

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

**Anonymous Referee #2**

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This paper analyzes the impact of the surface geometry used in glacier modelling studies on the obtained surface mass balance. For a study site with several dozens of glaciers in the Swiss Alps mass balances over the geometries of 1850 and 1985 are calculated using a model, and sensitivities are analyzed. The author reaches the conclusion that a large percentage of the glacier reaction to climate change is 'hidden' in the geometric adjustment.

The question which is addressed in this paper is important and worth investigating. Whereas I strongly agree with the statement that 'the climatic interpretation of mass balance data is rather complex', I have several, partly substantive, objections against the methodologies used. In particular, I do not agree with the main conclusions that are drawn from the experiments.

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I have two major, substantive points, several general comments on assumptions and methods applied in this paper, and a number of specific comments.

1. The main conclusion of the paper is that 50-70% of the glacier reaction to climate change are 'hidden' in the geometric adjustment. This number is presented as a general statement, which seems to be valid for *all* glacier mass balance studies dealing with long-term reconstructions. In my opinion this is highly problematic as it transmits the message to glaciologists measuring mass balance in the field: 'Whatever you measure, you will only capture a small part of the climatic signal'. Therefore, I ask the author to carefully put into context his conclusions: The number 50-70% is not valid *in general!* Based on the analysis presented in this paper only inferences on the period 1850-1985 in the Alps with the use of corresponding DEMs for mass balance modelling can be drawn.  
Mass balance studies, for example in the scope of long-term monitoring programs, are often based on aerial photographs that are available only over the last few decades. In the context of these studies, e.g. between the 1960s and present, the number of 50-70% that is 'hidden' in the geometric adjustment would be misleading. The effect necessarily converges to zero the smaller the changes (or the time period) between the DEMs are. Thus, I expect the 'hidden' percentage to be much (!) smaller in the case of e.g. a reconstruction since the 1960s. The author should be very clear about the fact that this study (and the presented numbers) is *specific* to the DEMs 1850-1973/85 and the study site. Mass balance reconstructions over other periods, using other DEMs will show significantly different percentages of glacier reaction that is 'hidden' in the geometric adjustment.
2. Figure 5b caused me to put a large question mark behind the main conclusions presented in this paper: Whereas the average change according to experiment 3a is +0.36 m w.e. (this number is discussed throughout the paper), the average

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change for experiment 3b is only +0.03 m w.e. Moreover, according to experiment 3b the large majority of the glaciers is even below the arithmetic average. They show a negative sign of the mass balance change when using the smaller (1973) instead of the larger (1850) glacier extent. The average slightly above zero is obviously due to a few glaciers with extremely positive changes (up to +2.9 m w.e.). Thus, Figure 5b, which only differs from Fig 5a / experiment 3a by the consideration of glacier entities, presents results that lead to completely reversed conclusions! If I believe the results of experiment 3b instead of 3a the conclusion would be that glacier wastage is even accelerated by the geometric adjustment. I am not saying that this interpretation is correct, but it indicates that this kind of model results can be highly ambiguous!

Some examples: GAG experiences a mass balance change of about +0.3 m w.e. in experiment 3a. In experiment 3b, however, this value drops to -0.1 m w.e. (which is hardly recognizable – even misleading – in the figures because the colour scales are not the same; it should be made consistent!). The only cause for this change in sign is that one minor tributary has split off. It seems that this tributary was virtually unconnected with the main glacier in 1850. Similar shifts occur for all larger glaciers in the study area. The important difference can therefore only be due to severe problems in the modelling procedure. To me, this is an indication that model applied here might not be suitable to answer the questions posed. As pointed out in the review of R. Giesen, it is not clear which results are based on a tuned, and which on an untuned run, which might explain some of the large differences between the glaciers in experiment 3b.

The author only discusses the results of experiment 3a; why are the results of experiment 3b with contradictory results not addressed in the paper? If there is a reason for this, it should be provided. I personally consider experiment 3b to be better suited to solve the questions posed in this paper because it is closer to 'reality' and reveals potentially erroneous mass balance distribution.

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#### General comments on assumptions and methods:

- I miss the link to reality in the modelling study. Normally, every model needs to be validated using some kind of field data before drawing any conclusions from the results. Validation is not present in this study! Model results cannot just be justified by comparing them to parameterizations developed for mountain range scale estimates. This missing validation is surprising, as for several glaciers within the study region a wide range of high-quality field data, partly covering almost the entire period of interest would be available. As long as the model can not (more or less) correctly reproduce reality (and you know that it does!) it cannot be applied to calculate sensitivity to climate and geometry change. As R. Giesen in her review, I therefore also ask the author to present a validation of the model results before interpreting them.
- The steady-state assumption is discussed in detail in the review by R. Giesen. I completely agree with the points that are well presented in this review, and I will not repeat them. This issue should absolutely be addressed by the author in a revised version of the manuscript.
- The tuning to a zero mass balance using precipitation as a tuning variable bears problems for several reasons (see also review of R. Giesen). In my opinion, the assumption that all glaciers have a zero mass balance in 1850 and 1973 (which seems to be quite central to the paper) cannot be justified. It is known from different studies (e.g. Paul and Haeberli, 2008) that glaciers show largely differing rates of mass loss for a similar climatic forcing. Why should they all have exactly the same mass balance in one certain year that is interpreted as a steady-state? This assumption seems to be in contradiction to observational evidence the author has published himself. The glaciers have strongly differing response times of up to one century (e.g. GAG). Whereas some small glaciers

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might be close, most larger glaciers are probably quite far from a steady-state, thus favouring *different* mass balances.

- The main point of the paper is that the interpretation of glacier mass balance series calculated over changing glacier geometry bears some problems (I agree with that!). The background given in the first chapter is long and quite comprehensive, and illustrated with many references. I do, however, not understand, why the author never mentions the concept of analyzing point mass balances instead of glacier-wide area-averaged quantities. The analysis of long-term mass balance series that refer to one measurement point on the glacier surface is a long established concept that is well recognized in the glaciological community. For the interpretation of climatic trends and variations this concept is almost unaffected by the problems with glacier-wide mass balance series, which is discussed here. I know that point mass balances are not the topic of the paper, but I think that this concept merits to be shortly discussed as well, because it provides relatively simple solutions to most of the biases highlighted in this study.
- The statistical significance of the results needs to be tested. I suggest that error bars should be given for all numbers. Based on statistical tests it should be checked whether the resulting effects are different from zero. Only considering mean values can be problematic as they might strongly be biased by outliers or unrealistic results from individual glaciers. Analyzing area-weighted quantities instead of arithmetic means, or calculating the effect for individual glaciers separately might also be a solution.
- The meaning of the term 'hidden' which is often used to illustrate the conclusions needs additional attention. What does it mean? How should it be interpreted? Let me consider the most negative mass balance year in the Alps, 2003, with measured balances of almost  $-3$  m w.e. If 50-70% are 'hidden' in the geometric adjustment does this mean that the mass balance in 2003 would have been  $-6$

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to  $-8$  m w.e. if the 1850 DEM would have been used to calculate surface mass balance with the same climate? I doubt that the author wants to imply this result is realistic, but it is a possible conclusion based on the percent-number given in the paper. And what about positive mass balance years?

My point here is that it is crucially important to tell the reader what this percentage of 'hidden glacier reaction' means. Maybe providing a percentage is the wrong type of measure for quantifying the effect.

Specific comments are provided below:

- p739, l5: This statement represents the common understanding in the scientific community (many more, also much older citation could be provided here). However, in my understanding it is in direct contradiction with the main conclusions of this paper. If 50-70% of the glacier reaction to climate change (see Abstract, page 738, l13) is 'hidden' in the geometric adjustment, then mass balance can barely be interpreted as the 'direct and undelayed reaction' to climate change. This contradiction needs to be solved in some way.
- p740, l29: Can the authors provide a short definition of 'down-wasting'. How can this process be easily distinguished from 'active retreat'. Is there really an observational proof that *all* Alpine glaciers are now affected by 'down-wasting'? Is there evidence that they are increasingly affected throughout the last century?
- p741, l9: Why is the interpretation 'less clear than in previous periods'? Also previous periods were characterized by mass balances below (or also above) the long-term average for more than just one year leading to length and area changes that are not completely incomparable to today. In order to be able to make this statement geometrical changes in 10-20 year periods throughout the

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entire century would need to be considered. To make this statements it is not sufficient to compare observations from the last (extreme) decades to averaged changes over the last century.

- p742, I10: It should be clearly stated which topographic maps were used here. To which year do these maps refer to? Exactly to the year 1850? Furthermore, it is not clear whether 100 m contour lines that were directly digitized from the maps, or reconstructed isolines were used for establishing the DEMs.  
My main point is that some estimates of map accuracy should be provided. This would allow a quantification of uncertainty in the final results arising from DEM uncertainty (which is probably considerable for the 1850 map).  
The accuracy of the 1850 maps could, for example, be assessed by drawing a comparison to the up-to-date DEM in non-glacierized areas.
- p743, I7: Where exactly was the 1850 DEM reset to the 1985 DEM? 'Towards the accumulation area ...' is not precise enough. The author should either state an elevation boundary or a percentage of the glacier surface for which this correction was applied. Is there any estimate what additional uncertainty this correction causes in the modelling results?  
It is probable that surface elevation in large parts of the accumulation area has significantly decreased throughout the last century. The impact on the overall mass balance can not just be neglected without verifying its importance.
- p743, I15: Some explanation might be of benefit here why the study does not rely on 'real' daily meteorological data, but on a rough approximation of the annual cycle based on a cosine function. I assume that the sensitivities could be quite a bit different when using a realistic weather variability throughout the year.
- p743, I18: The paper does not state anywhere which meteorological data were used. Do the data originate from measurements at a weather station?

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- p744, I24: Is this temperature gradient representative for the study region, i.e. does this value originate from comparison of different weather stations?
- p745, I1: Can the author provide a reference that confirms that turbulent heat fluxes are independent from wind speed? To my knowledge turbulent heat fluxes depend strongly on wind speed.
- p745, I9: Also the values of the other model parameters should be provided here.
- p745, I14: Do 'net balances' refer to area-averaged annual balances here? Provide a short definition.
- p746, I5: Here and elsewhere: Why 'us'? The paper is written by only one author.
- p747, I2-10: This paragraph sounds rather like a Figure caption and should be shortened.
- p747, I18 - 748, I4: The obtained mass balances in the study region for the same climatic forcing vary by 3.6 m w.e.! This is higher than the difference between the most negative and the most positive mass balance year in the Alps since the beginning of the measurements. Thus, this extreme variability is barely realistic, and physical interpretation of the data, as performed in this paragraph seems to be impossible to me.  
The interpretation of very positive mass balance at the Northern Alpine rim should be revised: Are there such mass gains in reality, or in the model? If it was in reality, then these glaciers would now be advancing quickly; if it was in the model precipitation is much too high there, or calculated global radiation to low.
- p748, I15: This statement should be verified using a statistical test! The difference between positive and negative effects is quite large. Is the mean really significantly different from zero? If the difference is not statistically significant the author should revise the related statements.

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- p748, l22: Here and elsewhere: It is not clearly stated how the mean 'for all glaciers' is calculated. I assume it is the arithmetical mean. It would be interesting to also provide the area-weighted mean, which is less affected by the large number of small glaciers with potentially much higher uncertainties in the modelling.
- p748, l29: 'special topographic conditions': Provide an example.
- p749, l20: It all also correlates well with the prescribed precipitation distribution pattern (Fig 2a). Glaciers with initially little precipitation (towards the border of the study area) need higher correction factors, and thus more precipitation, leading to a higher sensitivity. Is this interpretation consistent with the explanations provided in the paper?
- p750, l9: Are these two numbers really comparable? The estimate for the long-term mean mass balance refers to the hydrological mass balance, the calculated sensitivity is based on an unchanged geometry. In my opinion, this comparison could only be performed if the long-term mean mass balances would be based on constant glacier geometries.  
Moreover, the estimates referenced here cover the entire Alps, and not only to the study region. Also for this reason comparison is not possible.
- p751, l1: This conclusion should be formulated more clearly, or the estimate be completely removed. It is difficult to understand.
- p752, l7: According Nemeč et al. (2009) no hydrological balances were calculated. The result originates from 'a gradual linear interpolation' of the calculated mass balance for two different geometries. Hydrological mass balances obtained over dynamically changing ice extent and surface topography might be different from this value.

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- p753, l7: The author should be more careful with interpreting the model results. I assume the model is not accurate enough to allow inferring on effects of snow redistribution etc. Only if all other processes (melting, albedo variation, initial precipitation amounts) are absolutely 'correct' the importance of snow redistribution processes can be interpreted.

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Interactive comment on The Cryosphere Discuss., 4, 737, 2010.

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