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, between18∘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 differentmodelling 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 accurate 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 ob-served 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 acalibration/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 pretreatment 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 deWolde (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, non aspirated 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.
**Ok. We bias-corrected longwave radiative fluxes now, using the assumption that the glacier surfaces are melting ( are at zero degrees Celsius) during afternoon (1 p.m. to 6 p.m.). Data gap treatment is described in the section 3.1.**

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 temperatureTs 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.

**In the revised manuscript we do not assume Ts to be at 0∘C any more. We compute Ts now using the bias corrected values of LWout (except for Mocho, where we do not have this information). We now use the stability correction implemented in COSIMA for the turbulent fluxes for the reference database. The influence of different surface roughness values is discussed now.**

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.
**SWin are input data for the modeling approaches. SWout, that is parameterization for albedo, are calibrated and validated on Mocho Glacier (Figure9). LWin is validated in Figure10.**

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.
**Ok, now we discussed the influence of the roughness length on our results. Sadly no direct ablation measurements are available for the modeling periods.**

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; Gürgiseret al., 2013)). If subsurface fluxes are not considered, please justify this assumption.
**Subsurface fluxes are modeled by COSIMA but are not considered in the other approaches. Considering that we are modelling summer period, the temperature gradients in the snow/ice should be small and the corresponding subsurface fluxes as well ( see calculations below).**

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 re-fer 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.
**Ok. Since this work is about Chilean Glaciers we restricted the literature review mainly to studies on Chilean Glaciers. We are happy to receive additional recommendations for studies of relevance for our work to include them in the literature review.**

As a summary, I suggest that the authors take a slightly different approach in order too 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.
**Thank you for your constructive comments. We think that our re-submission is quite in the line of what you proposed.**

Minor Comments,
Line 5: Please write “Turbulent sensible heat flux”, “Turbulent latent heat flux” and“turbulent heat fluxes” **ok**
Line 9 : transport coefficient => do you mean bulk exchange coefficient? Or bulktransfer coefficient? **Bulk transfer coefficient (changed)**
Line 20: “These kind of models are sometimes called "physical melt models” => please include references, at least in the Andes. **Ok added.**

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”. **Ok.**

Page 2 Line 1 : “Chile is well-known for the climatic variety due to its north-southextension of the territory” => why do the authors focus on Chile only, and not onChile/Argentina? please rewrite. **We only analyze data from Chilean Glaciers in this manuscript, therefore we think that it is consistent to talk about the climate setting in Chile.**

Page 2 Line 2 : “Pacific anticyclone plays a key role” => on what?
**On the climate in general (formulation changed) .**
Page 2 Line 3: “sub-glaciological zones ” => please cite (Braun et al., 2019) and (Dus-saillant et al., 2019) **ok**
Page 2 line 14 : Are you sure that the mega-drought reached Patagonia?
**We are not sure if this drought in Patagonia in 2016 can be associated to the Central Chile mega-drought. We not associate the two phenomena in our manuscript..**
Page 2 line 16 : “Chile hosts the majority of glaciers in South America (more than 80% of the area),” => what about Argentina?
**There is much less glacier area in Argentina, again: our study focuses on Chilean glaciers.**

Page 2 line 17 : Â´ n which are mostly thinning and retreating in the last decades (e.g.Braun et al. (2019))." => please also cite (Dussaillant et al., 2019) **ok**

Page 2 line 17-20, and elsewhere: "The projections of future changes [...] climato-logical 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.

**Ok, now we mentioned in section 2 that Exploradores and Tyndall glaciers are calving glaciers.**

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

**Because our study is about Chilean Glaciers!**

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

**As mentioned above: since this study is about Chilean Glacier, we think it is more consistent to focus on Chilean glaciers in the intro. Again: suggestion of articles about Argentinean glaciers which you estimate of high relevance for our study are very welcome.**

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

**ok, we added: "During the monitoring period"**

Page 4, line 17: "glacier melt at equator near Zongo Glacier" => Zongo Glacier is inBolivia, not at the equator.

**Ok, we replaced "equator near" by "tropical"**

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.

**Modeled and observed fluxes are compared in Figures 5,6,7,8 and model parametrizations are compared to measurements in Figures 9 and 10. The only statistics that are computed up to now are overall mean values and daily mean values. Regarding the great similarity of the course of the melt rates observed in the Figures 6,7,8 we do not think that additional statistics (like correlations and standard deviations) would add some crucial new insights.**

"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. An-other interesting way to reach conclusions would be to make a sensitivity analysis on COSIMA model.

**We have chosen a very similar approach now: the reference database is composed of the measured radiative fluxes and turbulent fluxes based on measured surface temperature and using the stability correction which is implemented in COSIMA.**

"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.

**Now the stability correction in the reference database and COSIMA are the same.**

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.

**Ok!**

Please remind in the text that Tyndall and Exploradores are calving glaciers.

**Ok, we added that Tyndall and Exploradores glaciers experience some calving as well.**

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.

**Ok, we indicate the maximum difference between our simplified approach and the full COSIMA model now at the end of section 3.4 now.**

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 COSIMAmodel?

**Heat conduction through a solid is determined by two things: the thermal conductivity of the solid and temperature gradient. The thermal conductivity of ice is 2.1 W/(mK). The temperature gradient should be zero for the temperate glaciers of Patagonia where mean annual air temperatures are positive. This is also valid for the place on Mocho Glacier, where the energy balance is measured (Schaefer et al. 2017).**

**For the Glaciers in the Central Andes the temperature gradients in the ice should be small by January. Assuming an residual temperature gradient of 1 K/m the heat conduction into the ice would be 2.1 W/m², a very small value in comparison to the other fluxes.**

**Yes in COSIMA heat conduction is modeled, but for a more consistent comparison of the models we do not consider this fluxes in our study.**

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).

**Data gaps and data treatment are commented. Bias was removed from the longwave radiation data.**

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

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.
**What do yo mean with “fully” used? To our understanding the bulk aerodynamic approach is a way of quantifying the turbulent fluxes by making three important assumption (which are stated in the manuscript). We are happy to get feedback from you in the case you are thinking that we forget to mention another important assumption of this approach.**

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.
**Ok.**

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.
**Ok we now added an indication to this assumption that it is associated to a neutral atmosphere**

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.
**As indicated in the text, the value 0.5mm, which we chose for z_0 is an intermediate value which is in the range of recommended values for both smooth ice and snow surfaces (page 155, Cuffey&Paterson).**

Figure 3a. I don't understand why the authors don't use (Goff, J.A., and Gratch, S.1946). Relationships.

**Bolton(1980) seems to interpolate the measurements very well.**

In Particular, the COSIMA relationship looks like erroneous.

**You are right. This formula was implemented in the first version of COSIMA that we downloaded and was indicated in Huintjes et al. 2015a. But this seemed to be a bug which was changed now.**

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).

**All the curves are indicated below zero degrees as well, but the different parametrizations overlap for these temperatures, which is the reason why the three curves are not so easy to distinguish**

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

**Sorry, here we used an older version of the Graph where there was a problem with the units of P_vap to calculate the clearsky emissivity. The correct parametrization is shown now in the new graph and the different parametrizations are more similar now. The idea of this piece of work is not to derive new parametrizations but to test transferibility of parametrizations obtained at other glaciers.**

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?

**No, it considers only reflection from the surroundings in case of the installation of the AWS on a non-zero slope, where part of the slope should be visible for the sensor.**

How is it computed in the case of overcast conditions?

**Increased cloudiness increases the relative contribution of the diffuse radiation ( see formulas (2),(5) and (6) in Brock and Arnold 2000).**

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.

**The idea of this study is to test and compare the performance of different parametrizations. The COSIMA albedo scheme is validates against measurements on Mocho Glacier (Figure 9).**

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).
**We agree. However this assumption is part of this model that we decided to test in this piece of work.**

Page 9 line 10: “clear sky emissivity”=> again, validation of the equation may be easily done using field data.
**Correct. This is what we are doing in section 5.2 (Figure 10)**
Page 9 Line 12:” theoretically site-specific clear sky incoming solar radiation”: how is it computed?
**It is computed according to the formulas derived in 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,**
Moreover, do the authors compute n with same equation in every models?Is it computed with equation 9?
**Equation 9 is only used in the third method, EB-model has its own scheme and for method1 it is not necessary to calculate cloudiness, since the longwave radiation is directly measured.**
Equations 7 and 8 or only for stable conditions, which are generally not verified in the morning.
**During the measurements period air temperature is > 0 degrees Celsius also in the early morning, which should guaranty stability.**
Why do they assume this, when computing a Richardson number is very easy when surface temperature is available? Please justify.
**Again, in this study the primary goal is not to change the different assumptions made by the models employed but to show how these parametrizations vary between the model and what are consequences of this assumptions on the results.**

Page 9 line 22: “the roughness Reynolds number (Brock and Arnold, 2000).” => you mean using Andreas (1987) polynomials?
**Yes, reference added!**

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.
**A collection of codes are used in which the formula derived in Corripio (2003) are applied. These codes are written in IDL.**

Page 10 Line 6: please cite : U.S. Army Corps of Engineers (1956).
**We do not have access to this piece of work!**

Page 10, Line 22: "However here SH is multiplied by a correction factor which depends on the bulk Richardson number Ri" => Please precise.
**Ok. reformulated.**
Why do the authors use this formulae instead of the bulk approach?
**Again, here we use the parametrizations that are already implemented in the models.**
In particular, the assumptions behind equation13 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?
**Here it would be good to know what you call THE bulk method. To our understanding (and according to the authors of COSIMA), COSIMA IS using the bulk method. Perhaps it is using different stability corrections to the ones you are used to?**

Section 4.1 : Glacier climate => please reformulate this title
**Ok. Now we call this section: "Microclimatic conditions on the glaciers surfaces"**

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.
**Ok. The formulation of this complete paragraph has been changed now following your suggestions.**
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.
**Ok, we mention this now.**

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).
**Ok, we removed the bias now!**
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-TurbioGlacier (Brock et al., 2007).
**We do not have measured ablation data for Pirámide Glacier for the study period, which makes it impossible to compare with the results of 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.
**This is right, but this study is about comparing melt rates in the ablation area of the glaciers. We precised this in the text now.**

Figure 6, 7 and 8: show that the big differences between the 3 models are observed inLWnet 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.
**A stability correction of the turbulent fluxes was applied now to in the reference database and the resulting fluxes are very similar to the ones predicted by COSIMA now. Differences between the fluxes obtained by the different models are discussed in section 5.2.**

Page 15, Line 10: “although they have opposite exposition” => The studied surfaces are expected to be flat (no slope).
**Yes, but the exposition of the glacier still can make a difference due to shading of the the high peaks which constitute the accumulation area of the glaciers.**

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.
**Ok. We performed the bias correction now!**
Caption of Figure 9: “albedo. In COSIMA the following parameters have been cho-sen: 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?
**By comparison with the measured (daily) albedo.**

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.
**Ok!**
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 turbulentheat 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.
**Ok! Reformulated!**

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 thisTable, it would be interesting to include the elevation of ablation measurements, the latitude, the time period of measurements.
**Thank you for this suggestion. Sadly we do not have ablation data for the modeled summers which is why we have to compare with data from different years and/or different glaciers. This is why we also are not able to judge the results of the different methods on the basis of the observed melt rate. In this sense it would be probably not correct to include a "validation table".**

Page 18, line 23 :" observed melt range"=> "observed melt rates"
**Ok!**
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.
**Again the comparison with the values measured at Grey Glacier are not meant as a "validation" of one special method. We just want to show that the high melt rate that we obtained with one of the methods for Exploradores Glacier is in the range of observed melt rates at Patagonian Glaciers at a similar elevation range. We indicate this more clearly in the manuscript now.**

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).
**This statement refers to the two models tested in this contribution and is detailed in the paragraph which follows the statement. Specially the difficulties to transfer model parameterizations for me is a clear indication that even if the energy balance models are trying to reproduce correctly the energy fluxes their parametrizations are based on measurements and they are therefor empirical and not physical . Model Intercomparison Studies like the one you are citing are very valuable but their conclusions are mostly similar to ours: individual model results can be far away from the reality, but, if I use a huge set of models, the average between all these models is mostly doing fine. But this is due to statistics and not due to good physics!**

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.
**I am sure that the sentence is not trivial for all the readers of the Cryosphere. Indeed I have seen few publications of measurements of longwave radiative fluxes over glaciers. Again, the goal of this study is not to find THE best parametrization for every glacier (also we indicate them in Figure 10), but to test how transferable or “universal” are the parametrizations proposed in the tested models.**

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.
**We agree with this statement. This is why we encourage direct measurements of turbulent fluxes over glacier surfaces to be able to better judge the different parameterizations.**

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.
**In general there exist three different roughness lengths (z_0, z_H, z_T). Formulation was changed!**

Page 20, second paragraph: “Bringing the predicted melt rates by these highly paameterized 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.
**The SEB models we know, all have a high quantity of non-physical parameters (because their parameterization stem from empirical fits to data).**

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.
**Ok, we added: “in the ablation area of the glaciers”**
Page 20, Line 20: “The models underestimated the measured emissivity of the clearsky atmosphere in the Wet Andes.”=> please correct LWin before concluding.
**If we firstly “corrected” our model results according to the measurements, then the validation against the measurements would not make sense any more!**

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.
**We DO it (for the radiative fluxes): for example red dashed line against blue line in Figure 9 or black lines against colored lines in Figure 10.**