Our answers to the points raised by Reviewer No.2 in ***italic**:*

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.

For more than the half of the analysed glaciers the method is working! Where it it not working we try to find out why it is not working. Is it due to errors in the geodetic balances, due to errors in estimating the calving fluxes from the velocity field or due to errors of the SMB model? We find this analysis interesting, which is the reason why we want to share it with the community.

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.

The spatial resolution of GRACE data is far too low to make an analysis on glacier basin scale.

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.

Mass balance values are mostly expressed as meters water equivalent (mweq) as it is common practice for glacier mass balances. Sometimes we changes to km^3 of ice per year, which for us is a natural unit for calving fluxes and which can be converted into Gigatons (Gt) by multiplying with the density of ice (mostly used is 0.9 Gt/km^3).
The model includes a very simple firn model in which snow turns into firn after one year and firn turns into ice after another year. No detailed layering or compaction is modelled. This, together with the unknown layering in the beginning of the modelling period impedes that model could contribute to the interpretation of the observed volume changes (in the geodetic balances).

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).

We chose NCEP because we had easy access to this data. We used these data backwards until 1975, because we wanted to compare our model results with the geodetic balance of Rignot et al. (2003) which spans the period 1975-2000. We know that reanalysis products have lower quality in the pre-satellite area. However the good correlations found between our downscaled climate data and the few available observations (see Schaefer et al., 2013) make us confident about the quality of our final downscaled climate data, which were driving the SMB model.

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.

The principal lapse rate in the downscaling procedure is given by the 7 year WRF simulation, which determines the statistical downscaling to a 5 km resolution grid. Then, a further downscaling is effectuated inside of each 5 km^2 grid cell, where the constant lapse rates are applied for temperature and precipitation.

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.

Ok, we have revised it!

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

In order to give a upper bound to a process like volcanic heating, we assume that an increased geothermal heat flux of 1000mW/m^2 is evenly distributed below the SPI, which is roughly 15 times the average heat flux over the continental crust. Multiplying this additional heat flux with the number of seconds in a year and dividing this specific energy available in one year by the latent heat of fusion of water a and by the density of water we get a specific mass loss in mweq/year. This is standard calculation in mass balance modelling, so we are not sure if it has to be detailed much more in an experts journal. Editor?

Specific comments: ***(ok means we accept the proposed change, if not we comment why we do not accept)***

P3118
L2: model cannot be validated, only evaluated. ***ok***
L5: high: : : quantify ***we prefer to keep the abstract short, and hope that people that are interested in the numbers will read the paper, Coauthors?***
L7: positive and has been increasing during the period 1975-2011 ***we prefer: "was positive and increasing"***
P3119
L13: models. For the period 1975-2011, Rivera: : : ***ok***
L25: by an increase of calving ***we prefer "losses by calving"***
L26: in this paper ***ok***
P3120
L3: As a first step ***ok***
L4: one or two-way nesting? Specify resolutions of each domains. ***The nesting scheme was one-way involving three computational domains with spatial resolution of 45 km, 15 km and 5km.***
L22: define NPI ***is defined P3119, line 19***
L24: define correlations, of the linear fit, R of R2? ***R is the correlation and not the coefficient of adjustment (R^2)!***
P3121
L19: we present the annual mean incoming: : : ***ok***
L20: a sharp west-east gradient ***ok***
P3122
L6: this is unnessary information, this is a forcing and not a result ***it is both: result of the downscaling and a forcing of the smb model. We think it fits well here.***
L14: mass balance = SMB !!! ***ok***

L14: mweq: define ***ok***
L25: SMB values (please check the manuscript for these inconsistencies) ***ok***
L26-27: this is information for in the figure caption. ***ok***
P3123
L9: is sublimation accounted for in the model, and if yes, how? ***The physical process of sublimation is not modelled. However the model is a semi-empirical model with two tuning parameters for ablation and one tuning parameters for accumulation (Schaefer, 2013). So tuning the model's results to observations (Schaefer, 2013) should bring the modelled accumulation to value which in reality probably corresponds to accumulation minus sublimation.***
L17: if this is not the case, you should not use accumulation, but precipitation ***in our case it should be accumulation (see above).***
L19: albedo-melt feedback. Give a short explanation. ***ok***
L22 and further: why not present these as area-integrated values? ***We think that specific values are suitable, because they allow an easy comparison between glaciers and ice caps of different size.. Area-integrated values are presented in P3124 line 28ff, when comparing SMB processes with calving fluxes.***
P3127
L25: overestimate ***ok***
P3128
L2: I would remove this sentence, or elaborate. This adds to the feeling that the authors doubt their own results.
This sentence introduces the analysis which follows in the next paragraph.
P3129
L7 and L12: I see two different numbers for the same process. ***It is the same process but time intervals vary: line 7: 1975-2011, line 12: 1975-2000 and 2000-2011***
P3130
L4: rather then wind exposed peaks ***ok, we changed the last sentence to: Appropriate sites for accumulation measurements are smooth and flat areas in the central plateau of the accumulation area of the glaciers at elevations of about 1500 m.a.s.l., rather than wind exposed peaks and ridges where snow drift is dominating the accumulation patterns.***

References:

*Rignot, E., Rivera, A., and Casassa, G.: Contribution of the Patag*onia Icefields of South America to sea level rise, Science, 302, 434–437, doi:10.1126/science.1087393, 2003.

Schaefer, M., Machguth, H., Falvey, M. and G. Casassa, G.:Modeling past and future surface mass balance of the Northern Patagonia Icefield, J. Geophys. Res. Earth Surf., 118, 571–588, doi:10.1002/jgrf.20038, 2013.