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.

Ok. Section 4.1 was revised completely.

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.

Ok. This point is mentioned now.

Some applied methods are not suitable The NR-Lite sensor is less accurate than the CNR4. Thus, calculations of long wave radiation fluxes can be problematic on the Mocho glacier.
Ok. We agree that it is difficult to draw sound conclusions about longwave radiative fluxes using the data from NR-Lite sensor. Still, we think that the results for the longwave net fluxes obtained on Mocho fit well in the general North-South trend, which makes us more confident about them.

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.

Ok. For the computation of the Reference Database, we compute now the surface temperature using bias-corrected outgoing longwave radiation data. Comparing the performance of the different model parameterizations under different climatic conditions is the core part of this study.

P8, Equation 4: use the standard relationship of saturation vapor pressure as a function of temperature, no need to test different parametrizations.
It seems there exist several "standard" parameterizations in the literature. We used the one which we think interpolated best the measurements.
P8, line 13: why mentioning direct and diffuse components of solar irradiance if global radiation is directly measured?
Here we are describing the parameterizations of one of the models which we are applying. In this model the division into direct and diffuse components is used to infer the cloud cover.
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.
Ok we added some discussion on that.

P11, lines 9-12: no need to compare the two methods (especially if they give 'similar results').
We think that it makes our results more robust, when two different ways of computing albedo 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 etal., TAC, 2002 in the Alps; Sicart et al., JOG, 2010 in the tropical Andes; McDonell etal., 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.
For EB-Model and COSIMA the cloud-cover is estimated using incoming solar radiation fluxes measurements (see section 3.3 and 3.4). Figure 10 has been divided into 4 subplots now, to facilitate the visualization of our analysis.

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.

We do not agree here with your statement about the latent heat fluxes: these fluxes depend on the moisture content of the atmospheric layer next to the glacier surface. If the moisture content in this layer is less than the moisture content of a saturated air layer at zero degrees Celsius just above the glacier surface, then the latent heat flux has a negative sign (moisture is transported from the glacier surface to the atmosphere, which causes more evaporation or sublimation to happen on the glacier surface, which are processes that consume energy). This is happening in the Central Andes. If on the other hand the moisture content of the lowest atmospheric layer is higher than the moisture content of a saturated air layer at zero degrees Celsius just above the glacier surface, then the latent heat flux has a positive sign (moisture is transported from the atmosphere to the glacier surface, which causes condensation to happen on the glacier surface, which is processes that provides energy). This is happening in the Patagonian Andes (especially Exploradores Glacier).

- 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
ok, number was added.
- P5: the correct terms are 'irradiance' or 'radiationfluxes' (in W/m2)
The correct terms for what on page5? Not all energy fluxes on the glacier are radiative!
- P13, line 6 'ice equivalent'? Do you mean water equivalent?
As the glaciers surfaces to which we applied the model approach were mostly ice, we decided to present the results in 'ice equivalent'
- P13 where are the figures 12 and 13?
Sorry, these figures existed in an earlier version of the manuscript. They were removed now!
- P16, line 16: Equation 12?
Sorry, we meant equation (11)
- Where is Table 4?
Sorry. We refer to table A 1. Changed!
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].
Ok, now we bias-corrected the measured longwave radiation following a suggestion form reviewer2.
- 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?
We are using data from summer 2014/2015, since in summer 2013/2014 there was a problem with the sensor which is measuring shortwave radiation. Sadly for the summer 2014/2015 no ablation measurements are available at the location of the automatic weather station (see page 87 in CEAZA: Modelación del balance de masa y descarga de agua en glaciares del Norte Chico y Chile Central, Tech. rep., Dirección General de Aguas, S.I.T. No. 382, 2015.)