

Dear editor,

find attached our answers to the comments of the reviewers and the list of changes that were realized in the manuscript.

### **Answers reviewer 1:**

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I wish to thank the authors for their detailed response to the reviews. This clarifies many of the critical points addressed in the reviews. However, the issues which the authors are communicating in the response to the reviews have not been fully taken into account when revising the manuscript.

A main critical issue that is not well communicated is the comparatively high uncertainty in modelling of precipitation and the resulting values of net accumulation:

- In line 193 ff the authors state: “In Fig. 3 we compare the results of our simulations to these direct point measurements of the surface mass balance. Satisfactory agreement can be observed between the modelled and the measured data.” This is true for the ablation area, but by no means valid for the accumulation area, as obvious from the scatter of the 3 points of Fig. 3 (Nr. 3, 5, 6) in the accumulation area (which the authors correctly recognize as an inadequate sample).

***Ok, we changed the formulation.***

- In line 206 ff the authors state: “The process of wind drift ..... These local effects are important when comparing point measurements with modelled surface mass balance, but should not play an important role when estimating the surface mass balance of larger areas as glacier basins or even the entire SPI.” This is rather speculative, not supported by analysis or references. Elevation gradients of precipitation depend on several factors, not just wind drift. Also, there should be major differences in wind effects between the luv and lee sides of the ice field.

***Ok, we added a comment on the possibility of different effects of snow drift on glacier catchments situated at the luv and lee sides of the icefield.***

- On page 6 of the response document the authors state: “We can get an idea about the uncertainties of the individual mass balance components at every glacier by comparing columns 1 and 2 to columns 3 in Table1. We think that this is much more informative than inventing some arbitrarily high a priori uncertainty to the modelled accumulation.” These statements imply that a clearly defined and traceable error assessment is missing and that the uncertainty of modelled net accumulation is not known, and thus could possibly be quite high. This should be communicated not only in the author’s response, but also in the manuscript.

***This statement was already in the manuscript (line 265 in the new manuscript and section 3.5 new manuscript) and has now been additionally added in the conclusions section (lines 582-585).***

- “comparing columns 1 and 2 to columns 3 in Table1” in the statement above probably refers to Table 1 of manuscript Version 1 (Qc inferred and Qc from velocities). “Comparing” these columns does not enable any clear quantitative error assessment. On one hand, two multi-year periods are compared with a short term data set. On the other hand, column 4 (Qc from 2004 velocities) has high error bars; these translate into high error bars for the modelled surface mass balance if the relation of Equation 1 is used.

***We agree with you: using all the different kind of data that we had at our disposal to validate the model, we are still not able to make a clear quantitative error assessment. In the conclusions section we suggest measurements that hopefully will help to better constrain the uncertainties associated with the predictions of the model.***

- Table 1: overall uncertainties should be provided for Qc inferred rather than uncertainties considering only one of the error sources. The rather low error bars in the present version of Table 1 may lead to misinterpretation and are telling little about the uncertainty of the modelled mass balance.

***You are right each column on its own does not contain any informations about the uncertainty of the modelled mass balance. Comparing the different columns and using additional information given in text ( for example if the velocity given in Table 1 is representative for the long-term velocity or if the velocities were measured at the front), the interested reader can build a up an opinion of the quality of the models' results.***

- Table 1, columns 1 and 2 of revised manuscript: ELA data of the same years should be compared (2002 and 2004). It is not meaningful comparing the mean value of a long time series with data of 2 specific years.

***Me think that the modeled average 1975-2011 ELA is the more interesting parameter to share with the scientific community.***

- Conclusions: uncertainty estimates should be provided together with the mass balance numbers. The basis for estimating uncertainties needs to be explained in the manuscript (or in Supplementary Material).

***As you correctly noticed above: we are not able to rigorously quantify the uncertainties of the modelled surface mass balance. What we could do is to give personal educated guess (which would be probably not so far from the real uncertainties), but we think that it is more honest ( and also more scientific) not to do so, in a scientific publication in a journal with educated readers that can do there own guesses building on the data presented in our manuscript.***

Further issues:

- In the previous review there was a comment on the 1975 to 2000 (resp. 1968 to 2000 for Argentinean glaciers) volume change that has not been taken into account (or communicated) in the revised manuscript: Rignot et al.(2003) explain that the 1975 DEM did not cover areas at elevations above 1200 m. Obviously, this results in is a mismatch with the area covered by SMB simulations that extend over the whole ice field.

***This is right, but the Rignot et al.(2003) extrapolated the losses at higher elevations from the losses of lower elevations. All the numbers that were are citing from Rignot et al.(2003) include this extrapolated values.***

- Line 335 ff: “A literature reference on the 2004 SPI velocity field is missing. ***These are unpublished data, so no literature reference is possible.***
- In line 335 ff it is explained: “The model ELAs are considerably lower than the average SLA at the end of the summers 2002 and 2004 for the glaciers HPS12, HPS13, HPS29 and HPS34. This discrepancy can be explained by the possibility of snowfalls before the acquisition of the examined satellite images.” Snowfall effects should be rather the opposite. Summer snowfall in satellite images should lead to underestimation of inferred ELA.

***Thank you, of course you are right! We changed the entire paragraph which is treating the comparison between model ELA and SLA observed by de Angelis2014 (Line 335-365 new version), which made it much more coherent.***

## Answers reviewer 2:

Abstract: The abstract needs to be improved in clarity; what does "high measured accumulation" mean, "high ablation", "positive SMB"? Quantify.

***Ok, since they are extraordinary high, we indicate maximum measured values for accumulation and ablation in the abstract now.***

L60: define "mweq"

***ok***

Section 3.1: include some commas to improve readability, e.g. after W/m<sup>2</sup> (L148), after SPI (L155) and after peaks (L158)

***ok***

L211: I guess ".. the surface mass balance of the entire SPI, or even individual glacier basins,..." is more logical.

***We prefer the sentence as it is.***

L219: trend is in units mweq/year<sup>2</sup>

***ok***

Fig 3 and 4 are poorly readable. Increase font sizes, change "specific" to "surface" and re-arrange the legend in Fig 4 inside the graph.

***Ok, we increased the font sizes and changed specific to surface. The legend of Fig. 4, we prefer outside the graph.***

L230: melt is not equal to runoff! what is the amount of refreezing?

***Since the SPI is temperate, we assume that there is no refreezing. We added a sentence which clarifies that in the methods section.***

L279: melt = runoff

***should be melt here***

Section 3.6: Change section title to "Mass balance of Perito Moreno" or equivalent.

***ok***

Section 3.7 is clearly added after revision and has no link to the previous sections whatsoever. In section 3, I would advise to discuss first climate, then SMB (section 3.2, 3.4 and 3.7), then calving (section 3.5), and then mass balance (3.3 and 3.6).

*Ok, we moved the previous section 3.7 after previous "3.4 Surface mass balance of the individual glacier catchments" and before "3.6 Calving losses of the individual glaciers".*

**List of changes:**

- maximum numbers of measured ablation and accumulation were added in the abstract
- a sentence was added in the methods section, explaining that refreezing is disregarded on SPI
- the paragraph containing the ELA-SLA comparison was changed, following the comments of reviewer 1
- the font sizes in Figures 3 and 4 were increased
- a comment was added in section 3.5 (new manuscript) now, which states that the uncertainties discussed in this section are difficult to quantify
- a sentence was added in the conclusions section, pointing out the possibility of high uncertainties associated with the numbers obtained by the model