The Cryosphere Discuss., 6, C1741–C1746, 2012
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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "The footprint of Asian monsoon dynamics in the mass and energy balance of a Tibetan glacier" *by* T. Mölg et al.

Anonymous Referee #2

Received and published: 28 September 2012

General comments

This is a comprehensive and generally well-written manuscript which examines the impact of monsoon variability on the surface energy and mass balance of a glacier on the Tibetan Plateau. Overall, I find the quality of the science ito be high, and the authors have developed some important insights through their well-aimed research questions and methods. The writing can be quite dense at times, though this is mainly an issue of style as opposed to substance.

The authors use a physically based surface energy balance model, driven primarily with on-site meteorological observations and some dynamically downscaled inputs, to examine the variability of surface energy and mass balance over three (partial) melt seasons. The timing of monsoon onset, cessation, and the general strength of the monsoon active/break phase, determined through interpretation of reanalyses data, is used to diagnose surface energy and mass balance model results.

The main results of this study indicate that the mass balance of summer-accumulation type glaciers is strongly dependent on the timing of the Indian Summer Monsoon (ISM) onset, which controls glacier albedo and thus absorbed solar radiation. This is a particularly important point given the uncertainty with regards to changes in the timing and strength of the monsoon due to climate change and/or atmospheric black carbon and short-lived climate forcings.

I would recommend this manuscript for publication, but provide below specific comments and technical corrections that should be addressed.

Specific comments

- P3245, L24: The Fujita and Nuimura (2011) reference used a simple mass balance model to suggest spatially heterogenous trends in modelled ELA, not in glacier change as suggested here. A reference to Yao et al. (2012) may be more suitable here.
- P3249, L20 - Given the differences between observed and WRF-modelled precipitation, the authors properly spend time diagnosing the possible reasons for a scaling factor of 0.56. I also agree with their assertion that the differences are likely due to WRF overestimation and gauge undercatch. But are there other examples of WRF overestimation/gauge undercatch from the literature? How comparable is the WRF scaling factor used in this study to other snow/glacier studies?
- P3251, L7 - the use of a time-dependent vertical air temperature gradient is inter-

esting, though the authors do not show the development of their gradients for this particular site. Given that maximum air temperatures at the site rarely exceed 5C (Fig. 3), I am surprised that substantial boundary layer development occurs (as indicated by the separate temperature gradients). At the sites examined by Petersen and Pellicciotti (2011) and Shea and Moore (2010), for example, temperature suppression near the glacier surface is greatest at temperatures closer to 10-15C. The authors may wish to comment on this a bit further - how sensitive is the model the assumption of varying versus constant lapse rates?

- P3258, L6-10 - The authors find a high sensitivity of the mass balance results to the stability correction. As the sensitivity analyses were conducted by "deactivating" the various paremeterizations, it is unclear if this sensitivity is actually due to an earlier removal of the snow cover and thus greater absorbed solar radiation through the main portion of the melt season, as opposed to significant changes in the modelled turbulent heat fluxes. This requires some clarification.

  Also, digging a bit deeper into the Braithwaite (1995) stability correction used, it would appear that the correction factor will be low (approximately 0.8) for the range of observed temperatures and wind speeds (Figure 4, Braithwaite (1995)).

- P3258, L24: where does this multiplication factor (365/743) come from?

- P3258, L19 - 20 and Table 2: I find this analysis to be confusing, as the relative percentages appear to suggest a high static mass balance sensitivity (-48%) to winter temperature increases. Could the relative changes be related to the annual mass balance instead of seasonal components?

- P3259, L8-20 - Sensitivity analysis of the K_{WRF} correction factor is missing from Table 2, and the MB changes should also be expressed as a relative percentage for clarity. Furthermore, I was not expecting the densities to be varied as well in this sensitivity analysis, since the authors suggest earlier (P3249) that the difference between measured and modelled precipitation reflects both gauge undercatch and wind redistribution. Why not go with the most likely density, and vary only K_{WRF}? If gauge undercatch is a significant issue, than the value of K_{WRF} should be closer to 1, which would presumably have a large impact on the modelled mass balance.

- Figure 5d - unclear in the figure (and the text) if the global radiation for REF is derived from WRF. Please clarify.
- Figure 6a - mean glacier mass balance is captured well, and the authors write that individual correlations between single stakes and model locations are betwen 0.5 and 0.8, but I feel this still needs to be demonstrated more convincingly. How do the modelled mass balance profiles correspond to the observed?
- Figure 7 - this is a very busy (and important) figure that takes some time to decipher. I would suggest splitting the radiative components into a separate figure that includes net shortwave and net longwave. The individual radiation components and albedo could then be plotted together to help explain one of the main conclusions of the paper. Also, the use of a legend for the radiative components (instead of directly labelling the lines) might help the reader
- P3263, L10 (and abstract, L18) - a primary hypothesis and conclusion of this paper is that cryospheric evidence demonstrates "...local and regional modifications of large-scale air flow seem to prevail on the Tibetan Plateau from July to mid September..."

  While I am not a monsoon specialist, I would be surprised if the ISM expression over the Tibetan Plateau is *not* different from that in other monsoon-dominated regions (i.e. eastern Himalayas). Figure 11 demonstrates this point nicely, but the cryospheric evidence is limited to only 3 partial seasons, one of which excludes the core monsoon season! I would suggest that this result be given less emphasis, particularly in the conclusion.

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Technical corrections

- P4 L25 - "...senses..." - maybe a personal preference, but I would avoid assigning human characteristics to glaciers.
- Sec 2.1 - a summary table outlining where the SEB model inputs were obtained (i.e. AWS1, AWS2, WRF) might be helpful for the reader.
- Sec 2.2 - resolution of the WRF output?
- P3256, par.2 - units (W m^{-2}) should be introduced earlier
- P3261, L10 - sentence is missing a word or two: "...supports **the idea** that..."
- Table 2 - clarify that these are glacier-wide net mass balances (as opposed to point specfic mass balances)

References

R. J. Braithwaite. Positive degree-day factors for ablation on the Greenland ice sheet studied by energy-balance modelling. *Journal of Glaciology*, 41:153–160, 1995.

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