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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 #1

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General comments

In this paper, the authors provide an in-depth analysis of point mass and surface energy balance (SEB) of a tropical glacier, Lewis Glacier, Mount Kenya, based on 2.5 years of meteorological data collected on the upper part of the glacier. To process the data, and compute the surface energy fluxes at the glacier surface, they use a well-tested model from Mölg et al. (2008, 2009, 2012). They then provide a good analysis of the results to assess the climatic drivers of this inner tropics glacier. They finally compare with other results on tropical glaciers in Africa as well as in South America. Results are well discussed, conclusions are well supported and substantially new for this mountain

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Interactive Discussion
Discussion Paper

range. Consequently, this paper is worth being published in TC.

I have still two major comments regarding this study. (i) To validate their results of the SEB model, they have no other option than comparing measured and modelled surface temperature, and surface height (actually surface lowering only because surface increase is an input variable p5189, line 21) (Fig 3). This validation is rather weak. First, modelled surface height is computed from ablation rates in m water equivalent and then converted in surface height considering density values for fresh snow, bulk snow or ice. But these density values as well as the other parameters used to compute ablation and listed in Table 2 have been optimized to match the surface height changes (p5189, lines 17-19: "The parameter set resulting in the minimum RMSD between modelled and measured surface heights was used to calculate the SEB"). Therefore, the agreement between measured and modelled surface heights results from a tuning process and cannot serve as a validation because both fields are not independent. Second, measured surface temperature comes from measured outgoing long-wave radiation, assuming that emissivity is unity. I agree with this assumption which is well-accepted but considering that snow or ice emissivity might be as low as 0.96 or even lower in some cases, the error range on surface temperature is high (up to 3°C or even higher) which makes the validation against this variable rather weak. Consequently, I believe that the authors in this study cannot avoid making a sensitivity analysis on their input parameters, even if they have been optimized. An error range is given for every parameter in table 2 (how is the error range obtained?) and a sensitivity analysis should be conducted taking into account the upper and lower ranges of the range, and even considering values of the parameters outside this range which looks rather narrow. (see also specific comment 12).

(ii) Another important issue is the conductive heat flux. P5194, line 29: QC = 7.5 W/m2. I believe that the glacier is temperate i.e. at 0°C in depth. Therefore, at annual time scale, the conductive heat flux should be 0. Actually, this value of 7.5 looks high (same order of magnitude of other terms of the SEB) and there should be periods while QC is negative, which is not the case in Fig 5. Why? P 5195, line 3, it is reported that QC is negative only between 13:00 and 18:00 (flux directed towards the glacier and Tsurface =0°C probably). I am really surprised that there is no other hours of the day when QC<0 (mornings for example). If QC >0 all year round, from where does the energy come from to explain this energy supply to the surface?

Another minor issue regards the representativeness of the measurement period. This is well discussed in this paper for precipitation patterns (which I agree is the most relevant for the tropics), but a similar discussion for temperature might be useful, using reanalysis data for instance.

The paper is well-written and organized, conclusions are consistent, tables and figures are good but some major concerns reported above should be addressed prior to publication.

Specific comments

1. P 5182, line 7: comparable to South American tropical glaciers, for those located in the inner tropics (AR).

2. P 5183 and following: EEA, MAM, IO, IOZM, ENSO... so many acronyms. It would help the reader to limit the number of acronyms along the text when possible.

3. P5184, line 11-13: it would have been useful to quickly provide some mass balance results from these studies to quantify the LG recession here.

4. P5185, lines 5-7: additionally to points (i) and (ii) (and probably more effective) is the effect of reduced precipitation on ablation (decrease in precipitation -> depletion of albedo -> increase in ablation).

5. P5185, lines 9: the effect of ENSO is different between inner and outer tropics. In both cases, it is true that there is a warming usually observed in mountain areas, but in the outer tropics, also a precipitation depletion, which has the strongest impact on glacier mass balance.

6. P5186, lines 17-23: Considering that wind speed is low on this point site (Table 4), I assume that radiative heating of the Vaisala sensor may affect temperature measurements in a much more effective way than what is reported here. How can the author be sure that the sensor is barely affected?

7. P5187 line 3: so any correction for SWI has been applied in case of riming or snow?

8. P5187 line 6: How often has the AWS been visited and has any tilt been detected along the measurement period? This information is may be important to justify that no geometrical correction has been done.

9. P5187 line 6: how often are there gaps for V?

10. P5189, line 7: which density is used here to convert daily snow accumulation into accumulated mass? And the daily accumulated mass is divided into all hours of the day. However, the data logger is recording half-hourly values of SR50 sensor (p5186, line 15) so is there a more accurate way to distribute precipitation along the day? Considering that melting and accumulation are sometimes concomitant on such tropical glacier, this might have a non negligible impact on the results.

11. P5189, line 9: what is the glacier body temperature below 3 m? Is the glacier temperate? In Table 2 (last row), it is said that “the ice was assumed to be near the melting point” although T(ice)=-3°C at initialization? Why?

12. Table 2: in the last column, some references or field measurements are referred to support the results of the parameter optimization. For references, in some cases, it is easy to select other references that disagree with the parameter values obtained by optimization. For instance, there is a large scattering of z0 values (e.g. see the discussion regarding roughness parameters in Hock, Progress Phys. Geogr., 29(3), 362-391 2005 or see the standard deviation obtained for z0v and z0v on Kilimanjaro summit – table 2, Cullen et al, Ann Glaciol, 46, 227-233, 2007) and turbulent fluxes are very sensitive to z0 values. For field measurements, how fresh snow density measurements

TCD

6, C2935–C2941, 2013

Interactive Comment

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Interactive Discussion

Discussion Paper

were conducted? It is not so obvious to obtain accurate fresh snow density measurements, although it is important to have reliable measurements to convert snow height increase into accumulated mass. The density of fresh snow reported here is 400 $\pm$50 kg/m3 (from optimization) or 330 to 430 kg/m3 (from field measurements / the value of 420 kg/m3 is reported p5201, line 23). These values are very high, and, contrary to what is written in table 2, higher than the values reported by Sicart et al (2002) (i.e 250 kg/m3) which already were high. As a consequence and as already pointed out in General comments, a sensitivity analysis spanning a range of values much larger than the error range given in table 2 should be performed here, to know how sensitive the SEB results are to all model parameters.

13. Table 3: the authors should provide the period covered here, and given that there are gaps in the data series, it would have been interesting to provide annual values, for a complete year (i.e. oct 10-11) as well as mean values for all data.

14. P5191, lines 24-30 and Figure 3: there are data gaps for surface height, due to rotating mast or broken mast. After each data gap, the series starts again at 0, it would be worth mentioning it and also add an horizontal line at 0 on Fig3a to make it clear. Otherwise, we have the impression that it is a continuous series with some missing parts. Are there any ablation stakes nearby to help to reconstruct the surface height during the gaps? And if there are some, it might be interesting to show their records, to compare with SR50 measurements.

15. P5192, line 4: it might be useful to refer also to Fig 2a-h (and not only 2i) along the text in this section.

16. P5192, lines 10-12: which period for the 800 mm value given by S Hastenrath? Obviously outside the TRMM data period, 98-2012, so the comparison does not match the same period, which is not so much a problem I believe, but worth mentioning it.

17. P5192, line 9 and Fig 3: -2.55 m is not clear from Fig 3. Is it without considering positive surface height changes, and only considering surface lowering? Because from

TCD
6, C2935–C2941, 2013

fig 3, it looks like that the net surface lowering is rather closer to -2m than -2.55m

18. Fig4 and Fig5 : it might have been useful to provide somewhere in the paper (may be above Fig4?) the energy balance equation with the flux notations used in these figures, for sake of clarity. With this equation, we could have understood better the sign of the energy fluxes, and especially the sign of QM (negative when there is melt). On overall, I think both figures are not very convenient to read. It would have been more convenient to plot on the same subplot all terms of the energy balance equation, so that the reader can visualize easily QM, as the algebraic sum of all the other terms of the same subplot. For instance, on the first column (i) of Fig5, adding Net SW and Net LW would have facilitated the visualisation of full SEB, and the resulting term QM.

19. Table 3 and Table 4: It could have been interesting to provide mean values for all the energy fluxes of the energy budget equation, and not only SWI and LWI. (not only for LG, but also for the other glaciers in Table 4)

20. P5195 line 2: how can you explain that QS <0 between 17:00 -21:00? It looks strange because usually, at sunset, surface temperature decreases more rapidly than air temperature (due to the energy loss through net LW), making the near surface gradient positive and so is QS.

21. P5195 line 27: it is Fig5c-e and not 4c-e

22. P5197, line 5. There is still a 40% difference in LWI between dry and wet seasons. Consequently, there is not “only a very slight” difference in LWI at seasonal time scale. And this difference is almost as high as for outer tropics glaciers ZG and ARG (P5199 lines 9-11); may be a short discussion regarding this point and a comparison with other inner tropics glaciers could be interesting.

23. P5198, line 26: Wagnon et al (1999) deals with SEB on Zongo Glacier and does not support the statement here, which concerns only AG.

24. P5200, line 3: Table 3 should be removed from the () since it does not deal with

TCD

6, C2935–C2941, 2013

Interactive Comment

lapse rates.

25. Fig7 : blue dots are barely visible

26. P5202, lines 1-2: same as comment 12. I agree that due to temperature, fresh snow density might be higher at LG than at KG, but the difference reported here is rather high 420 kg/m3 against 255 kg/m3. How was these densities measured? How many measurements?

27. P5203, lines 12-16: Do the authors have an idea of the elevation of the rain-snow limit? Actually, the mean temperature recorded at the LG AWS site is close to 0°C, and the rain-snow limit might rise in a near future at the elevation of the glacier, which will severely affect its life expectancy.

28. P5203, line 26: Wagnon, 1999 is Wagnon et al., 1999

29. P5204, lines 20-22: looking at Fig5 (ii) I believe that the net LW difference between dry and wet months play a significant role in the SEB, and in turn, on the ablation melting. This effect is probably not as important as on outer tropics glaciers of South America, but it is significant on LG which shows a kind of intermediate behaviour between AG, EG, and ARG or ZG.

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

TCD

6, C2935–C2941, 2013

Interactive Comment