Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2018-21-AC1, 2018
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Open Access Earth System Science Data Discussions

Interactive comment on "The SUMup Dataset: Compiled measurements of surface mass balance components over ice sheets and sea ice with preliminary analysis over Greenland" *by* Lynn Montgomery et al.

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Received and published: 23 April 2018

General Remarks

Montgomery and colleagues have made a very valuable contribution to cryospheric sciences by compiling the very large SUMup database, containing snow/firn density snow accumulation on land ice and snow depth on sea ice. These data are very important in evaluating and calibrating, e.g., model output. While the database compiled and presented is very valuable, I am not entirely convinced by the quality of the manuscript.

I find the structure sometimes confusing and I wonder whether the relatively simple application of the database is really valuable. In its current shape, the comparison to MAR output appears somewhat appended and major components of a comparison between measurements and model output are missing or addressed superficially. Maybe this comparison could be removed and instead the manuscript fully focussed on a compelling presentation of the data itself? The current presentation of the data does not fully convince me. In particular, I find the use of terms confusing. For instance, it remains rather unclear what is meant under the term "measurement" and a number of similar terms is used without defining them. I am also confused to what is finally shown in the figures illustrating spatial and temporal distribution of the data: It appears that âĹij95% of the data points are excluded from the analysis because their spatial resolution is too high? As outlined below, describing your data set through a number of clearly defined parameters could make excluding the majority of your data obsolete. Finally, the authors need to improve the manuscript by addressing numerous inaccuracies (e.g. units in the figures) and wording.

The authors thank the reviewer for their comments. We believe we have address them and worked to improve the quality of the manuscript and more clearly shown why the comparisons made in this paper are relevant and useful. We additionally clarify that we use the word preliminary in the title to clearly convey that this data paper provides a preliminary study that can be expanded. We provide specific details below on how we further clarified and improved the paper based on the reviewers comments. Also attached as a supplement is an updated PDF of the manuscript. We can provide one with tracked changes if requested.

Line 20: I would remove this statement, as the database is probably not the largest set of "field measurements". Even when specifying that it is the largest data set of 'glaciological field measurements', I would still remove the statement, as the sheer number of data points says nothing about quality or value of the data

Statement removed: "Measurements in the dataset are sporadic in time and have spa-

tial gaps, however, they likely constitute the largest set of field measurements compiled, standardized and publicly available."

Lines 48-51: There have been efforts to (i) compile glaciological datasets in a consistent form, tailored for the use in models. (ii) Not only accumulation but also ablation datasets have been compiled. Recent examples that addressed both (i) and (ii) are, e.g. Ahlstrøm et al. (2008) and Machguth et al. (2016). I recommend that the authors provide a brief overview of similar work that has been done in the past, also highlighting the achievements rather than implying that previous efforts were either not publicly available or not formatted consistently.

We have added the following and changed lines 48-55 to provide an overview of a previous dataset and taken out the line about public availability/format of previous datasets.

"Some previous Arctic and Antarctic studies have compiled large sets of measurements, generally accumulation measurements (e.g. Mock 1967a; Mock 1967b; Ohmura and Reeh, 1999; Vaughan and others, 1999; Arthern and others, 2006; Machguth, 2016) most cover only a small region of the ice sheet and/or are not annually resolved. For instance, Machguth, 2016 provides a database spanning from 1892-2015 including over 3000 surface mass balance measurements from 46 sites in the ablation zone a standardized, formatted database from the Programme for Monitering of the Greenland Ice Sheet (PROMICE). "

Line 70: Stating here that 40 measurements have been excluded is puzzling. As a reader, I ask myself why these data have been excluded. I would rather move this statement and maybe also the comment on co-location to the figure caption (indeed, both statements are there already). Since this is the only time that excluded data are mentioned, I also ask myself whether any quality testing has been performed or not? Table 1 and 2 show that there is a column reserved for uncertainties, but how is that column used? I believe it is a viable approach to include all available data, but it needs to be stated whether quality test were performed or if the user should do such tests

We apologize for the lack of clarity here. These 40 measurements have not been excluded from the dataset, just from Figure 1 because it only shows Greenland and not Finland. We have changed the wording from "excluded" to "not shown" within the figure because of geographic location. This is also reflective in the figure caption.

Line 103: The title refers to structure and metadata, however, the following text has a strong focus on the data itself. Maybe consider changing the title or revising the structure? Furthermore, I think the paper would benefit from adding a figure that provides a graphical overview of the entire database, showing for instance its three subdatasets and, e.g., the number of measurements per subdataset together with a listing of the most typical types of measurement that contribute to the subdatasets.

We left the title Structure and Metadata as we feel is necessary to understand the type of data that the metadata is referencing. Here, we have added text to clarify is a method take a direct or indirect measurement of the desired parameter. We have also added taken the reviewers advice and added a bar graph to show the different measurement methods described by the metadata. We feel the bar graph will also help address the reviewers and, likely future readers, confusion about why we do not use certain data in our distribution analysis and now refer back to the bar graph at those points in the paper for further clarification. The new figure/bar graph is attached as a figure in these comments as well as in the new attached PDF of the manuscript.

Lines 111 – 112: I do not fully understand why these ten measurements are mentioned here. I would rather include a section to state your criteria for including or excluding data, e.g. "Snow density data that exceeded a physically plausible range from >0 kg m-3 and <1000 kg m-3 were rejected." Such criteria for data compilation are currently missing from the manuscript but I believe they are an integral part of presenting a database similar to yours.

We thank the reviewer for this suggestion and agree that it is necessary and adds clarity to the paper. We have implemented this change.

Line 134: Here you write of “data points”, above (line 116) of “point measurements at different depths”, on line 137 of “records” and elsewhere more generally of “measurements”. While I do not have a clear solution for the issue, I think there needs to be at least a definition of what is considered a “measurement” in the context of this paper, followed by consistent use of the terms. It might be confusing that a 2 m snow pit can be one measurement and a density measured over a depth range of a few millimetres (using, e.g., a neutron probe) is also a measurement (together with all the thousands of other density values derived from just one bore hole).

We agree with the reviewer that we should be more specific about how we define measurements. We have added the following to Section 2.4 to clarify: “Measurements can be separated into direct measurements, when the instrument measures the desired parameter directly, and derived measurements, when the instrument measures a parameter related to the primary parameter and uses a known relationship equation to derive the desired measurement. In this paper we refer to both direct and derived measurements as measurements.. For clarity, in the density dataset, methods 1-4, 6-9, and 13 are direct measurements (e.g. density cutters, ice core sections, etc.) while methods 5 and 10-12 are derived measurements (e.g. neutron density probe, X-ray microfocus computer tomography, Gamma ray attenuation, etc.). In the accumulation dataset, methods 1 and 3 are direct measurements (e.g. ice core sections and stake measurements) while method 2 is derived (radar isochrones). All snow depth on sea ice measurements are direct measurements.”

Lines 169-171: Maybe it would be worth briefly explaining what kind of ablation process took place at Summit Station (I assume wind erosion?).

We corrected this by adding a sentence stating “These negative accumulation measurements could be due to ablation processes of sublimation or wind redistribution.“

Line 177: remove “an” (maybe should read “one” instead?)

Corrected.

Lines 187 to 200: I find this rather confusing. It is hard to follow which measurements were finally included in the analysis (there is no statement on the exact number of measurements). Furthermore, I do not understand why more than 90 % of the data are simply excluded from the analysis. By excluding these data, you also change the distribution of measurements over time and space. Would it be a solution to bring the data, for the analysis, to a uniform depth resolution and then perform the analysis?

This can be addressed by the new plot discussed above about why we did not include these measurements (swamped this data).

Figures 2 and 3: Your study would benefit strongly from providing clear definitions of the different parameters defining your “measurements”. These could be “location”, “transect”, “depth”, “data point” and more. Such definitions could be given early in the manuscript. For instance, in Figure 2 mainly the location matters and I would recommend visualizing that parameter. Because all high resolution density data taken in one bore hole share the same location, they could be easily visualized in the figure.

We have added definitions of our measurements types as direct or derived to help clarify. We have also gone through the paper to make sure depth measurements and/or spatial measurements are clearly labeled.

Line 232: Maybe “contributes” rather than “contains”?

Corrected.

Line 233: What kind of spatial resolution do you mean? I assume horizontally, along track?

Yes, we have added in the word “horizontal” to clarify this. We have also added the word “vertical” to better describe the high resolution density measurements in line 193.

Lines 249/250: This is unclear to me, maybe consider rewording?

We have clarified these lines from “While understanding the date when accumulation

measurements are collected is important it is arguably more important to understand the years sampled by the ice core or snow pit." changed to "While understanding the date when accumulation measurements are taken is important, it is also important to understand the date, corresponding to the depth, represented by a sample."

Section 3.4: I generally found this section confusing and I think that using a consistent and clearly defined terminology (see comment to Figs. 2 and 3) would improve clarity. E.g. on line 293 you write of "collection bias" and on the following line of "sampling bias". I think it is mandatory to use a consistent terminology, even more when focussing on the presentation of a scientific data set in a journal like ESSD.

We have gone through the paper to make sure that the terminology is more consistent and tried to more clearly define what we mean. Specifically, we have changed all "bias" comments to "sampling bias" appropriately.

Section 3.4.1: What kind of density is meant in the title? The following section suggests you analyse both accumulation and density.

We have moved this section from 3.4.1 to 3.4 to clarify where the explanation of density begins vs. accumulation in section 3.4.2.

"Recent warming over the GrIS, including a melt event in 2012 that covered nearly the entire surface (Nghiem and others, 2012), has increased both snow density and snow accumulation in recent decades (e.g. Morris and Wingham, 2014; Machguth and others, 2016; Overly and others, 2016). Improved measurements, or models, of density and its evolution with time are needed to reduce uncertainties when converting altimetry measurements into total ice sheet mass balance using altimetry (e.g. Zwally and Li, 2002; Shepherd and others, 2012) and for converting radar isochrons into measurements of accumulation (e.g. Koenig and others, 2016). Many models use mean annual temperature and accumulation to model the spatial and temporal evolution of density (e.g. Harron and Langway, 1982; Reeh and others, 2005; Kuipers Munneke and others 2015). Some studies, however, show that density models generally under-

estimate surface (<1 m depth) density measurements (Koenig et al., 2016) while other studies point to the importance of the surface boundary condition for density models when comparing to measurements (Kuipers Munneke and others, 2015; Bellaire and others, 2017). Fausto and others (in revision) suggest two new snow surface (0-10 cm) density parameterizations, derived from a set of observations, using mean annual temperature and elevation to help modeling studies set surface boundary conditions. Here, we look more closely at the density and accumulation measurements within the SUMup dataset over the GrIS and their sampling distributions with respect to temperature, elevation and latitude." We hope this clears up this confusion.

Line 340: ">3000 m"

Corrected.

Line 354: no comma in "...a low density snow crystal, formation ..."

Corrected.

Line 357: what is meant with "1 m"?

In figure 12, we show that when you look at 1 m densities vs. more shallow densities you can see the seasonal cycle more clearly in the more shallow density time series. We have added the word "depth" to clarify this.

Section 5: unless this is a requirement from the journal, I would move this important information somewhere to the top of the manuscript.

This section is required by the journal. Please see (https://www.earth-system-science-data.net/for_authors/manuscript_preparation.html).

Line 378: What do you mean with "properly" described?

This has been corrected to say "has clearly defined metadata."

Line 381: As mentioned in the introduction, I do not think that such a statement is

helpful in any kind.

Changed to “As seen in SUMup, the measurements over the GrIS and AIS are sporadic in time and space, peaking during specific field campaigns and lapsing in between.”

Line 388: Please reword the sentence that starts with “in the future...”, something is wrong there.

Changed to “Once these cores are processed, they will be able to help fill some of the time gaps for the GrIS.“

Line 405: Not sure here, but would “scientists working in the field of remote sensing” be better than “remote sensors”?

Yes, that is more clear. It has been corrected.

Figure 2: Maybe use the same time periods for all three plots.

We understand the concern of the reviewer, but believe that the time periods here have too much range to use the same time period for all plots. We have put a) and c) on the same time scale but kept b) separate because of the larger time range. We have also added a note in the caption to make the reader aware of the different x-axis scales.

Figure 3: Unclear why the bins are so extremely different among the three plots.

We have changed the first two plots to have the same scale, but have kept the third distinct because of the majority of shallow pits in summit (other than the GISP2 ice core which is the 100+ measurements). This is also stated in the text in line 234 as “The deep 100 m plus measurements at Summit come from the GISP2 ice core (Alley, 1999).”

Figure 4: Using the same time frame for all three plots would substantially improve the comparison of the temporal distribution of measurements at the three locations. Why is this the case in Figure 5 but not in Figure 4?

We now use the same temporal distribution in all 3 plots in both Figure 4 and 5 (updated to be Figures 5 and 6).

Figure 8: You mention the grid cells but if I am not mistaken, the spatial resolution of the MAR output is never mentioned in the study. I assume it is 20 km? But even if you used a higher resolution model run, the question of how to compare very coarse gridded model output to point measurements is important and, to my understanding, not discussed in the current paper. For this and other reasons, I am not convinced that the comparison to MAR contributes substantially to your study.

We have added the grid size resolution of 25 km to the text to make this comparison more clear. The authors agree that there are many ways to compare point measurements to models which are beyond the scope of this paper. In this preliminary analysis we use a simple method, similar to other model/measurement comparisons (eg Arthern and others, 2006; Burgess and others, 2010; Koenig and others, 2016) and end up with similar results to Fausto and others, accepted.

Figure 9: Unit cannot be g m-3 if the maximum value is 0.6. Throughout the entire manuscript, please check for consistent use of units

Corrected. Units are consistent now throughout the whole paper as g/cmˆ3.

Figure 10: same as Figure 10.

Corrected.

Figure 12: Here you use kg cm-3 but rather mean kg m-3.

Corrected. We have also changed these units to match the rest of the plots as g/cmˆ3 now. Also, c and d were switched on this plot and that is now fixed too. Thanks for catching these unit errors.

Please also note the supplement to this comment:
https://www.earth-syst-sci-data-discuss.net/essd-2018-21/essd-2018-21-AC1-

supplement.pdf

Interactive comment on Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2018-21, 2018.

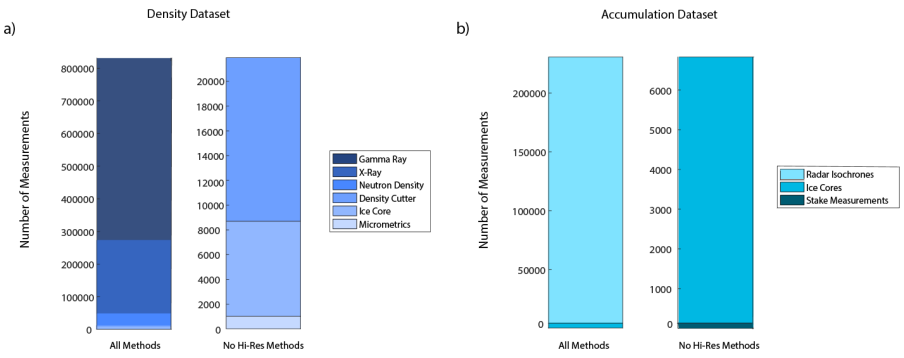

Fig. 1. New Bar Chart to show method of measurements