The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-51-RC2, 2019

The Cryosphere
Discussions
Open Access

Interactive comment on "Surface energy fluxes on Chilean glaciers: measurements and models" *by* Marius Schaefer et al.

Anonymous Referee #2

Received and published: 11 November 2019

This paper focuses on calculations of the surface energy balance (SEB) of glaciers along the Andes of Chile. The analysis is covering a large latitudinal range, between 18°S and 55°S, and tries to describe the main differences existing in the processes controlling melting under diverse climate settings. The paper compares three different modelling approaches applied on a dataset of 6 glaciers. The authors intend determining adequate parameterization of SEB models and conclude that the use of observed melt is not sufficient to assess model performance.

The dataset and SEB analysis are important for the community in particular for model validation and deserve to be published. However, in its present state, the modelling analysis is not sufficiently robust, because 1) raw data still present biases, 2) models are too different to be compared, 3) none of the models is currently sufficiently accu-

rate to be referred to as the reference. In particular, the authors write that "Bringing the predicted melt rates by these highly parameterized models in agreement with the observed ones seems to be rather a curve adjustment exercise than a indicator of correct physics.". However, I feel that this opinion is not supported by results.

I propose authors produce a (real) reference modelling, using adapted calibration for each glacier (see point 2), and then compare this reference with other "simplified" approaches (i.e. the EB-model and the COSIMA model as presented in the present version of the paper). When radiative fluxes are modelled, I propose to perform a calibration/validation step. Finally, in the discussion, I suggest that authors cautiously consider the differences in surface states, elevation and latitude (in particular when the authors compare ablation amounts). For this task, I have a few suggestions which may improve the accuracy of results:

1) A pre-treatment of field data has not been made in depth. Indeed, LW data are biased by very large artefacts, and this introduces large uncertainties in the SEB analysis. + Large biases are observed in LWin and LWout data. These are clearly visible in figure 4. They are likely due to CNR4 heating caused by solar radiation, or to an incorrect calibration. I personally worked during 20 years with these sensors (CNR1, CNR4) and I never observed continuous biases of about 20 W m-2. Obleitner and de Wolde (1999) proposed bias corrections as a function of SWin values, but this does not remove potential biases during the nights. In order to remove biases, I propose that authors analyse LWout values when the surface is melting (LWout should be 315 W m-2). LWnet is possibly correct because corrections may be similar for both LWin and LWout. + Potential other artefacts are not discussed in the text (existing shadows on sensor caused by the station mast, snow accumulation on CNR4, etc). Generally, nonaspirated temperature sensors may be biased high by solar radiation at wind speeds less than 3 m s−1 (Huwald et al., 2009; Georges and Kaser, 2002). A correction for the solar bias is complex but possible. At least, observed temperatures may be flagged when low wind speeds (i.e., <3 m s−1) are observed during the daytime. This has an

impact on turbulent heat fluxes calculations. + Data gaps are also not described. =>I suggest that authors accurately correct field data.

2) The "reference model" is not accurate enough due to assumptions made on Ts and turbulent heat flux calculations. SWnet and LWnet would possibly be accurate because these values are directly measured (if biases are removed), but turbulent heat fluxes are clearly not accurate in the reference model, because 1) surface temperature Ts is assumed to be at 0°C, and 2) calculations are done without considering stability conditions in the surface boundary layer. Conversely, the COMISA approach very likely produces more accurate LH and SH, but then the radiation terms are not accurate (see the important differences with observed fluxes in Table A1). =>I propose to run the COMISA model using the measured SWin, Albedo, LWin (after correction), T, Rh and wind speed, solid precipitation, the initial snow height and cloudiness (I am not sure that this variable will impact results since LWin and SWin will be already assimilated). I propose to force the model using specific surface roughness length values according to the surface state. Indeed, Bello Glacier (see photograph) presents small penitents at the surface when Tyndall presents a very smooth surface. Surface roughness length values have already been proposed in the literature for the different studied areas.

3) LW/SW schemes have never been calibrated/validated in the study. When modelled (i.e., in the simplified modelling approaches), albedo and LWin/SWin schemes could be calibrated using a simple monte carlo approach (scores could be computed using field measurements of albedo and LWin/SWin). A sensitivity analysis could also offer interesting information for the discussion. For turbulent heat fluxes, authors could test various surface roughness length values given in the literature. Validation of the "reference model" could also be done between modelled and observed Ts. The modelled ablation could be compared with observation on stakes or on sonic gauges (when available). =>I propose that authors calibrate the different schemes on one period and validate it on another one. This is possible at least for Bello and Tyndall glaciers.

4) How do authors compute melting? Do they use the mean daily energy excess,

or do they compute melting at a 1h time step? This is crucial because calculations must consider that surface melting depends on heat storage in the subsurface. In particular, the existence of subsurface fluxes is never mentioned (heat conduction and transmission of solar radiation in snow and ice) even in the COSIMA model. These fluxes are crucial to explain daily melt intensities. => Does COSIMA compute these fluxes? if not, would it be more accurate to run a model that includes these fluxes? (see for instance Thomas Mölg's model (e.g., Mölg et al., 2008, 2009b, 2012; Gürgiser et al., 2013)). If subsurface fluxes are not considered, please justify this assumption.

5) Finally, a deep review on SEB modelling in the Andes is lacking. Many SEB modelling are available along the Andes, but currently the review of literature is limited to (MacDonnell et al., 2013 ; Brock et al. 2007; Schneider et al., 2007 ; Pellicciotti et al., 2008 ; Ayala et al., 2017; Sicart et al., 2008), which is not up-to-date (papers mainly refer to studies published more than 10 years ago). A more exhaustive review of studies performed along the Andes would offer interesting information for the discussion.

As a summary, I suggest that the authors take a slightly different approach in order to present their study. I suggest to: a) compute a real reference SEB b) describe the differences in fluxes according to the altitude/latitude, c) compute simplified EB-model and COSIMA modelling. d) conclude on the differences existing between the "simplified approaches" and the reference model e) Please reconsider the conclusion of the paper if you don't clearly demonstrate that SEB modelling is harder to apply than an empirical model, and that it does not offer better results. f) I suggest that authors make a thorough editing of the text to improve the language style.

Minor Comments,

Line 5: Please write "Turbulent sensible heat flux", "Turbulent latent heat flux" and "turbulent heat fluxes"

Line 9 : transport coefficient => do you mean bulk exchange coefficient? Or bulk transfer coefficient?

Line 20: “These kind of models are sometimes called "physical melt models” => please include references, at least in the Andes.

Section 1 : Introduction => the introduction is confusing and is not focusing on the main objective of the paper. This introduction should present the interest of SEB modelling on glaciers, and a review of knowledge on the SEB in the Andes and under similar climates. I propose that authors reorganize the introduction and remove several sentences: For instance, the paragraph “Chile is well-known [. . .] future surface mass balance and melt water discharge of Chilean glaciers” could be included in a subsection of the section “sites” which could be titled “climate settings”.

Page 2 Line 1 : “Chile is well-known for the climatic variety due to its north-south extension of the territory” => why do the authors focus on Chile only, and not on Chile/Argentina? please rewrite.

Page 2 Line 2 : “Pacific anticyclone plays a key role” => on what?

Page 2 Line 3: “sub-glaciological zones ” => please cite (Braun et al., 2019) and (Dussaillant et al., 2019)

Page 2 line 14 : Are you sure that the mega-drought reached Patagonia?

Page 2 line 16 : “Chile hosts the majority of glaciers in South America (more than 80% of the area),” => what about Argentina?

Page 2 line 17 : Âń which are mostly thinning and retreating in the last decades (e.g. Braun et al. (2019)).” => please also cite (Dussaillant et al., 2019)

Page 2 line 17-20, and elsewhere: “The projections of future changes [. . .] climatological zones is necessary” => glacier wastage projections for calving glaciers are impossible if we only consider the SMB and the SEB (e.g., Collao-Barrios et al., 2018). The authors never write that Exploradores and Tyndall are calving glaciers. Please comment this point.

Page 2 line 21: “There exist few surface mass balance observation programs on Chilean glaciers:” => why considering only Chile?.

Page 2 line 23: “Echaurren Norte Glacier“ => please introduce also the Piloto glacier and other glaciers under study in Argentina.

Page 2 line 29 : “the limited accumulation of snow is not able to make up with the ablation processes” => If a glacier is present, this means that there was a time when the glacier had a positive SMB.

Page 4, line 17: “glacier melt at equator near Zongo Glacier” => Zongo Glacier is in Bolivia, not at the equator.

Page 4, line 26-29: “in this study we want to test their ability to reproduce the individual energy fluxes” => This is relevant, but this is not really done in the paper. I suggest that authors compare each modelled and observed fluxes, with figures and statistics.

“we want to emphasize the differences between the model parameterizations and their ability to reproduce the directly measured radiative fluxes at the glacier surfaces” => It is currently hard to conclude, because the authors use 3 very different models, with different assumptions on many variables (SWin, LWin, albedo, Ts, and turbulent heat fluxes). It would be easier to force COSIMA with observations in order to produce a reference modelling, then make simplified EB-model and COSIMA approaches. Another interesting way to reach conclusions would be to make a sensitivity analysis on COSIMA model.

“We also compare three different parameterization for the turbulent fluxes of sensible and latent heat” => Here, the authors used different equations, in which stability calculations were simplified, but they also changed the values of Ts. It is thus really hard to conclude on comparisons.

Section 2: Sites => please present different regions using a bulleted list and include here the paragraph which is currently in the introduction. Piramide, San Francisco, and Bello glaciers should enter in the same region; Tyndall and Exploradores glaciers would be in the of Patagonia. Please remind in the text that Tyndall and Exploradores are calving glaciers.

Section 3: Methods Page 5, Line 16: "to a very good approximation sum up to the melt energy available at the glacier surface" => Here and elsewhere in the text: please include values, scores, statistics.

Page 5, Line 18: Heat conduction and solar radiation transfer in the ice are neglected when they play a crucial role even in summer (See for instance Gürgiser et al., 2013). Please justify the choice of neglecting these fluxes. Are they considered in the COSIMA model?

Section 3.1: could be included in sites. Please inform on potential data gaps, and data treatment. Please remove biases in the data (see point 1).

Section 3.2 Reference database: I suggest that authors change the reference modelling as described in the introduction of my review.

Page 7, line 7 : "The bulk aerodynamic approach is employed" => The bulk aerodynamic approach is not never fully used here. The methods used here are simplified approaches.

Page 7 Line 14 : "T(s) is the temperature of the glacier-atmosphere interface, which is assumed to be 0°C"=> Please use corrected LWout (using obleitner and deWolde (1999) approach) to compute Ts.

Equation (2) : is only valid for neutral surface boundary layer and assuming that z0m = z0T = z0q, These points are suggested before in the text (see "the eddy diffusivity for heat has the same value as the eddy diffusivity for water vapor and the eddy viscosity"), but writing that the SBL is assumed to be neutral is more direct.

Page 7, line 22: "constant roughness length of z0=0.5mm" => please discuss this value because turbulent heat fluxes directly depend on it (they are twice larger if z0 is

10 times larger). z0 is expected to differ between snow and ice, and have very specific values over penitentes. There are many references proposing values assuming that z0 = z0m = z0T = z0q. Another option would be to consider different z0m for snow and for ice, and then apply Andreas [1987] polynomials.

Figure 3a. I don't understand why the authors don't use (Goff, J.A., and Gratch, S. 1946). Relationships. In Particular, the COSIMA relationship looks like erroneous. Moreover, I don't understand why two curves are given above 0°C (one for ice, and the other for snow), and only one below 0°C, when it should be the reverse (saturation against solid or liquid phase makes sense below 0°C).

Figure 3b. The differences are so large that the authors could calibrate a relationship between atmospheric emissivity, T and Rh, using their field data. They also could consider MacDonell et al., (2013) study.

Section 3.3, EB-Model Page8 Line 13: "the sum of the direct and diffuse incoming solar radiation" => Does EB-Model consider data from a DEM to compute Diffuse/direct components? How is it computed in the case of overcast conditions?

Page 9 line 1: It seems strange to assume a constant albedo on a glacier, and snow patches, when solid precipitation occurred. It would be better to consider the albedo scheme from COSIMA.

Page 9, Line 5: LWout= is 315.6 W/m2 => this assumption is really strong again. If you consider that melt is observed when values are maximum, then refreezing is observed at night in Figure 4 (except for Tyndall).

Page 9 line 10: "clear sky emissivity"=> again, validation of the equation may be easily done using field data.

Page 9 Line 12:" theoretically site-specific clear sky incoming solar radiation": how is it computed? Moreover, do the authors compute n with same equation in every models? Is it computed with equation 9?

Equations 7 and 8 or only for stable conditions, which are generally not verified in the morning. Why do they assume this, when computing a Richardson number is very easy when surface temperature is available? Please justify.

Page 9 line 22: "the roughness Reynolds number (Brock and Arnold, 2000)." => you mean using Andreas (1987) polynomials?

Page 9, Line 31 : "theoretical, site specific clearsky radiation computed by a code developed by Corripio (2003)"=> Do you mean SOLTRAN? Please give the exact reference.

Page 10 Line 6: please cite : U.S. Army Corps of Engineers (1956).

Page 10, Line 22: "However here SH is multiplied by a correction factor which depends on the bulk Richardson number Ri" => Please precise. Why do the authors use this formulae instead of the bulk approach? In particular, the assumptions behind equation 13 are not clear to me. Could they compute the turbulent heat fluxes offline using the bulk method and Ts, Tair, Rh, and U given by COSIMA and compare the results with COSIMA's turbulent heat fluxes?

Section 4.1 : Glacier climate => please reformulate this title Table 3: I suggest that the authors make a bulleted list and first compare glaciers in the same region, and then compare glaciers at different locations. Please discuss the differences in altitude and surface state in a same region.

Page 11, Line 15: "incoming longwave radiation increases from [...]increased cloudiness in the Wet Andes." => elevation and temperature also play a key role here. In particular, differences in LWin between Bello, Piramide and San Francisco are largely related to temperature.

Figure 4f: the surface at Tyndall glacier is constantly melting. This suggests that sensors are biased by a constant value of 15Wm-2. This bias seems to be retrieved on other glaciers (except on exploradores, where it seems to be even larger).

Section 4.2: Page 13, Line 3: "we exclude Pirámide Glacier from" => Instead of removing this glacier, I suggest that authors compare with results on Pichillancahue-Turbio Glacier (Brock et al., 2007).

Page 13, Line 13: "The predicted melt rates are higher for Patagonian Glaciers as compared to the Glacier in the Central Andes" => This sentence does not make sense, melting rates depend on elevation. At 4000 m asl, melting is zero in Patagonia.

Figure 6, 7 and 8: show that the big differences between the 3 models are observed in LWnet and SWnet, probably due to the very strong assumptions made on LWout and albedo variations. But if we analyse Table A1, the main differences are observed in the mean SH and LH values. I suggest that authors discuss this point.

Page 15, Line 10: "although they have opposite exposition" => The studied surfaces are expected to be flat (no slope).

Page 15, Line 16: "LWout detected on the glaciers are surprisingly higher than the expected 315.6 W/m2" => please correct biases in data before analysing the SEB.

Caption of Figure 9: "albedo. In COSIMA the following parameters have been chosen: frsnow=0.8, firn=0.5, time constant t= 2 days, snow depth constant d= 8 cm (see equations (11) and (12)" => How did the authors calibrate these parameters?

Page 17, Line 14 (and Page 18, Line 3): "At Exploradores Glacier the measured emissivity reaches values higher than one."=> Please correct LWin before computing the emissivity.

Page 18, Line 8 (and the end of the paragraph): "The variability of the modeled turbulent fluxes is very similar in all three methods[...] have very similar aspect: in all approaches the sensible heat flux is mainly driven by the temperature difference"=> please refer to Table A1. For instance, if we consider the Bello Glacier, SH is ranging from 6 to 30 W m-2 and LH from -23 to -53 Wm-2. Considering the sum of turbulent heat fluxes, SH+LH is ranging from -40 to +7 W m-2 . This maximum difference between the 3 models (47 W m-2) is larger than those observed in LWnet (22 W m-2) and in SWnet (15 W m-2). Perhaps I did not understand the end of the paragraph, but the large difference in turbulent heat fluxes results from the very different assumptions done on Ts and on stability corrections.

Section 5.3: melt rates: I propose that authors include a table to allow a quick validation between modelled ablation and observed ablation from stakes or sonic gauges. In this Table, it would be interesting to include the elevation of ablation measurements, the latitude, the time period of measurements.

Page 18, line 23 :" observed melt range"=> "observed melt rates"

Page 18, line 35: This comparison looks strange because Exploradores is located in campo de Hielo Norte, whereas Grey Glacier is located in Campo de Hielo Sur.

Page 19, Line 23: "The capacity of the models to reproduce the measured radiative fluxes is still improvable." => This conclusion looks strange when only 3 simple approaches are used. Please note that there is a large number of complex snow models and many complex experiments are done to improve these models (e.g., ESM-SnowMIP (Krinner et al., 2018).

Page 19, Line 28 : "This is because the used parameterizations are fits to data that were obtained in different climatic conditions." => This sentence seems trivial. I propose that authors calibrate their model with their observation, and then perform a model sensitivity analysis.

Page 19 Line 31: "Different parameterizations for the turbulent fluxes of sensible and latent heat were compared in this study,"=> because assumptions made on Ts and equations used were different in the 3 approaches, it is hard to conclude.

Page 19 Line 33 (and page 20, Line 1): "There are many parameters involved in these parameterizations"=> I don't agree, the bulk method only requires to define z0m.

Page 20, second paragraph: "Bringing the predicted melt rates by these highly pa-

rameterized models in agreement with the observed ones seems to be rather a curve adjustment exercise than a indicator of correct physics." => Perhaps this conclusion is real for EB-model and COSIMA, but I would not extrapolate this conclusion to all the SEB models.

Page 20, Line 18: "The inferred melt rates were higher for the Patagonian Andes than for the Central Andes."=> it depends on elevation of study sites. Melt is zero in Patagonia at 4000 m asl.

Page 20, Line 20: "The models underestimated the measured emissivity of the clearsky atmosphere in the Wet Andes."=> please correct LWin before concluding.

Page 20, line 23 : "To develop or improve physical models we have to validate every single model parameterization against data"=> this could be done in present study.

References:

Ayala, A., Pellicciotti, F., MacDonell, S., McPhee, J., and Burlando, P.: Patterns of glacier ablation across North-Central Chile: Identifying the limits of empirical melt models under sublimation-favorable conditions, Water Resources Research, 53, 5601–5625, https://doi.org/10.1002/2016WR020126, 2017.

Braun, M.H., Malz, P., Sommer, C. et al. Constraining glacier elevation and mass changes in South America. Nature Clim Change 9, 130–136 (2019) doi:10.1038/s41558-018-0375-7

Brock, B., Rivera, A., Casassa, G., Bown, F., and Acuna, C.: The surface energy balance of an active ice-covered volcano: Villarrica Volcano, Southern Chile, in: Annals of Glaciology, VOL 45, 2007, edited by Clarke, G. and Smellie, J., vol. 45 of Annals of Glaciology-Series, pp. 104–114, https://doi.org/10.3189/172756407782282372, international Symposium on Earth and Planetary Ice-Volcano Interactions, Univ Iceland, Inst Earth Sci, Reykjavik, ICELAND, JUN 19-23, 2006, 2007.

Collao-Barrios, G., F. Gillet-Chaulet, V. Favier, G. Casassa, E. Berthier, I. Dussaillant, P.

Gourlet, J. Mouginot, E. Rignot and M. Schaefer (2018) Ice-flow modelling to constrain the surface mass balance and ice discharge of San Rafael Glacier, Northern Patagonia Icefield, Journal of Glaciology, 1-15, https://doi.org/10.1017/jog.2018.46.

Corripio, J.: Vectorial algebra algorithms for calculating terrain parameters from DEMs and solar radiation modelling in mountainous terrain, International Journal of Geographical Information Science, 17, 1–23, https://doi.org/10.1080/713811744, 2003.

Dussaillant, I., Berthier, E., Brun, F. et al. Two decades of glacier mass loss along the Andes. Nat. Geosci. 12, 802–808 (2019) doi:10.1038/s41561-019-0432-5

Georges, C., and G. Kaser (2002), Ventilated and unventilated air temperature measurements for glacier? Climate studies on a tropical high mountain site, J. Geophys. Res., 107(D24), 4775, doi:10.1029/2002JD002503.

Goff, J.A., and Gratch, S. 1946. Low-pressure properties of water from −160 to 212 °F. In Transactions of the American Society of Heating and Ventilating Engineers, pp 95–122, presented at the 52nd annual meeting of the American Society of Heating and Ventilating Engineers, New York, 1946.

Gurgiser, W., Marzeion, B., Nicholson, L., Ortner, M., and Kaser, G.: Modeling energy and mass balance of Shallap Glacier, Peru, The Cryosphere, 7, 1787–1802, https://doi.org/10.5194/tc-7-1787-2013, 2013.

Huwald, H., C. W. Higgins, M.-O. Boldi, E. Bou-Zeid, M. Lehning, and M. B. Parlange (2009), Albedo effect on radiative errors in air temperature measurements, Water Resour. Res., 45, W08431, doi:10.1029/2008WR007600.

Krinner, G., Derksen, C., Essery, R., Flanner, M., Hagemann, S., Clark, M., Hall, A., Rott, H., Brutel-Vuilmet, C., Kim, H., Ménard, C. B., Mudryk, L., Thackeray, C., Wang, L., Arduini, G., Balsamo, G., Bartlett, P., Boike, J., Boone, A., Chéruy, F., Colin, J., Cuntz, M., Dai, Y., Decharme, B., Derry, J., Ducharne, A., Dutra, E., Fang, X., Fierz, C., Ghattas, J., Gusev, Y., Haverd, V., Kontu, A., Lafaysse, M., Law, R., Lawrence,

D., Li, W., Marke, T., Marks, D., Ménégoz, M., Nasonova, O., Nitta, T., Niwano, M., Pomeroy, J., Raleigh, M. S., Schaedler, G., Semenov, V., Smirnova, T. G., Stacke, T., Strasser, U., Svenson, S., Turkov, D., Wang, T., Wever, N., Yuan, H., Zhou, W., and Zhu, D.: ESM-SnowMIP: assessing snow models and quantifying snow-related climate feedbacks, Geosci. Model Dev., 11, 5027–5049, https://doi.org/10.5194/gmd-11-5027-2018, 2018.

MacDonell, S., Kinnard, C., Mölg, T., Nicholson, L., and Abermann, J.: Meteorological drivers of ablation processes on a cold glacier in the semi-arid Andes of Chile, The Cryosphere, 7, 1513, 2013a.

MacDonell, S., L. Nicholson, and C. Kinnard (2013b), Parameterization of incoming longwave radiation over glacier surfaces in the semiarid Andes of Chile, Theor. Appl. Climatol., 111, 513– 528, doi:10.1007/s00704-012-0675-1.

Mölg, T., Cullen, N. J., Hardy, D. R., Kaser, G., and Klok, L.: Mass balance of a slope glacier on Kilimanjaro and its sensitivity to climate, Int. J. Climatol., 892, 881–892, doi:10.1002/joc.1589, 2008.

Mölg, T., Cullen, N. J., and Kaser, G.: Solar radiation, cloudiness and longwave radiation over low-latitude glaciers: implications for mass-balance modelling, J. Glaciol., 55, 292–302, doi:10.3189/002214309788608822, 2009b.

Mölg, T., Maussion, F., Yang, W., and Scherer, D.: The footprint of Asian monsoon dynamics in the mass and energy balance of a Tibetan glacier, The Cryosphere, 6, 1445–1461, doi:10.5194/tc-6-1445-2012, 2012.

Obleitner, F., and J. de Wolde (1999), On intercomparison of instruments used within the VatnajoÂĺkull glacio-meteorological experiment, Boundary Layer Meteorol., 92, 27–37.

Pellicciotti, F., Helbing, J., Rivera, A., Favier, V., Corripio, J., Araos, J., Sicart, J.-E., and Carenzo, M.: A study of the energy balance and melt regime on Juncal Norte Glacier, semi-arid Andes of central Chile, using melt models of different complexity, HYDROLOGICAL PROCESSES, 22, 3980–3997, https://doi.org/10.1002/hyp.7085, workshop on Glaciers in Watershed and Global Hydrology, Obergurgl, AUSTRIA, AUG 27-31, 2007, 2008.

Schneider, C., Kilian, R., and Glaser, M.: Energy balance in the ablation zone during the summer season at the Gran Campo Nevado Ice Cap in the Southern Andes, Global and Planetary Change, 59, 175 – 188, https://doi.org/http://dx.doi.org/10.1016/j.gloplacha.2006.11.033, http://www.sciencedirect.com/science/article/pii/S092181810600292X, mass Balance of Andean Glaciers, 2007.

Sicart, J. E., Hock, R., and Six, D.: Glacier melt, air temperature, and energy balance in different climates: The Bolivian Tropics, the French Alps, and northern Sweden, Journal of Geophysical Research: Atmospheres, 113, 2008.

U.S. Army Corps of Engineers (1956), Summary report of the snow investigations, snow hydrology, North Pac. Div., Portland, Oreg.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-51, 2019.