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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 are grateful to the reviewer for providing detailed and constructive comments, which helped to improve the paper. All issues raised by the reviewer are addressed below.

General comments

RC: The sensitivities of mass balance, refreezing and ELA to air temperature and precipitation (Fig 15) are then used to project these quantities through the 21st century assuming a linear change in climate between 2010 and 2085 for the IPCC A1B emission scenario (Fig 16). I accept that this is a useful thing to do, although this methodology is gradually being superseded by one involving the use of downscaled time depen-

dent output from GCMs of future climate to drive glacier mass balance models into the future.

AC: We agree that using downscaled GCM output to drive the mass balance model would have been a good alternative to extend the mass balance time-series. After consulting the other two reviews, it has however been decided to leave out the section in which the mass balance time-series for 1912-2085 are presented, mainly because of the fact that without coupling of the mass balance model to an ice flow model, it is impossible to estimate the uncertainty in the presented mass balance time-series. In particular changes in the surface geometry over time may have a substantial impact on the mass balance over longer periods of time. We basically had 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. In order to still achieve these two goals, we extended the climate sensitivity section by doing the following: 1) a discussion of the SSC and trends in seasonal temperatures and precipitation during the 20th century has been included 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 has been added, and 3) the role of refreezing in a changing climate is now discussed in this section. Note that with these additions we avoid to present uncertain time-series and are still able to achieve the earlier mentioned goals. Our main conclusions are hence hardly affected.

RC: The paper is long (with 16 Figures) and I would encourage the authors to consider removing one or two sections (and Figures). The last paragraph of Section 6.3 (and Fig 14) is an obvious candidate.

AC: As mentioned before we decided to leave out Section 7. The last paragraph of section 6.3 is now more relevant since the sensitivities presented in Fig. 14 in combination with trends in historical seasonal temperatures and precipitation are now used to put the mass balance over the period 1990-2010 into a historical perspective.

RC: The Conclusions and Discussion could be shortened – the first 5 long paragraphs are really a summary of the whole paper and largely repeat a lot of what has been said. Could this be abbreviated to bullet points? The last two paragraphs only are what I would call “discussion” and usefully discus the key finding in the context of other work.

AC: We believe it is important to summarize the key points in the conclusions, especially because the paper is indeed long. To shorten the conclusions, we have removed several sentences and tried to include only the most relevant conclusions.

RC: I would like to see slightly more thought given to the role of calving on the mass balance of this glacier. It is rather hastily dismissed as unimportant on the basis that the glacier largely became land terminating part way through the 1989-2010 period.

AC: Calving may certainly have been a substantial component in the total mass budget of this glacier during the simulation period. Without thickness and frontal velocity estimates during the active calving phase it is however very hard to get an idea of how large this mass flux may have been. Due to this uncertainty, we therefore do not claim that the mass balance we present is the total mass budget of the glacier. It is regarded as the surface mass balance. This is now stated more explicitly in section 5.1 by including the following sentences: "In order to compute the actual mass budget of the glacier, the negative contribution of calving should be considered in the mass budget as well. Although the glacier snout has partly retreated on land over the course of the simulation period, the mass loss by calving may still have been significant. Estimating the mass loss by calving is hindered by the absence of frontal velocity and ice thickness estimates during the active calving phase. Hence, in this study we will only discuss the surface mass balance of the glacier."

Minor points

RC: Throughout. You could say “melt season” instead of “melting season”. Melt season is much more widely used.

AC: Corrected.

RC: P213. L3. Arnold et al, 1996 would be another good reference here (one of the first of its type).

AC: The reference has been included.

RC: P213. L6. Could do better than referencing Paterson’s text book here.

AC: Indeed. The reference to Paterson has been replaced by Jania et al. (1996): The thermal structure of Hansbreen, a tidewater glacier in southern Spitsbergen.

RC: P213. L20-21. Section on basal dynamics seems a little contrived / unnecessary here.

AC: We believe this sentence is useful, since being able to simulate the timing and rate of water transport to the base, which is mainly controlled by water storage in the snow model, is of great relevance for ice dynamical modelling in which the water input rate controls to a large extent the slipperiness of the bed and hence the amount of basal sliding. Note that output of the coupled model in this study may very well serve as a surface boundary forcing for an ice dynamical model.

RC: P214. L25. Refer to Fig 1 after . . . ..Lomonosovfonna

AC: Done.

RC: P215. L6-10. What resolution is the DEM?

AC: 40 m. This has now been included in this section as well (40 m).

RC: P215. L17 20. delete “above sea level” (everyone reading the paper should know what a.s.l. means.

AC: Done.

RC: P216. L19-24. The sentences “The mean observed. . .GPR measurements” need to be rewritten to make things clearer. It is not immediately obvious what has been

done here. Also, is 191mm an annual precipitation rate? What is meant by “mean maximum precipitation?” The word “yearly” should be deleted as the pptn is already expressed as a rate per year. Quote the mean accumulation rate found by Palli et al. (2002) rather than just saying it was “similar” to 540mm a^{-1}.

AC: The sentences have been reformulated as follows: "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: P217. L16. Better to say “data logger problems” rather than “short circuiting in the system”.

AC: Corrected

RC: P217. L27-8. Say “. . . .measurements have been made at. . . . . .readings are done. . . .”

AC: Corrected

RC: P218. L1. Say “Snow pits were dug. . . .”

AC: Corrected

RC: P218. L3-4. Consider moving the sentence “Furthermore, ...height.” to before the previous one. i.e. explain generally what is done each year since 2006, then describe extra data not done every year.

AC: Done

RC: P218. L22-24. Definition of specific mass balance is not quite right. Is this at a point? Over a grid cell? Or averaged over the whole glacier? Either way, units should be m w.e. a^{-1}, not m w.e. m^{-2} as stated.

AC: The sentence now reads: "The specific mass balance of a certain grid cell on the glacier is defined as the accumulated exchange of mass per unit area over a period of time."

RC: P218. L25-26. Say “Ice melt only influences the mass budget if the meltwater runs. . . ”
P223. L20. I think it should be “. . . are assigned. . . ”
P225. L5. “Finding”
P225. L10. “ . . . position (Davies and McKay, 1989). . . ”
P225. L25. The incoming long wave radiation flux depends. . . ”
P226. L7. say “data logger problems” rather than “short circuit problems”
P228. L17. “. . . altitude of -12oC. . . ”

AC: All corrected.

RC: P229. L14. “. . . ELA is 600m. . . ”

AC: A more accurate calculation of the ELA from observations (by linearly interpolating between nearest stake observations) led to an estimate of 591 m a.s.l.. The plus-minus symbol has been removed.

RC: P229. L16. Why not state how the calculated mass balance compares to the measured balance for the years for which there are measurements? Or if this is to be done later in the paper, state the section here.

AC: The following sentence has been added here: "In section 5.3 simulated specific mass balance at the stake sites are validated against observations."

RC: P229. L20-21. You say that the glacier “has almost fully retreated on land over the course of the simulation period” But where was it at the start of the period? Presumably it had an active calving front? Just because the glacier almost became land terminating at some point over the last 20 years does not necessarily imply calving is negligible. A better discussion of the likely calving contribution is needed here, perhaps

by comparison with other Svalbard glaciers where this has been calculated?

AC: A few sentences have been added to indicate that calving is an uncertain component in the mass budget, especially prior to the partial retreat of the glacier on land. We therefore stress that this study presents surface mass balance values rather than the total mass budget. Comparing the calving loss to other Svalbard glaciers does not seem to lead to a reliable estimate of the calving rate as calving for Svalbard glaciers is known to vary significantly from one glacier to another as indicated for example by Hagen et al. (2003): 'Glaciers in Svalbard: mass balance, runoff and freshwater flux'.

RC: P230. L6-7. Comma after "glacier". Delete "per unit area" and "annual". Add "a^{-1}" to discharge units.
P230. L9. Say "elevation profiles" rather than "height profiles". Could delete "defining".
P230. L16. Delete "approximately"
P230. L18-20. Say ". . . The sum of all the incoming and outgoing fluxes (Fig 6a) decreases with. . . "
P230 L21. Delete "at the surface"

AC: All corrected.

RC: P230. L29 / p231. L1. The sentence that straddles these pages doesn't quite make sense to me. Suggest rewrite.

AC: The sentence now reads: "Due to a high surface albedo in the accumulation zone less shortwave radiation is absorbed and surface temperatures are relatively low. This effect is partly compensated by an enhanced subsurface heat flux towards the surface."

RC: P231. L4/5. Delete sentence "As mentioned. . . .precipitation"
P231. L7. Say ". . . variations in both summer melt and winter accumulation"

AC: Both corrected.

RC: P231. L10-11. delete "in annually averaged temperatures". Also, what is meant by "not clear" with respect to the summer temperatures? It would be better to test to

see if the slope of the regression line through the annual and the summer data are significantly different to zero or not. Then you could state that the annual trend is significant and the summer trend is not (assuming this is actually what you find).

AC: We agree and performed a significance test of the trends. The following sentences have been included in the manuscript: "Trends in the annual and summer mean temperatures in Fig. 7(d) are tested for significance by taking the ratio of regression slope and the standard error of the slope to find t-scores of 2.21 and 0.61 respectively. Consequently, only the trend in annual mean air temperatures since 1989 is significant at a 95% confidence level."

RC: P231. L19. Change "significant" to "important" to avoid confusion with notions of statistical significance.

AC: Corrected.

RC: P231. L14-20. Wouldn't it also make sense to quote the correlation between annual mass balance and winter precipitation here too?

AC: The sentence "Winter snow fall is significant, owing to both its direct effect on the surface mass budget and the impact on the length of the melt season" has been rephrased to avoid confusion. It now reads: "In contrast to winter temperatures winter snow fall is important for the annual mass budget, owing to both its direct effect on the surface mass budget and the impact on the length of the melt season.". We do not want to say that snow fall is more significant during the winter season than other seasons. This also becomes clear in the climate sensitivity section.

RC: P231. L22. Say ". . . variability results mainly from. . . "
P231. L22-25. Should the word "affect" read "affects"? If so, with this change, the sentence is clear. Otherwise the sentence should be reworded to explain precisely what annual snowfall and air temperature control.

AC: Corrected. "affect" has been replaced by "affects".

RC: P231. L29 / P232 L1-2. This is confusing. Too many things are being discussed at once. Consider deleting or rewording.

AC: This has been reformulated as: "The simulated mean mass balance for Nordenskiöldbreen is 0.30 m w.e. $a^{?1}$ lower than observed on Kongsvegen and 0.05 m.w.e. $a^{?1}$ lower than observed on Hansbreen. Note that both the mass balance time-series of Hansbreen and Kongsvegen are also negative on average since 1990."

RC: P231. L13. ". . . at the melting. . . "
P231. L22. ". . . 2010 is shown. . . "
P231. L23-24. ". . . amount of runoff; when the snow pack is all gone, all available. . . immediately, inducing a clear. . . "

AC: All corrected.

RC: P232. L26-27. Delete reference to Morteratschgletscher here. What effect does the snowfall event have on the mass budget of Nordenskiöldbreen? This is surely more relevant here!

AC: The reference has been removed. The impact of a summer snowfall event on Nordenskiöldbreen depends very much on the severity of the event. We believe there is not much value in presenting absolute values here and stick with the qualitative description of the processes involved.

RC: P233. L 3. say "flow line" not "cross section" wrt Fig 1.
P233. L10. Say "measure of" or "proxy for"
P233. L22. Delete "extrema in"
P233. L24. say "these fluxes" not "the latter fluxes"
P233 L26-27. Say "Discrepancies between observed and simulated SWin result mainly from. . . , which also accounts for discrepancies between observed and simulated LWin"

AC: All corrected.

RC: P234. L11. "you say the situation at S1 and S2 "could be" related to windblown snow. But there could be other explanations? If so these should be stated and dismissed if there is evidence).

AC: Yes, there could indeed be other factors. The following sentences have been added: "Berthier et al. (2010) show that redistribution of snow by wind may lead to accumulation of snow in concave areas near the margins. Other factors that can explain the discrepancy in observed and simulated mass balance at the two sites are small scale shading and albedo effects. "

RC: P234. L14-15. It should be "compare. . . .with" not "compare. . . to"

AC: Corrected.

RC: P234. L15. "For this purpose.." Also, why only seven profiles used in the comparison? Seven out of how many? Why were these 7 chosen?

AC: These seven were selected out of a set of nine snow pit profiles, since these seven contained more than two data points in the vertical, which we used as a criterion. Note that variability in the observed density is typically quite large within a single snow pit, which was the main motivation to discard two of the nine profiles. A sentence has been added to indicate that two profiles were discarded.

RC: P234 L16. "adopted" is the wrong word". Just say "used" ?
P234 L19-20. Delete "In comparison to. . . ..density". Then say "The mean measured snow density of 367 kg m-3 is. . . higher than simulated."
P234 L24. "model's sensitivity"
P235 L7-8. Delete "Especially". Then say "The sensitivity. . . ..is particularly high. . . ."
P235 L21. "For this purpose"

AC: All corrected.

RC: P235. L22. Was the initial water content in the snowpack set to zero for the standard run too? Might be worth reiterating this if it was said earlier. Or explaining

why initial conditions were different here than for the standard run.

AC: The initial water content has only been set to zero for these particular sensitivity tests (not in the standard run). This has been done to avoid immediate refreezing when the subsurface temperature is lowered. A sentence has been added on this.

RC: P235. L24-5. Should this say ". . . by 3K increases the mass balance by 27 mm w.e. a^{-1} after one year and by 60 mm w.e. a^{-1} between 2-6 years after the start of the experiment." ?
P237. L2-4. Say ". . . .change can be offset by a 32%.....perturbations. On the other hand In the case of. . . ."
P237. L8-9. Say "Conversely" rather than "On the other hand"

AC: All corrected.

RC: P237. L11-12. "Reduced increase" This is hard to understand. Similarly "pronounced effect" is vague. I think you need to refer to Table 2 more frequently throughout this paragraph and explain these sensitivities a little more clearly.

AC: "reduced increase" has been rephrased as "relatively small increase", by which we mean that future temperature increase is expected to be relatively small during the summer months. The term "pronounced effect" refers to the large impact of summer air temperatures on the mass balance and the magnitude of this sensitivity is presented in the next paragraph.

RC: P237. L 16-23. This section is already long (the whole paper is very long) and this paragraph could be cut. It would also mean Fig 14 could be cut.

AC: In the old version of the manuscript this paragraph was indeed rather obsolete. In the revised form of the manuscript the discussion of the SSC is more relevant.

RC: P239. L9. delete apostrophes from 60's and 70's and say "1960s and 1970s"
P239. L17-18. Delete "w.r.t. the period 1990-2010" (as this was stated above). Say ". . . mass loss between 1910 and 2085 is 29m w.e. . . . . "

P239. L24. Delete "an increase in"
P239. L25-27. Say ". . . .climate, the effect of warming and enhanced precipitation on refreezing will, to a large extent, counteract each other"
P239. L28. "..in the case.."
P240. L7. Delete "it should be noted"
P240. L9-11. say "We regard the mass balance – dynamics – surface elevation feedback as. . . ..omission from our modelling; the presented. . . are likely, therefore, to be somewhat. . . ."

AC: The corresponding section has been removed.

RC: P241. L8. say "Conversely" not "On the other hand"

AC: Corrected.

RC: P241. L15. delete "disregardance of" and add "not considered by the model" to the end of the sentence.

AC: Corrected.

RC: Fig 5. Suggest "Elevation profiles" rather than "Height profiles"

AC: Corrected.

RC: Fig 7. The order in which a, b, c and d are discussed in the text is different to how they are presented. Consider reordering Figures d to b, b to c and c to d.

AC: The order has been changed in line with the reviewer's suggestion.

RC: Fig 9. Impossible to see difference between two shades of blue on the graph. Also, suggest 1 Apr, 1 May rather than Apr/1, May/1, etc.

AC: Different colors are now used and the dates have been corrected.

RC: Fig 10 caption. " ..along the main flow line marked. . . "

AC: Done.

RC: Fig 11. Add a) b) c) and d) to Figure and refer to these in Figure caption (rather than upper right panel, etc). Add time period over which observations are made to the Fig caption.

AC: Fixed.

RC: Fig 13. Difficult to see difference between reds and between blues (although I accept it is fairly obvious than the more extreme curves are for the more extreme climate variables)

AC: A different color is now used for the negative temperature perturbation lines.

RC: Fig 14. x axis units should be "(m w.e. a^{-1})" I assume.
Fig 15. Caption ". . . .mass balance, refreezing and ELA. . . "

AC: Both corrected.

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