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The Cryosphere
Discussions

Interactive comment on "Simulating melt, runoff and refreezing on Nordenskiöldbreen, Svalbard, using a coupled snow and energy balance model" *by* W. J. J. van Pelt et al.

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We would like to thank the reviewer for giving constructive comments and all the issues raised have been treated with great care. Our response to all the comments is given below.

General comments

RC: While the first part is presented in a very detail and the applied models are the 'state-of-the-art' in simulating surface mass balance, the second part is almost superfluous and unsubstantiated. It is superfluous, relative to the first part, because the findings do not expand our knowledge about modeling past and future mass balance.

The approach in the paper (using mass balance sensitivities) has been used in many studies before to assess mass balance changes on local, regional and global scales (e.g. Oerlemans et al, 1998; Gregory and Oerlemans, 1998; Oerlemans and Reichert, 2000, van de Wal and Wild, 2001, Slangen et al., 2011) and none of them was able to quantify actual uncertainties arising from omission of the glacier dynamics with all its effects (glacier thickness change, change in glacier area, subglacier hydrology) on net mass balance. Thus, applying the same approach here (in terms of neglecting ice dynamics) and just arguing that the presented 'mass balance scenarios are likely somewhat cautious estimate' is unnecessary addition to the first part of the study. Also, the way how the omission of ice dynamics is discussed (lines 1-11, page 240), especially the argument that the 'height feedback is the most significant omission' is unsubstantiated. If the authors have a reference showing that this is the most significant omission (in terms of quantitative error analysis) it would be good to include it here. My overall suggestion would be to omit the second part of the paper (since the first part is already sufficiently good study for itself), or provide quantitative error analysis for the past and future time series of mass balance.

AC: There were basically two reasons to include the time-series back to 1912 and into the future: 1) to once more illustrate the relevance of accounting for seasonality in climate change and 2) to put the simulated mass balance for 1990-2010 into a longer term perspective. We agree with the reviewer that due to uncertainties mainly related to ice dynamical factors, the presented longterm mass balance time-series do not have much value, since uncertainties cannot properly be quantified. This would only be possible if the mass balance model would have been coupled to an ice flow model. We therefore decided to follow the reviewer's suggestion to omit this part of the paper. In order to still achieve the goals mentioned in the first sentence of this comment, the climate sensitivity section has been extended. Added to this section are: 1) a discussion of the SSC and trends in seasonal temperatures and precipitation during the 20th century in order to indicate a negative trend in the mass balance during this period, 2) a discussion of uncertainties in longterm mass balance estimates when ice dynamics are not taken into account, 3) the role of refreezing in a changing climate is now discussed in this section. With these additions, we avoid presenting uncertain time-series and are still able to achieve the two goals in the first sentence of this comment. The main conclusions are hence hardly affected by these changes.

Specific comments:

RC: Page 213, Line 1: Change ‘Hock and Radic, 2007’ to ‘Hock et al, 2007’. Please correct this also in the references.

AC: This has been corrected.

RC: Page 214, Lines 1-12: I am not sure if introduction should cover the methodology in so much detail. Would it be possible to shorten this section since it almost reproduces the abstract?

AC: We believe the methods and strategy employed in this paper is one of the key elements of this study. We believe it is therefore good to include a brief description of the methodology in the introduction as well. In order to shorten this part somewhat, one sentence describing what the snow model does has been left out.

RC: Page 215, Line 24: The symbol ‘+-‘ is wrongly used throughout the text for the meaning of ‘roughly’. Please correct this to the symbol ‘$\sim$’.

AC: This has been corrected.

RC: Page 216, Lines 19-21: Shouldn't the precipitation rate be expressed in unit that contain time? Here is only given in mm, but referred as a rate. Also this sentence is a bit confusing (the precipitation rate is used to compute a mean altitude: : :?)

AC: The corresponding sentences have been reformulated: "The precipitation rate at 27 m a.s.l. is set equal to the precipitation rate at Svalbard Airport and above an altitude of 971 m a.s.l. the precipitation rate is assumed to be constant. The altitude of 971 m a.s.l. is chosen such that the parameterized mean maximum precipitation rate

is equal to observed mean maximum precipitation of 540 mm per year found by Pälli et al. (2002) on Nordenskiöldbreen for the period 1963-1999.

RC: Page 217, Lines 3-6: It is not clear how from the two RACMO grid cells interpolation and extrapolation is performed. Also, to which grid is it inter- and extra-polated?

AC: We reformulated the sentence to: "Every model time-step, altitudinal gradients of air temperature, specific humidity and potential temperature between the two RACMO grid points are computed and used to linearly inter- and extrapolate these variables onto the computational grid (40 m resolution)."

RC: Page 217, Lines 6-7: Why isn't sea level pressure taken directly from RACMO?

AC: Air pressure from RACMO at the two grid points has been used to compute the potential temperature at these points and the potential temperature is then linearly interpolated onto the grid. From the potential temperature field the air pressure field can then directly be computed for every grid cell. A sentence has been added here to clarify this.

RC: Page 217, Lines 24-25: It would be good to mention here where the data for cloud cover and precipitation) is taken from. Otherwise the reader needs to go back and forth in order to see which variables are taken from where.

AC: Agreed. The sentence has been reformulated.

RC: Page 231, Lines 25-26: Can these two glaciers be indicated (as dots) on the map in Figure 1?

AC: Done

RC: Page 234, Lines 8-9: Did the authors try to use a different way do convert height to mass changes, other than using the mean snow density from the snow pits? For example, did they try integrating the measured density profile? How sensitive is the conversion to different methods?

AC: The mean density we have used is the average density of the snow pit profiles that were available. For every snow pit, the mean density was computed by integration, hence accounting for variations in the vertical distance between consecutive measurements. Since the amount of snow density (and temperature) profiles was relatively small (compared to the amount of stake readings), we decided to use the mean snow density of all the snow pits, rather than using individual snow profiles to compute individual mass balance values from stake readings. When looking at the snow density profiles no clear dependence of the mean snow density on location (or altitude) could be identified. Mean values typically range from 320 to 400 kg m^{-3}. Due to the absence of a clear spatial pattern in mean snow density and the limited amount of snow pits, it seems like a reasonable assumption to take the mean snow density to compute the mass balance from stake readings. Other approaches have not been attempted. Note that for example an approach where snow pits and corresponding stake measurements are treated individually would have a very small effect for the stake measurements at lower altitudes, where the mass balance is dominated by ice melt in summer rather than variations in snow mass between two winter seasons. That approach would also not affect the mean observed mass balance averaged over all stake locations, since the overall mean snow density would be identical in both approaches.

RC: Page 236, Line 13: Which GCM is used? There are at least 22 GCMs with A1B scenarios produced for IPCC AR4.

AC: An ensemble of GCMs has been used for the statistical downscaling. According to Førland et al. (2010) the downscaling analysis included Multi-Model Dataset ensembles based on 50 integrations for temperature and 43 for precipitation. The sentence has been reformulated to stress that it involves an ensemble of GCM estimates.

RC: Page 238, Lines 5-14: Did the authors try to use transient run of temperature and precipitation, instead of just the delta change between 1980-2010 and 2070-2100? It would be interesting to validate this approach (with sensitivities) against the modeled mass balance, i.e. produce the transient runs for the period 1980-2010 (taking temper-

ature and precipitation from RACMO and then from GCM) and then compare it to the modeled time series.

AC: This comment is no longer relevant as this section is no longer included in the paper. We do agree with the reviewer that this would have been an interesting test.

RC: Figure 3. In the legend of the bottom plot the dot for ‘observed’ is barely visible.

AC: This has been fixed.

RC: Figure 4. It would be useful to have the same color distribution in plots a and c, i.e. it would ease the visual inspection of differences (as it is now the blue color is at the ablation zone in figure a and the red one in figure c).

AC: Corrected.

RC: Figure 9. In caption, it should be ‘Time-series of the ...’ not the ‘Runoff time-series of the ...’

AC: Fixed.

Interactive comment on The Cryosphere Discuss., 6, 211, 2012.