Response to anonymous referee #1

We thank the referee for their constructive comments, which will certainly help to shape this manuscript into an improved paper. We provide here our responses to each comment and/or question made and how we will modify the manuscript as a result. The referee's comments are in blue, and our response in normal text.

Review of "Climatic control of the surface mass balance of the Patagonian Icefields" by Carrasco-Escaff et al., submitted to The Cryosphere.

The authors present an interesting study that adds valuable new knowledge to climate and glacier science related to southern South America. The study has been carried out well and is sufficiently documented over large parts of the manuscript. Just the description of the sensitivity analysis is in parts hard to follow and some efforts should be undertaken to improve readability of this section. Apart from this, I have two major objections that prevent me from supporting publication of the article in its present form:

Major comment 1)

The downscaling of solar radiation as it is described in the one sentence provided in L183f has to be questioned. Bilinear interpolation of shortwave radiation on a non-systematically varying surface (like a DEM representing natural terrain) leads to wrong values at the higher-resolution scale. The angle between incoming direct solar radiation and surface slope/aspect (incidence angle) is crucial in determining the right amount of energy reaching the glacier surface. Hence, simply interpolating radiation values from low- to high-resolution grids introduces errors that could easily double or halve solar radiation energy reaching the surface. Regarding diffuse radiation, the skyview factors of the high-resolution grid cells might probably differ considerably from those of low-resolution fields. Taken together, it requires more to downscale solar radiation than just bilinear interpolation.

As spatiotemporal variability of solar geometry can easily be implemented in a downscaling model, the approach needs to be refined by considering incidence angles at each grid cell of the high-resolution topography. Otherwise, the resulting values are simply wrong. Moreover, a validation needs to presented that compares original and downscaled values to in situ measurements (ideally at an on-glacier weather stations). Such a validation must also be presented for T and P, as otherwise it is hard to argue why the RegCM fields can be used for reliable SMB modeling, especially as they show considerable biases to the reference CR2MET climate, which are corrected in a rather simple way only. I'm sure that the team of authors has access to such data even if it might cover only a short period of time.

These validations might also help to overcome the problem of validating the modeled SMB with respect to inter- and intra-annual variability. Assuming that downscaled T, P and R clearly show seasonal variability on a local scale, this would also suggest that the modeled SMB might be reliable in this respect.

We thank the referee for this valuable comment. We performed bilinear interpolation on the solar radiation field motivated by reproducing the temporal (year-to-year, winter-to-winter, and summer-to-summer) variability of the SMB. The slope and aspect, understood as fixed features of the terrain, would have a stronger importance in the assessing of the spatial variability of the SMB than in its temporal variability. Nonetheless, the referee's warning about the possible introduction of errors that could double or halve solar radiation needs to be addressed. In this regard, we will estimate the error introduced in the solar radiation by the use of bilinear interpolation in comparison with a downscaling technique considering incidence angles at each grid cell of the high-resolution topography. Then, we will compare the interannual variability of the modeled SMB using both techniques, and after that we will diagnose the need for changing the downscaling method for solar radiation. If there is a need to change the downscaling method for solar radiation to one considering incidence angles at each grid cell of the high-resolution topography, we will implement that change throughout the manuscript. If not, we will include the mentioned analysis in the supplementary material of a revised version of the manuscript and maintain the original bilinear interpolation technique.

Due to the nature of this study, we are interested in validating the meteorological variables on an interannual scale, which necessarily entails having large data periods. Other time scales than the one mentioned are beyond the scope of the investigation. As shown in fig. S1, there are few stations (and with few data) on or near the icefields to perform this validation. Bozkurt et al. (2019) perform a validation with these stations for the modeled variables of near surface temperature and precipitation. Furthermore, in this work we decided to compare the interannual variability of the RegCMv4 modeled variables with the CR2MET product, which combines the few observations in the area with reanalysis data. This comparison also allows us to have an idea of the possible bias of the modeled variables, which is useful to establish the intervals of possible bias in the sensitivity analysis. Given that, as mentioned, there is not enough data from meteorological stations to validate the interannual variability of the variables, the sensitivity analysis is the instrument with which we validate that the conclusions of our work do not vary substantially even in the presence of bias. Similarly, and despite the above, a revised version of the manuscript will compare the variables modeled with observational data for the stations that do have sufficient data.

Major comment 2)

Climate forcing is analyzed using the SMB integrated over NPI and SPI together. This spatially undifferentiated way of looking at the outcome of this study is a missed opportunity that should be accounted for in a revised and extended version of the study. In its present form the analysis prohibits to get an idea about potential regional variability of forcing mechanisms across Patagonia. I would like to see similar figures to Figs. 6-11 be added to the supplement that show the correlations with only NPI and SPI. Analyzing the differences of these two sets of maps/graphs would give valuable insight into regional variations of climate forcing across Patagonia. This would strengthen the interpretation of the so far presented results which just integrate over NPI and SPI. Sections 3.3-3.5, as well as discussion and conclusion should then be extended accordingly. As we know from the literate that NPI and SPI do not always show the same patterns of glacier change, such an analysis might be of really high value to science – even if it shows that climate forcing mechanisms do not differ significantly for NPI and SPI.

We thank the referee for this suggestion. We agree that performing the same analysis on NPI and SPI separately might be of really high value to science. Accordingly, we will develop figures similar to Figs. 6-11 for the NPI and the SPI, we will include them in the supplementary material, and we will discuss them in the manuscript.

In addition to these comments I have quite some minor comments that also needs some attention of the authors. Based on the two major comments above and the minor comments below, I suggest to return the manuscript for major revision.

Minor comments:

L9: better: ...fields of climate variables from the ERA-Interim…

We thank the referee for this suggestion. We will change this line of the manuscript accordingly.

L40: These positive trends fit to the recent southward shift and strengthening of the southern hemispheric westerly wind belt (e.g. Goyal et al. 2021, doi:10.1029/2020GL090849), which might be of interest here.

We thank the referee for this comment. We will refer to the strengthening and southward shift of the southern westerly wind belt when mentioning the observed precipitation trends in Patagonia.

L55-57: These moister than average conditions in southern Patagonia have already been suggested to significantly influence SMB (Möller et al. 2007, doi:10.3189/172756407782871530), which should be noted here.

We thank the referee for pointing this out. We will notice this in a revised version of the manuscript.

L80: better: …, i.e. the net change of mass at the surface, … "Gain" suggest an increasing mass of ice, but SMB has been positive and negative in the period studied. See Cogley et al. 2011 (Glossary of Glacier Mass Balance) for further details on the related terminology.

We thank the referee for this correction. We will change the line in the manuscript accordingly.

L81ff: I see no need to explain glacier mass balance in such detail as the manuscript is written for the cryosphere-centered journal. E.g. basal melting should only be mentioned if it is of interest at the glaciers modeled in the presented study.

We thank the referee for this recommendation. We will remove the detailed explanation of glacier mass balance from the manuscript.

L95ff: Braun et al. 2019 and Dussaillant et al. 2019 (both in the manuscript) should also be mentioned here. And it should be discussed that these two remote sensing studies have shown strong mass loss especially over the SPI, which contrasts the positive SMB mentioned before. In its present form the reader gets a picture of increasing ice masses in southern Patagonia, which is wrong.

We thank the referee for warning us about this possible misunderstanding. We will mention the references in the paragraph and we will emphasize the contrast between negative total mass balance quantified by remote sensing methods and the positive SMB obtained through modeling.

L129: Why ERA-Interim and not ERA5 which is available for quite a while now?

RegCMv4 simulations use initial and boundary conditions from ERA-Interim reanalysis because it was fully available at the time the simulations were designed and executed (year 2015), meanwhile ERA5 was not.

L134: Also provide reference to Alvarez-Garreton et al. 2018 here, and not only at the end of the paragraph.

We thank the referee for this recommendation. We will include a reference to Alvarez-Garreton et al. 2018 in L134.

L132-140: What makes the CR2MET dataset a reliable reference? I do not question here that it could be used as this, but I would greatly appreciate additional argumentation. It is necessary to outline and explain how well this dataset represents in situ conditions. Moreover, information about shortcomings and especially inaccuracies of the dataset are needed to be able to judge about its reliability. And finally (maybe most important) why are the RegCM fields created and used when CR2MET already exists? What is the advantage of RegCM over CR2MET and does this advantage justify the introduction of additional uncertainty (by comparing it to CR2MET before usage)?

Please see our response to major comment #1 in relation to CR2MET. We used the RegCMv4 simulations basically because, among the modeled variables, they include near surface temperature, precipitation and surface downward solar radiation, useful for the SMB modeling. Also, they come from a physical downscaling and therefore are physically coherent. Instead, the CR2MET uses statistical downscaling and does not have solar radiation among its output variables. We used the CR2MET product to estimate reasonable intervals for possible biases in near surface temperature and precipitation for the sensitivity analysis, especially in an area where there are only few stations with data between 1980-2015.

L147: better: "… of world-wide glacier extent at the beginning…", as "extension" implies a process of increase rather than a static condition

We thank the referee for pointing this out. We will modify the line accordingly.

L158: not clear what is meant here: "Lastly, we spatially unweighted averaged the meteorological forcing…"

We thank the referee for warning us about this unclear expression. In this step we computed the spatial average of the meteorological forcing assigning the same weight to each grid point. We will clarify this in a revised version of the manuscript.

L159: better: "Only grid points within…" (omit "Note that")

We thank the referee for this suggestion. We will change this line in the manuscript accordingly.

L192: provide reference for this representation of the fraction of solid precipitation

We thank the referee for this suggestion. We will provide reference for this representation in a revised version of the manuscript.

L209ff: It would be interesting to get some values on the distribution of snow/firn after the spin-up time: Give average numbers for snow-/firnline altitudes across the study area and discuss potential spatial variations in case they exist. Give reference to other studies which derived snowline altitudes in Patagonia and shortly compare your results to these findings.

We thank the referee for this valuable suggestion. Nonetheless, we think that an extension of our work analyzing the specific spatial distribution of the snow-/firnline altitude is beyond the scope of our work.

L231-235: This is a really nice idea. However, I strongly request that also information about the bias in SMB compared to the reference SMB is somehow incorporated in the Taylor diagram (e.g. by scaled sizes or color-scales of the points shown). The so far given information about correlation and standard deviation only give insight into how well the variability is represented, but do not tell anything about resulting biases.

We thank the referee for this suggestion. We agree that showing the information about the resulting biases would be valuable. We will incorporate the requested information in the Taylor diagram in a revised version of the manuscript.

L239-249: This is an interesting approach, but more information is needed here. First, give reference to studies that introduced or at least support your idea. Second, give more details on how you determined the variability in the dataset and how you subsequently removed it. Also here, a quantification of biases is needed in addition to the measures of variability.

We thank the referee for these recommendations. We will give more information about this step in a revised version of the manuscript. We will give references to studies that support our idea, give more details on the methods we used, and incorporate a table with the resulting biases in the supplementary material.

Fig. 5: I suggest to add a thin black line representing a zero SMB in the upper panel of the figure. This would increase readability and make positive and negative SMB years more easily distinguishable.

We thank the referee for this suggestion. We will add the suggested line in the upper panel of Fig. 5.

Table 2: Add information about the period represented by the given numbers to the caption.

We thank the referee for this suggestion. We will add the information in the caption.

L287: The fact that annual insolation shows a higher correlation to SMB than annual temperature further supports my initial request regarding a refined handling of solar radiation during downscaling.

Please see our reply to major comment #1.

L304ff: Isn’t that a necessary result of the over-simplified radiation downscaling that has been applied? I mean, how can a local-scale control over the SMB can be present when the applied downscaling is not able to produce the requited local-scale variability? (see my initial major comment) This analysis/interpretation must be redone after the radiation downscaling has been improved.

Please see our reply to major comment #1.

L307-318: It now entirely clear what was done here. A linear regression results in intercept and slope of a regression line, which are both important for interpretation. However, this full information is missing in Table 4 and has to be added. It must also be included in the following discussion.

We thank the referee for this suggestion. We will include the information about the intercepts in a separate table and discuss it in the text.

L325ff: Why is solar radiation not considered here?

We did not consider solar radiation in the regression analysis because we had already concluded from the sensitivity experiments (described in Sect. 2.3.4 and tabulated in Table 1) that solar radiation exerts a negligible control over the year-to-year, winter-to-winter and summer-to-summer variations of the SMB. In the regression analysis, we are interested only in the variables that exert control over the SMB directly (near surface temperature and precipitation) or indirectly (the rest of climatic variables).

Figs. 6b/7b: I recommend not to use red/green colors for the isolines as these colors are hard to differentiate for a lot of color-blind people.

We thank the referee for this suggestion. We will change the color of the isolines accordingly.

L410ff: It would greatly strengthen the findings of the study if comparisons to other long-term SMB time series at other Patagonian glaciers would be given. E.g. Möller & Schneider 2008 (doi:10.3189/172756408784700626) present a modeled SMB time series for Gran Campo Nevado ice cap south of the SPI. This time series e.g. shows the same strongly positive anomalies of SMB in 1990 and 1995, which supports the presented findings for SPI by showing that they fit nicely into the picture presented by other studies. Further south (e.g. Tierra del Fuego) other SMB pattern prevail (e.g. Buttstädt et al. 2009, doi:10.5194/adgeo-22-117-2009), suggesting a southward limitation of the regional pattern.

We thank the referee for this recommendation. We will compare the findings of our study with other modeled SMB including the ones mentioned.

L418: Doesn't this contradict the results that you presented before (see my comments on L287 and L304ff)? This should be clarified either here and/or above.

This does not contradict our results. On the one hand, the fact that insolation shows the second-highest correlation with annual SMB (r=-0.44*) does not necessarily imply that annual variations of insolation exert an effective control over the annual variation in SMB. We interpret the correlation between insolation and SMB as a mere consequence of the correlation between insolation and precipitation (r=-0.53*) due to the presence of clouds and the diminishing of solar radiation when precipitating. As precipitation shows the best correlation with SMB (r=0.69*), this covariance necessarily will be reflected in the insolation.

On the other hand, the local-scale analysis shows a negligible dependence of the annual SMB on insolation. To give a more detailed interpretation of this result, please consider again the eq. 5 and assume snow as the type of surface. To compute the effect on the surface energy flux of an insolation anomaly we have to multiply the anomaly by a factor 0.15, while in the case of temperature this factor increases to 9.5. For instance, a temperature anomaly of one standard deviation (0.37 ºC if we use the std. dev. of the annual time series to estimate the magnitude of typical deviations) implies an addition of 3.5 W/m^2 to the surface energy flux while an insolation anomaly of one standard deviation (4 W/m^2) adds only 0.6 W/m^2. Thus, temperature anomalies have a greater influence on the ablation field than insolation anomalies (with an approximate ratio of 7:1). This does not contradict the previous result about correlations because the local-scale analysis assesses the causes of the year-to-year variations of the SMB, while the correlation analysis does not assess causality.

L418-426: This paragraph would benefit from some references to either figures or tables.

We thank the referee for pointing this out. We will reference the proper tables and figures in a revised version of the manuscript.

L456ff: References to other studies dealing with this or comparable issues would support your speculation and should be added and discussed shortly.

We thank the referee for this suggestion. We will include references to other studies dealing with comparable issues in a revised version of the manuscript.

L474: This thought has not come to my mind until now: Is there any significant interannual variability in solar radiation? Or is it largely time-invariant? I'm asking because of the frequent presence of clouds in Patagonia. If there is no significant interannual variability, it would be a necessary consequence that SMB variations show almost not dependence on it. This needs to be analyzed (and outlined in the results section) before giving this broad statement, in order to potentially put it into the right context.

Please refer to our reply to the minor comment on L418. Although the low coefficient of variation of the annual insolation (std. dev./mean near 3%), the SMB variations show almost no dependence on the insolation due to the mathematical relation between the albedo, the c1 calibration parameter and the typical anomalies of near surface temperature and insolation. In a revised version of the manuscript, we will incorporate this analysis into the discussions and outline in the results section.

L490: "SBM" needs to be corrected to "SMB"

We thank the referee for pointing this out. We will correct this in the manuscript.