The Cryosphere Discuss., 5, C300–C306, 2011
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The Cryosphere Discussions

Interactive comment on "Ideal climatic variables for the present-day geometry of the Gregoriev Glacier, Inner Tien Shan, Kyrgyzstan, derived from GPS data and energy-mass balance measurements" *by* K. Fujita et al.

Anonymous Referee #1

Received and published: 2 May 2011

The paper treats an important topic from an underreported region. Especially in remote regions with scarce meteorological observations, the behavior of glaciers delivers valuable information on long-term climate trends. Understanding climate-glacier relations is a prerequisite for a correct interpretation of glacier changes. In the highly arid and continental Central Asia, glaciers are important water storages with high impact on the water balance.

Unfortunately, I see severe deficiencies in the methodology, especially what model validation and the extrapolation of observations to the total glacier area are concerned.

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Interactive Discussion
Discussion Paper

The determination of “ideal climatic variables” has profound shortcomings. Therefore, the manuscript cannot be published in its present form. However, I encourage the authors to revise the paper. Many deficiencies would be avoided if the authors step back from the concept of ideal climatic variables and focus on the long-term relation of climate and mass balance at the summit, especially in combination with reconstructed dust events from the ice core.

General comments:

In a first step, the authors have modeled energy- and mass balance of Gregoriev glacier for a two year period. The model is driven by AWS data, gap-filled by reanalysis data which was adjusted to a nearby long-term meteorological station. The model was validated by direct measurements at the summit of the ice cap, where also the influence of an albedo lowering dust event was considered. Validation at a single point is a drawback, since at least one stake from a (relatively) lower elevation was available. In this context, the following should be discussed:

1) Why are stakes not distributed over the full elevation range? 2) Why was stake number 6 not used for validation? 3) What is the information value of 1 year of mass balance observation at one location (which is also the highest part of the glacier)?

The study makes the impression that the results presented here are the bundled byproducts from an ice-core drilling campaign and were not planned originally. This is supported by the concentration of stakes around the summit, by the examination of only one snow pit (density varies with elevation) and by the fact that only the AWS was GPS-surveyed in 2005: Surveying the stakes would have doubled the observation period for flow direction/velocity and elevation change (mass balance) measurements (from 1 to 2 years).

In a second step, mass balance for Gregoriev glacier is derived from GPS measurements along one longitudinal profile only. Although the main part of the glacier is south exposed (as the profile), considerable parts have other, mostly eastern aspects. An

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extrapolation of the measured profile to the entire glacier will certainly introduce errors. These uncertainties should at least be discussed.

Another problem is the fact that the geodetic mass balance method can only be applied to entire glaciers and not to glacier parts, because it cannot account for ice dynamics (submergence and emergence). It is stated by the authors that the effect of glacier flow has little effect on geometry when only one year is considered. But: geometry changes are also small when only one year is considered, isn't the relative error due the ignorance of ice flow always the same?

One result of the study is the "ideal climate" to preserve the current glacier geometry, but I have some doubts concerning the approach: The consistency of the 1979-2007 mean precipitation with the long-term (1930-2003) mean is taken as an indication for the precipitation-regulation of glacier dynamics. I cannot follow this argumentation. Why does a constant precipitation prove that a glacier(that has retreated) is controlled by precipitation? Isn't the opposite the case? Correlations in chapter 3.6. prove that SMB is more closely related to air temperature.

The climate to preserve the glacier geometry is obtained by mean meteorological values for the period 1979-2007. For this period, the authors assume a constant glacier geometry and a mass balance of zero (which should be stated far earlier in the text). But: this was obviously not the case, regarding the areal retreat of the glacier! The authors state that taking into account an area loss would result in a cooler ideal temperature and they estimate that the difference is smaller than other uncertainties (867/7), but it remains unclear how this estimation was performed, especially how the area changes were transformed into volume changes (mass balances).

The conclusions are extremely weak, they are mainly repeating the results.

Minor general comments:

The manuscript has some weaknesses in structure that can easily be overcome. It

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misses a clear formulation of the overall objective, methods and results. Too many individual measurements and sub-objectives irritate the reader, the main goal should be clearly present at all stages of the manuscript. Moreover, it would clearly benefit from proof-reading by a native speaker. Is it really necessary to provide that much technical details on the hard- and software used?

Specific comments:

856/16-21: you should provide some citations! Define the region in line 20!

856/26: you should mention the reason for the discontinuation

857/20 this chapter misses information about mean climatic conditions.

858/6: since the area-altitude distribution is not shown in Figure 1, you should better speak of “contour lines” instead.

858/7-8: why both DEMs? In the figure caption, only SRTM is mentioned.

858/11-16: this is methods, not location.

859/4: you don′t measure the mass balance “of” one stake, but “at” one stake

859/8: “constructed” is not a good term here

860/25: is the surface temperature measured? Calculated? How?

860/26: how are the temperature profiles calculated?

861/2: it is not clear to me what Im stands for in this equation

861/7-17: There is too much reference to earlier studies without any explanation. The reader gets no idea what the model looks like, the authors should at least mention the basic features of their approaches.

862/15-21: This paragraph is difficult to understand, it should be extended or reworded.

863/9: “these variables are less important”: can you provide some numbers that justify

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this statement?

863/19-21: stakes 2 and 4 have lower horizontal velocities than AWS, why are they not considered?

864/4: why is the mass balance only calculated for AWS and not for all stakes with a low flow velocity? And why should the mass balance of one point for one year represent the mass balance required to maintain the present glacier geometry?

865/9: after the surface drop, there is still a significant deviation between observed surface and dust run which should be mentioned and discussed!

865/21-26: this part is hard to understand, I′m not sure if I understood it properly

866/27-28: this is an important (and arguable) assumption, it should be stated earlier.

867: 14: Here, the data of the 2 years of observation should be compared with modeled mass balances numerically. To me, it looks like deviations are larger than a first glimpse on the graphs might suggest. In case of the first year, for example, it looks like the observed mass balance was negative, while the modeled one was positive.

868/4: why “worse”? Do you mean “negative”?

868/7: why “smaller”, aren′t they higher than. . .?

868/3-11: is looks like temperature is stronger correlated to SMB than precipitation. How does this go along with the finding that glacier geometry is mainly controlled by precipitation?

868/24: Except for the last paragraph, the conclusions are repetitions of the results. In the last paragraph, a better reconstruction of mass balances after the detection of dust layers in the ice core is announced. This raises the question why the authors did not wait to publish the better version.

874: Table heading: “preliminary modified”, what does this mean?

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881: Fig. 4 (a): why glacier extent from 1999 AND 2006? (b) is not easy to read. Maybe periods should be indicated in the picture. Maybe one signature for modeled, one for observed? (c): how is this profile derived? It remains totally unclear to me, but would be very IMPORTANT to know!

882: Fig. 5 (a) it should become clear, without reading the text, that “calculation” refers to the dust run.

883: Figure captions: penultimate sentence: “. . .present-day glacier geometry and their variables”.

Technical corrections:

Title: "ideal climatic variables“: is this a common term? Inner Tien Shan, Kyrgyzstan: maybe delete one of the two geographic descriptions in order to make the title more concise. Energy-mass balance: maybe “energy- and mass balance” would be more precise, since you don′t mean the mass of the energy.

856/4: use “lowering” instead of deletion (also in the following)

856/7: maybe “current” is better than “modern”

856/7: maybe you want to round the numbers: -290 mm, -3.9°C

864/12: . . .”for 50 m elevation bands”. . .

869/20: geometry

878: Figure 1: it would be helpful to indicate the dates of the three glacier outlines in the figure. Figure captions: A date for the SRTM data should be given. Last sentence: “. . .were averaged. . .”

880/Figure 1: It is impossible to judge the match between observed and calculated values. Two lines with different color or widths would be more appropriate than lines and circles. In the precipitation graph, I can see only black OR grey bars.

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880: Figure captions: A date for the SRTM data should be given.

Interactive comment on The Cryosphere Discuss., 5, 855, 2011.