

RESPONSE TO REVIEWERS

We would like to thank the scientific editor for obtaining the review, and the two referees for providing detailed and thoughtful comments. We are glad to see that both referees evaluate our paper to be of high quality, and to be in a good position for publication. Below we describe point-by-point how we addressed the issues raised. There is no doubt that the referees' comments improve the revised version of the manuscript.

Sincerely,

T. Mölg, F. Maussion, W. Yang, and D. Scherer

Anonymous Referee #1

General remarks

This contribution seeks to gain a better understanding of the mass and energy balance of a Tibetan glacier by means of mass balance modeling. Because of the general interest in the response of Himalayan and Tibetan glaciers to climate change, it is a timely study, conducted with a physically based mass balance model that includes many of the processes thought to be important in this region. The authors use groundbased mass balance and AWS data from the Zhadang Glacier, along with climate modeling output to force and evaluate the modeling results. The model appears to perform well, and the authors apply parameter sensitivity tests to evaluate the climate sensitivity of the glacier. In addition, they assess the mass balance components during different phases of the Indian monsoon (onset, core, cessation), and conclude on the mass balance-effects of interannual monsoon variability.

The paper is very well written but pretty dense, containing a lot of detailed information. I had to read many sentences or paragraphs several times and go forth-and-back between text and figures to get all(?) the information. The figures are generally of very good quality and informative and the tables provide informative data as well. The only thing that bugged me is that the mass balance model as well as the climate modeling data, used to partly force the MB model, are not really described but merely referenced. I guess that's ok to do, but it requires the reader to pile up several more papers in order to understand the background. If the editors and authors don't have a problem with that, I think the paper will still stand out fine and be appreciated by those interested. Yet, elaborating a bit more on the modeling and the governing equations, even if it's partly repetitive with the previous work of the authors, would help the reader to follow without having to refer to several other papers, and potentially take along non-mass balance modeling experts, like me.

RESPONSE: We appreciate that the MB model builds on several other papers, since it has been under development for about eight years now. As the referee indicates, there are two possible ways: first, repeat the model details so that experienced MB modelers can skip this section; Or second, reference all relevant older papers so that interested readers can look up the details. We prefer to go with the second approach here, since the paper is already dense and thus it would be most advantageous to not increase the page number significantly further. However, to also allow non-model experts an easier comprehension of the model, we refer in the revised paper to a table in a previous publication (tab. 1 in Mölg et al., 2009, J. Clim. 22) that was put together under didactical viewpoints for non-MB modelers, showing how each atmospheric forcing variables affects the various energy and mass fluxes in the model.

Minor comments

P3247

L10-12: Could you briefly describe the procedure?

RESP: Unfortunately this can't be described briefly (cf. the rather brief description is one full page in the reference provided). However, we have added one sentence to better introduce the basic aim of the procedure (→ detect radiative heating of sensors or riming of instruments).

L18-22: These two sentences are unclear to me. Could you explain in more detail. Also, is reference to Table 1 for two numbers necessary? Maybe just give the depths, so I don't have to jump forth-an-back between text and figures/tables so often.

RESP: Sentence revised and reference to Table 1 at this location deleted;

P3248

L1: 'final 76 days' – out of how many?; L2: How long is this gap? Can you fill the entire gap with data from the other station?

RESP: We now give the precise date of the gap and its length as ratio (68% of period 3). Yes, we can fill the entire gap with the other station. However, the referee comment also showed us that we had forgotten to mention the gap starts a bit later than period 3 starts, which might have evoked the question about filling. This is now clarified in the text and in Fig. 5.

L6: Isn't it that there can be pretty strong melting during May, too?

RESP: May could be a strong melt month, but it was not in the year under consideration (2011), see Fig. 5a. We have revised this sentence and indicate now that, in the results section, it is shown that the gap coincides with a period of weak ablation.

L10: 'sufficient natural ventilation' – to result in what?

RESP: Text revised: "...and when there was high enough wind speed for sufficient natural ventilation of the Ta/RH sensor ...".

L16: 'obtain' -> 'estimate' more appropriate?

RESP: We prefer to leave "obtain", since we are referring to a measurement (and not a model estimate).

L19: How often and when were stakes read?

RESP: This information is given in Fig. 6. We revised the text to "... as will be detailed in Sect. 3.1".

P3249

L11: Again, could you briefly describe the method you refer to?

RESP: We added a brief description. The method is based on the condensate mixing ratio.

L12: Are there such strong altitudinal differences in the variable of interest?

RESP: Unfortunately we don't understand this comment. But altitudinal gradients in precipitation are controlled by a mass balance model parameter (shown as case 3 in Table A1).

P3250

L14-: Could add the respective letters of the variables?

RESP: Yes, no problem, we added the letters.

L28: 'the remaining subsurface layers are at. . .'

RESP: Changed;

P3250

L6-L19: This was frustrating for me to read. If I would want to know, I'd have to read so many more papers. Why not add 1-2 pages to the paper and describe the model in more detail. Those not interested can skip the section.

RESP: See response above;

P3251

L3: Doesn't the SRTM model have 90-m resolution here?

RESP: This is correct. We added to the text that the elevation model was re-sampled.

L18: What do you mean by can be chosen? What did you chose to do?

RESP: The model user can chose one of the two options. In our Monte Carlo approach both options were used (see Table A1). We revised the sentence to be clearer.

P3252

L20: Why these specific thresholds?

RESP: Please see the original paper that we cite (Prasad and Hayashi, 2007). It is not an arbitrary choice of us.

L23-24: Instead of the range, you could give each date. It's only three years, anyway.

RESP: We agree, but giving the range is a better connection to the sentence afterwards and the important Webster et al. (1998) reference.

P3256

L3: What do you mean by 'the' mid-latitude and subtropical VBP. Please explain.

RESP: We mean the "characteristic" VBP based on theory. We revised the text.

L6: The glacier-wide MB stands a little out of context here.

RESP: We agree and deleted this sentence. The number is repeated one paragraph later anyway.

L24: I think here you could also address some previous studies, like Rupper and Roe (2008, J. Climate), who performed rather simplified mass balance modeling. Do your modeling results support their findings, e.g., regarding sublimation, for example?

RESP: We cite the suggested paper in the revised paper, and indicate that both studies are in agreement regarding ablation characteristics.

P3257

L19: That's a pretty wide range and doesn't really tell us much, does it?

RESP: We only want to reproduce published values to let the reader know the measured range. We think this provides a better context for the modeled values.

P3258

L4: -0.20 - What unit?

RESP: Unit added;

Table 1

Caption: 'setting particular parameters' could be 'parameter setting'. 'The two radiation. . . albedo' – Is somehow unspecific to me. Table: I would probably prefer the column order to be: Instrument, variable, accuracy, usage. And also, for each variable one row. Finally, be consistent in using brackets or 'at' for the measurement height.

RESP: We have taken all suggestions into account, apart from changing the column order (as this doesn't change the table's significance to any degree).

Table A1

You could try to make this table easier to read. For example, create a column for the reference and the constraint (meas/atmos) or assumption.

RESP: Accepted and revised;

Figure 6a

The x-axis along with the data points suggests some continuous scale. Maybe use bars instead. In (b), can you give the dates for period 3?

RESP: We now give the full date details for period 3 (date was only given on a monthly resolution in the caption before). Regarding Figure 6a, the precise dates for every x-tick are specified, which should prevent readers from assuming a continuous scale.

Anonymous Referee #2

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 to 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.

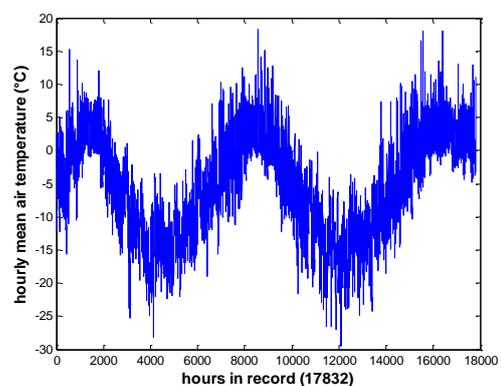
RESP: We agree that there are better references for this point, and now cite the suggested paper.

- 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?

RESP: We are not aware of any other example from the literature on this specific connection (WRF overestimation-gauge undercatch at glacier site). To better direct readers, however, we added a key reference for the undercatch issue.

- P3251, L7 - the use of a time-dependent vertical air temperature gradient is interesting, 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 to the assumption of varying versus constant lapse rates?

RESP: We assume there must be a misunderstanding. First, we do document the development of the gradients in the paper by mentioning that they are evident from the measurements (AWS1 vs. AWS2), see section 2.1, Table A1 (note a). Thus they are not chosen arbitrarily. Second, the referee most likely overlooked that Fig. 3 shows daily mean temperatures (as specified in the caption) in order to smooth the curves. Hence hourly air temperatures can also rise to 10 or more degrees C in summer (see plot). Taken together, field measurements do not support using a constant lapse rate.



• 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 parameterizations, 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.

RESP: We added some text detailing the contributions. The sensitivity is a result of both, as every change in energy flux will lead to a changed snow cover evolution in the course of the simulation.

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)).

RESP: As shown above, please note that hourly (the model time step) air temperatures can be significantly higher than values in Figure 3, and thus the correction factor is also greater than 20%. To avoid confusion we added a sentence to the text (Sect. 2.3) indicating that hourly values can fluctuate more strongly than daily means in Fig. 3, and now give the example of maximum air temperatures.

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

RESP: Days per year/total days of the study; We revised this sentence for clarification.

• 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?

RESP: We agree that this was confusing and have followed the referee's suggestion.

• P3259, L8-20 - Sensitivity analysis of the KWRF correction factor is missing from Table 2, and the MB changes should also be expressed as a relative percentage for clarity.

RESP: We now express the MB changes also as relative percentage. However, since KWRF is a particular sensitivity case, we prefer to not merge it with the other sensitivity cases in Table 2, in order to indicate the distinct nature (which we also express in the text: "the only parameter used to scale MB input").

Furthermore, I was not expecting the densities to be varied as well in this sensitivity analysis, since the authors suggest earlier 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 KWRF? If gauge undercatch is a significant issue, then the value of KWRF should be closer to 1, which would presumably have a large impact on the modelled mass balance.

RESP: We need to vary both together, because water equivalent precipitation (as controlled by KWRF) and the solid precipitation density together determine the actual precipitation height. The latter is the quantity that finally enters the model. As equally little is known about the density of solid precipitation as about KWRF, and the density is a free model parameter (so will always be adopted in the Monte Carlo runs to changes in KWRF), varying only KWRF would pretend a false accuracy about the precipitation height. We explain this in detail in Mölg and Scherer (2012), thus we cite them at this passage. Also, if we would not vary the density too, it would not be possible to detect the upper sensitivity region (i.e. when the density takes unrealistic values). – Nevertheless, we revised the text to make this clearer.

• Figure 5d - unclear in the figure (and the text) if the global radiation for REF is derived from WRF. Please clarify.

RESP: Global radiation is generated by the MB model, which is specified twice in (a) Fig. 3, which shows that only summer precipitation and cloud cover are from WRF; and (b) Fig. 5 caption, which starts with "Measurements versus MB model ...", so all model plots in Fig. 5 are from the MB model. We added another hint in Section 3.1: "... incoming shortwave radiation generated by the MB model ...".

• Figure 6a - mean glacier mass balance is captured well, and the authors write that individual correlations between single stakes and model locations are between 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?

RESP: Figure 6a is not showing the mean glacier mass balance, but the mean MB over the available stakes and associated model locations – as stated in the Fig. 6 caption. Thus Fig. 6a does demonstrate the

correlation (by the basic match in VBP gradient) and the absolute agreement (by the match in the net mass fluxes) between single stakes and model locations further.

- 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.

RESP: We agree that the figure contains a lot of information. To facilitate the readability, we have extended the caption but prefer to not separate the radiation terms from the other terms, in order to clearly demonstrate the quantitative relation between radiation and non-radiation fluxes.

- 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.

RESP: We agree that stating "there is modification of large-scale air flow" was too general, thus we revised the respective sentences in the abstract and in the conclusions. Actually the referee comment helped us realize what we really intended to say: local and regional weather systems *control atmospheric variability* on the Tibetan Plateau in the core monsoon season.

Technical corrections

- P4 L25 - "...senses..." - maybe a personal preference, but I would avoid assigning human characteristics to glaciers.

RESP: We removed the quotation marks, but – after talking to a native English speaking colleague – prefer to use the word.

- 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.

RESP: The input sources are specified in Figure 3 through different colors (as emphasized in the caption). However, we revised Fig. 3 to make this more obvious, by using more contrasting colors (black/blue instead of black/grey) and by indicating in panel 3d the measurement type of winter precipitation (SR50 or gauge). Also in the text, when Fig. 3 is introduced, we state that the figure summarizes the input source. AWS2 data are not used as meteorological input (only for estimation of instrument heights in the AWS1 data gap and for vertical gradient calculation).

- Sec 2.2 - resolution of the WRF output?

RESP: We added "from the 2 km-resolution grid" in the second paragraph. The first paragraph of Section 2.2, as well as Figure 1 caption, also mention the 2 km resolution.

- P3256, par.2 - units ($W m^{-2}$) should be introduced earlier

RESP: Done;

- P3261, L10 - sentence is missing a word or two: "...supports **the idea** that..."

RESP: Changed;

- Table 2 - clarify that these are glacier-wide net mass balances (as opposed to point specific mass balances)

RESP: Changed; "glacier-wide" added to the caption;