

General

The paper was improved, and the authors gave answers to my questions, but several points need further clarification before I can recommend the paper's publication. There are still many language issues, and I think this version requires still major English Editing.

In the following, I refer to line numbers of the version with marked changes.

Major revisions

- 1) The paper by Wang et al. (2020) has a very similar topic. It would strengthen the paper when in the introduction the differences of goals to those of the new paper would become clearer. I guess, the main difference is the comparison with North America, but perhaps there are others?
- 2) The hypothesis on the role of the TKE budget terms is better explained in Wang et al (2020). So, please refer also here to the Wang et al. (2020) paper. It can serve as a motivation for your study in the introduction!
- 3) Description of the determination of wind shear (equations 4-10): I strongly recommend writing L directly as a function of τ and H where H is heat flux (see equation 1 in Gryanik et al. 2020). Then one can tell that after prescribing values for τ and H from the model, L is determined and then via equation 4 the shear term. However, this method contains an inconsistency. Namely, when ERA5 takes other similarity functions than those of Dyer (1974). Please check this. If yes, then the shear does not correspond to the model equations and is just an approximation. This drawback needs to be explained.
- 4) After adding the 2500m contour lines in Figure 2, I see that not the whole TP region has high LCC, but roughly one third of the region is not concerned. This should be discussed!
- 5) I asked to explain results showing wind vectors (now Figure 4a and 4b). But I am not satisfied with the answer that the legend is now simply skipped. So, are the wind vectors now only a schematic? One needs to understand the effect quantitatively. Please note that this is one of the most important results (the central figure) for explaining the different LCC in the North American and Asian region. This needs explanation in the paper, not just for me!
- 6) I had asked for the discrepancy concerning the PBL and LCL (now in Figure 4). The authors answered this but this caused no change in the text (or did I oversee this?) Without further explanation, I would conclude from Figure 4 c) and d) that something is wrong with the definition of PBL since LCL cannot be so close to PBL over huge distances. This needs explanation in the text.
- 7) Language: in principle, the text can be understood but there are still many errors in almost every third sentence (smaller ones with just wrong articles but also larger ones with grammar and wording).

Minor revisions

Line 48: I think acronym TIPEX was not yet explained here.

Line 49: In which paper, the term popcorn like cumulus clouds was used the first time for this area? Please cite.

Line 69: sentence starting with 'according to'. It is too long and could be better understood by splitting the information into two sentences.

Figure 1 Caption needs to be changed. Is cloud fraction shown? Just 'digital number' is not enough information.

Figure 2: delete second occurrence of summer in the caption. Blue line is hard to see, replace it by white?1) Which cloud characteristic is changing? What is 'raised' cloud?

Line 174: I cannot understand the sentence. What is shown in Figure

Lines 178-180: Indices L, M,H are not explained.

Figure 7: write e) and f) in the same size as a) b) c) d).

Line 307-313. These sentences need language revision. It is difficult to follow. But I am also afraid that the difference PBLH-LCL should not be interpreted at all (see major point 6).

Reference

Gryanik, V. M., Lüpkes, C., Grachev, A., & Sidorenko, D. (2020). New modified and extended stability functions for the stable boundary layer based on SHEBA and parametrizations of bulk transfer coefficients for climate models. *Journal of the Atmospheric Sciences*, 77(8), 2687-2716.