Responses to comments from Referee #2

MANUSCRIPT: acp-2018-944

TITLE: From weak to intense downslope winds: origin, interaction with boundary-layer turbulence and impact on CO_2 variability

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MAIN CHANGES IN THE MANUSCRIPT:

o Title.

o Abstract and motivating aspects.

o Denomination: katabatic → downslope.

o Further information about data postprocessing in Sect. 2.2.

o New Sect. 4 in the revised manuscript: analysis of the heat and momentum budgets, profiles and the estimation of the jet-maximum height for three representative events.

o Summary and conclusions.

o Appendix A (footprint estimation) and B (assessment of the thermal profile).

o Removed figures (numbers from the old manuscript): Fig. 7, Fig. 10, Fig. 11 and Fig. 12.

o Merged figures (numbers from the old manuscript): Figs. 4 and 5, Figs. 8 and 9.

o New figures (numbers in the revised manuscript): Fig. 7, Fig. 8, Fig. 9, Fig. 10, Fig. A1 and Fig. B1.

o Slightly modified figures (numbers in the revised manuscript): Fig. 1 and Fig. 11.

o Wording and English review.

General comments:

The authors study downslope flows at a site close to the Guadarrama Mountain Range by means of mean and turbulence measurements at a 10 m tower. The authors use an algorithm to identify periods with low synoptic forcing and katabatic flows, and then separate the periods into those with weak, intermediate and strong katabatic flows. Finally, they study the conditions under which each of these occurs and the associated boundary layer structure and CO2. The study shows some interesting, though not surprising results on the correlation between the strong wind episodes that lead to weakly stable stratification, and weak wind episodes that lead to very stable stratification. Still, the study has major gaps and in general lacks a thorough analysis of physical processes and associated budgets needed to substantiate the explanations which are at the moment sometimes given without a thorough proof (see specific comments below on whether this flows are katabatic at all and what their origin is given that katabatic flows cannot develop in unstable stratification, on the need to look at budgets, or for example the text connected with Figure 3). Also, there is a lack of thorough understanding of the nature of these flows (both in terms of the driving mechanisms and the interaction with turbulence) and the fact that the location of the jet maximum is a vital information that is lacking from the entire study, if indeed this flows are shown to be katabatic. If the turbulence data are collected from above the jet maximum, then it is turbulence that is not connected with the ground and therefore is not expected to show standard boundary layer characteristics (such as MOST etc). Where the jet maximum within the tower depth for each averaging period is needs to be added in the discussion of all the results and all the discussions and conclusions adjusted accordingly. For a more in depth study of the turbulence characteristics of katabatic flows Grachev et al. (2016) paper gives excellent information.

We thank Referee #2 for his/her useful suggestions and comments about the manuscript. We have considered all the major points and modified the manuscript accordingly. First, and directly associated with the concern about the nature of katabatic flows in this study, we have decided to change the way we refer to them to the generic “downslope flows” (Zardi and Whiteman, 2013)). The algorithm ensures that they are to some extent thermally-driven, but since some of them have a dynamical input, we have decided to denominate them, as a whole, downslope flows. Some of them, in any case, behave as pure katabatic flows as it is indicated in the new manuscript. Moreover, the physical processes underlying the formation and development of the downslope flows have been more deeply explored. For that, the heat and momentum budgets have been calculated and investigated for the individual cases. Additionally, a moderate downslope flow has been added to the analysis of individual representative events. For these events, the interaction with turbulence, their dynamical and thermal characteristics, as well as the location of the jet maximum have been explored. Please notice that Sects. 4.2 and 5.1 have been eliminated from the old manuscript and new sections have been added in the new manuscript (Sect. 4, Appendix A and B). The responses to the specific queries from the referee, are provided point-by-point below. The modifications undertaken in the manuscript can be checked up both from the revised manuscript and the tracked-changes version provided.

Specific major comments:

1. Turbulence data processing. As already mentioned by the first referee, the authors fail to give vital information on the turbulence data processing. Apart from the missing information on rotation methods and turbulence corrections, the authors also fail to motivate why they use a 10min averaging time, which in stable boundary layer is generally too long, and even for strong wind conditions the more appropriate averaging time would be 5 min, while for weak winds it is most likely 1 min or less. I suggest the authors calculate ogives or multi-resolution flux decomposition (e.g., Vecenaj et al, 2012) for their 40 episodes and estimate the most appropriate averaging time. If they need to have 10 min averages for comparison to the slow sensors, then the fluxes can be afterwards averaged to that value.

Referee#2 is right when pointing out that information about the corrections and data post-processing procedures was missing. This was also a query from Referee#1. As stated in the responses to Referee#1, this information has been included in the new manuscript (Lines 22-34 on Page 5 and Lines 1-6 on Page 6).
With regard to the appropriate averaging time, Multi-resolution flux decomposition (MRFD) of the friction velocity and kinematic heat flux for the three individual events have been calculated at both 4 and 8 m, to support the election of the 10-min window. They represent the distinct turbulent conditions that are found within the database of 40 events. We show MRFDs for the intense-downslope (27/07/2017), moderate-downslope (25/07/2017) and weak-downslope (13/08/2017) events only at 8 m, since the interpretation is very similar at 4 m.
The MRFD of both the friction velocity and kinematic heat flux for the intense event (Fig. I) show that the centre of the spectral gap is between 5 and 10 min. However, an averaging time of 10 min seems to be more appropriate than 5 min to capture all the turbulent scales. The MRFD of the friction velocity for the moderate event (Fig. IIa), shows the centre of the spectral gap also between 5 and 10 min. On the other hand, the spectral gap from the kinematic heat flux is unclear.
Finally for the weak downslope event, in which the uncertainty is greater, the centre of the spectral gap between 1600 and 1800 UTC is located between 5 and 10 min as for the two other events. If we choose 5 min as the averaging time we lose some scales, whereas when choosing 10 min we may include a few non-turbulent scales. After 1800 UTC turbulence is very weak and the spectral gap is hardly distinguished.
After exploring the MRFDs for the three events we can conclude that in general 10 min is the most appropriate averaging time in order to include all the turbulent scales for the distinct downslope times. Many studies have found that due to factors such as stability, mesoscale circulations and the synoptic forcing, the spectral gap can turn vague or highly variable (e.g. Hess and Clarke, 1973; Viana et al. 2010; Román-Cascón et al. 2015; Schalkwijk et al., 2015; Babic et al., 2017). In particular, when working with a large database, changing the

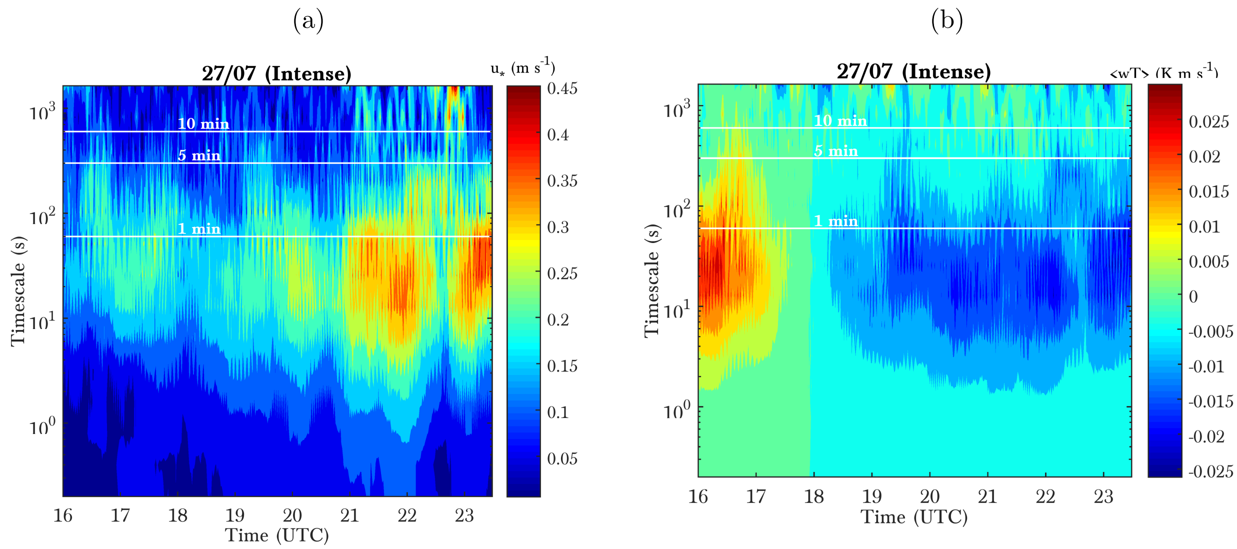

Figure I: *MRFD analysis of the friction velocity and (b) kinematic heat flux at 8 m for the intense downslope event (27/07/2017) between 1600 and 2330 UTC. Horizontal white lines indicate timescales of 1, 5 and 10 min.*

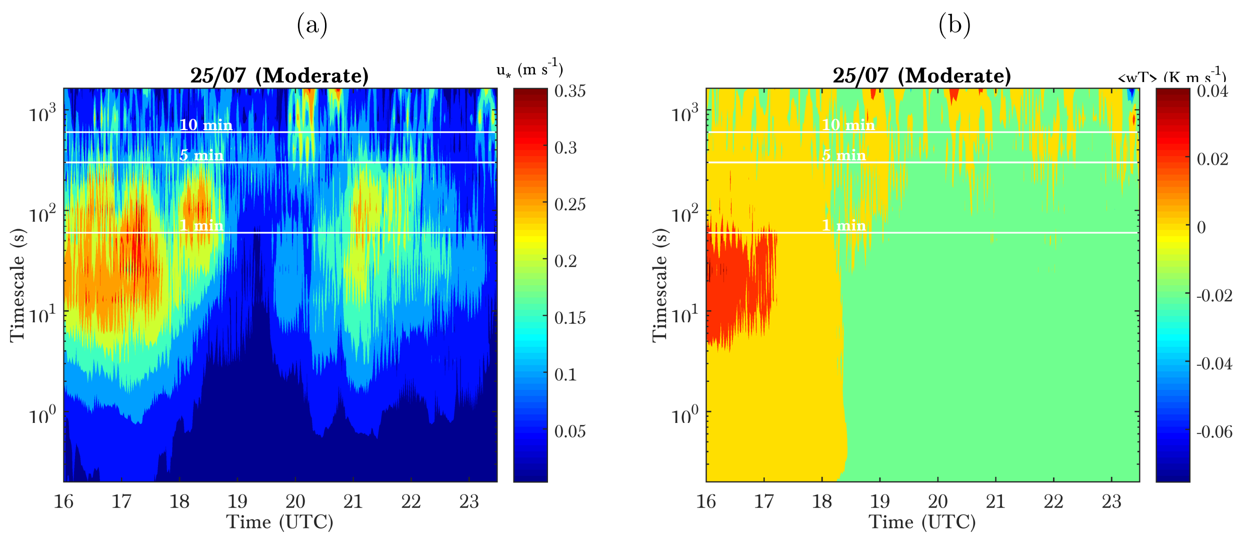

Figure II: *Same as Fig. I for the moderate downslope event (25/07/2017).*

averaging time by adapting it to different turbulent conditions is impractical and subjective, so it is preferable using a standard averaging time. 10 min is considered standard for micrometeorological datasets (Mauritsen et al., 2017).

References:

Babić, N., Večenaj, Z., and De Wekker, S. F. J. (2017). Spectral gap characteristics in a daytime valley boundary layer, Q. J. R. Meteorol. Soc., 143, 2509–2523. DOI: `https:`

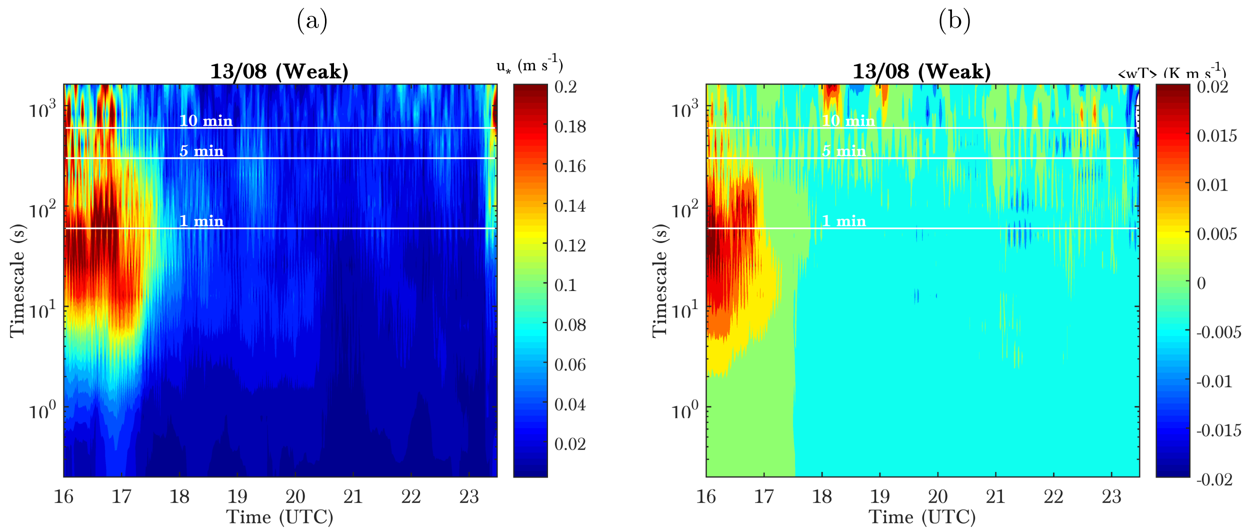

Figure III: *Same as Fig. I for the weak downslope event (13/08/2017).*

//doi.org/10.1002/qj.3103.

Hess, G. D. and Clarke, R. H. (1973). Time spectra and cross-spectra of kinetic energy in the planetary boundary layer, Q. J. R. Meteorol. Soc., 99, 130–153. DOI: `https://doi.org/10.1002/qj.49709941912`.

Mauritsen, T. and Svensson, G. (2007). Observations of Stably Stratified Shear-Driven Atmospheric Turbulence at Low and High Richardson Numbers, J. Atmos. Sci., 64, 645–655. DOI: `https://doi.org/10.1175/JAS3856.1`.

Román-Cascón, C., Yagüe, C., Mahrt, L., Sastre, M., Steeneveld, G.-J., Pardyjak, E., van de Boer, A., and Hartogensis, O. (2015). Interactions among drainage flows, gravity waves and turbulence: a BLLAST case study, Atmos. Chem. Phys., 15, 9031–9047. DOI: https://doi.org/10.5194/acp-15-9031-2015.

Schalkwijk, J., Jonker, H. J. J., Siebesma, A. P., and Bosveld, F. C. (2015). A Year-Long Large-Eddy Simulation of the Weather over Cabauw: An Overview, Mon. Wea. Rev., 143, 828–844. DOI: `https://doi.org/10.1175/MWR-D-14-00293.1`.

Viana, S., Terradellas, E., and Yagüe, C. (2010). Analysis of Gravity Waves Generated at the Top of a Drainage Flow, J. Atmos. Sci., 67, 3949-3966. DOI: `https://doi.org/10.1175/MWR-D-14-00293.1`.

2. Footprint analysis. The authors themselves talk about the footprints influencing the values of the fluxes (Pg 4, ln 33 or Pg. 14, ln 23-24), however, no footprint analysis is provided. I suggest the authors use the footprint model of Kljun et al. (2015) to examine the differences in the source area of the turbulent fluxes for the three different categories. But on another note, the sentence on

Pg. 4 is erroneous: the footprint does not induce "uncertainty in estimation of fluxes" if the fluxes are calculated from the eddy covariance, they might just represent turbulence originating from other locations.

As suggested by the referee, we have added the footprint analysis in the new manuscript. It has been added in Appendix A. To estimate the footprint we have employed the approximate analytical model from Hsieh et al. (2000), which is based on lagrangian stochastic dispersion models and dimensional analysis, in combination with the 2D extension from Detto et al. (2006), to include the contribution of lateral spread. This footprint model was chosen because it is developed for thermally stratified surface layers, it is practical and has been applied in many studies, giving satisfactory results when compared with other footprint models. In order to use the model from Kljun et al. (2015), however, we would have to estimate the boundary-layer height. But, as pointed out by Referee#2, when having a low-level jet the turbulence above the jet maximum might be disassociated with the surface, and therefore the estimation of the boundary-layer height has a great uncertainty. Therefore, we have chosen the models from Hsieh et al. (2000) and Detto et al. (2006) instead of the one from Kljun et al. (2015).
With respect to the sentence from the old manuscript brought by the referee, we do agree it is erroneous. Hence, it has been eliminated from the new manuscript, and the explanation associated to the footprint analysis has been revised.

3. Structure of the katabatic flow. I find myself wondering if not doubting if the flow the authors are studying should be classified as katabatic at all or not. Katabatic flows possess very specific characteristics: a low level jet, formation due to surface temperature deficit and retardation due to surface friction (turbulent momentum transport towards the surface), very specific turbulent structure associated with its jet profile: negative momentum and positive horizontal heat flux below the jet and the opposite above, minimum in TKE at the jet maximum (see Grachev et al. 2016). The profiles in Fig. 11, particularly for the strong cases do not resemble katabatic ones at all, and the weak cases have a low level maximum that could be also just due to the interpolation scheme, and then appears to have a secondary maximum above the height of the tower. On a side note: why are the sonic measurements not used in the wind speed profiles such as in Figure 11? Could it be that it actually is the basin flow (Pg 2, ln 15. How do you ascertain that your flow is not actually influenced by the Madrid basin and is purely katabatic). The authors should look at the profiles of the turbulence quantities to first identify if their flow qualifies as katabatic and second to actually show if their profiles in Fig. 11 are physical at all or the low level jet in the weak case is purely a construct of interpolation scheme, and what is happening with the secondary maximum. The profiles of turbulent quantities would also allow them to estimate the jet maximum height in each individual period. The jet maximum height is indeed the vital parameter when studying anything related to katabatic flows since at jet maximum wind speed will be maximal but the turbulence will be zero – and thus exactly the opposite of standard flat-terrain stable boundary layer structure that the authors so heav-

ily rely on in the HOST, MOST, shear capacity and other diagnostics. In that respect, if there is really a low level jet maximum below 3 m in the weak cases, then the turbulence above that height light be disassociated with the surface and therefore not exhibit standard boundary layer characteristics. Not taking this fact into account invalidates the conclusions.

Regarding referee's first wonder, and as previously explained, we have changed the denomination of all the events as a whole to "downslope flows". As explained in Sect. 4 of the new manuscript, weak downslope flows share the characteristics of katabatic flows, but moderate and particularly intense downslope flows show a distinct behaviour due to the dynamical input from the nearby slope. We have explored the physical mechanisms responsible for their formation by calculating the heat and momentum budgets for the three representative events in Sect. 4.1. In addition, we have included the profiles of the turbulent fluxes in Figs. 8–10, which support the existence of the katabatic jets on the weak downslope events (rejecting therefore the idea about being a construct of the interpolation scheme), and of the jet maxima above 10 m particularly on the intense events. A deep analysis about the physical mechanisms underlying, the thermal structure of the flow, as well as their interaction with turbulence for a representative weak, moderate and intense downslope event has been included in Sect. 4.
With respect to the wind-speed measurements from the sonic anemometer, they have not been included in the profiles because they introduce an extra instrumental bias on the wind profile, which is based on the cup-anemometer measurements at 3, 6 and 10 m. In some weak events, wind speed is very weak at all levels ($\simeq 1$ m s^{-1}), and therefore the instrumental bias could introduce an important deviation from the real profile. An example of this instrumental bias for the investigated weak event (13/08/2017) is shown in the following Fig. IV. It can be observed that the form of the peaks and minima is not always coincident between both instruments. Since from the cup anemometers we cover a larger vertical profile, we use those measurements to characterise the wind profile.
With regard to the downbasin flow, it is easily discriminated from the local downslope flow in our study site. Downslope flows are directed from the Guadarrama mountain range (i.e. from the NW), whereas downbasin winds approximately follow the direction of the main rivers of the area (i.e. from the NE during night-time). This distinction was also made in Plaza et al. (1997). Despite some of the events are affected by the irruption of downbasin winds, in the figures we just represent the profiles at times in which downslope flows are blowing, as required by the selection algorithm. Fig. 1a in the new manuscript has been changed, so that local slopes and the basin are better distinguished.
And regarding the last comment about the connection of the measured turbulence with the surface, the analysis of the regime transition from non-dimensional parameters (Sect. 4.2 from the old manuscript) has been eliminated, and therefore MOST does not need to be assumed anymore. With regard to the figure with the HOST transition (Fig. 7b), we believe that the transition of nocturnal regimes explained in Sun et al. (2012) is clearly identified from the represented turbulence data at 8 m. Furthermore, Sun et al. (2012) relate the occurrence of the distinct turbulent regimes with the existence of low-level jets during CASES-99.

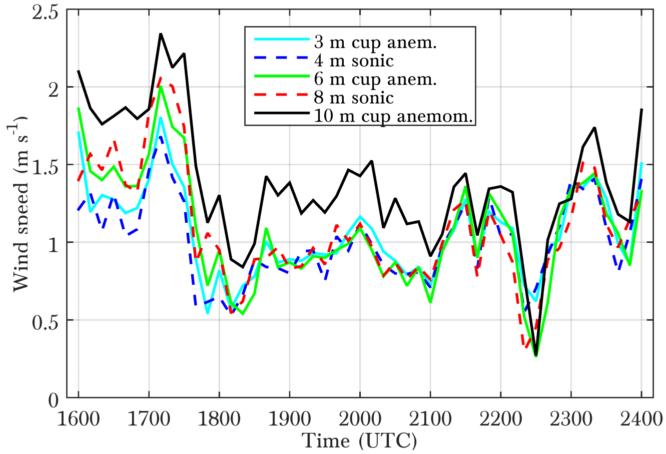

Figure IV: *Times series of the wind speed from 1600 to 2400 UTC on the 13/08/2017. Solid lines represent the measurements from cup anemometers, and dashed lines from sonic anemometers.*

4. Study of budgets The authors should present budget of the momentum and heat to substantiate their claims (such as the section 3 and 4 when talking about the development of the flow and its interaction with turbulence and transition to very or weakly stable boundary layer), and also to more fully understand the processes at hand. By examining the budgets of the katabatic flow one could isolate the importance of individual terms (local generation, dissipation, advection etc) on the weak, intermediate and strong katabatic flows and therefore show if the weak katabatic flow for example is locally driven and the strong katabatic flow is advected from the steep slopes 2km away, whereby the change in slope (from 25° to 2°) leads to the deepening of the flow as observed by Smith and Skyllingstaad (2005). The budgets will also show the importance of mesoscale and not just the large-scale pressure gradients on the flow, even if only as a residual term. The budgets could answer where the claims that stronger unstable turbulence facilitates intense katabatics. This indeed is counterintuitive as for the katabatic flow to develop one needs a large temperature deficit (i.e. cooling) and turbulence suppresses the katabatic flow while unstable stratification does not even allow the development of katabatic flow (Pg. 8, ln 7-13).

As requested by the referee, the analysis of the heat and momentum budgets has been included in the revised manuscript (Sect. 4.1). Indeed, we have been able to prove that weak downslope flows (which behave as pure katabatic flows) are driven by the buoyancy acceleration triggered by the local temperature deficit, whereas moderate and particularly intense downslope flows, have an important dynamical contribution from the nearby steep slope. Hence, the arrival of the downslope flow can take place before the thermal profile becomes stable at the measuring point, and the strengthening of turbulence is not constrained by negative buoyancy.

5. Stratification How was the virtual potential temperature calculated? Was the humidity needed to convert air temperature to virtual potential temperature used from Irgason and at which level? Also about the calculation of the potential temperature gradient: On Pg. 7, ln 20 and Equation 1, if you are using a 3th order polynomial why is the stratification calculated only from delta? The true temperature gradient dTheta/dz (if one takes the derivative of Eq 1) has contributions from beta, gamma and delta and depends on height.

As explained in Lines 22-23 on Page 8, virtual potential temperature was calculated from temperature measurements from aspirated thermometers at 3, 6 and 10 m, and humidity measurements in a T/RH probe at 2 m.
With regard to the logarithmic fit of the virtual potential temperature profile, even though θ_v also depends on β and γ, the static stability is best described from the value of δ. This information has actually been extended and included in Appendix B, by showing an example of the distinct static stabilities of the thermal profiles during a moderate event. We did not mean to say that the gradient is independent of β and γ, but that the static stability of the profile can easily be inferred from this parameter. The classification from Eq. B2 was designed after a thorough check.

6. Origins of the flow. Tied to the previous comments, the paper fails to determine the origin of the katabatic flows. For example, on Pg. 8, ln 5 the authors mention that the stratification is unstable and net radiation positive during the onset of the strong katabatic episodes. Given that the katabatic flow is caused by stable stratification (temperature deficit) and therefore cannot develop in unstable stratification, the authors should show evidence of why they think their flow is katabatic, and how and where it originates from (does it originate on the steep slope where the stratification has already turned stable due to shadowing? And is now merely advected to the study site?)

As explained in previous responses to queries, the investigation of the budgets has provided further proof about the origin of the distinct downslope flows: weak downslope being katabatic, and moderate and intense downslope being partly or mostly dynamically induced by the nearby slope. Considering that the axis of the mountain range is directed SW-NE, sunset takes place at the back of it, and therefore the cooling down of the surface starts earlier in the slope than at the foothill. If the low soil moisture favours the stronger cooling and the synoptic wind, despite being weak, is from the NW, the downslope advection of cold drainage fronts is possible, arriving at the foothill when still the thermal profile is unstable and net radiation positive. Such scenario was for instance observed in Papadopoulos and Helmis (1999).

7. Figure 7 and the correlation to soil moisture Putting the 4th order fit through the data presented in Figure 7 is stretching it beyond any justifiability. Indeed, the spread of the data is so large that a linear fit would be possible at maximum to show that for low soil moisture G is slightly positive, but for high it is mostly negative. The results for longwave-radiative loss show no correlation between the data, both linear or non-linear. I therefore protest against any conclusions

based on these fits and the identified two maxima.

We agree with the referee that the spread of the data is large and that the correlation is low. Therefore, we have removed the figure and the associated explanation from the manuscript. Instead, a short comment about this vague correlation has been included in the revised manuscript (Lines 21-24 on Page 9).

8. Energy balance closure and the ground heat flux. It is interesting to note that the level of energy balance closure is so high in the study site. How did the authors calculate the ground heat flux and the heat storage in the layer above the heat flux plate? Given the nature of the weak and the strong flows, one could also argue on the importance of advective processes, but the energy balance appears to suggest that advection is not important for strong katabatic flows.

Given the uncertainty in the calculation of the energy-balance closure, the figure in which we showed the different components has been eliminated. Instead, we have preferred to explore the nature of different downslope flows by analysing the heat and momentum budgets, as suggested by this referee. In fact, the analysis of the energy-balance closure is out of the scope of this study.

9. Interaction with turbulence The second motivation of the study talks about the interaction between turbulence in SBL and katabatics but actually, the relevant turbulence that is interacting was found to be unstable stratification before the onset of the wind itself.

The referee is right when stating that not just turbulence in the SBL is interacting with downslope flows. Indeed, the moderate and intense downslope flows that are taken as case studies, are established before the onset of the SBL. For that reason, the second motivating aspect of the paper has been changed from "The interaction of katabatic winds with local turbulence in the SBL and the implication in turbulent characteristics" to "The interaction of downslope winds with local turbulence and the implication in the characteristics of the SBL". Note that we still mention the SBL, since the characteristics of the SBL (once it is established) are strongly affected by the nature of the different downslope flows, and is one of the motivating aspects of the study. On the other hand, the term "stable boundary layer" has been replaced by "boundary layer".

10. Language I suggest a professional or native speaker to check the language as I didn't want to enumerate all the things that need to be changed (e.g. "avoids" should be "prevents", "striked" should be "stricken" or rather something more appropriate, "fogs" as plural does not exist, it is always "fog", "emplacement" sounds awkward etc.)

As suggested by Referee#2 and Referee#3, a native speaker has revised the manuscript and given some language corrections and improvements, apart from those provided by the referees.

Individual Major Corrections

2. Pg. 1, ln 1. What are the "dynamic and turbulent" features of SBL?

We have changed it to "the turbulent characteristics and thermal structure".

3. Pg. 1, ln 8: In Figure 6, the limits is 3.5 m/s not 6. What is the correct number?

The lower limit is 3.5 and the upper limit 6: it has been clarified in the new manuscript.

4. Pg. 3, ln 8-14: No, even over flat terrain with the existence of a low level jet or even in very stable stratification without a low level jet MOST is not valid

As commented in the response of Query 6 from Referee #1, Fig. 10 from the old manuscript was eliminated for various reasons. Therefore, the validity of MOST is not assumed anymore in this analysis.

5. Pg 4, ln 24: why would a strong surface thermal inversion necessarily be allowed to develop just because the slope angle is low, if there is enough wind to prevent its development?

This referee is right when stating that strong wind can prevent the development of the thermal inversion. However, what we mean is that over a shallow slope the buoyancy acceleration is smaller than over a steep slope, and therefore the thermal inversion can be stronger, just with the locally generated flow and without the erosion of drained downslope flows from the nearby steep slope. This has been clarified now.

6. Pg. 4, ln 35: you have not mentioned the CO2 fluxes at all until the moment when you mention the negligible effect of the urban area. Or do you mean the effect of urban area on all the fluxes?

We meant the effect on all the turbulent fluxes. In any case, the explanation associated with the footprint has been changed in the new manuscript (Lines 4-8 on Page 5 and Appendix A).

7. Figure 3. Why is there no data from the sonics? Also, the way the data are presented all lumped together does not show if there are wind maxima at different heights for the different episodes, periods. I suggest the authors calculate the jet maximum if possible, normalize the height with it and then plot the normalized profiles.

With respect to the wind data from the sonic anemometers, as explained in the Specific Major Comment 3, they have not been included in the profiles or Fig. 3 due to the instrumental bias they introduce. Apart from that bias, due to the closeness between the levels and to the fact that sonic levels are in between cup-anemometer measurements, they do not provide further information of interest about the wind profile, as can be seen from the following Fig. V.

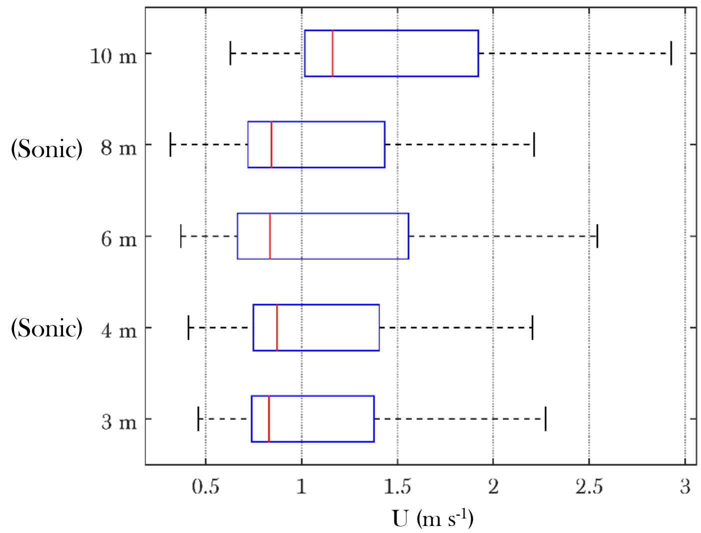

Figure V: *Same as Fig. 3a from the manuscript but including sonic measurements at 4 and 8 m.*

On the other hand, the referee is right that this plot does not show the location of wind maxima. In fact, as inferred from the sign of the turbulent fluxes in Figs. 8–10, the jet maximum in many cases is probably located above 10 m. This plot was included in order to show the wind-speed frequency distributions and range of values at different levels, and to introduce and motivate the classification of events. In addition, as shown for the three representative events in Sect. 4.2, the determination of the jet-maximum height is not always possible only with two sonic measurements. Therefore, even though the normalisation of the height with the jet-maximum location is an interesting exercise, it is not feasible in this study without carrying great uncertainty.

8. Pg. 6, ln 20-26: Nowhere in Figure 3 is it visible that there is a development of skin flow or where the jet maximum is. Indeed, the figures seem to suggest that the jet maximum is always above 10 m.

We agree that from Fig. 3 the development of a skin flow and the position of the jet maximum are not visible. We only state that the median at 3 m is similar to that at 6 m, and the first quartile is even smaller at 6 m than at 3 m, which does not occur for the third quartile. This occurs as a consequence of the skin flow on the weak downslope events, but not for all the downslope events, and therefore it is not observed for the whole distribution. The skin flow is shown in Fig. 10b (weak event), and is clearly absent in Fig. 12b (intense event), since the jet maximum is above 10 m.

9. Figure 3 should also include the information on the temperature profiles and turbulence.

We acknowledge this suggestion but we prefer just to include the box plots for the wind speed. At the point of the manuscript where this Fig. 3 is introduced, frequency distributions for the temperature and turbulence would not contribute to the motivation of the classification into the three downslope events, which is the main purpose of this figure. On the other hand, thermal and turbulence profiles are shown in Figs. 8–10, and we think they provide more interesting information on the thermal structure and interaction with turbulence than the frequency distribution of these variables itself.

10. Pg 7, ln 6-7: I find it very strange that the results at 3 and 10 m do not show conformation to the classification and only 6m is so good. The sonic data should be used to study this more in detail and give a physical explanation why this is so.

In those lines (Lines 10-11 on Page 8 of the new manuscript) we state the following: "At the levels of 3 and 10 m the events showing different features cannot be so clearly detached, and therefore the level of 6 m is employed for the classification". We did not mean that the levels of 3 and 10 m do not show conformation to the classification, but that the classification into the three downslope types with contrasting turbulence conditions is better produced using the level of 6 m. On the other hand, as commented above, due to the instrumental bias we prefer not to use measurements from sonic anemometers, but from cup anemometers.

11. Flocas et al. reference is missing from the list of references

Thank you for pointing out the missing reference. It has been included in the revised manuscript.

12. Pg. 7, ln 17: is it the soil or the skin temperature?

It is actually the skin temperature. It has been corrected.

13. Pg. 8, ln 1: is the low value of TKE due to the jet maximum being close to the measurement height or because one is above the jet?

In some cases it could be due to the closeness of the jet maximum, but in that group some moderate downslope flows are also included, and the skin flow is not always developed. With the available information, we cannot specify the position of the jet maximum for all the events within the group of low TKE. Note that Figs. 4 and 5 from the old manuscript have been merged into Fig. 4 of the new manuscript.

14. Pg 8, ln 6: katabatic flow develops turbulence through shear generation. What you mean to say by "relation between katabatic flow and turbulence" I

guess is, the turbulence before the onset of the flow

We think that the interaction is bidirectional. The turbulence at the onset influences the downslope flow, and at the same time, the downslope flow itself affects the turbulent characteristics of the boundary layer. This can be checked up for instance from Figs. 8–10.

15. Pg. 9, ln 3: why would large soil moisture after precipitation during nighttime lead to the enhanced cooling of the soil?

From the available data, we cannot draw conclusions about the mechanism explaining that fact without being speculative. Additionally, the figure associated with this explanation has been eliminated from the manuscript. Hence, investigating the mechanism that explains why after strong precipitation surface cooling can be enhanced is not within the scope of the paper.

16. Pg 9, 7-13: The study on the influence of stratification needs to be associated with momentum and heat budgets of the flow to show whether the conclusions drawn are substantiated in the text.

Now, the heat and momentum budgets have been calculated and analysed in Sect. 4.1.

17. Pg. 10, ln 8-19: the transition will depend on the location of the jet maximum and if it is below 6m or above or is moving between. The exact value that is lower than in Sun is indeed no wonder given the fact that there is a jet maximum present and not in Cases-99.

As explained in the Specific Major Comment 3, Sun et al. (2012) relate the occurrence of the distinct turbulent regimes with the existence of low-level jets during CASES-99. In our case, the transition occurs independently of whether the jet is located below or above the sonic measurements. Even if the skin flow is present (below 3 m) for many weak downslope events, we cannot assure that the jet maximum is close to the sonic measurements whenever $U < 1.5$ m s^{-1} (including some moderate events in black). We can only comment about this possibility (sentence included now on Page 11 Lines 3-4).

18. Pg. 11, ln 1-3. The two sentences on MOST cannot be applied to the current study without more understanding of the processes studied. MOST will only be applicable very close to the surface if the stratification is weakly stable and turbulence is well developed, and if the terrain is horizontally homogeneous. If there is a low level jet close to the tower height or even worse, within the tower height, MOST by definition is not valid as there is another height scale that is more important than z/L.

For the reasons presented in the response to Major Point 6 from Referee#1, Fig. 10 from the old manuscript has been eliminated. Therefore, the compliance of MOST is not assumed anymore in this work.

19. Pg. 11, ln 25: Isn't the fact that Fig 10b resembles Fig. 8b by construction since you change the definition of shear capacity to match the HOST?

Idem to the previous response, Fig. 10 has been eliminated.

20. Pg 12, ln 4-6: The calculation on Rb will again depend on the existence of the jet maximum if it is below, and therefore it doesn't make much sense as a measure. A better measure would be the gradient Richardson number Ri which the authors could calculate from the interpolated profiles and therefore obtain a profile of Ri.

Idem to the previous response, Fig. 10 has been eliminated.

21. Pg 12, ln 24: say that the value of the diurnal peak is not shown, or do you refer to the little part before the transition shown in Fig. 11?

We refered to the diurnal peak throughout the day, so we include "(not shown)" in the manuscript.

22. Pg. 12, ln 25: does the flow arrive or develop? Show the budgets

This is an important and interesting point that has been clarified in the new manuscript after showing the budgets. As it has been demonstrated, the weak downslope events are mainly locally generated katabatic flows, whereas some moderate and particularly intense downslope flows are dynamically induced and propagated from the nearby steep slope, so that we can say that the flow "arrives" for them.

23. Pg. 12, ln 1-2: how accurate is this wind maximum that does not exist in the measurements but only in the interpolation?

From the measurements, particularly at 2230 UTC, the existence of the jet maximum around or below 3 m can be guessed ($U_{3m} > U_{6m}, U_{10m}$; Fig. 10b). Moreover, the profiles of the momentum and horizontal-heat turbulent fluxes (Fig. 10d) support the existence of the jet below 4 m.

24. Pg. 12, 5-6: In Grachev et al. (2016) paper it says that the flow is stationary but only in the well-developed phase. You are focusing on the transition.

Thank you for pointing this out. That sentence has been eliminated from the manuscript.

25. Pg. 15, ln 23: "form when maximum wind speed is kept" should be "have maximum wind speeds below", because indeed you are talking about the katabatic wind speed of 1.5 and not the ambient wind speed into which the katabatic wind impinges.

Thank you for the suggestion, it has been accordingly changed.

26. Pg. 15, 26: Wind shear is not driving katabatic flow, the driver is the negative buoyancy and wind shear is the product of the katabatic flow itself.

We do agree with the referee. That sentence has been eliminated and the whole explanation has been revised.

27. Pg. 15, ln 29: "intense katabatics are found" should be "int. kat. have maximum wind speeds..". It is again a question of cause and effect.

Thank you for the suggestion, it has been accordingly changed.

28. Pg. 16, ln 10: the scaling regime expected to be valid for at least very stable conditions is local or most likely z-less scaling.

Idem from Specific Major Comments 19 and 20. Fig. 10 from the old manuscript and the associated conclusions have been withdrawn.

29. Pg. 16, ln 25: influence of submesoscale phenomena will be visible in the calculated ogives or multi-resolution flux decomposition.

The referee is right that the influence of submeso motions is visible from the calculated MRFD plots (Figs. I-III), particularly over a timescale of around 10^3 s. In any case, the analysis of submeso phenomena was not within the scope of the article, and therefore, MRFD analyses have not been included in the revised manuscript.