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Interactive comment on "Modeling energy and mass balance of Shallap Glacier, Peru" *by* W. Gurgiser et al.

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We thank Valentina Radic for obtaining the reviews, and we thank all reviewers for their careful reviews of the manuscript, for their constructive suggestions and notes that markedly improved the revised manuscript. We have addressed all points individually below.

Response to Andrew MacDougall

General Comments:

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1. **Comment:** The Monte−Carlo method used in this study to tune the distributed energy balance model is an innovative approach to bypass the large data requirements of this class of melt models. However, the approach – as applied in this study – does not leave an independent source of validation data. The cross−validation method used to evaluate the transferability of the model gives some confidence in the validity of the model, but pro−glacier stream−flow data or comparison to photographs of snowline position could have improved the confidence in the conclusions of this study. This lack of validation data is not unique to this study and I appreciate that the authors only have the data that was collected during field campaigns. I would, however, like to see an acknowledgement of this lack of independent validation data in discussion section of the paper.
   Response: We agree with reviewer 1 (and reviewer 4) and added a paragraph at the end of Section 4.3.

2. **Comment:** To make the manuscript more self−contained I suggest that a brief summary of the temperature transfer function and meteorological instruments be included in the methods section, instead of referring readers to Gurgiser et al. (2013).
   Response: We added some information at the end of section 2.1.1 and included a list of the applied sensors in the supplement to keep the main text short.

3. **Comment:** The manuscript has a large number of figures for a paper of this length (17!). I believe that many of these figures can be deleted without harming the presentation of the study results. In particular I would delete figures 6, 9, 10, A1, and A3. Figures 9 and 10 are in particular incomprehensible. There is simply

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too much information in these figures to extract useful information from them.
Response: We agree with reviewer 1 that Figures 6, A1 and A3 can be removed without harming the value of our study. However, we think data presented in Figures 9 and 10 are new for our region and the figures give a compressed overview of the annual course of several factors of mass and energy balance of an Andean glacier. Therefore, we would like to leave them in the manuscript.

4. **Comment:** Why are some figures in an appendix when references are made to them in the main text of the paper?
Response: The figures in the appendix were not relevant with regard to the main findings of this study but seem to be interesting background information. To improve the clarity of the manuscript, we moved all figures from the appendix to the supplement.

5. **Comment:** I do not understand how the error ranges are being computed for the values given in the abstract and Table 1. Table 1 refers the reader to a section 2.3, a section that does not exist.
Response: We apologize for the wrong reference and have corrected and improved the caption of Table 1.

Specific Comments:

6. Page 4016 line 14: 2 ± 0.68 m w.e. is strange notation. The error range seems overly precise for a value that is approximately 2.
Response: Adapted.

7. Page 4018 line 15: This sentence is too long and would benefit from being re–written for clarity.
   Response: Adapted.

8. Page 4022 line 20: The symbol S↓ is used here, while SWin is used in the rest of the text.
   Response: Corrected.

9. Equation 4: I think p1–21 is typo.
   Response: Corrected.

10. Equations 4–6: np, ni, and ny would be clearer as np, ni, and ny
    Response: Adapted.

11. Page 4028 lines 13–16: Figure six is probably superfluous, simply stating the r and p values in this paragraph is sufficient.
    Response: Adapted.

12. Page 4028 line 15: "so" could be replaced with "therefore".
    Response: Adapted.

13. Page 4028 line 20 and 21: the adverb "slightly" is unnecessary.
    Response: Corrected.

14. Section 3.2: In high latitude distributed energy balance model studies (e.g. MacDougall and Flowers (2011)) the ELA varies across the surface of the glacier

TCD
7, C1976–C1992, 2013

due to topographic shading. Is this not so here due to the high elevation angle of the sun in the tropics? Or does the glacier have little topographic shading?
Response: Due to high elevation angles throughout the year potential shading effects of the topography are generally lower in low latitudes and show little seasonal variation. However, all ELA values are spatially averaged and we added this information in Section 3.2.

15. Page 4031 line 24: How can the ground heat flux always be negative? Unless the glacier is warming up (non−temperate), however, it is stated that the glacier is assumed to be at freezing point below your 14 layer ice heat flow model component?
Response: On average the ground heat flux transfers energy from the surface in the subsurface that is consumed by subsurface melt. So this energy sink at the surface is an energy source for melt in the subsurface (Section 3.3, paragraph 5).

16. Table A1: It is difficult to tell where one parameter description ends and another begins. Maybe you should treat the descriptions as sentences beginning with a capital letter and ending with a period.
Response: Adapted.

17. Figures: Labeled ticks should all have the same precision. That is 0, 0.5, 1.0, 1.5 not 0, 0.5, 1, 1.5
Response: Adapted.

18. Figure 4: The dates are difficult to read. Maybe you should use a dash or backslash instead of a period.

Response: Adapted.

19. Figure 7: Please include North arrows on these maps. Labeling the year in panels a. and b. would also help with clarity.
Response: Adapted.

Response to Yang Wei:

General Comments:

1. **Page 4016 Line 19 – 20:** The authors stated that lower surface albedo was caused by higher snow line. However, in Page 4034 Line 19 – 28 and Page 4035 Line 1 – 10, much attention is paid to the discrepancy of precipitation amount, snow cover and fraction of rain/snow in these two years. Actually, the higher snow line was the consequence of energy–mass balance and cannot be used to explain the albedo variation. The reasons of lower surface albedo should be rephrased in the Abstract.
Response: We adapted the misleading statement.

2. **Model description and parameters:** Two threshold temperatures (Table A1) are used to calculate solid and liquid precipitation. My first question is how to calculate the percent of rain/snow between this two threshold values (1.1 and 2.6°C). Linear interpolation or some other methods? In addition, these two thresholds are critical to albedo parameterization and accumulation calculation. The authors used a narrow range (1.0 – 2.5±0.5°C) to optimize these two

important parameters. I wonder whether such narrow precipitation range from Rohrer(1989) is appropriate for Peru glacier in tropic region. The discussion of modeling uncertainty from these narrow range of precipitation phase is welcomed in the revised paper.
Response: The percentage of rain/snow is calculated by linearly interpolating between the two threshold temperatures. We added this information in the text. Concerning the range Rohrer (1989) found that the amount of solid precipitation occurring at relatively high temperatures (T$>$3°C) is very small. Besides, relative humidity is high during strong precipitation at our site which means that there is only little cooling of snow crystals by sublimation or evaporation. Assuming that the temperature gradient is $-$0.55°C/100m (which seems reliable for almost saturated conditions), the snowflakes melt between 200m and 470m below the 0°C isotherm when the optimized threshold values (1.1 and 2.6°C) are applied. Furthermore, the suitability of these thresholds was well captured by our evaluation method as variability between liquid and solid precipitation was highest in the lowest parts of the glacier where the stake measurements were conducted. We addressed this issue in Section 4.3.

3. **Air temperature:** The performance of air temperature interpolated from lateral moraine is linking to the modeled albedo, incoming longwave radiation and turbulent heat flux. The author developed a good temperature transfer function. The reader would like to check the performance of this function. One figure showing the diurnal variation of transferred temperature and measured temperature in ablation season is necessary to evaluate the robust ability. In addition, the vertical air temperature gradient ($-$0.55°C/100m) is from the Zongo glacier in the outer tropics. The possible uncertainty by this gradient should be briefly discussed since the meteorological condition is different with your glacier as your statements in Page 4017 Line 28$-$29.

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Response: A figure showing the mean diurnal cycle of measured and transferred temperature (with transfer function and constant gradient) was added in the supplement. We also calculated mass balance sensitivity to a stronger temperature gradient (-0.65 instead of -0.55°C/100m) that has been applied e.g. by Kaser (2001) and added the values in Section 4.3.

4. **Discussion:** A short comparison of energy/mass balance characteristics for three glaciers: Zongo glacier, Antisana glacier and Shallap glacier will strength the scientific significance of this paper.
Response: We agree with the reviewer that the comparison of mass and energy balance between Zongo, Antisana and Shallap glacier is of scientific interest. However, information is already given in Section 1, and the main differences are already presented in Section 4.3 and Section 5.

Specific comments:

5. Page 4019 Line 7: Provide the satellite information.
Response: Information added in the intro of Section 2.

6. Page 4023 Line 16−17. Why d* is a fixed value of 1 cm? It is different with the constant in Gurgiser et al. (2013) in Journal of Glaciology.
Response: The function of d* is basically to smooth the transition in albedo between snow and ice. To reduce the numbers of free albedo model parameters we applied fixed values of d* for the optimization of t* (described at the end of Section 2.2.1) and found good results for d*=1cm. Regarding equation 2 this means that ice albedo affects surface albedo approx. as long as snow depth is

TCD
7, C1976–C1992, 2013

<4cm. Low penetration depths of shortwave radiation seem reliable for typically wet and thus dense snow in the ablation zone of Shallap glacier.

7. Figure 1: Add the location of Zongo glacier and Antisana glacier in the inset box.
   Response: Information added.

8. Table 2: Please list the wind speed and relative humidity.
   Response: Information added.

<u>Technical corrections:</u>

9. Equation 2: Please check this equation again. I think the parenthesis is missing.
   Response: We apologize, correction applied.

10. Page 4034 Line 15–16: the sign of net shortwave radiation should be positive.
    Response: Correction applied.

    11. Figure 8's Caption: No Section 2.3 in manuscript.
    Response: Correction applied.

11. Structure: There are two Model evaluation in Section 2.2.3 and Section 3.1. Maybe Section 2.2.3 can be changed to model uncertainty evaluation?
    Response: We followed the suggestion.

TCD
7, C1976–C1992, 2013

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Response to Marco Carenzo

General Comments:

1. **Comment:** Section 2.1.1. Model design – Please indicate which SWE values were used to initialize the model runs. The 2006–2007 surface mass balance above 4850 m – where no information about the surface condition is available – is certainly influenced by the amount of snow assumed in the modelling. Although three months of spin–up are included in the modeling, they are "located" in the dry period (when precipitation is low, Fig. A2). The mass loss due to sublimation occurring in these three months could anticipate the ice exposure and lead to the negative mass balance on most of the glacier area. Did you investigate the effect of different initial SWE maps? You can eventually discuss it briefly in Section 4.3 (Uncertainties).
   Response: For all runs SWE values had to be developed within the spin–up time. Thus, we have tested the sensitivity of mass balance (especially of year 1) to spin–up times of different length. We found that in our case SWE values from 3 month spin–up time (as finally applied because of lower computation time) produced the almost same mass balance (Δ 0.03 m w.e. in year 1, Δ 0.01m w.e. in year 2) as 11 month spin–up time (maximum possible; including the wet season before the investigation period). This result was mainly caused by relatively low snow pack formation in the wet season preceding the investigation period. Out of interest we also initialized a model run with the modeled SWE values of 01.06.2008 which was markedly more pronounced and found that mass balance would have increased by 0.15 m we. in year 1 and 0.08m w.e. in year 2. Thus, the length of the required spin–up time to form SWE values is of course determined by the conditions preceding the investigation period. For

clarity we added a note in Section 2.2.1 (..."to include three months of spin−up which was proofed to be sufficient to produce reliable snow pack patterns").

2. **Comment:** Section 2.1.1. Model design – Please indicate how relative humidity and wind speed are extrapolated to the DEM grid points. In a data scarce context such as that of the outer tropical Andes, not many extrapolation techniques can be obviously derived from the measurements. Meteorological data (relative humidity and wind speed) are generally assumed invariant in space. However, recently attention has been paid on the correct characterization of meteorological variables, and the comparison between on− and off−glacier input data has been generally conducted to assess the influence of forcing observed at not−glacierized locations (in your case an AWS installed on a steep moraine). How is the correlation between on− and off−glacier relative humidity and wind speed? Gurgiser et al. (2013) describe carefully only the local transfer function for air temperature and its effect on the glacier vapor pressure using RH_M. In this paper the comparison is conducted only in terms of wind speed values averaged over 184 days. I would suggest to mention and to describe these assumptions in the discussion section of the paper.
Response: Both relative humidity and wind speed were adopted from AWS_M and kept constant in space as no data were available for deriving gradients. However, the correlations and mean values between hourly on and off glacier humidity (r=0.95, Δ RH=1%) and wind speed (r=0.89, Δ ws=0.1 ms^{-1}) were very high during the 214 days (184 was the wrong number of days) of overlapping measurements. Thus, the humidity and wind speed data measured at AWS_M seem to be well suited as model input without modifications. We improved the description of the assumptions concerning wind speed and included relative humidity in Section 4.3.

3. **Comment:** Section 2.1.1. Model design – Is QL entirely converted into mass fluxes of sublimation or, when surface conditions allow the process, is partly used for the evaporation of surface meltwater?
   Response: If surface temperatures are below 0°C QL is converted into mass fluxes of sublimation, else it is converted into mass flux of evaporation. We added a note in Section 2.2.1.

Specific Comments:

4. Page 4017 Line 12 – Although it has gained acceptability as singular word, data is plural (plural form of datum). Please replace "is" with "are".
   Response: Correction applied.

5. Page 4017 Line 16 – I would replace "degree day" with "empirical temperature–index".
   Response: Changed.

6. Page 4019 Line 24 – Data as plural word. Please replace "was" with "were".
   Response: Correction applied.

7. Page 4019 Line 24 – Wrong unit. Please remove "°".
   Response: Correction applied.

8. Page 4022 Line 10 – Wrong unit. Please add "°C" or replace "°C" with "K".
   Response: Correction applied.

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9. Page 4022 Line 18 – Wrong unit. Please remove "°".
Response: Correction applied.

10. Page 4027 Line 18 – Consistency, you always use the B.E. Please replace "modelled" with "modeled".
Response: Correction applied.

11. Page 4031 Line 28 – For clarity, I would remove "net shortwave energy fluxes and".
Response: Correction applied.

12. Page 4034 Lines 14–15 – The net shortwave radiation and the surplus of energy for melting should be positive. Please replace "−" with "+".
Response: Correction applied.

13. Page 4034 Line 14 – Please replace "mean" with "means".
Response: Correction applied.

14. Page 4036 Line 22 – Please replace "horrizontal" with "horizontal".
Response: Correction applied.

15. Page 4036 Line 25 – Please replace "likley" with "likely".
Response: Correction applied.

TCD
7, C1976–C1992, 2013

16. Page 4047 Table A1 – Wrong unit. Please remove "°" in the fixed bottom temperature.
Response: Correction applied.

17. Page 4056 Fig 9 – Please specify which accumulation value corresponds to white in (b). I guess it corresponds to no accumulation, but no white color is included in the colorbar.
Response: Information added.

Response to Anonymous Referee 4

General Comments:

1. **Comment:** The energy and mass balance model has a few weak points, for example where assumptions from other tropical glaciers (Zongo) are transferred one to one (e.g. the lapse rate or the assumption that precipitation does not change with elevation; an assumption which is almost certainly incorrect). The model is trained (tuned) with data from stake measurements in the ablation zone, but there is no additional validation of the model. This is not a criticism, since such data is hard to come by, but it leaves room for improvement during future studies, which will have to independently asses the accuracy of this model from an observational perspective. Overall however, the authors are to be commended for trying to develop the best possible model, in the absence of better available observations.
Response: We fully agree with the author that further studies including additional evaluation methods are desirable. We added a note at the end of Section

4.3.

2. **Comment:** The paper is very long and the discussion often dwells on very specific and minor details. As a reader it is not easy to follow along and filter out the main thrust of the paper from the many technical details. I think shortening the paper with a more focused description of methods, results and discussion would help to make it much more attractive.
Response: We see this point and could slightly shorten Section 2, 3 and 4 while adhering to our concern that the applied methods and some remarkable details remain comprehensible for the benefit of future studies.

Specific comments:

3. Page 4020, line 5: "Unidad"
Response: Correction applied.

4. Page 4020, line 6: "Autoridad Nacional de Agua"
Response: Correction applied.

5. Page 4025: lines 13 (equation (4)) and 15: the letter p should be a subscript in the term "np" otherwise it might be mistaken as the product of "n" times "p". The same comment also applies for the terms "ni" and "ny" in equations (5) and (6) and the subsequent discussion in the text (page 4026).
Response: Adapted.

6. Page 4025, line 14: "the number of model runs"
Response: "the number of the model run" is correct as we calculated the RMSD for each run separately. However, we slightly adapted the formulations to improve clarity.

7. Page 4025, line 16 and page 4026, line 5: if "n" is the number of runs, then a specific run should not be named "n"
Response: See above, n (now changed to r) refers to a specific model run.

8. Page 4033, line 17: "resultant"
Response: Correction applied.

9. Page 4034, lines 1−6: This sentence is too long. Break it up into two.
Response: Adapted.

10. Page 4035, line22: "spatially"
Response: Correction applied.

11. Page 4036, line 22: "horizontal"
Response: Correction applied.

12. Page 4040, line 4: "McKenzie"
Response: Correction applied.

TCD
7, C1976–C1992, 2013

13. Page 4041, line 1: "Caceres"
Response: Correction applied.

14. Page 4043, line 14: The title of the reference Porter (1975) is incorrect
Response: Correction applied.

15. Page 4043, line 30: "Ribstein"
Response: Correction applied.

16. Figure caption 2: R2 is commonly referred to as "coefficient of determination", not "squared coefficient of correlation"
Response: Correction applied.

References

Kaser, G.: Glacier−climate interaction at low latitudes, J. Glaciol., 47, 195−204, doi10.3189/172756501781832296, 2001.

Rohrer, M. B.: Determination of the transition air temperature from snow to rain and intensity of precipitation, in: IAHS/WMO/ETH International Workshop on Precipitation Measurement, St. Moritz, Switzerland, 475−482, 1989.

TCD
7, C1976–C1992, 2013