

RC2:

General Comments:

This study presents, validates and interprets a comprehensive and impressive data set, which covers a range of parameters in the variable environments of the Susitna Basin, Alaska. The data set includes meteorological, glaciological and soil parameters. The data set is unique, as many of the measurements were done in complex terrain where measurements generally are sparse. It is effortful and requires extensive planning to acquire meaningful data in this terrain. Problems in the data are addressed and generally, implications that arise with these problems are described in detail. Overall, the manuscript is well structured and provides a good overview of the data. The data set itself could be extremely valuable for model validation or comparison with future field studies.

Thanks!

However, the manuscript is not always coherent and suffers from redundant information (e.g. section 3.3, section 5.2, figure 9), **None of these sections or figure are redundant. Perhaps the reviewer meant to say relevant? That critique was addressed in response to reviewer 1 and can be summarized with: "Readers who are familiar with the prior work or the field site will be interested in this information."**

which distracts the reader from following the key points and weakens the focus of the paper (see specific comments).

Addressed below

The introduction does not adequately motivate the manuscript, as it does not really make clear why the data set is important and what the purpose of the data set is (see specific comments).

Addressed below

In addition, some of the presented data appear isolated and need to be put into context better (e.g. section 4.4., how do continuous mass balance measurements compare to stake measurements nearby?) **See P22L10: "The net depth of snow lost at the sonic distance sensor site was 1.97 m (17 April - 30 June), which is comparable to the 2.15 m snow depth measured in a snow pit about 6 m away on 14 April 2013." And see figure 5 which shows the ablation stake and continuous measurements together. Added reference to figure 5 in section 4.4.**

Therefore, I suggest a number of minor revisions to focus the main messages of the manuscript and to emphasize the uniqueness of the data set.

Specific Comments:

Introduction:

Mentioning climate change in the beginning of the introduction is not convincing, since you only acquire three years of data. Either remove the link to climate change, or emphasize that the data set is meant to be used for comparison with future studies (as you do in the conclusions).

We also compare our data to data from 30 years ago and see evidence of climate changes, so we assert that climate change is in fact relevant to the paper.

P2, l9-10: using changes in river flow on dam operations as a motivation here seems misplaced, since you mention in the beginning that the dam was not built. In addition, river flow is not covered in this study. Please rephrase or remove. You could motivate each of the data types (meteorological/climatological, glaciological, snow, soil) individually, as you do later in the manuscript. E.g., p4,l23-26 motivates meteorological/climatological measurements, p25,4-5 motivates soil measurements and should be placed in the introduction

This was the main motivation for the project (and the reason the project was funded) so we believe it to be relevant context. The wording makes it clear that the paper focuses on the data, this section of the intro is providing the context for why the data was collected. We added a sentence about data scarcity to bolster the motivations.

- p1, l6: since only the years 1981-83 were investigated, please do not write 1980s here but refer specifically to the years 1981-83

DONE

- p2, l5: state precise number instead of “more than 120 glaciers”

Changed to “more than 100”, see notes to reviewer 1

- p3, l5: please add reference here

Added: Scherler, Dirk, Bodo Bookhagen, and Manfred R. Strecker. "Spatially variable response of Himalayan glaciers to climate change affected by debris cover." *Nature geoscience* 4.3 (2011): 156.

- p3, l10-p4,l3: detailed description of surge history seems unnecessary here, please shorten

We focus on mass balance, which can be affected by surging (and vice versa). The latest surge of West Fork occurred shortly after the 1981-83 mass balance measurements to which we are comparing our data. While the effect of a surge on the mass balance may not be quantifiable given the data that is available, the surge still needs to be mentioned.

- p6, l4: what does the number 3 in brackets mean?

Added “Figure” to the 3.

- p6, l26: first time abbreviation “DGGS” is mentioned, please provide full name

Spelled out: Alaska Division of Geological & Geophysical Surveys

- p7, figure 4: please provide figure in higher resolution

Done

- p8, l13: delete sentence “Data are not available from 18 January 2014 to 22 April 2014 when the station was buried in snow.” since this was mentioned before.

Done

- p9, table 2 caption: delete “18 January 2014 to 22 April 2014.” from caption, since it was mentioned before

Because readers may take values from the table without closely reading the text, we believe this mention should remain.

- p11, figure 5: please provide higher resolution figure

Done

- p12, figure 6: figure is not mentioned in text, please add reference to figure in text. Also, please provide figure in higher resolution

Added reference and higher resolution figure

- p13, l8: reference to figure 4: is this right or do you mean figure 7?

Good catch, thank you

- p13, l11: rather say “very close to 1.0”

Point taken that it is not exactly 1.000000. But with two significant digits, I think it is appropriate to report it as 1.0 rather than “close to 1.0” implying it could have been 0.99.

- p13, l12/13: “The lack of a consistent pattern in these comparisons prevented us from adjusting the HOBO temperature data to match the Campbell data.” This is confusing since you mention an average offset the sentence before. Can you clarify this?

Added references to figures to clarify: "The lack of a consistent pattern in these comparisons (Figure 7) prevented us from adjusting the HOBO temperature data to match the Campbell data. The high correlation and low temperature offsets (Figure 8) among sensors gave us confidence that using HOBO stations to assess temperature patterns across the basin was valid."

- p.14, l1/2: What is your confidence that no double tips are missed or that normal tips are identified as double tips?
For a complete discussion, please see Wolken et al 2015. Reference added here.

- p15, figure 7: how do you explain very high RH offset of some HOBO-sensor at higher RH, especially in 2013?
Simply different sensor performance, as mentioned in the text. It is notoriously difficult to accurately measure humidity at cold temperatures (e.g. -10 C).

- p14, l11: "Precipitation amounts did not correlate significantly with elevation, slope, aspect, or location." How do you define "location"? What drives the variations in precipitation amounts?
Location can be defined with lat/long coordinates, or by logical grouping in mountain ranges. We chose not to speculate on what drives variation, but we can rule out the factors mentioned.

- p14, l15-17: katabatic wind flow: Can you back this with references or add a more thorough analysis based on your data, e.g. wind direction analyses? Or is this just an assumption you make?
Added a paragraph on wind direction to the "Meteorological data" section.

- p15, figure 7: colour codes are missing
Added to caption: Each colored line represents a HOBO sensor, blue dots are for the reference Rotronic sensor, and red dots are for a Campbell 107 Temperature Probe.

- p16, figure 9: what is the purpose of figure 9? There is no in-depth analysis provided in the text and patterns are trivial (yearly temperature cycle, lower temperatures with higher elevation). In addition, it figure again suffers from relatively low resolution. Please either remove figure or provide more detailed analysis
We believe the patterns are not trivial, but chose to spend more space in the paper describing mass balance data. Future papers may delve deeper into the spatial and temporal variations introduced here. We also feel it is best to present this "raw" data before looking at it in terms of lapse rates (figures 10 and 11). Fixed resolution issue.

- p16, section 3.3: this section does not provide a thorough analysis and is not useful for the manuscript since it is not based on your data. Either please remove or transform; rather than a trend analysis, the section could provide an assessment whether the years 2013-2014 were exceptional (in terms of temperature) or normal.
We later reference this section when discussing mass balance differences between the 1980s and recent periods.

- p18, figure 11: what purpose do the different colours serve? If none, please use black or dark grey
The colors help distinguish which data goes with each week. A grayscale version of this figure is illegible.

- p19, l12: Did you think about adding the line fit to the figure? This would allow the reader to clearly identify the equilibrium line altitude you derived
Given the inconsistent recovery of data from different sites in different years, we chose not to display best fit lines. It doesn't make statistical sense to try to compare these lines from year to year.

- p19, l1/2: Since you used only point measurements, it is also possible that the stake measurements do not fully represent the area that was covered by satellite. In addition, you mentioned earlier that most of the stakes were placed on the centreline of the glacier, which is typically higher than the margins (which are included in the satellite estimation?), potentially leading to higher estimation of the equilibrium line altitude
Reworded

- p19, l7/8: "Glacier-wide mass balance estimates were then calculated by summing the distributed mass balance over the whole glacier." Did you use hypsometry of each individual glacier? Or did you use hypsometry of the entire glacier area for the calculation of the individual glacier mass balances? If so, the numbers you get probably have very high uncertainties. Please clarify.

Changed "based on the glacier hypsometry" to "based on the individual glacier's hypsometry"

- p20, l2: "East Fork Glacier had a similar mass balance as Maclaren Glacier." Why do you stress this here? Seems misplaced, please delete

Done

- p20, l17: "To robustly validate model simulations of snow accumulation..."; please move to introduction, since this provides a motivation for your measurements and you are presenting results in this section

Removed reference to modeling as suggested by reviewer 1.

- p21, l2/3: "...at 2000 m measuring 2-3 times higher than at 1000 m." looks more like 2-4 times higher (see year 2014)
True, changed to 2-4.

- p21, l4: "A notable south-north decrease in total SWE and accumulation gradient indicates a strong orographic influence." Please remove: sentence is redundant since you mention elevation dependence the sentence before. In addition, this is not always necessarily a north-south gradient

Done

- p22, figure 14 caption: "In early August 2013, the sensor's mounting pole began to tip over and give bad readings. On 1-2 September 2014, 18 cm of snow accumulation was recorded, consistent with observations during a site visit. The sensor pinged off falling snow, so some points in that window are labeled as bad data." please remove or move to text

I think it is valid to leave this short description of the bad data in the figure caption, so the viewer doesn't have to search the text for the description of the bad data.

- p22, l16: "data became noisier as the surface transitioned from snow to ice." can this be seen in figure 14?

Yes, when zoomed in.

- p22, l21/22: "This leads to an average melt rate of 0.016 m w.e. d⁻¹ for the summer of 2013 and 0.012 m w.e. d⁻¹ for 2014." This information seems a bit isolated from the previous, interesting mass balance investigations. Can you provide a comparison here? How does this compare to nearby ablation stakes summer mass balances?

The previous paragraph mentions this comparison, and it is plotted in figure 5 (added reference here). "The net depth of snow lost at the sonic distance sensor site was 1.97 m (17 April - 30 June), which is comparable to the 2.15 m snow depth measured in a snow pit about 6 m away on 14 April 2013."

- p23, l7: "Snow water equivalent (SWE)": abbreviation has been initialized before

Deleted "SWE"

- p23, l14: "...generally showed a strong elevation dependence." dependence of what type? Maybe just write "increased with elevation"

Changed

- p23, l16/17: "At the Lower Windy Cr. site, about 40 mm of SWE (25%) was lost due to melt of the end-of-winter snowpack between 9 April and 22 April 2014 (Table 6)" Why is this stressed here? Please remove

Added context: "Lower Windy Creek was the only site where we collected SWE data more than once in a year. About 40 mm of SWE (25%) was lost due to melt of the end-of-winter snowpack between 9 April and 22 April 2014 (Table \ref{tab:Snow})."

- p24, section 5.2: This section does not add any value to the paper but is very distracting; please remove
Readers who are familiar with the prior work or the field site will be interested in this information.
- p25, l1: “characteristics we observed”; please specify these characteristics so the agreement between soil pits and STATSGO becomes clearer
Done: “our observations of soil texture, organic content, and drainage characteristics”
- p25, l4/5: “Understanding the distribution of permafrost and seasonally-frozen ground across the basin is important for modeling of water moving across the landscape”; again, this provides a motivation for your measurements and you are presenting results in this section, so please move to introduction
- p27, l28/29: “Summer air temperatures in 2012-2014 were 1.1 °C warmer than 1981-1983. Annual temperatures were 0.5°C warmer in the recent period.” Why do you add this here? This is not based on your own measurements and thus not a significant outcome. Please consider removing.
It is significant because it helps explain the glacial mass balance changes we observe. Since we do not do temperature index modeling in this paper, we do not directly attribute the mass balance change to the temperature change, though we assume (hope!) the reader will see the connection.
- p28, l2: since only the years 2012-14 vs. 1981-83 were investigated, please do not generally say 2010s vs. 1980s since you have no information on the other years
Done

Technical Corrections:

- p8, l12: move “On-Ice” to the end of this sentence
Done
- p6, l33: please remove “instead of every minute”
Done
- p16, l4: “most distinct” instead of “least complicated”
Done
- p18, l2: “refers to the period October...” instead of “refers to October...”
Done
- p20, l11/12: “Therefore, lower annual balance in the latter period (-1.72±0.87 m w.e.) compared to the former period (0.04±0.25 m w.e.) were driven by the more negative summer balances.”
should be “balances... were driven...” or “balance... was driven”
Done
- p26, figure 17 caption: “soil” instead of “soils”; remove “,” after data
Done