) does not demonstrate an important role of surface air density to the problem in any manner.

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Worse than that, the discussion of the figure does not lead to any questions to be resolved by further analyses. At least, I missed to read such a clear statement in the text.

It seems to me that the figures of the present manuscript are presented in random order, and text could be read equally well even by re–shuffling the order of figures in any different manner. In this manner, the manuscript totally lacks with a clear logical order of a presentation. At least, I fail to follow such a logical sequence.

Most seriously, the manuscript totally fails to explain why the authors focus on the effect of low density with high altitudes, rather than any other attributes (e.g., pressure, potential temperature, etc) of a high altitude. In other words, even a choice of a subject of the study is just a random choice without any justification.

Geographical Focuses:

The effect of low density in ABL, if it ever exists in any sensible manner, is clearly a universal question that applies to any ABL regardless of any geographical locations. However, the manuscript strangely focuses exclusively on the Tibetan Plateau without clearly stating a reason for this focus for addressing this very question.

Indeed, the first paragraph of the introduction addresses a uniqueness of the Tibetan Plateau in a very general context of the climate dynamics. However, as far as I can read, the introduction totally fails to explain why we need this particular geographical focus, more specifically, for studying the effect of low density in ABL.

Reading through the manuscript, I even begin to see a nationalistic motivation to study the Tibetan Plateau rather than based on any real scientific motivations. An expression "over the rest of China" at Line 39 suggests that the authors are only interested with Chinese geography. In support of this speculation, Fig. 1 only maps the values over China. This is in spite of the fact that ERA reanalysis that the authors use cover the whole globe. The mnauscript lacks in scientific objectivity, even when it is possible to maintain it.

LES Experiment Design:

Arguably, a main result of the present study is a set of LES experiments performed for investigating the so–called effect of low density in ABL. However, the actual design is rather naitve, at the best, and most likely very misleading: the surface heat flux, H, (given in unit of W/m^2) is simplify fixed, and the surface–level density is varied. As a result, the vertical eddy potential–temperature flux (given in unit of Km/s), $\overline{w'\theta'}$, simply increases with a decreasing density. In other words, the authors are *not* examining any genuine effect of density on ABL, but simply changing $\overline{w'\theta'}$, and examining resulting change of the ABL behaviors. The main results presented by Fig. 2 are rather trivial, and I do not see anything particular to report.

Change of Surface Flux with a Change of Air Density:

The authors do not provide any sensible argument to justify this experimental design. However, a following simple simply consideration on a principle of the bulk surface–flux formulation suggests that it is rather the vertical eddy flux, $\overline{w'\theta'}$, rather than the heat flux, H, itself that remains overall invariant with a change of air density.

A standard bulk formulation writes the surface vertical eddy flux, $\overline{w'\theta'}$, as

$$\overline{w'\theta'} = C u_H (\bar{\theta}^* - \bar{\theta})$$

where C is a bulk coefficient, which generally has only a weak dependence on the environmental state; u_H is a horizontal–wind scale. Thus, if all the other factors are identical, $\overline{w'\theta'}$ remains unchanged with a change of air density, but H decreases with a decreasing air density.

It follows that the present LES design does not reveal anything about the density effect on ABL.

Appendix A:

• In the main text, the role of environmental descent, $w_e \neq 0$, is explicitly taken into account (cf., Eq. 2). However, this effect is simply neglected (cf., Eq. A1) without any justifications.

• Throughout the Appendix, the prime sign is missing everywhere for indicating the eddy values. The temperature, T, must be replaced by the potential temperature, θ, throughout.

• Eq. (A9) does not make any sense.

• Eq. (A11) is clearly not a solution of Eq. (A10), thus all the subsequent discussions are irrelevant. In fact, a solution for Eq. (A10) is given by Eq. (5) with $\alpha = 2$, as explicitly stated in Zhu et al (2005). [It is obvious that the authors are citing this paper without reading it. Also note that this solution (5) is earlier derived by Betts (1973, see his Eq. 42). However, the authors simply fail to give such a simple credit.]

Presentation Style:

See the next section for more specific problems. However, the manuscript generally suffers from the following problems in the presentation style:

• The references are more than often chosen in totally arbitrary manner. It is always important to cite an original source of the idea in citation references. The authors totally dismiss this very basic rule.

• Various equations are quoted rather in an arbitrary manner without any conscious considerations of a relevance.

• Substantial re–writing of the text will be required. I will point selective examples in the next section.

Specific Comments

Line 88, EDDYPRO: It is a totally non–essential matter for readers what software the authors have used. However, it is crucial to present how turbulent fluxes are actually computed based on which theory, formula, etc.

Line 107, LCC: Please spell this out

Sec. 3 (Lines 106–120): Please re–write the text to the point. One has to read it dozen times to understand this rather conjugated text to understand what the authors really want to say. As it stands for now, the remaining part of the main text could be read equally well even by totally removing Sec. 3.

Lines 137–146: This paragraph is not linked to any other part of the text. One can simply remove it.

Line 169: Sullivan et al (1998) is just one of many papers quoting an earlier claim with a constant 0.2. This is just an example of arbitrary quotation system that the authors use. The earliest claim of this value that I could dig is Ball (1960). However, the authors must search carefully.

Eq. (5): Indeed this is a valid solution for Eq. (A10) with $\alpha = 2$ when an environmental descent is absent, i.e., $w_e = 0$. However, Zhu et al. (2002) *did not* derive (cf., Line 169) this expression as a general solution for, say, the system with Eq. (2). They merely *suggest* it as a phenomenological generalization of the solution with $\alpha = 2$.

This is an example of the present authors' system of arbitrary quotations of earlier results out of context.

Lines 179–181: This premise is hardly justified, which also makes the present manuscript invalid. A work based on such an ill–posed premise should not be published.

Eq. (7): This is just another example of the present authors' system of arbitrary quotations. This formula is just a curve fit out of an LES, and there is no reason to believe its universality.

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Line 311, “Water vapor is relatively abundant over ECMR in summer”: This is a very odd manner to start a final section of a paper considering the role of the low density with a high altitude.

Lines 316–317, “This density effect is demonstrated with a simple mixed–layer model in Appendix A”: NO

Lines 321–322, “Stronger ascending motions”: This is nothing to do with a low density

Lines 452–454, “a simple microphysics scheme (Grabowski et al 1998) that considers the impact of the relatively low temperature over the TP”: NO

Line 477, “corrected”: Please describe what kind of corrections are made

Fig. 4: plots are so scattered that I do not think that we can draw any sensible conclusions out of them.

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