Responses to comments from Referee #1

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.

Judgement:

I think that obtained results may be useful for further understanding of the katabatic flows and manuscript is suitable for publication in the ACP, however not before a major revision. I recommend acceptance with major revisions although most of the comments are not that major and related to lack of clarity. My specific comments are listed below.

We thank Referee #1 for his/her review about the manuscript and for highlighting its suitability for publication in ACP. Responses to the specific comments are given point-by-point below, and the changes undertaken in the manuscript can be checked up both from the revised manuscript and the tracked-changes version of the manuscript provided.

Revision issues:

1. Although it is appropriate to refer readers to other papers for the details of the field campaign and instrumentations, more info is needed in this paper than is currently provided (see my remarks detailed below).

As suggested by this referee, we have included further information in the new manuscript regarding instrumentation, data post-processing and corrections. Specifications about each of the posed queries are provided below.

2. Data (post) processing. Data processing is only briefly mentioned (p. 5). More info is needed in this paper for the details of the turbulent flux measurements than is currently provided. A reader (in order to acknowledge the results) would want to know: 1) how the filtering was done (block average, high-pass, other?), 2) what data-quality control checks were used, 3) how the wind stress (or momentum flux, friction velocity) was computed in Fig. 11? Based only on the longitudinal (or downstream) $< u'w' >$ wind stress component or both longitudinal and lateral (or crosswind) $< v'w' >$ stress components? Why?

The referee is right when stating that the data processing and corrections were briefly mentioned. Following his/her query, we have included further information about the filtering, data quality control checks and correction of the turbulent fluxes, as well as minor manual checks, in the new manuscript (Page 5 Lines 22-34, and Page 6 Lines 1-6).
Regarding the calculation of the friction velocity, since the double rotation (and not the triple rotation) has been applied to the sonic coordinate system (as specified in the new manuscript), the friction velocity was calculated considering both the longitudinal- and lateral-stress components.

3. Large errors in the measurement of the turbulent fluxes can result from relatively small errors in the alignment of a sonic anemometer due to the cross contamination of velocities (i.e. fluctuations in the longitudinal wind speed components appear as vertical velocity fluctuations, and vice versa). To avoid these

errors rotation of the anemometers' axes is needed to place the measured wind components in a streamline coordinate system. The most common method is a double rotation of the anemometer coordinate system to compute the longitudinal, lateral, and vertical velocity components (Kaimal and Finnigan, 1994, section 6.6). Was this done?

We agree that rotation of the sonic-anemometer axes is needed to prevent errors in the estimation of the turbulent parameters. Indeed, the double-rotation method was applied to compute the longitudinal, lateral and vertical velocity components. It has now been specified in the new manuscript (Lines 32-34 on Page 5).

4. Since the sonic anemometer measures the so-called 'sonic' virtual temperature (which is close to the virtual temperature) the moisture correction in the sonic anemometer signal is necessary to obtain the correct value of temperature itself and sensible heat flux (e.g. Kaimal and Finnigan, 1994). Authors reported the sensible heat flux (Figure 9). To value the present results the authors should either show that the moisture corrections and their impact on the results are small, or (if otherwise) apply moisture corrections to the sonic temperature following Schotanus et al. (1983) based on the data collected by the Campbell fast-response open path infrared gas analyzer listed in Table 1.

Moisture corrections were applied to the sonic temperature following Schotanus et al. (1983) to derive the air temperature and sensible-heat flux. In fact, all the parameters based on the fast-temperature measurements shown in the figures along the manuscript have undergone this correction. It is described in the Easyflux DL software (Campbell Scientific, 2017) and now explained in the new manuscript (Line 34 on Page 5 to Line 2 on Page 6).
The effect of applying both moisture and Webb corrections (linked with next query by Referee#1) to the calculation of respectively the sensible and latent heat fluxes, is shown below in Fig. I for a selected downslope event within the analysed period.

References:

Cambell Scientific: EasyFlux DL Eddy-Covariance CR3000 Datalogger Program, `https://s.campbellsci.com/documents/us/product-brochures/b_easyflux-dl.pdf`*, 2017*

Schotanus P., Nieuwstadt F.T.M., De Bruin H.A.R. (1983) Temperature measurement with a sonic anemometer and its application to heat and moisture fluxes. Boundary-Layer Meteorol. 26(1): 81–93. DOI:`10.1007/BF00164332`
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5. Authors say nothing about the Webb correction (also referred as WPL or Webb effect after the paper by Webb et al. [1980]). This correction must be taken into account when the turbulent fluxes of minor constituents such as carbon dioxide or, in some cases, water vapor are measured (Webb et al. 1980).

The referee is right that nothing about the WPL correction was mentioned in the manuscript,

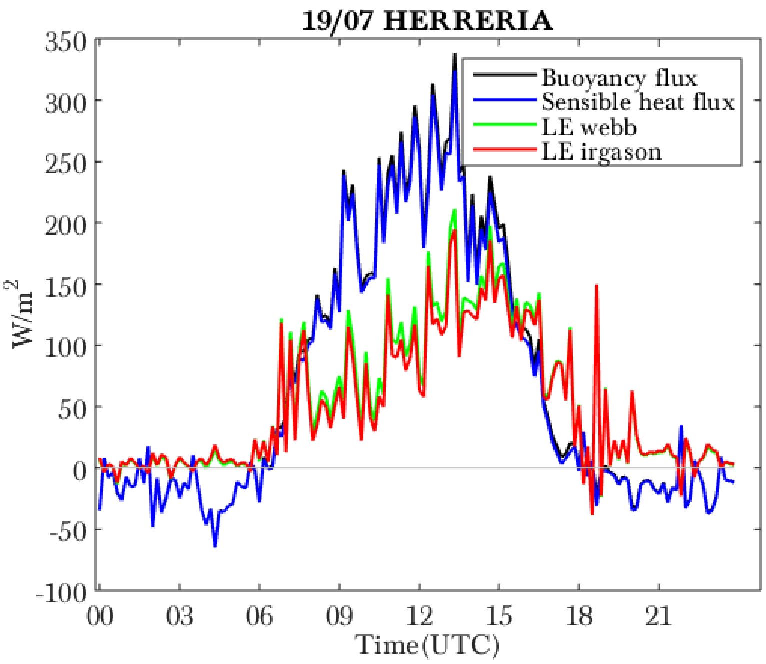

Figure I: Buoyancy flux, sensible heat flux (after moisture correction) and the latent-heat flux before and after the Webb correction on July 19 2017 in La Herrería.

although it had been considered to correct latent-heat and CO_2 turbulent fluxes. It is mentioned now in the new manuscript (Line 2 on Page 6) so that the reader will know that this correction has been applied. As an illustrative proof, Fig. II compares the CO_2 turbulent fluxes before and after applying this correction for the same downslope event from Fig. I. It can be observed how the Webb correction is considerably important during daytime.

6. In a slope-following coordinate system, the horizontal (along the slope) heat (buoyancy) flux contributes to the net buoyancy term and, therefore, the Monin-Obukhov stability parameter z/L (see page 11 and Fig. 10) contains this additional term (e.g., see Grachev et al. 2016, their Eq. (3) and references therein). Authors say nothing about this issue for calculation z/L which is very important point for katabatic flows.

We thank the referee for this interesting and important aspect, but given the issues with the horizontal heat fluxes, the challenging application of the MOST theory stressed by Referee#2, and the fact that the calculated stability parameters are not strictly needed for the conclusions drawn in this study, we have removed former Fig. 10 from the old manuscript.

Minor and editorial/technical comments:

I. Page 5, Line 8. Replace CO^2 by CO_2.

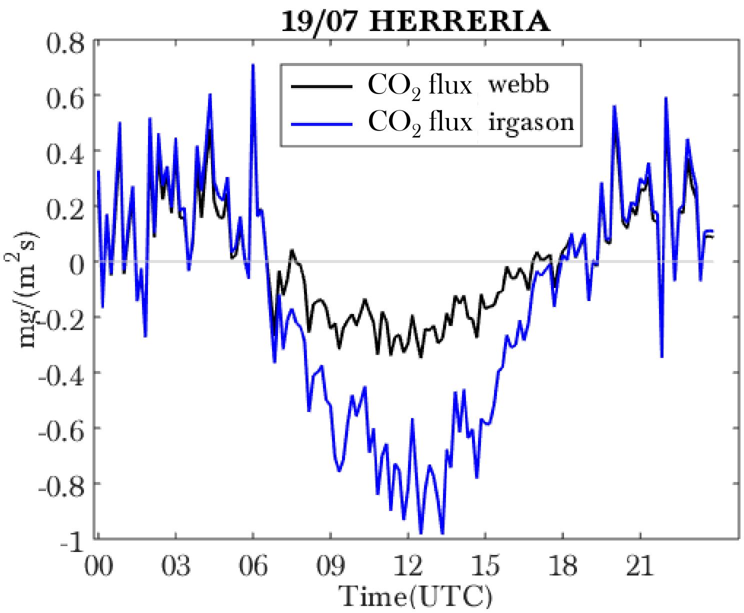

Figure II: CO_2 turbulent fluxes before and after the WPL correction on July 19 2017 in La Herrería.

Corrected, thank you.

II. Page 11, Line 28. I suggest to provide a definition of the Monin-Obukhov stability parameter (z/L) and the bulk Richardson number (R_B) for a layman reader.

We thank Referee#1 for this suggestion, but for the reasons given in the response to Query 6 above, those definitions are not necessary anymore in the new version.

III. Page 12, Lines 2-9. I would like also to see here a discussion on difficulties and controversy of interpretation associated with the critical Richardson number (e.g., Grachev et al. 2013 and references therein).

For the reasons explained in the preceding comment, that discussion is not needed anymore.

References: Replace 'Boundary Layer Meteorol.' by 'Boundary-Layer Meteorol.' (dash is missed).

Corrected, thank you.

Page 19, Line 1. Please correct Silvana's name: Di Sabatino S. instead Sabatino, S.D.

Corrected, thank you.

Page 20, Line 9. Please correct reference Pardyjak et al. as follows: Pardyjak E.R., Fernando H.J.S., Hunt J.C.R, Grachev A.A., Anderson J.A. (2009) A case study of the development of nocturnal slope flows in a wide open valley and associated air quality implications. Meteorologische Zeitschrift, 18(1), 85–100. DOI: 10.1127/0941-2948/2009/362

Accordingly corrected, thank you.