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The Cryosphere
Discussions

Interactive comment on "Micrometeorological conditions and surface mass and energy fluxes on Lewis glacier, Mt Kenya, in relation to other tropical glaciers" *by* L. Nicholson et al.

Anonymous Referee #2

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see PDF for formatted comments

General Comments This is an important, well-written manuscript nearly ready for publication. New measurements from the highest-elevations of a glacier on Mt Kenya would be worthy enough, but the authors provide more – with extensive energy- and mass-balance modeling as well as a larger-scale context provided by other tropical glaciers. In all respects it is a very good paper.

My only general, overall comment on the manuscript is best expressed by the first sentence on page 5192, especially with the bracketed addition: "These data highlight the high degree of inter-annual variability in the duration and intensity of the rainy [and

dry] seasons in the summit region." The authors do a careful and comprehensive analysis with 773 days of measurements over 2.5 years, yet there are multiple ideas expressed where potential bias introduced by this variability is not explicitly re-iterated. (One example is on p. 5200, line 23: snow accumulation on "one in five days" at both LG and KG; this interpretation must remain tentative, due to the short less-than-one-annual-cycle PCM – as well as uncertainty in ultrasonic sensor data (e.g., snowfall vs. wind redistribution).) Because high-quality data such as that the authors report on here are rare and precious, generalizations must be made very carefully – perhaps to a greater extent than in some sections of this paper. Underscoring the importance of this caution is that these LG data will next be employed for distributed modeling, to further investigate how climate has influenced the glaciers of Mt Kenya (cf. abstract).

One illustration of the inter-annual variability generalization issue is discussed nicely on page 5196, where the authors point out that the wet season "usually" sees mass gain, but can also be a period of "vigorous ablation". Also, I note that the first paragraph of section 4.3 begins with a nicely-stated caveat.

Specific Comments A. p. 5184, line 14: My interpretation of "short-term" here would be roughly 6 months or less (i.e., less than an annual cycle). If measurements exist spanning a longer time period, I suggest mentioning a time period – as they could prove useful for further work. B. p. 5186, beginning of section 2.1: I suggest a relatively-recent, aerial oblique view of Lewis Glacier, in the context of Mt Kenya's upper slopes would be helpful as figure 1. This would nicely show the topography and setting for the study site as only a photo can, and given that the paper reports new data from a new site I think it would be warranted. C. section 2.1, second paragraph: The discussion of T/RH should begin by informing the reader – in text or the table - as to what type of radiation shield houses the sensors. I assume it is a multiplate, naturally-ventilated shield, in which case errors due to radiation loading likely overwhelm those due to the sensor membrane. These errors would impact both the thermistor and the Vaisala sensor. Elsewhere the paper reports a median wind speed of 2.5 m/s, and diurnal speeds that are ~1 m/s lower. Under such conditions over snow or ice, there will be considerable error in maximum daily temperatures for both instruments. However, this error will likely only influence daily means on the calmest days with fresh snowcover. D. Line 25 of the same paragraph: please state whether e is computed after each T and RH measurement, prior to averaging, or done from the 30-min averages of each in post-processing. With rapidly-changing values of both due to large diurnal fluctuations, these will yield different values of mean vapor pressure. E. p. 5187, line 1: why not just delete negative values of SWI and SWO rather than use a TOA time series to exclude nocturnal readings? Otherwise, how can a TOA time series account for cloud reflection, diffuse radiation and other effects of low sun angle? F. same page, line 15: As I'm sure the authors are aware, snow cover on the radiometer dome can create a situation where SWI is <35% of the clear sky value. Perhaps a model should be considered which also looks at SWO before defining conditions as overcast, although there may not be much difference. G. p. 5189, line 13: It would be useful to indicate (perhaps parenthetically) what the proportion was of "input parameters that were poorly constrained by field data", and thus optimized. H. section 3.1, second sentence: I suggest a different word than "clear" as this is too vague, in light of the relatively-short measurement period. In subsequent text there are numerous examples cited of seasonality in many of the variables. While I appreciate that the authors wish to stress that low latitudes have generally less seasonality than many readers may be aware, I suspect that monthly or daily means over >3 years would reveal a greater degree of seasonality. The word "clear" seems too subjective. I. p. 5190, line 26: I suggest that RH > 99% may be too precise in defining saturation conditions, especially with sensor accuracy when new of +/- 3%. This is a minor point perhaps, but would saturation conditions be reached in 5% more sampled days with a RH value of 97%? 10% more? J. p. 5191, line 26: The meaning of "enhanced accumulation" is not clear. Does this mean greater than normal (average), or no accumulation? I believe that the 2011 long rains failed completely, bringing widespread drought to much of Equatorial East Africa (termed "catastrophic" by some). See also p. 5201 line 27;

what is "slightly elevated" accumulation? On Kilimanjaro, the 2009 short rains were distinctly above normal. K. p. 5193, ~line14: As an example of the caution required in generalizations based on limited time periods (i.e., General Comment above), October 2010 was chosen to represent "typical wet season conditions" – yet on Kilimanjaro this was a rather dry interval of little precipitation, low albedo, and high ablation. L. p. 5197, line 23: why not use Hastenrath (2005) to assess the relative reliability (1981-90), or to corroborate the short record from LG? M. same page, line 28 referring to JF snowfall on Kilimanjaro: this is unclear and/or inaccurate, because the JF period is fundamentally different from the JJAS dry season there. It is much shorter than 2 months in duration, which may account for the notion that is comparable with wet seasons; often the short rains extend into January, for example. N. p. 5198, lines 13-18: this is a wonderful sentence, beautifully summarizing the situation in EEA! O. p. 5204 at top: I question the wisdom of stating that 65% of mass loss on KG is due sublimation, consuming 94% of the energy for ablation. These values are from point models representing short intervals of time. They may indeed be accurate for a particular point over a particular interval, but 10 m in any direction over a different time period one is almost certain to get differing values. So is it reasonable to characterize and generalize these important processes with such precision? I don't have an answer, for the numbers are what the robust model provides. Perhaps some form of caveat could be used in such cases, e.g., "point modeling suggests that" or "about 2/3" or "most of the energy"? P. Table 4 shows the periods from which data from other AWS are used, and it appears that these are ~1 year for all the South American sites. These data sources are discussed on p. 5187 in the final paragraph (i.e., line 28). Since one year is a very short interval for sites with high inter-annual variability, please consider adding a caveat to this effect. Otherwise, the reader may not realize that only short portions of the relatively-long ZG and AG records are used.

Technical Corrections 1. p. 5187, line 28: Table 4 is referenced after Table 1 but prior to Table 3 2. consider more precisely specifying what "ERA-interim" wind fields are, although a quick Google search will reveal this to any reader 3. p. 5194, line13: I think you need a comma after “freezing” – unless I am misinterpreting the sentence 4. p. 5205, line 14: Especially in the Conclusions section, the clarity of wording is important. I suggest “...meteorological conditions high on Mt Kenya...” rather than “...on the summit of...”, even though the station is within 400 m of the summit. Likewise, I suggest “...little variability on an annual timescale, in accordance...” to clarify that it is temporal variability being discussed. Finally, I think a comma after “...regional hygric seasonality” would be helpful. 5. p. 5205 line 18: I hope there is a typo in that “...whereby JJAS (JF) is...” should be “...whereby JF (JJAS) is...” – because JJAS is decidedly more arid on Kilimanjaro than JF. Indeed, if based on mean monthly vapor pressure, the short dry season on Kilimanjaro can be more narrowly defined as February only. 6. Table 2: How can the permissible value range for “% of refreezing meltwater forming superimposed ice” be 0.3 +/- 20%?

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C3039/2013/tcd-6-C3039-2013-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 6, 5181, 2012.