EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-603-RC1, 2022

Comment on egusphere-2022-603

Anonymous Referee #1

Referee comment on "Climatic control of the surface mass balance of the Patagonian Icefields" by Tomás Carrasco-Escaff et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-603-RC1, 2022

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.

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.

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…

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.

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.

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.

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.

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.

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

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

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)?

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

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

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

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

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.

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.

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.

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.

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

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.

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.

L325ff: Why is solar radiation not considered here?

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.

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.

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.

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

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

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.

L490: “SBM” needs to be corrected to “SMB”