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Interactive comment on "Quantifying mass balance processes on the Southern Patagonia Icefield" *by* M. Schaefer et al.

Anonymous Referee #2

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Review of Quantifying mass balance processes on the Southern Patagonia Icefield by Schaefer et al.

Introduction

Using a physical downscaling of atmospheric reanalyses, the authors present the long-term surface mass balance of the SPI. They use this SMB field and reported mass loss to infer calving rates for each individual glacier catchment. The subject fits the scope of TC, and the paper is relatively well written. However, I have several general comments and specific comments that, altogether, inhibit publication of the manuscript in its current form.

Major comments

The method to determine the SPI SMB is sound, although very hard to evaluate (I follow the suggestions of the other comments posted in the discussion). However, the inferred calving rates are associated with too high uncertainties to present as such, since the uncertainties in the SMB fields, ice thickness, as well as the volume-mass conversion are high, but difficult to estimate. Therefore, I suggest deleting the part on the calving, or, at least, give it less weight in the paper and give more weight to the associated uncertainties. The large differences presented in Table 1, column 3 and 4, already indicate that the method is not working. This is not a critique to the method or to the authors, as this is the best available as yet, but I don't think it can be presented in this form. The text on page 3127 and 3128 suggests that the authors also have strong doubts regarding the results, and try to collect all possible evidence why this might not be the case. An alternative could be to use GRACE data as a tool to evaluate the modeled SMB (e.g. looking at the seasonal amplitude) and/or the total mass balance.

Does the model include a firn model, and if so, why are all the results presented as volumes, and not as mass? The authors could interpret the observed volume changes, and convert them to mass changes, using their model, which would be a great addition to the paper.

P3120, L 9: Why did the authors choose for NCEP, and why backwards until 1975? Reanalyses on the southern hemisphere are known to perform very poorly before the satellite era (1979, see e.g. Bromwich et al., 2004), and NCEP appears to perform poorly even after 1979 in high southern latitudes (Nicolas and Bromwich, 2011).

P 3121, L 4: why is this constant lapse rate used? In this moist environment, I expect strong temporal (i.e. seasonal) and spatial variability of lapse rate. Why is the lapse rate not take directly from the NCEP output?

Naming conventions: The authors continuously switch between SMB, accumulation and mass balance. For instance, Figure 2c does not show glacier mass balance, but area-integrated glacier SMB. This should be considerable revised and improved in a

potential revision.

P3128: how is the potential change in SMB from volcanic heating calculated? More details should be added here.

Specific comments

P3118

L2: model cannot be validated, only evaluated.

L5: high. . . quantify

L7: positive and has been increasing during the period 1975-2011

P3119

L13: models. For the period 1975-2011, Rivera. . .

L25: by an increase of calving

L26: in this paper

P3120

L3: As a first step

L4: one or two-way nesting? Specify resolutions of each domains.

L22: define NPI

L24: define correlations, of the linear fit, R of R2?

P3121

L19: we present the annual mean incoming. . . L20: a sharp west-east gradient

P3122

L6: this is unnessary information, this is a forcing and not a result

L14: mass balance = SMB !!!

L14: mweq: define

L25: SMB values (please check the manuscript for these inconsistencies)

L26-27: this is information for in the figure caption.

P3123

L9: is sublimation accounted for in the model, and if yes, how?

L17: if this is not the case, you should not use accumulation, but precipitation

L19: albedo-melt feedback. Give a short explanation.

L22 and further: why not present these as area-integrated values?

P3127

L25: overestimate

P3128

L2: I would remove this sentence, or elaborate. This adds to the feeling that the authors doubt their own results.

P3129

L7 and L12: I see two different numbers for the same process.

P3130

L4: rather then wind exposed peaks

References

Bromwich, D. H., R. L. Fogt, 2004: Strong Trends in the Skill of the ERA-40 and NCEP–NCAR Reanalyses in the High and Midlatitudes of the Southern Hemisphere, 1958–

2001. J. Climate, 17, 4603–4619. doi: http://dx.doi.org/10.1175/3241.1

Nicolas, J. P. and D. H. Bromwich, 2011: Precipitation Changes in High Southern Latitudes from Global Reanalyses: A Cautionary Tale. Surv. Geophys., 32, 4-5, 475-494, doi: 10.1007/s10712-011-9114-6

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