

Interactive comment on “Multi-decadal mass balance series of three Kyrgyz glaciers inferred from transient snowline observations” by Martina Barandun et al.

Anonymous Referee #1

Received and published: 22 December 2017

Overall comment:

This paper by Barandun and colleagues presents an interesting approach to model the glacier surface mass balance using a degree-day approach for ablation, precipitations for accumulation and remotely-sensed snowline observations to constrain the model.

This approach is applied on three glaciers located in Kyrgyzstan where glacier surface mass balance data from the glaciological and geodetic methods are available to validate the modeling approach.

The approach is also used to reconstruct glacier surface mass balance time series over a longer time period than available in situ glaciological data.

C1

The paper reads well. However, I have some comments, and I consider that once integrated by the authors, the paper will be ready to be published in The Cryosphere.

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Main comments:

(1) Title of the paper

The title of the paper is misleading and needs to be changed. Indeed, in your approach the surface mass balance is modeled with meteorological data and this modeling is constrained by snowline observations during the ablation season. Thus, the title has to mention that the surface mass balance time series are quantified with a model. As it stands, one can have the impression that the surface mass balance is only inferred from snowline observations.

I suggest:

"Glacier surface mass balance modeling constrained by remote sensing derived snowlines. Application on three kyrgyz glaciers to quantify multi-decadal series".

or

"The use of remote sensing derived snowlines to constrain glacier surface mass balance modeling. Application on ..."

(2) Name of the proposed approach.

In the same way, the name you give to the proposed approach (snowline-derived mass balance) is not adequate, and so is the abbreviate in the tables and figures (Msnl).

For the name of the approach, I propose: Modeled SMB constrained by snowline.

And for the abbreviate: Mmod-tsl. TSL stands for Transient Snow Line and must be preferred to SNL.

(3) Terminology. "Mass balance", "glacier balance", etc. . .

C2

Be careful with the terminology and follow Cogley et al. Glossary of mass balance terminology. For example, use “surface mass balance” instead of “mass balance” or “glacier balance”, except when you refer to the geodetic mass balance.

(4) Glaciological surface mass balance.

The used method is not the classical glaciological one. Indeed, a model-based extrapolation is used and optimized to best fit the point measurements. What about the input data for accumulation and ablation (e.g. precipitation and temperature data)? Where do they come from? Are they the same as used in the model constrained by the snowline?

If the data are the same, are the methods "constrained by the snowline observations" and “constrained by the point measurements” strictly independent as they rely on the same meteorological data?

(5) High resolution images recorded in November => impact of the snow cover on the derived DEM.

Two of the stereo-pairs used to make DEMs and quantify the geodetic mass balance were recorded in November for Abramov and Golubin glaciers, respectively.

I had a look at the SPOT catalogue and for Golubin I could find the SPOT7 images from 01/11/2014. Regarding Abramov, the images are not in the catalogue but I assume it is because these have been acquired within the SPIRIT acquisition campaign.

Anyway, at least for 2014 and I assume it is the same for 2011, all the more because the SPOT5 images date from late November, the glaciers and surrounding terrains are completely snow covered.

This implies several challenges for DEM generation:

- low contrast because of snow brightness
- unknown snow thickness

C3

- impossibility to delineate the glacier outline.

How these issues have been tackled and what is their impact on the uncertainties?

In addition, Table 7 shows that the geodetic mass balances for Abramov (2011-2015) and for Golubin (2006-2014) are less negative than the average annual surface mass balances quantified by your model constrained by the snowline. Can the snow-cover on the images implying a higher surface elevation (of an unknown value!) be a cause of this difference (or at least part of it)?

(6) Surface mass balance interannual variability.

It is pity that your discussion about the interannual variability of the SMB is short and only dedicated on the annual values.

You should have a look on the two terms of the annual surface mass balance, and discuss about their interannual variability. You could see if the interannual variabilities of accumulation and ablation are homogeneous, comparable between the three sites and if the difference you mention regarding the interannual variability of Glacier No. 354 annual SMB is more likely related to a different ablation or accumulation interannual variability.

(7) Application of the proposed approach.

You mention that the approach you propose can be useful to quantify surface mass balance time series for a number of remote glaciers. Although I mostly agree with this statement, I wonder if the approach can indeed be applicable for summer accumulation glaciers (like in the tropics, or in monsoon regime regions) or for high latitudes glaciers where superimposed ice can be more important than in Kyrgyzstan.

You can probably add some sentences on this point in the conclusion.

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Specific comments (PX, LX = Page X, Line X)

C4

Abstract

- P1, L5-6: the sentence "A combination of 3 independent [...] Golubin and No. 354" needs to be reformulated. Indeed, the methods are not combined to reconstruct the surface mass balance. The methods are compared/cross-validated but not combined. For me, you would have a combination if, for example, your modeled surface mass balance time series had been adjusted with the geodetic method.

- P1, L8-9: "satellite optical imagery" instead of "satellite imagery".

- P1, L12, 13 and in the entire paper: should write "yr-1", instead of "a-1". "yr-1" is most common except for IGS journals.

- P1, L15: prefer "unmonitored" to "inaccessible". I do not know about an inaccessible glacier on Earth.

1. Introduction

- P2, L2: should the IPCC reference be quoted Stoecker et al., 2013?

- P2, L18: remove a comma after "e.g.,"

2. Study site and data

- P4, L12-13: this statement is useless if you do not give any quantification.

- P5, L11-12: idem, give a value.

- P5, L6 and 15: should mention the elevation of the AWS.

- Figure 3: for Glacier No. 354, the Quickbird and Pléiades images recorded in 2003 and 2015 respectively have not been used for snowline mapping? If yes, these images should appear in the Figure. If not, why?

- Table 1: should write "snowline" instead of SNL. You should also indicate the sensor in brackets for the high resolution images. If I am correct these are SPOT5 and Pléiades for Abramov, ALOS and SPOT7 for Golubin and Quickbird, GeoEye and Pléiades for

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No. 354.

3.4 Glaciological mass balance

See my main comment related to this section (#4), and the one regarding the terminology for the title of this sub-section.

3.5 Geodetic mass balance

See my main comment related to this section (#5).

3.6 Mass balance modeling...

- P10, L23: the paper by Huintjes et al. has not been finally published. I wonder if papers that stayed in discussion and/or were rejected can be quoted. You can probably remove this reference from the text and the ref list, all the more that it is quoted within a list of 5 references starting by e.g.

- In Table 2 and in the text (e.g. P11, L7): I recommend using 'Z' instead of 'H' for the elevation criteria. H usually stands for thickness.

4.3 Snowline-derived mass balance

- P15, L3-8: you should provide an illustration for the test that uses average daily temperature and precipitation series. In addition, you have to explain the low sensitivity of your model to the input meteorological data. I assume this is because the parameters are adjusted for each year and for each glacier.

- Figure 5b: change JAJ by JJA.

5.1 Long-term snowline...

- P16, L3-6: you should provide an illustration for the comparison between SCAFs given by the model and the images for all the used images, glacier by glacier.

- P16, L7-8: the period 1998 to 2016 stands for Abramov Glacier only. You have to mention the specific periods for each glacier. Golubin starts in 2000 and No. 354

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in 2004. In addition “over the two last decades” can be removed from the sentence because the time periods for each glacier will be mentioned.

- P16, L8: you refer to Figure 8, but because figures 6 and 7 have not been quoted yet, this figure should be Figure 6. Anyway, because I suggest adding two more figures, it will probably remain Figure 8, but must appear before the current figures 6 and 7.

- Table 5: you must indicate in the table itself (not only the caption) on which period the STD is quantified.

- P17, L1: should the first close-to-zero SMB period be extended to 2005?

- P17, L1: “Glacier No. 354, situated in a more continental climate regime, [...]”. I am a bit skeptical with this statement! All three glaciers are in a continental regime. This glacier being located in the inner range, it might receive less precipitation than the two others. Is it what you mean? See also my main comment related to interannual variability of the SMB (#6).

- P17, L4-5: this sentence refers to the years 2006 and 2008 mentioned in the previous sentence? If yes, the two sentences might be separated by a semi-colon not a dot.

- P17, L8-9: same thing here, the two sentences could be separated by a semi-colon not a dot.

5.2 Comparison to glaciological. . .

- Table 6: you should mention in the caption that the values differ from Table 5 because the glacier-wide annual surface mass balances are not computed over the same number of days. However, the difference is really high for some years, for example 2014 for Golubin Glacier (more than 0.8 m w.e. different!). You could indicate the number of days differing from the quantification given in Table 5.

- Table 7. A dot is missing after ‘e’ in “m w.e a-1” In addition, regarding Abramov Glacier, why the first period is 2003-2015 and not 2003-2011?

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6.1 More accurate. . .

- P20, L3: replace “integrating” by “using”

- P21, L6: discuss why the difference is opposite for Glacier No. 354

6.2 Intercomparison. . .

- P22, L27: replace “too positive” by “not negative enough”

- P22, L33: change “shows” by “showed”

6.3 Comparison to other studies

- P23, L18-20: you indicate that the average glacier-wide annual surface mass balance quantified by Brun et al. (2017) shows a stronger mass loss than your study. This is not really exact. The difference is important with your modeling approach, but the estimate by Brun et al. and your geodetic estimate are really close.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-256>, 2017.

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