

Response to referee comments

Dear referees,

Here is our response to your comments. We paste them as-is and write our response under each one.

[Our comments are in blue color.](#)

The manuscript has undergone major revisions, especially in the Methods and Results sections.

The main changes are:

1. Addition of FLATu10: A new experiment (FLATu10) was added to better isolate the impacts of orography and wind shear.
2. Background wind in FLAT and REAL: A weak background wind was introduced in FLAT and REAL (constant profile of 1 m/s) to break the W-E symmetry. This modification was successful (see Figs. 3, 6, and 7).
3. Terrain smoothing settings: The standard deviation σ of the 2D Gaussian filter applied to the terrain was reduced from 200 m to 75 m (a factor of 4 to 1.5 grid spacings) to retain more small-scale terrain features. While the overall results do not change, the differences are noticeable in 99th percentiles, which reinforces our main conclusions.
4. New Appendix B and additional figure: Appendix B was added to describe the super-Gaussian tapering of the lateral boundaries, and Fig. C1 now shows w' perturbations at lower levels, which were previously only mentioned in the text.
5. New ABL-terrain diagnostic: Figure 9 was added to show ABL depth (in km AGL) and ABL height (km MSL) as a function of underlying terrain elevation, providing additional insight into the influence of the small-scale orography.
6. Greater emphasis on the upper tail of the ABL height distribution: we highlighted the behaviour of the upper tail (99th percentile) of the ABL height distribution (Fig. 8; Table 2; Section 3.4; Conclusions). This clarifies how small-scale orography disproportionately affects the deepest convective columns.
7. We introduced new height abbreviations to avoid confusion.
 - MSL (above mean sea level),
 - APL (above plain height, where plain height is 4.2 km MSL),
 - AGL (above ground level, i.e., "above terrain height". These heights are not interpolated but terrain-following).

RC1

General

In this paper the authors use high-resolution LES (CM1) in idealized experiments to study the impact of 'subgrid-scale orography' (subgrid to typical global NWP or regional climate models) on the development of extremely deep convective boundary layers on the Tibetan Plateau. It is found that beyond the earlier identified conditions of weak stratification aloft and not too weak surface heating, 'local orography' indeed has an enhancing effect on CBL growth – thus leading to order 10% higher CBL. Overall, the paper is well designed (numerical experiment) and the results are presented in a consistent manner. I have, however, two 'major comments' addressing some details of the experiment. They are called 'major' because they cannot be attributed to a line (as the minor comments) – but will easily be addressed. In this sense, I think, the paper can be published after having addressed the minor comments.

Thank you very much for reading the manuscript thoroughly and for your excellent (and very clear) insight. We hope this response is satisfactory. Further comments and/or suggestions regarding the revised manuscript are welcome.

Major comment

- Why is mechanical forcing introduced in such an unrealistic way (i.e., the steep gradient from zero wind speed in the valley to 10 m/s over just 500 m)? If there are conditions of forced convection, i.e. nonzero mechanical turbulence, this will result in a more gradual decrease of mean wind speed towards the mean plateau level. Can the authors comment on that?
The imposed wind profile is used only in the sensitivity experiment REALu10 (and now in FLATu10 as well) and is not intended to represent a realistic valley–ridge wind structure. Our main motivation was to introduce a non-zero, large-scale background wind within the boundary layer, with a magnitude comparable to the conditions documented by Chen et al. (2016, their Fig. 5). We set the initial wind to zero near the surface to avoid exciting strong orographic waves during model initialization. As the simulation evolves, there is downward momentum transport resulting in a non-zero mean wind within the BL and a shear layer across the entrainment zone.

We revised section 2.2 to briefly explain this; and section 3.1 where we comment this evolution (Fig. 2) in more detail.

- The numerical experiment is placed over the TiP and the local orography is used to define 'REAL' case. However, the experiment has no other ingredient of an elevated plateau (with the exception of pressure, perhaps). The question is therefore, to which degree the conclusions can be transferred to lower plateaus. Clearly, an enhanced CBL height over a lower plateau would not lead to those record-high CBLs with the potential to even reach the stratosphere – but still, a several-km deep CBL has been observed either under conditions of strong convection at desert locations, or on the TiP. This aspect should be worked out to some detail in a revised version.

In our configuration, the primary ingredient enabling extreme CBL growth is the weak stability aloft, associated with the passage of an upper-level trough (PU anomaly) (Chen et al. 2016). Further ingredients include the high elevation (lower air density), the arid land surface, and mechanical forcing (in REAL and REALu10) that together favor strong dry convection. We agree that the same mechanism—weak stability combined with strong surface heating and small-scale orography—can also operate over lower plateaus or desert regions, even though the resulting CBLs would not reach the extreme heights found on the TiP. Thus, our FLAT vs REAL comparison should be understood as illustrating a general mechanism whose qualitative behavior is transferable to lower plateaus, while the quantitative CBL depths will depend on elevation, background stability, and surface forcing.

Revision: we have clarified this point in the revised conclusions section -- that a systematic exploration for other plateau regimes remains a topic for future work.

Minor comments

I.16 'TiP': the abbreviation is introduced in the abstract – but this usually not counted. So, please introduce the abbreviation here as well.

Revised as suggested. We also applied it to the other abbreviations, like CBL.

I.70 'FLAT (no orography)': this may be a matter of definition, but I consider the TiP to be part of 'orography', too. So, maybe the authors want to consider to re-label this to locally flat ('no local orography', no 'subgrid-scale orography') or alike.

Revised: "FLAT (no orography)" -> "FLAT (no local orography)"

I.91 this 'delta20 m' notation seems to be odd. Why not 'the vertical grid spacing is set to 20 m below...'? Several occurrences on this and the next line.

revised as suggested

I.94 'the vertical grid spacing...': this is repeated (and this time the 'delta' is already removed....)

Revised: removed repetition

I.96 what is a semi-slip condition? Please explain.

In the revised version, there is a line added in section 2.1 explaining the semi-slip condition.

I.98 'idealized sounding': please specify whether this only refers to temperature, or also includes humidity and wind speed (the u10 simulation suggests that the reference has no wind?)

Revised as suggested: we specify in section 2.1

I.103 '...starting a 09LT...': is this the start of the simulation? Or is there a spin-up period considered? Please specify.

Revised. In section 2.1 we write the simulation start and how long it runs. (09-20 LT)

Tab 1 'Flat orography' seems to be a contradiction. In the text it is referred to as 'no orography', which also seems to be counter intuitive...(see comment to I.70). Again, I suggest to consider 'no subgrid scale orography', or 'elevated plain' or similar).

Revised as suggested. Also, additional revisions to Table 1 were introduced.

I.139 'to quantify' is probably not appropriate here – consider 'to diagnose' or 'to determine' instead.

Revised: "to quantify" -> "to determine"

Fig.3, caption: what does 'see 2.32.3.2' refer to? Also, the black contour line should be explained, as 800 m above plateau level (i.e., 4200 m ALS)

Revised Fig. 3 caption.

Fig.3 In the caption it says ,at 1800 m AGL (6 km ASL)': for the ,REAL' simulations the equivalence of these two statements is not given. Please specify, which applies.

Revised: we write only 6 km ASL to avoid confusion.

Fig. 4, caption: must specify the time, when the instantaneous fields are obtained.

Revised: "at 15 LT" added.

Eqs (4) and (5): the lhs must be u'^2 , v'^2 respectively. Furthermore, if using the according to (1) and using (2) the 'difference between the time-averaged second and first moments' is not obvious. I suggest to explicitly derive this (maybe in an appendix). However, in the nomenclature of the present work, it would, correspond to the resolved variance. This should be added.

Added resolved variance part (eq 4) in 2.3.1. We refer to this equation in 3.3.

We have rewritten this section altogether, but without changing the core calculations.

Fig.7 you have introduced the tracers as '1' and '2' – and here, they are referred to as 'surface-based' and 'upper-level, respectively. I suggest to introduce this convention where the tracers are introduced. Also, in the caption of Fig. 7, the units of the isolines must be specified.

Revision:

- there are no tracer 1 or 2, but surface-based and upper-level sect 2.1.
- the units of the isolines are specified now, and we changed the units of the upper-level tracer to dimensionless percentage.

Fig.8, caption: 'horizontal distribution...': if I understand the accompanying text correctly, these are the horizontal statistics (median, interquartile range) of the diagnosed mixing height (or CBL height) based on the surface emitted tracer and the elevated tracer (and not: tracer concentrations). I furthermore cannot understand how 'the mean concentration' can be marked (with an x) in a height domain (time) diagram. Please explain.

Caption is revised.

A quick explanation (just in case, since the previous caption was wrong and misleading):

1. Ensemble average the surface-based tracer at 15 and 19 LT. (Fig. 7 shows how the vertical cross-section looks).
2. Diagnose the ABL heights from this (500*500 columns == 500*500 ABL heights).
3. Obtain statistics.

l.249 '...by 383 m...': I don't think that 'meter resolution' here is appropriate (I much more like the 'about 10%', even if in the summary then it should be indicated percent of what). Putting into context could also be done by expressing the change in percent of the terrain height.

Revised this section so it uses percentages instead of meters and uses meter resolution (and percentages) in Table 2.

We introduced different height abbreviations:

- APL (above plain height),
- MSL (above mean sea level),
- AGL (above ground level, i.e., "above terrain height").

The mentioned percentages refer to ABL height APL.

Appendix 1: this appendix is never mentioned in the manuscript, nor is the parcel method. So, either the appendix can be removed from the ms, or the parcel method is included into the analysis.

Revision: we rewrote section 2.3.2 to mention the parcel method and refer to the appendix for a detailed/quantitative comparison between the methods. We chose to focus on the tracer method and keep the parcel method in the appendix.

RC2

The authors conduct semi-idealized LES experiments under dry conditions to compare three scenarios: a flat plateau (FLAT), realistic terrain (REAL), and realistic terrain with added upper-level wind (REALu10). They analyze boundary layer depth, turbulence structure, tracer dispersion, and vertical mixing. Key findings include:

Small-scale orography accelerates early CBL growth and leads to localized deepening (up to ~500 m over mountains). Wind shear organizes convection into roll vortices, enhancing mixing and further increasing CBL depth (up to 9.4 km ASL). Terrain and shear effects are roughly additive, with the deepest and most uniform CBL occurring in REALu10. A tracer-based method for diagnosing CBL height is introduced and validated against a parcel method.

The manuscript in its current form has minor weaknesses that must be addressed before it can be considered for publication. These primarily concern a lack of methodological clarity, and occasional overstatement of conclusions.

We thank Reviewer 2 for their constructive and thoughtful comments. We appreciate the reviewer's recognition of the manuscript's strengths and their helpful suggestions for improving clarity and moderation of our conclusions. We have carefully addressed all points and revised the text accordingly.

This study makes an original contribution by quantifying the role of small-scale orography and shear in driving extreme CBL depths over the Tibetan Plateau. I am curious, compared with the stability aloft, the fine-scale terrain and shear effects are still the first-order factor for the extreme high CBL? Please add more sensitivity simulation to compare their respective importance.

The weak stability aloft is indeed the primary control on extreme CBL growth, as it sets the background conditions for deep dry convection (see energy balance model in Chen et al.). The present study focuses on how small-scale orography and wind shear further modulate this growth under otherwise identical, weak-stability conditions. In other words: small-scale orography and shear are the sensitivity test to the already favorable conditions for deep CBL growth.

Revision: we mention this in section 2.1 and make it clearer overall.

The description of the Terrain Processing methodology is vague and omits critical details necessary for reproducibility and full assessment. The statement that the terrain was "tapered" and then "smoothed with a Gaussian filter (200 m)" is insufficient. What was the standard deviation of the Gaussian kernel? Was the filter

applied once or iteratively? A figure showing the original SRTM data, the tapered terrain, and the final smoothed terrain used in the REAL case would be immensely helpful to understand the degree of modification and its potential impact.

Revisions:

- (1) improved wording of the terrain processing methodology (section 2.2),
- (2) added Appendix B discussing the super-gaussian filter,
- (3) added additional panel in Fig. 1 for even more clarity.

Flow Decomposition: The description of the Reynolds averaging procedure is confusing. The authors use a 2D Gaussian filter on time-averaged fields. This is a spatial filter, not a classic ensemble average. The justification for this hybrid approach over a pure temporal or spatial average needs to be strengthened. Furthermore, the choice of a 500 m standard deviation and a 4 km support width seems arbitrary; Please give an explanation why these values were used. A sensitivity analysis or a stronger justification based on the dominant turbulent length scales would be beneficial.

It was applied to isolate sub-mesoscale variability in the time-averaged fields, serving as a spatial decomposition rather than a strict Reynolds average. The so-called "ensemble average" is then used. The filter parameters ($\sigma = 500$ m and support width = 4 km) were chosen deliberately to match the dominant turbulent length scales observed in the simulations, corresponding to the typical horizontal size of large thermals and roll vortices in the CBL.

Additional tests with smaller ($\sigma = 250$ m) and larger ($\sigma = 1$ km) smoothing lengths yielded consistent qualitative patterns, confirming that the decomposition is not sensitive to the exact parameter choice. We have clarified this rationale in the revised manuscript.

Why did we chose to include spatial-average as well as the time-average? With only the time averaging, the turbulent statistics are poor - but domain-means are not applicable due to orography. So we are choosing time averaging AND 2D Gaussian, to improve turbulent statistics. This approach could be called a "poor-man's" approximation to ensemble-average.

Revision: we restate these points in Section 2.3.1.

Initial Conditions and Forcing: The use of an idealized sounding with a constant weak lapse rate above a 300 m mixed layer is a major simplification. The authors must more explicitly discuss the limitations this imposes, as the real atmosphere over the TP likely has multiple layers of varying stability that could significantly modulate entrainment and CBL growth.

The weak lapse rate above the 300 m mixed layer was indeed chosen intentionally to reproduce the favorable thermodynamic conditions observed during deep boundary-layer development over the Tibetan Plateau. In particular, Chen et al. (2016) report weak free-tropospheric stability (near-dry-adiabatic lapse rates) during strong surface heating episodes, which we use as a representative reference.

We acknowledge that real soundings often exhibit multiple stable layers and inversions; however, simplified profile was chosen to isolate the effects of local orography and shear without introducing additional complexity from vertical layering. The goal was to conduct a sensitivity study, not reproduce observations accurately.

We have clarified these limitations in the revised manuscript (conclusions)

Local vs. Domain-Averaged Impacts: While the local enhancement over the ridge (R-TOPO) is well-quantified (~500 m), the domain-averaged impact of orography (REAL vs. FLAT) is surprisingly small (~80 m). The authors should discuss this dichotomy more thoroughly. Does this imply that the overall impact of small-scale orography on the plateau-scale CBL is minor, but it creates strong local heterogeneity? The current narrative emphasizes the importance of orography, but the domain-mean results tell a more nuanced story.

You are exactly right and we tried to point it out in the manuscript, but it was not emphasized enough. What you observed reflects the fact that the orographic influence is spatially heterogeneous — it strongly amplifies local entrainment and turbulence over elevated slopes but averages out when integrated over regions that also include valleys and flat areas.

Revisions:

- We have clarified in the revised results section and conclusions that subgrid orography mainly enhances spatial variability rather than substantially altering the domain-mean CBL height.
- We added a new Fig. 9 where we show how the boundary layer is actually less deep over orography - although may reach higher heights. This distinction is made clear (height above ground level vs. height above mean sea level).

The REAL terrain is described as featuring slopes up to 30° and ridges up to 1400 m. It would be helpful to contextualize this within the broader Tibetan Plateau to justify its representativeness.

The chosen terrain represents a typical ridge–valley structure within the central Tibetan Plateau, with elevation differences of 1000–1500 m and slopes up to ~30°,

consistent with regional SRTM data. The periodic boundary conditions ensure that the domain effectively repeats this representative mountain structure, rather than emphasizing any single extreme feature. The local context of the selected region is already illustrated in Fig. 1a, where the surrounding orography can be seen. We have added this clarification to the Methods section.

In the revised manuscript, the slopes go up to $\sim 36^\circ$ and there is less smoothing for the final terrain (was $\sigma=200$ m, now $\sigma=75$ m).

While the idealized setup is appropriate for isolating mechanisms, the authors should more explicitly discuss how the exclusion of moisture, clouds, and synoptic variability might affect the generalizability of the results.

We have expanded the discussion of limitations in the Conclusions to more explicitly address how the exclusion of moisture, clouds, and synoptic variability affects the generalizability of our results. As suggested, we now clarify that the objective of our idealized setup was to isolate the mechanisms by which orography and shear influence CBL development, and that these dynamical mechanisms can reasonably be generalized to other high-elevation or weakly stratified environments. We also note that moist convection and cloud processes would likely enhance vertical exchange and turbulence compared to our dry configuration, potentially amplifying some of the effects identified here. Finally, we specify that the synoptic background in our case corresponds to unusually weak stability, which is a key condition favoring deep CBL growth, and we point to future work needed to explore a wider range of synoptic regimes.

The authors mention that unresolved orography may bias CBL representation in models. A more quantitative discussion—e.g., how much CBL growth is underestimated in models that smooth terrain—would enhance the impact of the study.

Section 3.4. is now rewritten and contains many points regarding this. We elaborate on this and clarify that models underestimate extreme BLs, and that relative ABL depth is smaller over the mountains (new Fig. 9).

The results section frequently references figures (e.g., Fig. 3, 4, 7) before the reader has any context for what these figures show. The narrative would be much clearer if the description of each figure was immediately followed by the interpretation of its results.

Thank you for pointing this out. We moved the figures so they appear earlier in the document, i.e., before they are referenced.

The abstract and conclusions are somewhat repetitive. The conclusions could be strengthened by synthesizing the findings into a broader conceptual model of how terrain and shear interact, rather than just restating the results.

Abstract is revised and the conclusions contain future guidelines and also how can it be expanded.

Line 6, Small-scale orography substantially accelerates early growth: by midday the CBL in REAL is ~80 m higher than in FLAT, 80m higher than FLAT is substantially? Compare surface flux and stability in the free air, is it still substantially?

Line 7, locally above the mountain it is ~500 m deeper. This sentence is confusing, please rephrase it.

Line 7, This terrain-induced advantage narrows later in the day, what is narrows? Please rephrase this sentence.

Revision for three comments: we rephrased this part in the abstract to (also) include some ideas from the previous comment about local vs. domain-averaged impacts.

Line 12, Under clear-sky conditions, the plateau's CBL can exceed 9 km within a single day given strong surface heating and weak stability aloft. This conclusion has been approved by Chen` study. It may not be proper to be included in the abstract and taken as a conclusion of this study.

We removed the Chen part as well.

Line 18, One distinguishing feature of the TiP's ABL is its extraordinary depth. It`s better to cite the reference here: X., C. and Y., M., 2025. The unique atmospheric boundary layer over the Tibetan Plateau (in Chinese). Chin Sci Bull, 70: 4180–4187. Reference added. Thank you for the suggestion.

Line 37, it is better to further point out the weak stability in the lower troposphere is associated to the tropopause folds activity (Chen et al. 2011 ACP).

Chen, X., Ma, Y., Kelder, H., Su, Z. and Yang, K., 2011. On the behaviour of the tropopause folding events over the Tibetan Plateau. Atmos. Chem. Phys, 11: 5113-5122.

The reference has been substituted.

P3, L75: Specify what "REAL terrain" represents in terms of maximum height AGL and horizontal scales.

Revised as suggested.

P4, L105: The vertical grid spacing is described twice with slightly different wording. Consolidate this into a single, clear description.

Revised as suggested.

P9, eq. 4-6, The equations for TKE components are incorrectly formatted. w'^2 is repeated for all three components. This must be corrected to u'^2 , v'^2 , and w'^2 .
[Revised as suggested.](#)

RC3

This manuscript investigates the impact that small-scale orography features have on the evolution and structure of the convective boundary layer over the Tibetan Plateau. The author used an LES model (CM1) and ran three idealized simulations for a special case using three different configurations that differ in the small-scale orography features included and in the presence/absence of upper-level wind. The results show these small-scale features and shear effects enhance the CBL growth by up to 15%. The paper is well designed, clearly states the objectives and methodology, and the results are properly presented and discussed. I recommend the paper for publication, although some minor comments should be addressed before that.

[We thank Reviewer 3 for the comments and corrections. The attention to detail is really helpful and it improved the manuscript.](#)

Minor corrections

- Line 4: replace “a upper-level” by “an upper-level”.
[revised as suggested.](#)
- Line 8: please explain what LT means (first time that appears in the text).
[revised as suggested.](#)
- Line 16: please explain what the “TiB” abbreviation means.
[revised as suggested.](#)
- Line 19: same for the abbreviation “CBL”.
[revised as suggested.](#)
- Line 94 to 95: sentence repeated (already explained in line 91).
Repetition removed.
- Line 96: please include a brief explanation of what semi-slip condition means.
[In the revised version, there is a line added in section 2.1 explaining the semi-slip condition.](#)
- Line 116: I think “FLAT” is a bit confusing cause this simulation is not flat itself, but does not include small-scale features. Please consider changing the name, like NOSSO or something that states more clearly how the orography is represented in that simulation.
[When introducing FLAT, we write: FLAT \(no local orography\).](#)

- Section “Experiments and Data”: could the authors include whether the simulations were run with a spinup period?
The first 2 hours are considered spinup. They are discarded as the model spinup to achieve a reasonable physical atmospheric state.
We added a sentence to section 2.1 regarding this.
- Lines 145-147: these two sentences say the same in a slightly different way. Please consider keeping just one to avoid repetition.
revised as suggested.
- Line 153: include the symbol q after potential temperature, as this symbol will be used in the caption of several figures throughout the manuscript.
revised as suggested.
- Figure 2: this figure is just briefly discussed in lines 154 and 155. I would suggest moving it to the Appendix.
The authors consider Figure 2 to be an important part of the paper clarity and reading flow and we decide to keep it as-is - but we expanded the discussion around it in Section 3.1.
- Line 157: add “ w ” after vertical velocity as this will be used in the caption of the following figures.
revised as suggested.
- Figure 3: in the caption, the authors mentioned that the vertical cross-sections are calculated for $y=0$. To improve the clarity of the manuscript, I suggest including a line in figure 1 showing where this cross-section is located.
In the caption of this figure, there is a reference to 2.32.3.2. Please correct.
The authors think adding the horizontal line is unnecessary, and that the one added in the top row of Fig. 3 serves this purpose. However, if the reviewer insists, we will add the horizontal line.

Caption is revised.

- Line 182: the authors say they also analysed the instantaneous vertical velocity at lower levels and that those results are not shown. Perhaps the one at 500 m AGL could be included in the Appendix.
We added appendix C with a four-panel figure showing at horizontal cross-section at 500 m AGL, at 12 LT and 15 LT, for FLAT and REAL.
- Line 189 and 190: w^2 should be replaced by u^2 and v^2 , respectively.
revised as suggested.
- Figure 7: please indicate the units used and explain the meaning of the lines shown.
We added the units and we revised the figure so that the upper-level tracer is dimensionless (percentages) rather than actual values, since it is more helpful for a qualitative read.

- Line 202: FLAT should be replaced by R-FLAT if I am not mistaken.
[The authors carefully checked the line, and "FLAT" is correctly placed.](#)
- Figure 8: if I am not mistaken, y-axis is showing the altitude of the CBL height and not the surface-based tracer concentration. Please clarify this.
[revised as suggested.](#)
- Line 281: the Appendix is never mentioned throughout the manuscript. Perhaps, you could refer to it in section 2.3.2 when talking about the method used to diagnose the boundary layer height.
[revised as suggested.](#)

RC4

Dear authors

As a first note, I want to inform you that I already read through the three other review statements. In general, I agree with all of them, and henceforth, my comments might be less than a typical review, but please consider them as equally relevant.

General comments

This is a clear, well-written manuscript exploring the reasons for a very high-ranging boundary layer over the Tibetan Plateau. Given the Tibetan Plateau's role in the global climate system, this research is relevant for the community. The authors employ the CM1 model for idealized simulations and analyze the boundary-layer structure in simulations with/without orography, and imposing different background wind conditions.

The manuscript is well-written, but additional analysis (TKE budget) and possibly one more simulation (FLAT/u10m) are necessary to strengthen the conclusions of the authors and to make the MS suitable for publication in ACP.

[Thank you for your valuable comments and insights, and very helpful suggestions. We address each of them carefully.](#)

Major comments

You identified shear-induced turbulence as one of the major reasons for the high-ranging ABL over your plateau. However, although you mention shear throughout the manuscript, you never show its actual quantity, e.g., by calculating the shear production term in the TKE budget equation compared to buoyancy forcing. Creating a figure and the accompanying text of the TKE budget will strengthen your

manuscript massively.

As suggested, we add additional panel to Fig. 5, which was previously just a resolved TKE profile at 15 LT. Now it includes the shear and buoyancy production term at 15 LT as well.

We expand the accompanying text in section 3.3.

As a second point, you talk about 'terrain forcing'. I understand that this conclusion is drawn from a comparison of flat vs. complex terrain. However, if I understood it right, the FLAT simulation is a sub-domain of the domain with real topography.

The FLAT simulation is a stand-alone simulation with no local orography. There is a subdomain called R-FLAT and it is only analysed in Fig. 8 and Table 2.

1) How can you make sure that the FLAT subdomain is not affected by a plain to mountain flow towards your plateau?

There is no plain-to-mountain flow in FLAT. However, there is plain-to-mountain flow in R-FLAT, but it is intentional since it is a subdomain and analyzed as such.

2) As a second point, it would be also advisable to add a simulation/analysis of a flat-terrain, but shear-induced simulation, i.e., FLATu10, so that you can make a direct comparison with the plateau simulation - and - especially relevant - to isolate the 'terrain' and 'shear' effects accordingly.

We added an additional simulation to the study: FLATu10, as per reviewer's suggestion. It helped us show our results and concluding points more clearly.

Minor comments

As other referees already pointed out minor formulation or spelling errors, I would only want to encourage the authors to avoid statements like 'typical development' or 'expected behaviour' without citing the relevant literature. Not all readers are aware of how boundary-layer development over orography is 'supposed to look like'.

We added the relevant literature (Stull, 1988) and rephrased the sentence that mentions the typical TKE profile in the late afternoon to make the statement more clear. Also, there are a few other instances we use "classical" and "typical". They are now placed in a (hopefully) better context.

Furthermore, the last paragraph of the introduction might better fit in the methods Section.

Last paragraph of the introduction was rewritten to only contain an overview of what was done with no details. The details were moved to the Methods section, as suggested.

Therefore, I recommend major revisions for the manuscript and look forward to the revised version.

Best regards