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AUTHORS' RESPONSE TO REVIEWERS

We greatly appreciate the constructive and detailed comments of both anonymous reviewers and of Mauri Pelto. Below we respond to the comments (in bold, following each comment).

<u>RESPONSE TO MAURI PELTO</u>
Significant suggestions:
1. replace the use of degree day function for determining snow ablation from ice ablation derived on the Greenland Ice Sheet, with one from a more temperate and comparable region.
Good idea; we have now adapted a Hock-type parameterization, which is based on data from Storglacieren, Iceland and French Alps listed in Hock (2003) (see below).

2. Second is to determine the volume flux at or near the ELA and compare that to the assessed accumulation zone balance, and to the combined calving flux and ablation volume loss from below the ELA.
Yes, this would be a good additional but unfortunately we do not have any ice thickness data or time series of surface velocity at the ELA that would be needed to make such a calculation.

1135-15 **By "former of these two plots," we presume Dr. Pelto suggests to plot b vs ELA, which is first of the three types of plots he mentions are found in the WGMS inventories. As our measurement of the ELA of San Rafael is not independent but is derivative of our balance plots (i.e. ELA data in Figure 9 are derivative of balance profiles in Figure 8, where b=0), we feel there would not be added benefit to creating such a plot.**

Detailed comments
1128-15: Can cite lapse rates from the Southern Patagonia Icefield from Rivera (2004) and Kerr and Sugden (1994) are supportive of this value they have 6.0 to 6.5 C km^{-1}.
Thanks. We have included these references

1130-22: What is ELA0 for San Rafael?
The mean (1960-2005) modeled ELA0 is 1310m; see Figure 8. We have added this number to the figure caption.

1133-24: Compare to Table 1 values from Fukami and Naruse (1987) and Figure 6.15 in Rivera (2004).
Yes, good idea, thanks.

1134-4: Braithwaite and Oleson (1988) not a good reference here, and does not generate confidence in the resultant degree-day factor used. The Greenland Ice Sheet is not a good analog for the NPI when comparing snow and ice ablation. Hock (2003) provides a table comparing the degree-day factor for melt from snow and ice regions from a number of glaciers. Hock (2003) finds that the ratio is closer to 0.7 for snow ablation to ice ablation for temperate glaciers. Takeuchi et al (1995 and 1996) and Rott (1998) on Patagonia glaciers generated a degree-day factor as well that should be consulted.
Thank you for this suggestion. Both Takeuchi et al. and Rott use ice degree day factors only. However, Hock did find ratios between snow and ice ablation ranging from 0.5 to 0.69 for temperate glaciers. We have rerun the model using these values, which were

derived from measurements on temperate glaciers in Storglacieren, Iceland, and the French Alps, and have included the revised model results in our estimates of Q_{abl}.

1134-15: The rate of rise of the transient snow line during the melt season can provide another test of the model. Hock et al (2008) discuss the use of this method. The movement of the TSL provides a measure of ablation if the balance gradient is known or a measure of the balance gradient if ablation is known.
Yes, we agree, but unfortunately apart from the snowline/ELA mentioned by Rignot in 1994 and Rivera in 2001, we do not have a record of transient snowlines for San Rafael (as noted in Rivera et al., (2007), this region is notoriously cloud-covered and there are very few satellite observations from which to determine a snow line, particularly during the end of the melt season).

1135-15: One of the key measures for mass balance is the balance gradient, which is not constant with elevation of course but is yet a key measure that should be reported.
What is balance gradient for Glaciar San Rafael? Naruse and others (1997) found .010-.015 on Moreno and Upsala Glacier and Rivera (2004) .013 on Chico Glacier. These data can be used for verification and support. The World Glacier Monitoring Service in its glacier mass balance bulletin provides three key diagrams for each glacier examined in detail: mass balance versus ELA, mass balance versus AAR and the balance gradient. I would recommend that since the ELA has been determined (Fig. 5) that the former of these two plots be added.
Figure 8 does show the mean modeled mass balance gradient over the period 1960-2005. Between 0-1800m, mass balance gradient is ~0.012 m/yr/m; at higher elevations mass balance is constant (3.5 m/yr/m). We have added this description to the Figure 8 caption.

1138-5: Rott et al (1998) contrasted volume flux balance and surface mass balance on Moreno Glacier. This provides an excellent means of comparison of modeled surface mass balance. In Koppes et al.(2011) data is provided for the ELA region that would allow volume flux determination at 1100 meters and volume flux is determined at the calving front. The ELA flux or somewhere near the ELA provides an independent check of the surface mass balance determination in the accumulation zone (Pelto et al. 2008). The change in volume flux from the ELA to the calving front provides an independent check on the ablation zone balance losses. Given the data is readily at hand for determining an approximate volume flux at the ELA and the calving flux is already determined, this should be done.
Indeed this would be a good check, but unfortunately we do not have a time series of surface velocities in the vicinity of the ELA, and nor are we confident that the estimate of ice thickness at the ELA (Rignot et al., 1996) is accurate. As Dr. Pelto mentions at 1138-5, to do so would require "data readily at hand," which unfortunately does not exist.

RESPONSE TO REVIEWER #1 (TCD-5-C554)

Major concern:
The construction of the statistical models (eq. 1, 2, 4), all field data are used for model training but none of them for model evaluation. In my mind this is a general problem in the geosciences with short data records and statistical downscaling (so cryospheric sciences should be amongst the first to resolve it!), where people use all data to fit the statistical relation, but do not check its validity on independent data. If you do such a split sample analysis you will obtain a range of model parameters for each statistical model, and this range could be used to define different cases in Table 1 – more objectively than now (and I would delete case #1).

Great idea – additional analysis of uncertainty is always good. We used the leave-out-out cross-validation approach for our sediment yield study in Laguna San Rafael (Koppes et al., 2010). We have rerun the climate data regression (eq. 1-4) using this approach and compare the results using this method with those from our error propagation analysis, and both ranges are now stated in Table 1.

Minor points in the "general comments" are:
(1) could you discuss more explicitly why the local relation between calving and front retreat differs from other places?
We expect calving and terminus retreat to vary widely from glacier to glacier, as each are dependent upon other factors including the balance flux to the terminus and the rate of thinning (see Equation 6). We have included explanatory text to further discuss this.

(2) The description of models used in the study is distributed throughout the text. I think the fluency would be enhanced if there is one section for all models employed. But this is the authors' decision.
Yes, we have followed this suggestion and restructured the sections to combine the various climate, mass balance, and calving models to make the article easier to follow.

(3) Replace "manipulation" in the acknowledgments (media can misuse such words). You mean "analyses" or "post-processing".
Thank you for catching this – we have altered the text as recommended.

SPECIFIC COMMENTS AND TECHNICAL CORRECTIONS:
1) Please don't use the term "most equatorial" for a glacier at 46 deg latitude it is confusing (and not important for the study). Something like "northernmost Patagonian glacier".
We agree this terminology is confusing and have changed the description as suggested to 'northernmost tidewater glacier in the Southern Hemisphere'.

2) P 1127-1128: You should mention that air temp. and precip. in the reanalysis data are of different quality by nature (type A versus type C variable), so a poor performance of reanalysis precip. (Fig. 2b) can almost be expected.
Yes, it is a very good idea to make this distinction.

3) P 1132, L 3: "derived" (typo)
Thanks, we have fixed this.

4) P 1133, L 17-18: Can you clarify what you mean by "change in retreat rate"? The slope of this curve prior to 1980 shows similarly steep sections than afterwards.
Yes, it is confusing as written. Retreat rate actually slows down in the 1980's when the glacier retreats into the channel; we have clarified this in the text.

5) Equation 4a: needs to start with the symbol for ablation (as 4b does);
Thanks, we have fixed this.

6) Equation 5 and associated text: please clarify if you want to show specific or total mass balance in this equation;
Mass balance is calculated at each elevation z, and summed to calculate total mass balance.

7) P 1136, L 6-7: "calving and mass loss due to melting": any idea if sublimation is important at the terminus face?
We do not know, but we expect sublimation to be small in such a cool, humid environment. We have clarified this as "mass losses from submarine melting and from surface ablation".

8) Section 5.1: please make an immediate reference to Table 1 (not at the end of the first paragraph);
Yes, good idea.

9) P 1138, L 13: +1.06 m/year – is this figure from Table 1? I cannot find it there.
Thank you for catching this, which is a typo from an earlier run. The average surface mass balance for case 4 is +0.78 m/a over 1960-2005 (Q_{acc} –Q_{abl}). We have revised this.

10) P 1141, L 25: "1960-1976" (as in Table 1);
Noted.

11) Equation 9: are roh_sw and roh_i defined in the text?
No – thanks for catching this; also in Eqn. 11 we have roh_w, which was not defined. We have added this.

12) P 1143, L 13: "estimate" (typo)
Thanks

13) P 1143, L 18: what does "consistent" mean? (constant?);
Yes, "constant", thanks

14) Figure 3: please provide the altitude of Laguna San Rafael in the caption;
As mentioned in the first sentence of Section 3, it is connected to the ocean, hence at sea level.

15) Figure 12: "grey shading" – I cannot see shading in this figure. Please revise
Yes, we have reworked Figure 12 and split into two panels to improve the figure significantly.

<u>RESPONSE TO REVIEWER #2 (TCD-5-C558)</u>

1. Accumulation model: One of the most important uncertainties in the analysis seems to be the definition of the accumulation model. In a sensitivity test this is shown by the authors. First of all, the description of the accumulation model should be enhanced. It is for example not clear to me how the authors exactly arrive at the function k(z) shown in Figure 5. They write that k=3 and k=5 was used in the test runs. But if I plug these constants into Eq. 3, where does the elevation dependence come from? Is k a function of z (which?), or a constant? If some relation with x (distance from coast) is included, this should also be visible in the Equation! Moreover, the calibration of the accumulation model not appears to be very robust. Isn't there some more data that could be used for calibration of the accumulation side? Mass balance measurements, transient snow line observations etc.?
We agree that there is much uncertainty in the accumulation model. We have revised the text to make it clear that the k=3 and k=5 scenarios are for constant precipitation over the glacier (of course, snowfall is not constant because some precipitation falls as

rain). We also provide more details about the spatially dependent enhancement factor k(z): while it is true that z, meaning glacier surface altitude, is a function of x, so that Eq. (3) could be written as k(z[x]), we do not see any advantage in doing so. Unfortunately, data for tuning the models are sparse (see comment above). To help clarify this key point in the paper, we have added a table of all measurements made on or near San Rafael glacier (which we discuss in Section 3), including precipitation on the coast, precipitation at SR1, the three snowline/ELA observations, the 1983 ablation measurements, the four terminus velocity measurements and the three point mass balance measurements.

2. Degree-day melt model: I agree that using a simple degree-day model makes sense in the context of this study. However, I see some problems here: The treatment of the snow in the model looks strange to me. Equation 4b (melting of snow instead of bare ice) is only used if precipitation on the previous day was in the form of snow (page 1134, line 7). For example in the accumulation zone, the surface condition is snow throughout the entire year, independent of the temperature on the previous day. Also in the lower reaches of the glacier, several meters of winter snow are probable that will endure some of the summer months when precipitation falls as rain due to higher temperatures, and not as snow. Therefore, I expect that Eq. 4b is used in too few cases. Mass balance models normally track the snow height as a separate variable through the entire year and decide from this variable whether the surface condition is snow or ice.
This is obviously a large source of uncertainty in the model. The model looks for whether T>2 deg C at any elevation z the day before, whether or not there was precipitation, and then assumes that any snow that might have fallen when T<2 deg C will be ablated away if in the following days T>0 deg C, and thereafter the ice ablation co-efficient will apply. This will overestimate the ablation rate, but only during the start of the melt season near the snowline, as the snowline is rising but the model is estimating snow removal faster than may be occurring (if snowfall is greater at z is greater than can be melted away in a day).

My second point agrees with the comment of Mauri Pelto. Just taking a literature value (referring to Greenland with a completely different climate) for the ratio between DDFice and DDFsnow is not sufficient. I strongly recommend a more careful calibration and validation of the mass balance model (both the accumulation and the ablation side) using the given mass balance observations.
Yes, we have changed the DDF for snow (see reply to Mauri Pelto, above). Although observations are sparse, we tried to make the comparison of the model with observations more transparent by adding a table of data, as explained above.

3. Structure: Section 4.3. states (using a reference) that 'calving laws' are used in the study. The reader has no idea yet what kind of calving laws the authors are talking about. The equations are then presented later in the discussion section. I suggest to assemble all model approaches in a method section at the beginning of the paper. It would also be of benefit to present the description of the accumulation and the ablation model together. Also these two models are presently described in different chapters.
We have moved the calving model in with the surface mass balance model and the climate reconstruction (see above).

Detailed comments:
page 1124, line 22: "last remaining large ice reserves ..." might be put better into context. There is as well Alaska and Canada etc. that represent quite important ice reserves outside of

the polar ice sheets.
Yes, good suggestion thanks, We have added text to clarify that that Patagonia is one of the 3 largest reserves of ice outside the polar regions, as per Dyurgerov and Meier (2005).

page 1125, line 15: In the context of the current glacier retreat related to climate warming I do not completely understand this motivation of the study.
Thanks for pointing this out - we have clarified the motivation for this study, which seeks to understand how the glacier response to recent (50 yr) climate conditions might pertain to earlier ones.

page 1127, line 8: Obviously, the authors are aware of the uncertainties in the re-analysis data between 1950-1960. Why are the results for this period shown nevertheless? Even more, as it is stated that results for this period are outside of the focus of the paper? Could this period just be omitted for reaching a better accuracy in the results?

As we are aware of the limitations of the reanalysis data prior to 1960, we omitted this data from the regression analysis (Eqs. 1-4), however, we did feel it was appealing to include the entire record available in our model runs, with caveat, particularly as the terminus record suggests the ice front was unusually stable from 1950-1959.

page 1127, line 12: From a climatological point of view it might be questionable whether these weather data covering only 13 months are sufficient for deriving statistically valid statements for their relation to the re-analysis data. It is clear that no other data are available, but some lines discussing the problems induced by the use of these short time series might be required here.
Yes, good idea, we have added some discussion at that effect. We emphasize that a sample size of 398 daily measurements were used to compare the local weather data with the reanalysis data, which we feel is a statistically significant sample set.

page 1128, line 15: I was surprised to see that the dry temperature lapse rate is smaller than the wet lapse rate. Normally, it is rather the other way around. Is there some explanation for this (inversions etc.)? Are these lapse rates that are derived (modeled) for the free atmosphere also representative for glacierized environments / the glacier surface?
Yes, the result for "dry" days is surprising. We derived the lapse rate directly from the NCEP-NCAR upper-air database (temperatures culled directly from the 1000, 925, 850,700 and 600 hPa datasets). We suspect that this arises because of the high humidity in the region where the 'dry' days are never very dry; note that the lapse rate does not vary significantly. We have added a note about this, and to simplify use a single value for the lapse rate.

page 1132, line 26: Here and elsewhere numbers for the thinning in metres of the HPN are provided (mostly taken from the literature). In my opinion, these numbers are not very useful without putting them into context. What does 'around the margins of HPN' mean? This does not tell you anything about the mass balance of the ice cap – it could be thickening in the interior. Moreover, how large (area?) is the 'margin' of the HPN? If the thinning is spatially clearly attributed (e.g. at the current glacier terminus) the statement is clearer.
Yes, you are correct. The description 'around the margins of the HPN' that we used is as stated in Rivera et al. (2007), from which the observations were made. According to Rivera et al., their average thinning rate was measured from a region that included both the termini of the outlet glaciers (including San Rafael) and retraction of the lateral

margins of the icefield from the valley walls. We have clarified this spatial pattern of thinning in the text.

page 1133, line 16: How uncertain is the assumption that the height of the ice cliff was constantly at 40m above the water line throughout the entire study period? What is the basis for this assumption? Presently the ice cliff height strongly varies between 30 and 70m indicating that it is not spatially constant. What about the temporal stability?
This is an approximation based on our observations in 2005 and Warren's in 1993 (Warren et al., 2005); we do not have any data to help constrain the temporal stability of the ice cliff.

page 1133, line 23: I have troubles with the statement of ablation rates per day. This implies that ablation rates remain constant throughout the year which is most probably not the case. It is not clear if these ablation rates were measured by Ohata et al. (1985) over the summer period only, or over an entire year. If the former is the case, the extrapolation to annual mass balances (as done by the authors of this study if I correctly understand) would not be correct. Clarify.

On page 1133, we identify that stated ablation rate was measured in December 1983, and that these rates are compared to the local and reanalysis temperatures to derive a PDD melt factor. Given the relatively constant cool, humid maritime climate of the HPN, we do not expect PDD factors to change significantly between seasons in this region. This is an assumption however, as we do not have any other data to help constrain ablation rates. We are not assuming that the *rate* of ablation is constant throughout the year, but that the ice ablation co-efficient (ice-PDD factor) is unchanging.

page 1134, line 14: The 2.2 m w.e. accumulation observed on another glacier (having a different exposure than San Rafael) is not comparable to the study site, and consequently should not be used for model calibration / validation.
We have demoted this observation as you suggest – thanks.

page 1134, line 19: Another possible inhomogeneity in the calibration / validation data that should be verified before its use is the date the snowline / ELA observations are referring to. I assume these numbers refer to satellite images taken at a given date. Numbers for the elevation of the snowline at this date do not have to be the ELA (snowline at the end of the ablation season). The model however provides the effective ELA. Thus, a bias in the comparison is possible due to varying survey dates in the different studies.
This is an important point. We have taken ELA observations from the elevations published in the tables provided in Rignot et al. (1996) and (2003) and Rivera et al., (2007), which do not indicate the exact date of measurement. We have contact the authors of these studies to determine whether the published ELA refer to end-of-season or during the season.

page 1137, line 9: Why 'various' degree-day models? Normally, every degree-day model includes the distinction between snow and ice. So, I would only talk about ONE degree-day model in general. 'Various degree-day models' rather implies that completely different modelling approaches based on the temperature index methods were used.
Yes, it is one model with a range of input values for the DDF factor; we have clarified this in the text.

page 1138, line 5: It would be very useful if the goodness of the fit, is shown somehow. The

Figures 8 and 9 that are referenced here only provide model results and no validation.
We have added a measure of rms comparing each model scenario and the three mass balance observations and ELA observations in Table 1 and Figure 9 to show the goodness of fit.

page 1139, line 22: How was the value of 19 km3 for the volume loss by thinning calculated? Some observations are available for the ablation area of the glacier. But what about the accumulation zone? How were given thinning rates at low elevation extrapolated to unmeasured areas?
The volume loss by thinning is calculated as explained in the subsequent paragraphs, by extrapolating the thinning rate at the margins to vanishing values at the headwall. (see below).

page 1140, line 27: Option (1) is probably not realistic. Option (2) seems to be better, but I have troubles understanding the quantitative implications by the results that are presented hereafter. How certain are they? Clarify.
Most glaciers with large volume loss have a thinning pattern that decreases with altitude. We have added a reference (Schwitter and Raymond, 1993) and clarifying text to further explain our approach.

page 1143, line 6: The sliding law model was tuned to three velocity measurements (calibration). With only three data points it is not surprising that a good correlation with modelled and observed velocities are achieved (the same data points are later used for validation, see page 1143, line 25). From a statistical point of view this is problematic.
Yes, this is a problem with the approach; we have included more discussion of likely pitfalls in the model

page 1143, line 19: How realistic is the assumption of attribution the entire surface velocity to basal sliding? Discuss.
For extremely fast-moving, calving glaciers such as Glaciar San Rafael, we have followed the practices of prior studies in assuming that internal deformation (creep) rates are small when compared to >3 km/yr surface velocities, particularly near the terminus where the glaciers are noted to speed up (see Howat et al., 2005; Humphrey and Raymond, 1992; Venteris, 1999). We have taken the approach stated in Benn et al. (2007): "For fast-flowing calving glaciers, the creep component can be assumed to be small compared to basal sliding, and in the work presented here we assume that the calculated basal velocity, *U*B, is equal to the vertically averaged ice velocity, *U."* This is of course an assumption, and will tend to overestimate the downslope ice fluxes. We have added text to clarify.

page 1144, line 11: I think it would be very important to provide a possible explanation for the large divergence between calving rates obtained with the mass balance model and the calving laws. Otherwise a discussion of the sensitivities of modelled calving rates based on the sliding law is difficult.
Yes, the widely divergent results between the model results of the mass balance model and the sliding law model is an intriguing and mysterious result of the study. It should be noted that calving laws are notoriously inexact, and have often been tuned to a single glacier system, where causal relationships may not translate to other systems. One possible reason for this is that stretching rates and crevasse formation are not taken into account in either the calving laws or the observed calving rates based on surface feature tracking to derive downslope ice speeds (but not the calving flux based

on iceberg volume, as measured by Warren et al. (1995) in 1993), and hence both approaches may grossly overestimate the actual flux of ice (see Venteris, 1999). This would result in the mass balance model (which is tuned to the sparse observations of ELA, accumulation and ablation, but not calving) producing calving fluxes that are less than the observed rates of calving and the modeled calving rates based on the sliding law (as we have found, and shown in Figure 12).

Figure 1: The information displayed in this figure is too small. Better focus the plot on San Rafael. Glacier outlines are difficult to recognize, the star in the inset is almost impossible to find.
Good idea, we will zoom in on the glacier.

Figure 3: Enlarge axis labeling
Thanks, we will also check font on other plots.

Figure 4: The meaning of the colors is not clear. The figure would be easier to read if contour lines for the elevation are displayed.
Yes, contours would be an improvement; we have done this as suggested.

Figure 8: The elevation of peak accumulation according to this figure is on 1800 masl. This is strange, as the authors force the accumulation model with the function k(z) that shows a maximum on about 1000 masl (see Fig. 5 and text). Can the authors explain this divergence?
Thanks for pointing this out; we will check the calculations. Some difference arises because this plot shows snowfall (rather than precipitation), but it is not clear that explains it all.

Figure 11: Grey line difficult to see. A legend (also Fig. 12) would be helpful and increase the clarity of the caption.
Thanks, we have made sure the lines are visible and added legends to clarify the plots.

Figure 12: Obviously there is some smoothing (cubic spline?) done to get these time series. This procedure should probably also be shortly explained in the caption. But wouldn't it be fair to show annual values of the calculated quantities? In general, this figure puts a question mark behind to whole analysis: At least visually I cannot see any correlation at all between the modelled calving flux (black line) and the orange/brown (difficult to discriminate) dots (measured calving rates / simulated using calving law). Especially before 1990 the observations are completely off. Why? Is this within the uncertainty range of the model?
We have revised Figure 12 to present annual values in lieu of a smoothing function, and have added discussion of the differences between observed calving fluxes and those derived from the mass balance model (tuned to sparse observations of ELA and mass balance) and the calving laws (see comment above).