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Interactive comment on "Weak and intense katabatic winds: impacts on turbulent characteristics in the stable boundary layer and CO_2 transport" *by* Jon A. Arrillaga et al.

Anonymous Referee #2

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Review of the "Weak and intense katabatic winds: impacts on turbulent characteristics in the stable boundary layer and CO2 transport" by Jon A. Arrillaga, Carlos Yagüe, Carlos Román-Cascón, Mariano Sastre, Gregorio Maqueda, and Jordi Vilà-Guerau de Arellano

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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

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strong katabatic flows. Finally, they study the conditions under which each of these occurs and the associated boundary layer structure and CO_2. 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.

Specific Major Comments 1. Turbulence data processing As already mentioned by the first reviewer, 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. 2. Footprint analysis 1. 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. 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 heavily 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 might be disassociated with the surface and therefore not exhibit standard boundary layer characteristics. Not taking this fact into account invalidates the conclusions. 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). 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. 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

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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?) 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. 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. 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. 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.)

Individual Major Corrections 2. Pg. 1, ln 1. What are the "dynamic and turbulent" features of SBL? 3. Pg. 1, ln 8: In Figure 6, the limits is 3.5 m/s not 6. What is the correct number? 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. 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? 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? 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. 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 10m. 9. Figure 3 should also include the information on the temperature profiles and turbulence. 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. 11. Flocas et al. reference is missing from the list of references 12. Pg. 7, ln 17: is it the soil or the skin temperature? 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? 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 15. Pg. 9, ln 3: why would large soil moisture after precipitation during nighttime lead to the enhanced cooling of the soil? 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. 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. 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. 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? 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. 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? 22. Pg. 12, ln 25: does the flow arrive or develop? Show the budgets 23. Pg. 12, ln 1-2: how accurate is this wind maximum that does not exist in the measurements but only in the interpolation? 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. 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. 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 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 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. 29. Pg. 16, ln 25: influence of submesoscale phenomena will be visible in the calculated ogives or multi-resolution flux decomposition.

Extra references: Kljun, N., Calanca, P., Rotach, M. W., and Schmid, H. P.: A simple two-dimensional parameterisation for Flux Footprint Prediction (FFP), Geosci. Model Dev., 8, 3695-3713, 2015. Večenaj, Ž., Belušić, D., Grubišić, V. et al. Boundary-Layer Meteorol (2012) 143: 527. https://doi.org/10.1007/s10546-012-9697-6

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