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The Cryosphere
Discussions

Interactive comment on "The influence of changes in glacier extent and surface elevation on modeled mass balance" *by* F. Paul

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General comments

This paper addresses an important issue in the response of glaciers to a climate change and is thereby an contribution of great interest to the cryospheric community. As clearly outlined in the 'Background' section, changes in the surface mass balance due to the dynamical behaviour of a glacier are not always taken into account in mass balance studies. Based on a set of experiments with a mass balance model, this study assesses whether this is justified. Even though the (quantitative) results may only apply to the European Alps, the author illustrates that dynamic changes of a glacier can have a significant effect on the mass balance and therefore cannot be disregarded in long-term mass balance studies. The paper is well-written, concise and has a clear

structure. I recommend this paper for publication in The Cryosphere, provided that the points below are addressed.

At some points, the author should be more careful in his statements, especially when the dynamical behaviour of a glacier is concerned. Of these, two points need particular attention:

1. Steady-state conditions. The author correctly states that steady-state glaciers are a theoretical concept, due to the continuously fluctuating climate. While a glacier in an equilibrium-state would have a stationary front, this does not imply that glaciers showing only small length changes for a number of consecutive years can be called stationary. Only a forcing with a period considerably longer than the response time can lead to a near-steady state; this does not apply to the climatic fluctuations observed in the real world. When periods with a near-stationary front do occur, they most likely indicate either a slowdown of a negative (or positive) trend due to some years with more positive (negative) mass balance or a change from advance to retreat or vice versa. This concept can be compared to a harmonic oscillation: at the point where the amplitude is largest, the movement is small, but this is certainly not an equilibrium-state! The author assumes steady-state conditions for the two model periods because the front was more or less stationary. This assumption is very likely not correct and should be avoided throughout the paper. The author uses the steady-state assumption to tune the precipitation towards zero mass balance for all glaciers. From the model description it is not clear to me whether this is done for all model runs or only when determining the mass balance sensitivity. Except perhaps for the sensitivity experiments, zero mass balance is not required for the comparison between the two periods and tuning of the precipitation is therefore not necessary. I would suggest to keep the tuning to a minimum, as this might affect the results, especially when correction factors are large. One representative set of gridded air temperature and precipitation could be chosen to use in all model runs; modelled mass balance can perhaps be validated with mass balance measurements that are likely available for some of the glaciers in

the 1970s and 1980s?

2. Change of surface elevation and extent. Although the experiments performed allow for a separation of the effects of surface lowering and area change, which is very interesting indeed, the author should be careful in describing them as two independent processes. Changes in glacier length and area are a response to changes in surface height, i.e. glacier thickening or thinning and are therefore dynamically coupled.

Further questions and remarks are specified below.

Specific comments
738_3: What is small?

738_23-25: Although glacier changes are easily linked to climate change, their records are relatively short and sparse compared to other proxies. Stating that glaciers are 'the best' indicators is in my opinion too bold, especially outside the glaciological community many researchers will not agree.

740_17-19: What is exactly meant by an 'ongoing forcing'? In my opinion, this could be anything from a constant change with respect to the initial condition to highly variable conditions. It seems that the author here refers to a linearly increasing temperature?

740_21-22: Did this one degree temperature increase occur as a step change around 1985 or gradually in the period after 1985? This is not clear from the text.

742_15-18: The assumption that the 1980s DEM is also representative for the 1970s is logical, changes in surface elevation were probably not dramatic in this period. These are the best data available and the argument of steady-state conditions is not needed in this respect.

743_7-8: Please indicate which percentage of the data was reset to the DEM25.

743_16-17: If I understand it correctly, all experiments use the same climate, except perhaps for the mass balance sensitivity experiments. Why not use present-day air

temperature in the experiments instead of searching for mean annual values that give near-zero mass balance in 1850?

744_1-3: Is this value of 0.5 based on measurements? If so, please mention this and provide a reference. Does this factor also include attenuation in the cloud-free atmosphere or is this accounted for in SRAD?

744_22: What is the annual range/amplitude of the temperature cycle?

744_23-24: Is the lapse rate based on measurements? How large is the uncertainty and how does this affect the results?

745_1: The calculation of the longwave radiation is not described, which parameterizations were used?

745-746: I read through the 'Experiments' section a couple of times and still had difficulties understanding how the experiments were exactly configured. The author attempts to supply a structure by using letters and numbers, but I would suggest to provide the overview in a slightly different way. Instead of distinguishing between runs A and B and the experiments numbered with letters E, the individual model runs could be named more precisely (and given in a table) and then the difference between two model runs can be referred to. E.g.:
A = 1850 DEM and 1850 extent
B = 1850 DEM and 1973 extent
C = 1980s DEM and 1850 extent
D1 = 1980s DEM and 1973 extent
D2 = 1980s DEM and 1973 extent+split up of glaciers
Then experiment E1 would become A-C (or C-A?), and so forth.

746_10-26: The sensitivity experiments need additional clarification as well. Do I understand it right that in these two runs the corresponding DEM and extent are used? In the terminology described above, configurations A and D1? Then it seems that also different climates are used for the two runs, with a one degree warmer climate for D1. This was probably done to obtain zero mass balance for both geometries. But now it becomes impossible to distinguish between changes in mass balance sensitivity because of the different geometry and changes due to a different initial climate state. By performing two additional runs (geometry A and one degree warmer climate, geometry D1 and reference climate) these effects could be separated. However, considering the uncertainty in the mass balance distribution for both periods, the paper would benefit from a focus on the other experiments (E1-E3), where the actual mass balance distribution is of less importance. To achieve this, the mass balance sensitivity calculations could be omitted altogether or reduced to one geometry and climate.

746_15-18: Are large correction factors needed to obtain zero mass balance? Please quantify this by giving the range of correction factors.

747_4-5: How are these standard deviations computed, from the glacier's total mass balance or from the gridpoint mass balances? The same question applies to the mean values.

748_10-13: It would be very interesting to separate these two opposing effects, which could be done by performing a model run with the 1980s DEM, but using the SRAD output from the 1850 DEM.

748_23-27: Considering the largely overestimated ablation on UAG mentioned before (lines 2-4), the results of the model experiments are probably not realistic for this glacier. Drawing conclusions about the effect of extent changes for UAG can therefore better be avoided.

749_16-20: How can you be sure that the artificial tuning towards a zero mass balance has not affected the mass balance sensitivity? The glaciers that are far from zero mass balance apparently are most sensitive. This could be realistic, but the precipitation correction is also largest for these glaciers and this may be reflected in the mass balance sensitivity. The uncertainty introduced by the precipitation correction could be

quantified by calculating the sensitivities for the situation without tuning, which could for example be added to Fig. 7a. However, as mentioned before, I would prefer to have the mass balance sensitivity discussion reduced altogether.

749_27: I assume that a 150 m upward shift of the equilibrium line corresponds to a one degree increase in temperature?

750_24-751_2: This estimate is based on many assumptions and does not add any new information obtained from the results in this paper, I would suggest to omit it. Would it be possible to say something about the effect of ongoing and future changes in glacier extent on the (measured) mass balance, based on the results from this paper?

751_16-24: Does this imply that the results of this paper are mainly valid for rather healthy glaciers?

751_25-28: More negative than a constant extent and elevation, I assume? Do mass balances always become more negative? Is this not dependent on the change in geometry?

753_7-9: It is still not clear to me whether precipitation has been adjusted for all runs or only for part of the experiments. Reading this sentence, it seems that precipitation was always adjusted, but in other sections the author refers to 'untuned' model runs. This should be more clearly stated in Section 4, preferably in a table. Furthermore, the terms should be used consistently throughout the paper.

Figs. 4 and 5: By his choice of the colouring in the figures, the author aims to stress the spatial variability in the results. However, the main conclusions are drawn on the differences between the various experiments, but with the chosen colour scales, comparison between figures is difficult. I would suggest not to derive the colour scaling from the mean and standard deviation per experiment, but to use the same colouring for the four experiments. The mean value and the standard deviation can be presented in a table. This would ease the comparison of the four maps, while the spatial variability would still be visible. Moreover, when the colour interval is the same for all maps, the spatial variability can be compared between maps.

Technical corrections

738_16: perhaps ‘Moreover’ is more appropriate here than ‘Thereby’

738_20: change to ‘long-term mass balance’

738_20: ‘also’ can be omitted

739_7: ‘much’ can be omitted

740_16: ‘again’ is only valid when the initial state was an equilibrium-state

741_21: introduce ‘DEM’

742_5: remove ‘still’, as all other glaciers also get smaller, the Aletschglacier will undoubtedly remain the largest.

742_5: replace ‘one’ by ‘glacier’, now it refers to the region instead of the glacier.

742_9: ‘statistical’ can be omitted

744_6-7: this line can be moved to the end of the paragraph.

744_9: ‘is’ should be ‘are’

750_9: -0.65 m w.e. is mentioned in the previous section.

750_18: change to ‘near zero mass balances’

750_22: ‘again’ can be omitted, as it assumes a change from steady-state conditions

751_16: ‘the’ instead of ‘he’

753_12: ‘glacier’s response’

754_15: ‘Switzerland’ with a capital S

754_19: ‘1760-1998’

757_14: ‘Giesen’ with one s

757_15: ‘Morteratsch’

Figs. 2-5: The font size in the legends is very small and hard to read, please use a larger font.

Fig. 6: In the legend, the mass balance sensitivities are positive instead of negative.

Fig. 7: The font size of the labels and axes titles is rather small, I would suggest using a larger font. In panel b, the axes labels are positive instead of negative. Furthermore, I would advise to use the same range for both axes (0.2-0.9).

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