

## ***Interactive comment on “Characteristics of the summer atmospheric boundary layer height over the Tibetan Plateau and influential factors” by Junhui Che and Ping Zhao***

### **Anonymous Referee #3**

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In the ACP manuscript “Characteristics of the summer atmospheric boundary layer height over the Tibetan Plateau and influential factors” by Che and Zhao, authors used a great set of rawinsonde launches for more than two year period to analyze the surface-forcing governing the ABL depth variability in summer time. Of course, authors did not bring up any discussion why the other seasons were missing in the manuscript. The authors have made some nice time-series analyses and presented a great set of observational findings. However, at many instances, the manuscript lacks the interpretation of the results. I will encourage to consider the following points during the revisions. – In the abstract: A big picture of the problem for the region for ABL research needs to be mentioned. – It is mentioned “The SBL accounts for 85% of

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the TP ABL” and also mentioned in the very next line, “The CBL accounts for 77% of the TP ABL” needs some clarification or needs to be rephrased. Otherwise, they contradict in general sense. – “The ABL height exhibits a large west-east difference, with a mean height above 2000 m in the western TP and around 1500 m in the eastern TP.” Did you refer to the daytime well-mixed CBL here? – In the numbers, authors need to mention whether this is m AGL or m MSL. Since spatial variability is mentioned underlying orography will play a role if these numbers are in MSL. Please clarify. – Line 30: “ABL height in climate prediction”. Authors need to bring an appropriate reference here. There is only one study that directly refers to climate projection. Please refer to the following one: “Differences in the efficacy of climate forcings explained by variations in atmospheric boundary layer depth” – Line 41: “The ABL height can be calculated from temperature, humidity, and wind profiles (Seibert et al., 2000; Seidel et al., 2010; Davy, 2018).” Please add a reference for numerical simulation as well since researchers are using models as well for this purpose. – Line 48: “solar altitude angle with respect to latitude” Please refer to Seidel et al., 2010 – Line 69: “results has certain limitations” What are those? Please be specific here. – Line 94: Quality check – Line 108: “operational observation of total cloudiness” Are these from reanalysis or from ceilometers? – Line 150: “This west-east difference increases from noon to the late afternoon.” Authors need to bring the concept of west-to-east march of the solar timing given span of 20 deg longitude would cause some “real” solar timing issue although in the region there is no time zone separations and BJT is used here. According to classical rule of “15 degrees of longitude per hour”, it will result in at least local time difference of 80 minutes or little more from western site to eastern site. Thus, the increase in the west-east gradient is also attributed to some extent to this “real” local timing differences. See Seidel et al. 2010 and other relevant studies as well. – Additionally, authors need to acknowledge the above-mentioned topic in other discussion where they brought up the west-east gradient changes from noon to late afternoon. – “Figure 3a-c shows the spatial distribution of the SBL height” How did they classify SBL regime during daytime soundings? Please clarify. – remark-

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able diurnal variation.” This is a qualitative statement unless some other SBL regimes are referred here for the contrasting scenario since SBL variability is in general low. Did you refer to the spatial variability? Please justify the causes for this then! Throughout the results section, authors need to bring some discussion of the causes for these findings. Otherwise, it appears as reporting of the observed variability. For section “3.2 Characteristics of SBL, NBL, and CBL heights” I will highly recommend authors performing the analyses of ABL depth growth rates which is most appropriate parameters that they wanted to discuss mentioned in the title and the abstract. Please see the feasibility of applying estimation of daily ABL depth growth rates Several discussion via the frequency distribution analyses for ETP/WTP, authors need to decide the aim of these analyses. The results are presented with respect to findings and results without taking care of their interpretations. Interpretation part. Line 23: “when SHF is strong, the turbulent motion is strong and the ABL height develops” True in general. What about the lag of ABL development since a number of studies showed that even after SHF attains its maximum daytime value, ABL depth growth continues till the time of early evening transitions. I would like to see some results in this respect between ETP and WTP and that will clearly illustrate the differences in the surface forcings the authors have tried to engage the readers.

Finally, authors should consider that some comparisons with regional scale variability of ABL depths (m MSL or m AGL, be consistent) should be presented and main conclusions why this study makes an unique contribution to the field emphasizing the new processes learned for very deep ABL over the region as reported in a number of past studies.

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