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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.

Anonymous Referee #2

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General comments

The paper presents a detailed study of surface mass balance of a glacier in Svalbard. The authors used previously developed surface energy balance model and subsurface model, which are here coupled and forced with climate data from a regional climate model and a weather station in the glacier vicinity. The model is shown to successfully reproduce stake measurements of mass balance as well as several climate variables from automated weather station on the glacier. Authors also investigate parameter sensitivity, non-linearity in mass balance sensitivity to temperature and precipitation changes, and expand the annual mass balance series into the past and future.

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Overall, the flow of ideas, paper organization and the use of language are proficient and clear. Nevertheless, if one divides the paper in two parts, where the first one is the mass balance modeling (calibration, validation and sensitivity assessment) while the second is extension of the mass balance series in the past and the future, one notices the discrepancies in quality between the two. 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.

Specific comments:

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

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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?

Page 215, Line 24: The symbol '+-' is wrongly used throughout the text for the meaning of 'roughly'. Please correct this to the symbol '~'.

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. . .?)

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?

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

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.

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

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?

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

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

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mass balance, i.e. produce the transient runs for the period 1980-2010 (taking temperature and precipitation from RACMO and then from GCM) and then compare it to the modeled time series.

Figure 3. In the legend of the bottom plot the dot for 'observed' is barely visible.

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).

Figure 9. In caption, it should be 'Time-series of the ...' not the 'Runoff time-series of the ...'

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

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