The Cryosphere Discussions

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Comment

Interactive comment on "Deriving mass balance and calving variations from reanalysis data and sparse observations, Glaciar San Rafael, northern Patagonia, 1950–2005" *by* M. Koppes et al.

Anonymous Referee #2

This paper presents a comprehensive modelling study of the evolution of Glaciar San Rafael over the last five decades. The authors do a good job in assembling sparse data and using simple model approaches. Despite of the important simplifications in the models and the considerable uncertainties in the analysis – that are quantified and discussed in the paper – a number of interesting findings originates from this study. However, I have several major concerns about modelling and calibration/validation issues that should be addressed by the authors:

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

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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.?

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.
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 DDF_{ice} and DDF_{snow} 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.

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.

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.

- **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.

- **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?

- **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.

- **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 (modelled) for the free atmosphere also representative for glacierized environments / the glacier surface?

- **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.

- **page 1133, line 16:** How uncertain is the assumption that the height of the ice cliff was constantly at 40 m 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 70 m indicating that it is not spatially constant. What about the temporal stability?

- **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.

- **page 1134, line 14:** The 2.2 m w.e. accumulation observed on an other 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.

- **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.

• **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.

• **page 1137, line 14:** rather 'standard deviation'? How did the authors obtain this standard deviation? Clarify. It is stated that Table 1 shows ranges of uncertainty based on these standard deviations in the input parameters. But there are only four different combinations (differing model complexity) shown that could never cover the entire uncertainty range spanned by the poorly defined parameter values.

• **page 1137, line 17:** These results refer to which scenario?

• **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.

• **page 1139, line 22:** How was the value of $19 \, \text{km}^3$ 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?

• **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.

- **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.

- **page 1143, line 19:** How realistic is the assumption of attribution the entire surface velocity to basal sliding? Discuss.

- **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.

- **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.

- **Figure 3:** Enlarge axis labelling

- **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.

- **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?

- **Figure 11:** Grey line difficult to see. A legend (also Fig. 12) would be helpful and increase the clarity of the caption.

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- **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?

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