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Atmospheric Measurement Techniques Discussions

AMTD

8, C135–C138, 2015

Interactive Comment

Interactive comment on "A study of turbulent fluxes and their measurement errors for different wind regimes over the tropical Zongo glacier (16 S) during the dry season" *by* M. Litt et al.

Anonymous Referee #1

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General Comment and Recommendation for Publication

This paper is suitable for publication and quite appropriate for Atmospheric Measurement Technology. I have no major issues with the general approach the authors are taking, formally it is correct, and except for a three minor concerns discussed below under editorial/technical comments (comments 3-5), the paper is quite reasonable and acceptable for publication. Nonetheless, I was left wondering what is the motivation or purpose of this paper, how does it advance the science, and what is the final application of the results. Here two questions/thoughts come to mind, which are addressed below under specific comments (comments 1 and 2): (1) What is the surface energy balance closure (SEBC) of the data set they discuss in this paper? And (2) Were any of the authors in attendance at the Dec 2014 AGU meeting in San Francisco where the latest results concerning the sonic anemometry were presented?

Specific Comments

(1) I think the paper would be far more interesting (at least to me) if it included a discussion of the SEBC. How well do the methods close the energy balance? Is it underestimated as is so often the case with EC measurements? Does the degree of closure vary much with wind regime? Given the present formal error analysis, how much of any imbalance in the SEBC can be attributed to possible inherent uncertainties in the method and instrumentation? It may certainly be the case that the authors are intending to address these SEBC questions in a follow-on paper, but my own personal preference (and recommendation) would be to expand the present paper to include them.

(2) The recent controversy concerning the underestimation of vertical wind speed by non-orthogonal sonics has largely been resolved. The consensus at the last AGU meeting is that some sonics (CSAT-3 and other sonics) do not include any shadowing correction or if they do they may not be doing so correctly, whereas other sonics (ATI) do. The authors appear to be aware of this bias (e.g., Abstract, Page 1056, sentence spanning lines 20-22; Conclusions, Page 1082, sentences spanned by lines 13-18), but unaware of its resolution.

I recommend the authors download Tom Horst's AGU talk, available at his website http://www.eol.ucar.edu/homes/horst/. They should also request a copy of the talk (also presented at the 2014 Fall AGU meeting) by Frank et al. Frank's email address is jfrank@fs.fed.us. (I also have a copy of both talks, but it would be better for all if the present authors went directly to the original sources first.) Finally, the authors should be made aware that both Horst and Frank et al. have papers that are either in press (Horst, Boundary-Layer Meteorology) or in review (Frank et al., Journal of Atmospheric and Oceanic Technology). I think both papers are relevant and should be cited by the authors in their revised manuscript. Both papers should be available from their respective authors, but Horst's paper will be appearing shortly at the journal website as an Online First Article.

Once the present authors have had time to review Horst's and Frank et al.'s work, I think they should be able to clarify for themselves and for their readers the present understanding of sonic anemometer issues and to improve on their claim that non-orthogonal sonics can underestimate fluxes by up to 16% (Page 1082, lines 15-16). But, I would also encourage the authors to do more than just update the reference list and the 16% value. Both Horst and Frank et al. provide (somewhat different) templates (or algorithms) for making shadowing transducer corrections. With some effort (but not necessarily great effort), the authors could include as part of their own sensitivity analysis, the sensitivity of the EC fluxes to the different shadowing correction algorithms. Expanding the authors' current sensitivity/error analysis to include the shadowing correction would improve the present paper and advance the science of micrometeorological measurement technology.

Editorial/Technical Comments

(3) Page 1063, Equation (2) – I believe that the 'Wpl' should be all upper case, i.e., 'WPL'.

(4) Page 1074, lines 10-11 – The authors claim that "relative random errors derived from the ML method canceled out to a mean of 12%". Their statement is at the very least not clear and at worst contradicts their Section 3.3.1 and Equations (15) and (16). According to these last two equations random errors are estimated as positive definite quantities. As such they cannot cancel each other out; they can only add to an ever increasing estimate.

(5) Page 1081, paragraph defined by lines 5-9 – The authors suggest that the BA method severely underestimates the magnitude of the net turbulent fluxes due to its

inability to account for the flux induced by katabatic oscillations or outside-layer interactions with the surface layer. This is certainly plausible, but I also think that it is also one aspect of the larger failure of the similarity and non-stationarity assumptions upon which the BA method is based. I think the authors should also include this point in their discussion.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1055, 2015.

AMTD

8, C135–C138, 2015

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