Answer to reviewer #2 comments.

We kindly thank the reviewer for his useful comments and his reference suggestions. We carefully read the proposed papers and included the references along the text when necessary. In the revised manuscript, we provide a short comparison of the random error on the Eddy Covariance fluxes found in this study with the error obtained in the other suggested studies. Nevertheless, since the context of these other studies was very different than our, we were not able to comment in detail all of these differences.

Answer to all general, specific, and editorial comments can be found below. References to changes in the main text have been introduced when necessary. We provide an edited version of the manuscript, and a final version. Pages and lines numbers refer to those of the final version of the manuscript. Added text related to reviewer #1 comments is shown in red in the edited version, and in blue for answers related to reviewer #2 comments.

Specific comments.

Page 1056, Abstract and through the text. "Upslope" flows are also referred as "anabatic flows" (e.g., Fedorovich and Shapiro 2009).

We agree with the reviewer. The name “upslope” was used in previous publications (Sicart et al., 2014; Litt et al. 2015) dealing with the same dataset, so we wanted to keep it for consistency. We changed the term “upslope” for “anabatic” in the introduction and the first sections of the paper. We then specified (in Section 2.3) that these types of flows were referred to as “upslope”, and use this latter word in the rest of the text. We inserted the proposed reference when introducing katabatic and anabatic (upslope) winds in the Introduction.

Changes in the text:

Section 1, Introduction, page 4, lines 27-28,

“At night and during the morning, a marked temperature inversion at low height above the surface (2–3 m) favors the development of katabatic flows (Fedorovitch and Shapiro, 2009)”

Section 1, Introduction, page 5, lines 2-3,

“During afternoons of the dry season, ~~upslope~~anabatic valley winds are frequently observed (Fedorovitch and Shapiro, 2009), while the temperature inversion is less marked.”

Section 1, Introduction, page 5, lines 15-18,

“For each of the three wind regimes observed, i.e., pure katabatic flows, strong downslope flows and anabatic ~~upslope~~ flows, […]”

Section 2.3, Meteorological conditions and wind regimes, page 7, line 23,

~~“Upslope winds~~Anabatic flows (referred to as “upslope” herein) generally occur around midday and […]”

Page 1056, Abstract and page 1081, Conclusions. Authors wrote: "On average, both fluxes exhibit similar magnitudes and cancel each other out." It's interesting to estimate contribution of water vapour (latent heat) flux in the net buoyancy term in your case.

Sensible and latent heat fluxes frequently cancel each other out over snow or ice surfaces (Harding et al. 1996), especially on high altitudes sites, where sublimation can be strong (Wagnon et al., 2003; Sicart et al., 2005; Winkler et al., 2009).

Page 1063, Eq. (2). Did you use Eq. (23) from Webb et al (1980, QJRMS) for corrections? Specify and provide estimations of the Wpl term in your Eq. (2). I guess, it should small.

The WPL term is provided directly by the Edire software (University of Edinburgh) that we used to treat our EC data. The software calculation is based on equation eq. (24) in Webb 1980. Estimations of the WPL term are indeed very small, as specified in the text at the end of the same paragraph at lines […] (new version):

"The Webb–Pearman–Leuning term (WPL) (Webb et al., 1980) generally remained below 2 W m−2 throughout the campaign"

It might be of interest to provide a plot for Bower ratio, Bo = H/LE based on the turbulent and bulk estimates.

Since the paper includes already many figures and since the focus of the paper is not about Bowen ratio, we only provide the plot herein (See Fig.1). This figure could be added in the paper if the reviewer requires it. A plot of the Bowen ratio is indeed interesting to see if the Bulk method underestimates are similar on *H* and on LE.

The Bowen ratio is highly variable (from 3 to -6), resulting from the fact that *H* is often near zero during the day and exhibits large magnitudes during the night, whereas LE remains significantly negative most of the time. In these cases the bulk method provides lower magnitudes for the Bowen ratio than the Eddy-covariance method. When the sum of *H* and LE is near zero during the night the Bowen ratio is negative since *H* and LE are opposed in sign. The EC method provides more negative Bowen ratio than the bulk method. This is because the bulk method does not underestimate the magnitude of LE to the same extent than it underestimates that of *H*. The *H* magnitude underestimation is more important than LE magnitude underestimation. During the day, *H* is near zero while LE losses remain large and the Bowen ratio is near zero and both the EC and bulk method provide similar, small Bowen ratios.

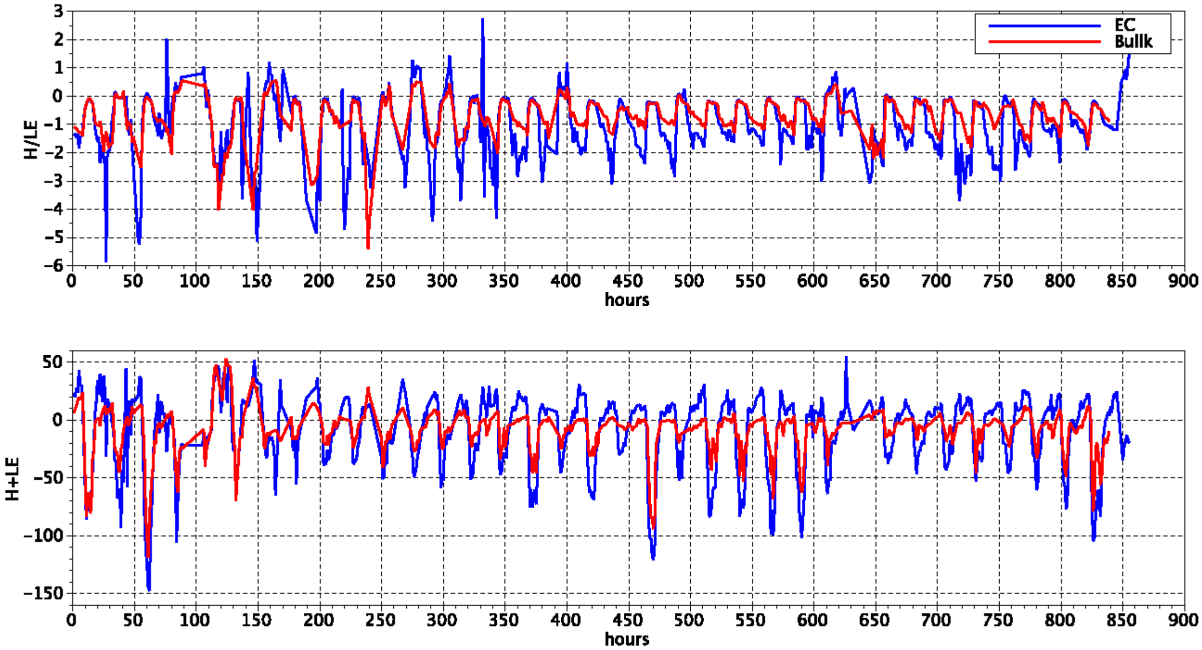

Figure 1: Upper panel: change with time of the Bowen ratio evaluated with (blue) the EC method and (red) the Bulk method. Lower panel: change with time of the sum *H*+LE evaluated with (blue) the EC method and (red) the Bulk method.

Pages 1091-1096, References: Important papers by Vickers et al. (2010) and Salesky et al. (2010) on the random errors of turbulent fluxes have been missed. I would have liked to see a comparison of typical errors/uncertainty for turbulent fluxes from these papers and current study.

Thank you, we carefully read these interesting papers. We added their references in the text in the method section when presenting our method for determining the random errors on EC fluxes, and in the results section where we also provide a short comparison of our results with the results from these two studies.

In the comparison with Vickers et al. (2010), we used the results obtained from nocturnal data collected over their maize site, since it is probably the most similar context compared to our site (stable stratification and short vegetation). From Salesky et al. (2012), we used the single value (10%) of the random error on w'θ' (no evaluation is provided for the latent heat flux) provided in their Figure 6.

Interpreting the differences between our results and those obtained in the proposed literature is not straightforward since the context of the sites is quite different from the context on Zongo glacier.

Changes in the text

Section 3.2.1, Potential random errors in eddy-covariance fluxes, page 9, lines 17-20, *“To characterize this error we applied ~~two different methods:~~ the familiar and straightforward Mann and Lenschow (1994) (ML) and Hollinger and Richardson (2005) (HR) methods. Results are compared with those from studies implying other methods (i.e., Vickers et al., 2010; Salesky et al., 2012).”*

Section 3.2.1, Potential random errors in eddy-covariance fluxes, page 10, lines 18-25,

“The Vickers et al. (2010) method is based on the calculation of sub-record fluxes, i.e., fluxes computed with time scales shorter than 1 hour. It evaluates the random error from the within-run variance of the sub-record fluxes. The Salesky et al. (2012) method is based on similar assumptions than the ML method. A filter of changing time constant is applied to the 1-h flux. Filtered fluxes with increasing time constant converge to the 1-h averaged flux following a power law. Salesky et al. (2012) show that the parameters of this power law can be related to the random error on the flux. Both methods have the advantage of not requiring an estimation of the integral time scale.”

Section 4.1.1, Random error calculations, page 18, lines 11-18,

“In relative terms, for hourly fluxes, the random errors on Hec (resp. LEec) derived from the ML method were between 22 % and 60 % (resp. between 36 % and 98 %). Same orders of magnitudes but slightly lower values were derived by Vickers et al. (2010) with their method, during the night over a maize field (19 % for H and 23 % for LE). With their filtering method Salesky et al. (2012) found much lower values (10 %) for the random error on H for data collected in California. Interpreting these differences is not straightforward since the context of Vickers et al. (2010) and Salesky et al. (2012) was very different from ours. This comparison suggests that our method tends to maximize the errors”

Technical corrections.

Update reference of your paper Litt et al (2015) BLM instead Litt et al (2014) in the text and the reference list.

Done.

Added references in the paper (see answers above):

Fedorovich E, Shapiro A (2009) Structure of numerically simulated katabatic and anabatic flows along steep slopes. Acta Geophysica 57(4):981–1010, doi:10.2478/s11600-009-0027-4

S.T. Salesky, M. Chamecki and N.L. Dias (2012) Estimating the random error in eddy-covariance based fluxes and other turbulence statistics: the filtering method, Boundary-Layer Meteorol.144(1):113-135.

Vickers, D., Gockede, M., Law, B.E. (2010) Uncertainty estimates for 1-h averaged turbulence fluxes of carbon dioxide, latent heat and sensible heat. Tellus B, 62(2),87-99

References cited in this response

Harding, R. J., & Pomeroy, J. W. (1996). The energy balance of the winter boreal landscape. Journal of Climate, 9(11), 2778-2787.

Sicart, J. E., Wagnon, P., & Ribstein, P. (2005). Atmospheric controls of the heat balance of Zongo Glacier (16 S, Bolivia). Journal of Geophysical Research: Atmospheres (1984–2012), 110(D12).

Wagnon, P., Sicart, J. E., Berthier, E., & Chazarin, J. P. (2003). Wintertime high-altitude surface energy balance of a Bolivian glacier, Illimani, 6340 m above sea level. Journal of Geophysical Research: Atmospheres (1984–2012), 108(D6).

Winkler, M., Juen, I., Mölg, T., Wagnon, P., Gómez, J., & Kaser, G. (2009). Measured and modelled sublimation on the tropical Glaciar Artesonraju, Perú. The Cryosphere, 3(1), 21-30.