Article reference: essd-2023-116

Title: Quality-controlled meteorological datasets from SIGMA automatic weather stations in northwest Greenland, 2012–2020

Author(s): Motoshi Nishimura et al.

MS type: Data description paper

Thank you for sending a decision letter that includes instruction of future revisions, and I appreciate giving a specific evaluation and valuable comments for improving our article from you and reviewers. Considering the comments, I have revised our manuscript as showing below. The given referee comments are written in gothic font, each comment's responses are in blue type, and correction details are in red type. Revised sentences and context are highlighted using track changes in MS Word in the text.

11 May 2023

Referee Comment #1

Citation: https://doi.org/10.5194/essd-2023-116-RC1

This article is appropriate to support the publication of this data set. I was able to download the data and plot samples. I felt the accompanying metadata files and the readme files did a nice job in explaining the dataset. I felt this submission was of high quality and I would trust the dataset as useful.

Thank you for your comment. We appreciate your evaluation. We hope that this dataset will be published and will contribute to many studies.

No specific correction was done.

18 May 2023
Referee Comment #2
Citation: https://doi.org/10.5194/essd-2023-116-RC2

General:

This paper presents approximately eight years of quality-controlled datasets of two automatic weather stations (AWS) on the ice in NW Greenland, one situated on the contiguous ice sheet and one on a detached coastal ice cap. This region is climatologically very interesting as well as rapidly changing, as described in numerous recent publications. These AWS data are highly valuable for process understanding, climate monitoring, and model evaluation/satellite validation, and deserve to be published. The data have been extensively quality controlled as described in this paper. The resulting dataset appears clean and robust and useful for users. My main problem is the non-concise and often unclear writing style in this paper, which makes the paper hard to digest and, more seriously, in places leads to confusion. Although it is a relatively minor remark, it will require a significant effort by the authors to remedy this.

Thank you for your comments; in accordance with the Major comment and the comments on Section 4.1, we have revised the text to reduce its length and improve readability. In addition, we will correct the unclear explanations in accordance with your comments.

Major comments:

I would like to encourage the authors to critically go through the MS text again to improve the readability and accuracy of the text. The writing can be more concise and precise. Some examples (not exhaustive) are listed below as minor comments. And in the process please aim for a shorter paper.

We will revise and improve the readability of this manuscript by following the reviewer's comments as

> *Section 4.1: I suggest listing all range values in Table 3 and not to repeat these in the text, to improve readability. Instead, for each correction it would be nice to mention the % data affected.*

and reducing the amount of text by deleting the text and figures in the sections on PDD (Fig. 5) and air temperature lapse rate (Fig. 7b).

The entire text has been reread, redundant parts have been reorganized, and unnecessary explanations have been deleted. I believe this work has improved the readability of the text.

We added the Tables summarized the ranges for each parameter as Table 3 in Sect. 4.1 and showing the percentage of unmasked data as Table 4 in Sect. 4.2. In Section 4.1, the description of the range

test written in Table 3 has been minimized, and the text has been modified as well. In section 4.2, the formula for the anomaly range of the anomaly test (Eq. 2.0.1) is noted at the beginning of the section and avoided being repeated in the text that follows.
In addition, with this correction, we found a new error in the lower limit of the range test for snow temperature, which is noted here and corrected.

Following parts were deleted.

- Figure 5 and the second paragraph of section 5.1 (both related to PDD)
- Figure 7b and the second paragraph of Section 5.2 (discussion on the rate of temperature decrease between the two locations)

Table 2 would be better placed at the very beginning or end of the text.
We have modified Table 2 to be placed at the front of the main text.
Table 2 has been moved to the beginning of Chapter 2. With this change, Table 1 was also moved to the same section to make the flow of the text more natural, and the title of Chapter 2 was changed.

This paper presents an observational dataset, so it is more logical to start the introduction with the history and importance of in situ observations in Greenland.
We thought I included an explanation in the second paragraph of the Introduction, but it seems it was insufficient, so we will add a few more sentences about the AWS network of GC-Net, K-transect, and PROMICE, citing their references and the position of SIGMA AWSs.
The position of SIGMA-AWS is clarified by adding an explanation of the history of the meteorological observation network in Greenland in the first part of Introduction. In addition, the first paragraph of the former version Introduction was deleted to make the flow of the text more natural.

Section 5 also discusses derived data, such as positive degree days, lapse rates but also average seasonal cycles, etc. Not sure such (admittedly basic) analysis has a place in a data journal.
Following the comment, we will delete the following parts

- Figure 5 and the second paragraph of section 5.1 (both related to PDD)
- Figure 7b and the second paragraph of Section 5.2 (discussion on the rate of temperature decrease between the two locations)

Those two parts (indicated above) were deleted.

Minor comments:

l. 17: an -> the

We will revise following the comment.

Corrected the relevant the part (L17 in the revised manuscript).

l. 25: ""snow height degradation", unclear, do you mean snow height decrease or snow metamorphism?

We will correct to "snow height decrease" since it means "snow height decrease".

Corrected the relevant the part (L28 in the revised manuscript).

l. 37: "however, the existing in situ meteorological data are insufficient for these purposes", unclear, do you mean that current observational coverage is insufficient? It is quite good in Greenland when compared to e.g., Antarctica.

“Insufficient" is an overstatement in some cases, so we will rephrase as "continuous accumulation of measured data will be more valuable”.

No correction was done because the relevant part was deleted in the revision process.

l. 51: "analytical values of various numerical models", unclear, do you mean "output of numerical models"?

We will correct to "output of numerical models" as the reviewer's comment is correct.

Corrected the relevant the part (L53 in the revised manuscript).

l. 54: please explain 'sensor noise' and 'natural factors'.

Sensor noise mainly refers to a few watts of radiation that occurs at night. Currently, we use the terms "electric pulse," "electric noise," or "sensor noise" to refer to this error, but this was not appropriate. This phenomenon and its errors are generally referred to as "Zero Offset" (Behrens, 2021), so we will modify the terminology to use that terminology.

Natural factors include riming, ice accretion, snow accumulation on sensors, etc. Since this expression is indeed abstract, we will modify this part of the initial publication to add the following explanation:

“natural factors”

-> “natural factors (e.g., riming, ice accretion, snow accumulation on sensors)”

Corrected the relevant the part (L55–57, 234–237, and 246 in the revised manuscript).

l. 56: please explain QC or better simply write out throughout.

Since QC was first mentioned here, we will add an explanation that QC is a process to improve the quality of data by removing outliers and modify the notation to Quality Control (QC).

Corrected the relevant the part (L58–59 in the revised manuscript).

l. 76: " It is considered" the fact that the surface consists of accumulating snow/firn proves that this is the accumulation area.

We will rephrase “It is considered to be” to “This site is”.

Rephrased the relevant the part (L80 in the revised manuscript).

Fig. 1a: I suggest including the GC-Net stations as well.

We will consider adding GC-Net and K-transect observation sites.

The AWS locations of GC-Net and K-transect were added in Figure 1.

l. 78: "is supposed to be", unclear, was it intended to be at the equilibrium line, or is it thought to be there?

We intended that the SIGMA-B site is thought to be located at near the equilibrium line.

We will modify to mean that the SIGMA-B site is considered to be located at near the equilibrium line.

Corrected the relevant the part (L82 in the revised manuscript).

l. 80: " The surface condition at this site varies (see Fig. 2), and surface melting has occurred in warm years". Obviously, surface melting occurs at the equilibrium line. Did you perhaps mean "net ablation"?

Surface melting here was intended to mean "significant surface height decreasing," so “surface melting” will be changed to " significant surface height decreasing”.

Corrected the relevant the part (L84–85 in the revised manuscript).

l. 97, Figure 2: mainmast -> main mast (also elsewhere in text).

We will correct following the comment.

Corrected the relevant the parts (many parts in the revised manuscript).

Figure 2: why is date given only in lower plots?

Since the surface condition of the SIGMA-B site varies greatly depending on the year and the time of year, we have included photographs of the different surface conditions at the site and the timing of each photograph in the figure as reference information. Specifically, in July in years with high temperatures, the entire snow layer may melt, exposing the bare ice surface, while in years with low temperatures, there may be almost no surface melting. This is a characteristic of the SIGMA-B site environment and is explained in the text.

No specific correction was done.

l. 113: cyclone battery?

Since "cyclone battery" was a proper noun, we will change the term to "lead-acid battery".

Corrected the relevant the part (L123 in the revised manuscript).

Table 1 caption typo: observaion -> observation

The submitted version spells it correctly, so it may be some kind of mistake on Referee's part.

In any case, it should be correct in the the revised manuscript.

No specific correction was done.

Table 1: accuracy of wind direction, unclear what is meant here.

We will check and correct the sensor specifications.

We checked the manual. Corrected the section on wind direction accuracy in Table 1. The accuracy of wind speed was also corrected, as it was not properly described.

Table 1: It appears that for the radiation measurement the sensitivity rather than the accuracy is listed?

We will check and correct the sensor specifications.

The manual was checked and the section on radiometer accuracy was corrected in Table 1.

l. 131: some, not all?

I thought it would be easier to explain LW_{std} and others in the section explaining the QC process, and since they are not explained in the L131 section (section 3.2), I left them as some.

Since this is the intent, I will explain the intent and leave it as some instead of all.

No specific correction was done.

l. 133: "Because the vertical radiant flux against the inclined surface needed to accurately calculate the surface albedo and surface energy balance is affected by the sloping surface at the SIGMA-B site, we calculated the slope-corrected downward shortwave radiation (SWd_slope) from the corresponding observations using the correction method in Jonsell et al. (2003) and Hock and Holmgren (2005)." This sentence is unclear.

We will revise this sentence to be more concise and clearer.

We rephrased the relevant the part (L137–141 in the revised manuscript)

Table 2: in line 147, 'transmittance' is indicated by lowercase 't_r', in Table 2 we see an uppercase 'T_r' which is called 'transmissivity'. Are these the same things?

T_r in Table 2 is a constant (0.881; SIGMA-A, 0.872; SIGMA-B) defined to explain the QC of shortwave and near-infrared radiation in secondary control, and t_r in line 147 refers to the general

atmospheric transmission coefficient used in the SW_{d_slope} calculation, which is a different parameter. It may be confusing, so we will change the variable names Tr to T_{rA} and T_{rB}, and t_r is left unchanged to make it easier to distinguish them.

The name of the atmospheric transmittance variable in Table 2 have been modified so that they are general atmospheric transmittance variables (t_r) rather than site-specific constants for SIGMA-A and SIGMA-B. T_r in the text has been modified to site-specific constant names, such as T_{rA} and T_{rB} (L339–340 in the revised manuscript).

Section 4.1: I suggest listing all range values in Table 3 and not to repeat these in the text, to improve readability. Instead, for each correction it would be nice to mention the % data affected.

We would like to reduce the amount of text by summarizing the range of the range test in a table or something. Also, it would be useful to know what percentage of the total data is masked, so we will add this information as a summary in a table or something.

The range test coverage is summarized in Table 3 and the percentage of masked data is summarized in Table 5.

l. 214: I do not understand this correction: why giving a clearly wrong measurement an arbitrary physical value?

Indeed, a negative wind direction is understood to mean that the sensor is making some kind of error, so we change the wind direction to be masked rather than set to 0 when the wind direction is negative.

Corrected the relevant the part (L226–227 in the revised manuscript).

l. 224 and 236: 'electrical noise', what is this? Earlier you used 'sensor noise', is this the same?

Since they mean the same thing, we will unify them with “Zero Offset” (See Reviewer#1 L.54 comment response).

Corrected the relevant the part (L234–237, and 246 in the revised manuscript).

l. 226: why can alfa_sw and alfa_nir not be lower than 0.95 and 0.90? Or do you mean 'higher'?

It was a “higher” typo. We will correct it as such.

Corrected the relevant the part (L240 in the revised manuscript).

l. 232: Are these the conditions for which the data are flagged as erroneous? It seems to be the other way around.

It seems that the notation was not clear. These parts will be changed from

“$SW_d < SW_{TOA\_max}$,” to "$SW_d > SW_{TOA\_max} \rightarrow SW_d = -9999$,"

and modify the text a little to match this modification.

In the revision process, above parts were decided to summarize in Table 3 and to delete the relevant part in the text.

l. 237-240: You give the data a physical value (zero), would it not be better to not do that unless for instance when SW_TOA < 0?

The negative emission that occurs when the solar zenith angle is greater than 90°, which means SW_{TOA} =0, is considered to be Zero Offset, so the negative value itself has no meaning, and there is no emission in such a case, so setting the value to 0 is not a problem.

Therefore, we will not modify this part.

No specific correction was done.

l. 249: the surface consists of snow or ice, so how can its temperature become positive?

Although the snow surface temperature is not higher than 0˚C, a threshold of upward longwave radiation equivalent to a snow surface temperature of +10˚C is set, taking into account the effect of radiation from AWS poles and other sources. This threshold does not imply that the snow surface temperature is positive.

Added explanation of above intention and changed the text in L258.

l. 299: Six hours of calm weather is not impossible, why this arbitrary value? Why not use the wind speed at the other AWS to check this?

Since there are no other AWS nearby, we cannot confirm the validity of this process.

Although the 6-hour period has an arbitrary, it is possible to be in a calm weather environment with infinitesimally small wind speeds for several hours, so the 6-hour threshold was set to avoid accidentally masking such a situation.

No specific correction was done.

l. 355: Why is wet snow treated differently at both sites?

The SIGMA-A site has a lower albedo limit of 0.3 because it is extremely unlikely that bare or dark ice will be exposed on the surface even if surface melting occurs, while the SIGMA-B site has a lower albedo limit of 0.1 because even dark ice may be exposed.

No specific correction was done.

Figure 4, 6, 8: Consider reducing symbol size.

We will revise as following the comment.

Corrected the relevant the part (Figures 4, 6, and 8).

09 Jun 2023
Referee Comment #3
Citation: https://doi.org/10.5194/essd-2023-116-RC3

Review of "Quality-controlled meteorological datasets from SIGMA automatic weather stations in northwest Greenland, 2012-2020" by M. Nishimura et al.

B. Vandecrux (bav@geus.dk)

The article describes the valuable meteorological data collected by two weather stations in northwest Greenland and the processing thereof. The article is clearly written, the key elements of the AWS systems are thoroughly described and the figures are of very good quality. I only have minor comments on the manuscript. It is great that this data is being published and distributed freely. Nevertheless I am concerned that many data users will wonder why there is only data up to 2020. Adding recent data will certainly increase reuse and citations. If this is not possible for some reason, then it should be stated clearly in the article. I am also strongly encouraging (and I think ESSD does as well) the publication of the scripts that are behind the data processing. This is key to making this dataset and article fully reproducible. After addressing these two points and the minor comments listed below, the article will be a great asset for ESSD.

Since July 2020, SIGMA-A observation data has been continuously showing erroneous and missing values due to some kind of malfunction. Due to the global pandemic of the COVID-19, field work has not been possible, and this situation continues to this day. Therefore, in this paper, we intend to publish the data set up to August 2020, when we can obtain the data reliably and when the mass balance year is well delimited.

As your comment, we are considering the possibility of releasing the processing code, as we believe it would benefit the scientific community. However, more time is needed to prepare for the release of the code, as more testing and code organization are needed.

We are working on those tasks now, but due to the large amount of work, we think it may be difficult to complete those tasks by the deadline of this revision, and we hope to complete the work and release the code by the time of publication.

Comments on the article:

- abstract: ESSD requires that the dataset DOI appears in the abstract. Please add the two DOIs of the two level 1.3 datasets.

We will add the dois of Level 1.3 datasets to abstract.

Corrected the relevant the part (L20–22 in the revised manuscript).

- l.22: "snow height increased" by how much? The use of "snow height" is misleading in the accumulation area. In many studies, snow more than one year old is not refered to as snow anymore but firn, so I first misunderstood this statement as the "annual snowfall is increasing". If the author do not distinguish snow and firn, then the total snow (+firn) thickness, and thickness change, are actually not measured. I recommend changing to "surface height" or "snow surface height". For Sigma-B, it would be nice to state clearly if it is standing on bare glacial ice. In that case "snow height" can be used.

We agree with the intention of the comment. The first installation was on bare ice in July 2012, but it is possible that refreezing ice (when it formed is unknown) is now forming above the bare ice surface in 2012. Therefore, “snow height” in the manuscript will be changed to “surface height” because a location higher than the surface height at the time of installation is not necessarily snow.

Rephrased the relevant the part (many parts in this manuscript), Tables 1 and 2, and Figures 3 and 4.

- l.24: "decrease" by how much? Again, for snow height, do you mean that the annual maximum snow height is decreasing or that the surface height is generally decreasing?

“decrease” means the surface is lowering. We will rephrase “snow height” to “surface height”.

Corrected and rephrased the relevant the part (L26 in the revised manuscript).

- l.26: "notable snow height degradation" Not clear why it is notable or with regards to which normal it is a degradation. Please rephrase.

We will rephrase “notable” to ”apparent”.

Corrected the relevant the part (L28 in the revised manuscript).

- l.97: "mainmast" two words?

We will rephrase “mainmast” to ”main mast”.

Corrected the relevant the parts (many parts in the revised manuscript).

- l.123: "cm" line 104-105 you use m for instrument depth, now cm for height. Please be consistent. Potentially use only SI units.

We will change the “m” notation of the depth of snow temperature sensor installation to “cm” notation.

Corrected the relevant the part (L114–116 in the revised manuscript).

- table 1: It should be stated whether RH is provided with regards to water or with regards to ice (in subfreezing conditions). Some sensors do the conversion automatically, some don't. If it is with regards to water, then a corrected RH could be provided accounting for the different

saturation point in subfreezing conditions. Or at least potential correction methods should be listed.

Since this sensor calculates relative humidity based on the saturated water vapor pressure for liquid water, we will add a note to that in the table. The intent of this paper is to describe the QC method of the observed data and the observed values themselves, and it is our policy not to make any corrections or process the data including such a way that the intention of the implementer may intervene. Including further data processing methods in this paper would be redundant and would obscure the point of the discussion. We understand that accurate data analysis may require correction for shelter heating effects of air temperature and humidity in freezing environments, so we will discuss those treatments when we publish such a paper. However, We will revise the text to add a note to that effect, for alerting readers to this issue.

Added a paragraph with explanations in Sect.3.2 and a note in Table 1.

- Section 3: I am missing a discussion of the sensors' known limitations, it could be either included under the AWS system description subsections or in a section of its own at the end of the manuscript. It should estimate how often those problems my occur and point at potential way to remidiate them. Some of these limitations are:

It may be necessary to describe the errors that the observed values contain, and we will add a subsection in Chapter 3 to explain this. The response to the correction and limitation of individual observations is described in detail below.

However, as noted in the response to the comment on Table 1, the intention of this paper is to publish the observed values themselves, without any correction or data processing that might involve the intervention of the implementer's intention. Therefore, we will note that the data published in this paper possibly contain some errors only and will not conduct any additional analysis or corrections that would show the corrected values.

Added a paragraph with explanations in Sect.3.2 that some data may require correction.

- RH sensor clogging up with rime (https://doi.org/10.1007/s10546-004-7955-y)

The temperature and humidity sensors used at both sites may be affected by icing and riming as you have indicated. We will revise the text to cite this paper and add a note to that effect, for alerting readers to this issue.

Added a paragraph with alert explanations in Sect.3.2 that RH data may require correction.

- Unventilated thermometer overheating in low wind and clear sky conditions.

As noted in the response to the comments on Table 1, this paper does not include any correction or

data processing that might include the possibility of intervening intentions of the implementer, and the intention is to publish the observed values themselves, so I will not discuss such issues. However, We will note the shelter heating effect, which has been pointed out in many previous studies, in Chapter 3.

Added a paragraph with alert explanations in Sect.3.2 that the possibility of positive bias due to the shelter heating.

- Radiation sensors and anemometers being shadowed/sheltered by the station mast (https://doi.org/10.1029/2010JD015507)

At least radiation sensors of SIGMA-A is placed far enough away from the AWS main mast, and the pole of the radiometric sensor is placed in such a way that it does not affect the sensor. So we think that the station mast has almost no influence to those sensors. If SIGMA-B is affected, it would be by the shadow of the satellite communication antenna mounted at the top of the main mast of AWS. I cannot make a quantitative assessment of the presence or absence of this effect, but a detailed review of the hourly data showed that the effect was not pronounced. According to this, it is highly unlikely that the antenna's shadow is affecting the radiation, and if it is, it is likely to be slight. Therefore, we think no specific treatment is required. Nevertheless, your point is a valid one, and I will add a brief summary of the above explanation to Chapter 3.

Added a paragraph with explanations in Sect.3.2 that although there is a possibility of being made shadow on the radiation sensors by the AWS mast, the effect did not confirm by checking the dataset.

- l.218: if RH is given with regards to ice, then supersaturation is not uncommon on the ice sheet up to ~110% and this filter may be too strict. If RH is given with regards to water then values >100 are unlikely.

Since the humidity sensor measurement is based on relative humidity relative to liquid water, we would leave the upper threshold at 100%.

Added a paragraph with explanations in Sect.3.2 and a note in Table 1.

- l.226: "lower" higher?

The indication is correct, we will correct it to “higher”.

Rephrased the relevant the part (L240 in this manuscript).

- l.243: Please avoid this use of brackets in equations to indicate interchangeable variables. Brackets have a defined meaning in equations. Either spell out two equations or use a subscript "i" in the equation and define it in the text like: "i being either u or d"

We will correct as per the comment with some subscripts.

We decided to use χ as a subscript and added a note to L197–198 that χ indicates the downward or upward direction of the radiation. The specific change in the text in L247–249, and 344 in the revised manuscript.

Although the comments were only for the upward and downward emissions, Procedure 1.4.2 also contained parentheses, so the variable was changed and an explanation for the variable was added to the L253–255.

- Section 4.1.5: Do you use the same filters for "sensor_height" as for "sh"? It should be mentioned in the text.

Since the sensor height is calculated after the QC of the snow height was completed, we do not set any no filter for the sensor height. I will add an explanation to the text about this.

The explanation of above was added in the last part of Sect. 4.2.6.

- l.317: "weak electric pulse" where does that pulse come from, why is it weak and how does this relates to the radiation measurements?

“weak electric pulse” mainly refers to a few watts of radiation that occurs at night. The radiation amount is an observation error caused by the specifications of the instrument, and the error is caused by the slight temperature difference between the two detectors (inside of the dome shelter and sensor body), which occurs when there is a large temperature difference between the outside air temperature and the temperature inside the sensor body.

This radiometric error may cause the shortwave radiation to be recorded as an observed value at night. However, since the value is an observation error, the observed value may be different from the original radiation balance.

The explanation of above was added in the first paragraph of Sect. 4.1.3.

- l.331: same comment as line 243

We will correct as per the comment with some subscripts.

Same as the response for the L243 comment, we corrected the relevant the part.

- l.409: since the snow temperature sensors' installation depths were given in meter, I misunderstood the "-1" as meter. Please be consistent with the units.

We will correct the notation of depth for snow temperature sensor installation to “cm”.

No specific correction for this comment was done, because the notation of the unit of initial sensor installation depth has been corrected.

- l.445: please give mean annual PDD and its standard deviation to support this statement.

In accordance with RC2, we are going to reduce the text, so we will delete the part about the analysis of the PDD. Therefore, the relevant part of this comment will also be deleted, so I will not respond to it.

No specific correction for this comment was done.

-l.454: Is there any net ablation years? Does the station allow to measure the ice ablation? Is there any measurement (e.g. stakes) of the ice ablation? Please elaborate on this.

Since no ice thickness changes or stake observations were made, it is not possible to discuss the mass balance. Since this discussion is based on observations at AWS, the discussion is based on meteorological observation data.

Incidentally, Sugiyama et al. (2021) reported the result of stake observations of the SMB for the years 2012/13-2018/19. The result showed that the SMB at the same elevation zone, the clearly negative SMB year is 2014/15. 2015/16 and 2018/19 are ±0, and the rest are POSITIVE. However, since this is not an observation at the SIGMA-B site and we did not observe it at the same elevation as the SIGMA-B site, we do not know if its SMB is the same at the SIGMA-B site. This verification is beyond the scope of this paper and will not be done in this paper.

No specific correction was done.

- l.472: shouldn't the lapse rates be negative?

The point is correct, but I will delete this section and will skip responding to your comment.

No specific correction was done.

Comments on the data files:

- Commercial formats like Microsoft Word should be avoided. Please replace by a text file.

The temporary Dropbox data link may have included MS word files, but the official dataset data and doc do not include MS word, so please check the doi link page.

No specific correction was done.

- The station coordinates (potentially through time?) and a table giving the meaning of each variable (as they are named in the data files) should be provided at least in the readme file, or even better: in separate, machine-readable files (e.g. csv, tsv).

We will change information published as pdf files to text format, etc.

The station coordinates, and easy information of each variables were added in the read me files.

Details for each variable were not written in the read me file, because it makes the readme file redundant and decreases a readability, and they are also included in the body of the paper.

- The date format used is non-standard. ESSD encourages ISO 8601 (or alike). Please specify if time stamp is local time or UTC.

We will correct the time data format.

We will update the dataset again after the peer review process is over, and I will correct the timestamps at that time.

26 Jun 2023

Editor Comment #1

Citation: https://doi.org/10.5194/essd-2023-116-EC1

I would like to take a moment and thank the reviewers for their work. There are a lot of good comments, which I would like to encourage the authors to respond to and then to prepare a revised submission.

In general, I believe that the manuscript would benefit from careful incorporation of the reviewers' comments.

Additionally, I would like to encourage the authors to:

- include the GC-net stations in figure 1, to show how this gap has been closed

We will revise as following the comment.

The AWS locations of GC-Net and K-transect was added in Figure 1.

- clearly distinguish in the manuscript between measured and derived parameters

The observed values are summarized in Table 1, and the other parameters are derived parameters. If the distinction is made in the table, it would be more unclear, so we will revise the text to clearly state that "the observed values are summarized in Table 1 and the other parameters are derived parameters.

Added explanation of above intention in L75.

- consider publishing the processing code to increase confidence in the dataset.

As stated in our response to RC3's General comment, we will consider it positively and would prefer to publish it.

The program is now available as an asset of this paper.