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The Cryosphere Discussions

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

Anonymous Referee #1

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Comments for the Authors

The paper addresses relevant scientific questions within the scope of TC. It shows a good data set of meteorological measurements and estimates of the surface energy balance of Chilean glaciers of contrasting climates. Unfortunately, in my opinion the manuscript does not represent substantial progress beyond current scientific understanding. The overall presentation is well structured and clear. However, the text can be improved; it should be more concise and accurate. I found that some scientific methods were not suitable and that some assumptions were not clearly outlined. Finally, the discussion does not reach substantial conclusions. In my opinion, the main problem is that the objectives of the study are not clearly defined. The manuscript presents various results but the overall purpose of the study remains imprecise. The analysis of this

interesting dataset could be an interesting contribution to the literature if the objectives were articulated more clearly and reflected in an appropriate methodology. The paper would certainly need major improvements to be innovative and merit publication in TC.

These points are detailed in the comments below.

Major comments

- The objectives should be redefined.

The main contribution of the study is the presentation of a comprehensive set of meteorological measurements on glaciers of contrasting climates along the Andes. The data certainly allow for correct estimation of the energy fluxes at the surface of the glaciers. Radiation flux measurements seem appropriate and accurate. Turbulent flows are not measured directly, but meteorological measurements probably allow correct estimates (if the appropriate methods are applied, see comments below). Thus, I would suggest focusing on a comparison of the energy fluxes partitioning in the different climates from the dry Andes to the wet Andes (the measurements above debris-covered ice are not useful here). The effects of latitude and of altitude on the energy fluxes need to be discussed in more detail.

One limitation of the study is that general interpretations of different climates are deduced from point-scale measurements. The partitioning of the energy fluxes depends on the position of the weather station. For instance, the albedo varies greatly over short distances near the snow line, so that the interpretation of punctual energy flux measurements can lead to an erroneous generalization of the melting characteristics to the entire ablation area. This point should be discussed.

- Some applied methods are not suitable

The NR-Lite sensor is less accurate than the CNR4. Thus, calculations of longwave radiation fluxes can be problematic on the Mocho glacier.

Comparing different models does not bring much newness here. Some assumptions

are not valid (Ts=0°C, constant albedo...) or some formulations are not adequately described (see below). I would suggest discussing the energy fluxes derived from the most direct approach: the 'reference database' based on measurements. For example, there is no need to assume that the surface temperature is fixed to 0°C (P7, line 14) if outgoing longwave measurements are available. This assumption (which does not seem valid on the San Francisco and on Bello glaciers) has a significant impact on the turbulent sensible heat fluxes derived from the bulk method.

P8, Equation 4: use the standard relationship of saturation vapor pressure as a function of temperature, no need to test different parametrizations.

P8, line 13: why mentioning direct and diffuse components of solar irradiance if global radiation is directly measured?

Sections 3.2, 3.3 and 3.4: the comparison of the different turbulent 'transfer coefficient' (P7, line 16) should refer to stability concepts. A stability correction must be included over glacier surfaces, using the Monin-Obukhov length scale or the Richardson number. This important point should be clarified. The values of the roughness lengths for momentum, temperature and humidity should also be discussed in more detail with reference to the state of art.

P11, lines 9-12: no need to compare the two methods (especially if they give 'similar results').

The effects of cloud cover in the different climates should be investigated more rigorously. The applied method is inappropriate (the text does not say how the cloud cover is calculated) and appropriate references are missing. Figure 10 is unclear and its interpretation P17-18 remains vague. Robust methods have been proposed for estimating cloud cover from solar and longwave radiation fluxes measurements (e.g., Marty et al., TAC, 2002 in the Alps; Sicart et al., JOG, 2010 in the tropical Andes; McDonell et al., TAC, 2013 in the semiarid Andes of Chile; Munneke et al., IJC, 2011...). These parametrizations, once calibrated at each site, will make it possible to distinguish clear

skies from cloudy conditions. This point, with an adequate methodology, could be an interesting contribution of the study.

- Many results are presented without proper interpretation

Figures 5, 6, 7 and 8 show many results, but most of them remain poorly analysed.

The turbulent fluxes are not measured directly, so their estimates are not very accurate. The large differences in sensible heat flux in the different climates are certainly significant. However, I think no much can be said about the latent heat fluxes shown in figure 5; the fluxes remain close to zero and the uncertainties are certainly large.

- The text must be carefully proofread.

Be more specific and accurate, for instances: - P2, lines 23-25: give numbers to quantify the trends in mass balance - P5: the correct terms are 'irradiance' or 'radiation fluxes' (in W/m2) - P13, line 6 'ice equivalent'? Do you mean water equivalent? - P13 where are the figures 12 and 13? - P16, line 16: Equation 12? - Where is Table 4?

Minor comments

- P16, lines 2-4: the effects on longwave radiation of the "very humid and temperate air column between the sensor and the glacier surface" is probably small and can be estimated [e.g., Pluss and Ohmura, 1997].

- P19: why not using the meteorological and ablation measurements on the Bello glacier during the summers 2013/14 and 2014/2015 to validate (in a rigorous way) the calculations of the energy fluxes?

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