The Cryosphere Discuss.,
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Interactive comment on "Can katabatic winds directly force retreat of Greenland outlet glaciers? Hypothesis test on Helheim Glacier in Sermilik Fjord" *by* Iain Wheel et al.

Anonymous Referee #1

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General comments:

Wheel et al. present their findings of how katabatic winds draining off the southeast Greenland ice sheet influence Sermilik fjord circulation and sea ice/ice melange break up in front of Helheim Glacier. A catalogue or climatology of katabatic winds, categorised into Downslope Wind Events (DWEs), is created using ERA5 data and compared to two weather stations. They use a combination of atmospheric and ocean observations with satellite products to investigate a number of events in more detail and provide convincing evidence that katabatic winds have a direct impact on the fjord circulation. Furthermore, they provide evidence for glacier retreat and calving directly

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caused by the katabatic winds in a particularly strong event.

The combination of atmosphere and oceanographic observations over a number of years highlights the results in a comprehensive way. Some of their results are interesting and do add to the research and knowledge in this field, however some are not novel and look very similar to the findings of Oltmanns et al. 2014. The discussion is thorough and well balanced, with them highlighting the new research and the gaps that remain. The limitations of the study should be made clearer though, and the motivation of the study should be more obvious in the introduction.

My reservations lie in four main areas, two of which the authors could remedy without too much additional analysis. However, two may require more work. These are described more below. The research fits well in The Cryosphere journal and will be useful for the atmosphere and coastal oceanography community. The authors mention a number of areas that future research could go into, based on their results. If the major and minor comments are addressed, then it should be accepted for publication.

Firstly, the more major changes:

Some of your results, specifically the catalogue of DWEs is not novel, as it is almost identical to the Oltmanns et al 2014 study. In Oltmanns et al. 2014, they use the same two weather stations as you, use the same method but with slightly different thresholds, and present the findings using the same figures. However, you fail to mention that your study uses the Oltmanns method, with some changes. You also do not mention the Oltmanns study in the introduction- which you could use to your advantage, as there were gaps remaining in this research field, and your findings fill these gaps by building on the Oltmanns et al. 2014 foundation. There is also little comparison to Oltmanns et al. 2014 in your results or discussion, even though their results are relevant, as they use the same data as you, and also created a catalogue of katabatic winds. For instance, Figure 2 in your manuscript is the same as Figure 3 in Oltmanns et al. 2014, therefore this is not a new finding, but more of an extension of the Oltmanns study but using ERA5 data. You need to make clear in the figure caption that it is an adaptation of the Oltmanns et al 2014 one. Similarly, Oltmanns et al. 2014 also looked at the impact of the March 2011 katabatic event on sea ice cover (Figure 10 in Oltmanns et al. 2014), which is similar to your finding in Figure 7. This means that two aspects of your results are not quite as novel as they could be (catalogue of DWEs and March 2011 sea ice analysis). The Oltmanns et al. 2014 study didn't include any hydrographic data or discussion on fjord circulation, so this is where your research comes in, but that isn't clear enough currently. You should strengthen your motivation for the study in the introduction. If you: 1) explain that you use the Oltmanns method but alter it for X,Y,Z reasons, 2) explain that you have used Oltmanns study as a basis but updated it for ERA5, 3) discuss the ERA-Interim/ERA5 differences in the discussion, 4) describe why you also look at the March 2011 episode (e.g improved resolution of satellite imagery provides more information about ice melange break up and movement of ice bergs), then I think this manuscript could be improved.

What is the evidence for sea ice break-up related to katabatic winds if satellite imagery is only available (due to cloud cover) for three events? As there are only three events, and the March 2011 one has previously been published, why didn't you present one or both of the other events? This would be more novel and provide stronger evidence for your conclusions that katabatic winds influence sea ice concentration. Currently, you make quite bold statements 'most DWE events removed sea ice from the fjord…' (line 212) but there isn't much evidence to back up this statement. You show evidence of this from March 2011, which has been shown previously, and say that you only have imagery from two other events. So how do you know that most events remove the sea ice?

Secondly, the two minor changes:

The black bars in figure 2 and results/discussion surrounding the extreme events are not included in the results. Similarly, the 20m/s threshold of 'extreme' events is not introduced in the methodology. Currently, the reader can gain very little about where this 20m/s threshold came from, and what can be interpreted from this. Only further into the results, where the sea ice break-up is presented, is the 20m/s value introduced. This should come earlier in the methodology and in the first section of the results where you present the catalogue of DWEs.

Section 2.3 of the results needs restructuring to make it clearer where the authors are presenting the general characteristics of the DWEs, or where they are pointing directly to the case studies. This part of the manuscript needs improvement, as I have made many smaller/specific comments below also. The results are robust and interesting, but they are currently difficult to interpret, and it took me a number of reads through the manuscript to understand. The specific comments relate largely to confusion over dates in the text and figures (which differ) and highlighting specific periods in the figures for ease of understanding. For example, adding dashed lines to show when the DWE starts and ends. Citing specific panels of figures (e.g Fig 3c instead of Fig 3) might make this section easier to understand also. Please see the specific comments below for other suggestions.

Specific comments:

Intro, Ln 27-30: What is the citation for this 40% calving, 60% surface melt statistic?

Intro, Ln 54: reorganise sentence to make it clearer. For example: 'As the katabatic air mass is colder than ambient conditions, they can reduce in situ temperatures . . .'. With the -20°C, do you mean that the airmass reduces temperatures by 20°C (i.e a change) or that absolute temperatures can reach -20°C? This isn't clear.

Data/Methods: In your methodology you do not say that the method you use is taken from Oltmanns et al. 2014. The citations of the study are not enough, as it is not made clear to the reader that this is not a new method, but slightly adapted values from a previous study. You need to make this clearer and give justification for using different values if a previous catalogue is available.

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Ln 80-85: More information about the AWSs and ERA5 is needed. What is observed at these stations and what data do you use? E.g. Wind direction and wind speed only. At what interval are the observations made and what interval do you use? Introduce your abbreviations here too, so that it is consistent throughout the manuscript. What resolution is ERA5? Did you select one grid point for analysis or an area average? Cite Figure 1 so that readers can see the ERA5 location.

Ln 84: The Olauson 2018 citation is not necessary here, as they looked at ERA5 for wind power in different countries and is not related to this study. Use a citation provided by ECMWF or their doi for this.

Ln 104: The Oltmanns et al. 2014 citation isn't needed here, as that study using ERA Interim, not ERA5, so did not show its reliability.

Ln 125 and 132: What time period prior to the DWE do you consider for this calculation? Is it a certain number of hours/days/weeks? As you only mention the 'duration' of the DWE on ln 125.

Ln 140: Later in the manuscript, you say that due to cloudy conditions, you can only assess ice break up for specific events in 2005, 2011 and 2013. Please give an indication of this lack of data in this paragraph. As it reads currently, it sounds like you have a daily timeseries of such data.

Results, Ln 155-157: remind the reader of the time period of each data set here. E.g '. . . the DMI station record (1958-2018) showed. . .'.

Ln 164: This is the first time that 20m/s has been mentioned as a threshold, so it confused me that it was mentioned here. I see on Figure 2 that this corresponds to an extreme event, but this is not explained in the data/methods section, or elsewhere in the results. Please put the black bars from Figure 2 into context in the results section. It becomes more important later on with the discussion of sea ice breakup, but I only know that from reading the manuscript a few times.

line 176: flow rate increases with little lag, then Figure 3 is cited. Is this a general comment, or only specific to the events shown in Figure 3?

Figure 3/4/5 would also benefit from a dashed vertical line of some kind to highlight when the DWE starts/ends.

For fig 3a there are 2 peaks in WS- is this 1 DWE or 2? Some dates should be included in the results section to highlight which DWE you are talking about. E.g Line 185: 'the weaker of the two events'- which one does that relate to? Include a date and/or highlight the specific panel in Figure 3.

Ln 178: 'first event in 2012' but the dates in figure 3 are 2013. Same for line 185.

Ln 191: Does the figure 4 citation here relate to where the thermocline became shallower, or when it was 'not always the case'? Point to a specific panel if needed.

Ln 204: 'events in 2010': none of your figures are from 2010, so are you now looking at 2010, or should that be a different year?

Figure 4/5: There are some date issues on these figures. Figure 4c/d is 2011-03-05 whereas Figure 5c/d is 2011-03-04, but the wind speed/direction panel looks identical, so which date is correct? Figure 5: temperature and salinity taken from 10_4 and 11_5 buoys: is this respectively, or are the lines on the plot an average of them? The panel letters (a,b,c etc) are inside the figure in Fig 5 but outside of the panels in all other plots (outside the plot is clearer in my opinion, but as long as they are consistent, whichever is fine). Fig 5 caption: 'Hydrographic changes during the events in winter of 2010 are shown in Fig 4'. The dates in Figure 4 are for 2011 and 2013.

Discussion, Ln 244: Is this 18% underestimation compared to the DMI station? Perhaps make that clearer if so, with 'underestimates downslope flow by XX%, compared to observations at DMI, for an. . .'

Ln 317: Here you say that a typical DWE removed the sea ice: is this statement related to the March 2011 event from Figures 7+8? If so, I wouldn't call this event a 'typical'

DWE, as this was over 20m/s winds, and therefore an ‘extreme’ case by your definition. If this statement isn’t related to the March event, where is your evidence for sea ice remove during typical events?

Ln 356/357: you do not mention or analyse the surface melt water or surface ablation in the manuscript, so how are you able to assume that there is no melt water or ablation? Is this purely because it is in the winter? There are instances of winter melting in Greenland and Antarctica, however. Perhaps reword this sentence to make it clear that your inferences are from winter characteristics as opposed to actually analysing the surface conditions.

Ln 370-373: I don’t quite understand this sentence- please reword or restructure to make it clear.

Technical comments: Abstract, Ln 14: change ‘impact on glaciers stability’ to ‘impact on the stability of glaciers’. Abstract, Ln 18: ‘dependant’ should be ‘dependent’. Intro, Ln 27: full stop missing after Mouginot citation. Intro, Ln 27: ‘calving on’ should be ‘calving of’. Intro, Ln 61/62: Put all citations at the end of the sentence to make it easier to read. Data/Method, Ln 116: ‘Temperature profiles for each site was…’ should be ‘temperature profiles for each site were…’ Data/Method, Ln 117: ADCP has not been defined. Results, Ln 158: ‘had’ should be ‘have’ Throughout: Be consistent with use of FS or Fjord Station for the AWS. Throughout: Point to specific panels in Figures where necessary to aid understanding. E.g line 158 is highlighting Figure 2a, whereas line 160 is Figure 2b. This becomes quite important for Figure 3,4 and 5. Ln 200: ‘subsequence’ should be ‘subsequent’. Ln 232: ‘the prevalence of DWEs was…’ to ‘the prevalence of DWEs are…’. Check your tenses throughout, as there are other instances where present tense would be better. Ln 255: singular/plural issue here: ‘During a DWE’ or ‘During DWEs’. Ln 262: Katabatic is with capital K here- check consistency throughout. Ln 321: should facilitates be facilitated? I think this sentence should be re-worded to make clearer. Ln 339: ‘on’ should be ‘from’ Ln 346: ‘retreat’ is missing after ‘rapid’ Ln 354/355: ‘indirectly’ and ‘directly’ should be ‘indirect’ and ‘direct’. Ln 361 to 364: A long sentence without punctuation to break it up- perhaps re-write into some shorter sentences, which would also make it clearer to understand your main point. Ln 415: ‘the’ should be ‘there’, and remove ‘in’ after trends.

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