Lateral meltwater transfer across an Antarctic ice shelf

Dell *et al.*

The Cryosphere

Anonymous referee

General comments:

This manuscript presents a quantitative analysis of meltwater distribution, storage and transfer on the Nivlisen Ice Shelf, Dronning Maud Land in East Antarctica during one melt season (2016-2017). The authors use a modified version of a previously published algorithm to classify meltwater into four different categories from Landsat-8 and Sentinel-2 imagery and track meltwater area and volume through the melt season.

Meltwater has been linked to the instability and collapse of Antarctic ice shelves, but detailed observations of the seasonal evolution of meltwater on ice shelves in Antarctica are lacking. Therefore, it is my view that the findings from this manuscript are of broad interest to the cryosphere community.

In general, the manuscript is well-written and clearly structured. The rationale for the study is well justified, including the specific choice of ice shelf. The quantification of meltwater area and volume stored and transferred on the ice shelf during one melt season builds upon previous work that has identified surface meltwater systems present on numerous Antarctic outlet glaciers and ice shelves (Langley et al., 2016; Kingslake et al., 2017; Stokes et al., 2019).

However, it is not immediately clear how the authors have adapted the previously published FASTER algorithm into the algorithm used here, FASTISh, and its novelty could be communicated more clearly. Its ability to differentiate between lakes and streams represents an important advance because it gives insight into the lateral transport of meltwater across ice shelves, not just the *in-situ* storage, but this needs to be made clearer in the Introduction. On a related note, I am slightly apprehensive about the binary classification of lake versus stream – what about elongate, linear lakes, which are common on Antarctic ice shelves?

There is a lack of discussion of the limitations associated with the methods used in Section 3, including classifying water body types, and applying the depth retrieval algorithm. The depth algorithm is known to only retrieve depths up to a certain point because rapid attenuation of red wavelengths means that only shallower lakes can be measured in this band (Pope et al., 2016), and makes several assumptions (see Sneed and Hamilton, 2011). The reader should be made aware of these limitations. Another point for the authors to consider, which deserves mention in the manuscript, is that this method will be unable to extract depth from lakes with floating ice cover (and hence will miss their deepest points).

The authors use surface and ground surface air temperature from one weather station to assess meltwater changes against temperature changes. However, this is only at a single point location, and I suggest the authors could compare reanalysis or regional climate model temperature data over the whole ice shelf, or at least over a broader area, at least to see if a similar pattern is shown through the melt season. Such models are spatially distributed and provide daily outputs. I believe that some regional climate models have outputs at 5.5 km resolution over parts of Dronning Maud Land. I leave it up to the authors to decide whether to include this additional analysis.

I would recommend that to strengthen the discussion in Section 5 the authors carry out flow routing analysis on a DEM in order to estimate the pathways in which surface meltwater would be routed. This would be particularly useful at end of third paragraph where the authors discuss the potential for excess meltwater export off the ice shelf.

The authors discuss the evolution of meltwater stored in the two largest meltwater systems. The meltwater volume loss from upstream lakes near the grounding line, and the gain in meltwater volume in these two large meltwater systems (WS and ES) through the melt season, should be more explicitly calculated in addition to the time series of total are and volume presented in Figure 6. This would help their discussion of lateral meltwater transfer across the shelf. A further optional suggestion is that the authors could include evidence that the firn contains shallow-sub-surface meltwater, e.g. visible as low backscatter in Sentinel 1 (Miles et al., 2017, Frontiers in Earth Science), which could aid their discussion of the likely processes responsible for meltwater transfer across the shelf.

Once the authors address these points, I can therefore recommend that this manuscript is suitable for publication in The Cryosphere.

Specific comments:

Line 30: Quantify how large (e.g. up to ~8/5 km long).

Line 55: Consider also citing Bevan et al. (2017) here, who present four boreholes on Larsen C in addition to that reported by Hubbard et al. (2016).

Line 90: See general comment above about highlighting the difference in algorithms.

Line 103: 'flow velocities of around 100 m a^{-1}' – is this at the grounding line or near the calving front?

Line 104: It would be helpful to label these regions on Figure 1, i.e. bare/blue ice area, Shirmacheroasen and the ice shelf itself. Red star is missing from Figure 1 inset.

Line 106: It would be useful to mention surface melt rates on the ice shelf in this section.

Line 117: Perhaps refer the reader to Figure 2 here.

Line 127: Perhaps explain why you have chosen to conduct your analysis on a single melt season, and why this specific melt season has been chosen. You state yourselves in line 82 the importance of tracking meltwater evolution through entire summer melt seasons, so why not do it here? If this was because your focus was on testing FASTish over a relatively small area and time window, make this clearer. Or was this purely based on imagery availability? Also, why not change 30th April 2017 to 24th March 2017, given this is the latest image you use. You also say you identified 12 scenes with 'minimal' cloud cover – did you select a specific % threshold?

Line 141: How well does this cloud mask perform compared to other cloud masks? And why was this method used but then you computed a cloud mask using a different method for Sentinel 2 scenes?

Line 166: As with Line 127, state the specific % cloud threshold used.

Line 184: I feel this is a bit vague – what was unsuccessful about this cloud masking approach? Were there false positives? Clouds not captured in the cloud mask? In addition, a little more detail might be added about how individual masks were manually created in these cases – were polygons manually digitised in GIS?

Line 209: Explain what a two-dimensional connective threshold is.

Line 210: ≤ two pixels in total size, or in width? Same with Sentinel-2?

Line 216: Rather than qualitative depth assessments, I would say that NDWI has been primarily used previously to derive water body extents and area, so would delete this second part of the sentence.

Line 239: The description of how A_d was calculated could be explained in slightly more detail.

Line 244: Did all images include deep water? What range of R_∞ values were used here? I believe Sneed and Hamilton (2011) conclude that optically-deep water is not required in every scene, and indeed Banwell et al. (2019) found a negligible difference of using a R_∞ value of zero. I am interested whether you explored different values to see what difference it made.

Line 250: Figure 2 mentions the removal of negative depths from the area and depth matrix, yet this is not discussed in-text. Where did negative depths originate from, was this in the cases with floating ice on lake surfaces?

Line 261: I think the solidity score needs to be explained in greater detail here, i.e. that the score can range from 0 to 1 and denotes the proportion of pixels within the convex hull of the water body, with streams or linear lakes having a score closer to 0, and perfectly circular lakes having a score of 1.

Line 276: Does FASTISh use the same minimum size threshold for tracking water bodies as in Williamson et al. (2018), i.e. 495 pixels (0.0495 km^2)? I couldn't see mention of this.

Line 278: I think this needs to be better highlighted that this is what sets FASTISh apart from the previous algorithm, FASTER (which did not track water body type).

Line 300: I think it needs to be made clear to the reader that a 'loss event' could either be a lake drainage or freeze-through. Given the previous sentence, one might assume this is just referring to lake drainage events only.

Line 309: I assume the single 2 m data strip was also REMA, but maybe best to clarify.

Line 314: See broader comments above.

Line 331: I suggest you include total area of meltwater bodies in each time-step (i.e. when discussing Fig 4A-D), as well as mean and maximum water depths.

Line 355: Interesting that the mean and maximum depth at the peak of the melt season is lower than in early December, near the start of the melt season? Have the authors considered why this may be?

Line 423: Is it not possible to distinguish whether a water volume loss event is freeze-through or surface drainage? This would help quantify specifically how much meltwater is transferred across the ice shelf.

Line 520: I would suggest moving the last sentence of this paragraph (explaining FAC) to here.

Line 546: How did you derive albedo over water-free distal areas of the ice shelf?

Line 572: Consider changing this to 'Around the peak of the melt season (26^{th} January 2017)'

Line 576: Perhaps consider adding a sentence or two about the wider application of your findings. For example, your mapped lake extents could be used as constraints in surface hydrology modelling simulations?

Line 581: What about sharing your dataset publicly, i.e. lake extents containing attribute information of elevation, mean area, maximum area, mean depth, maximum depth.

Line 817: Looking at your study area extent, it seems odd to include a couple of lakes above the grounding line but not all, when there are visibly quite a few more within the blue ice area above the grounding line which fall outside your study area boundary. Given these are probably 'feeder lakes' which help route meltwater to lakes and channels on the ice shelf, it seems incomplete not to track seasonal changes in their area and volume as well.

Line 839: Are the white stripes across the figure artefacts? Consider changing the colour ramp to make values easier to read, and outlining lakes in black for visibility.

Line 852: Again, the colours make visibility difficult. I struggle to pick out areas in the 'always a stream' category

Line 877: I find this figure slightly confusing, though perhaps I have mis-interpreted it – why is one bar not shown per individual date studied in the 2016-2017 melt season (i.e. only 8 bars are shown, not 11). Clarify caption to show that loss events record either lake drainage *or* freeze-through.

Technical corrections

Line 23: inconsistent hyphenation of Landsat-8 and Sentinel-2 throughout the manuscript; please check throughout for consistency

Line 68: inconsistent hyphenation of sub-surface; please check throughout for consistency

Line 69: re-order citations chronologically; please check and modify throughout

Line 74: delete 'events'

Line 76: delete comma after 'front'

Line 81: change 'on' to 'in'

Line 102: is 'approximately' 123 km wide by 92 km long.

Line 161: 'change 'in' to 'into'

Line 175: Change 'give' to 'produce'

Line 201: Hyphenate 'water covered'

Line 218: Delete 'different'

Line 206: Change 'as' to 'because'

Line 220: Correct (Philpot, 1989) to Philpot (1989)

Line 294: Change 'track' to 'tracked'

Line 393: Maximum should be ~3 $x10^6$ m^2, not 2.5 $x10^7$?

Line 426: change ‘by’ to ‘on’ (as this is showing loss events on that particular date rather than cumulative events, if I have understood correctly)

Line 449: Change ‘weather’ to ‘temperature fluctuations’

Line 467: New sentence after ‘bodies’. ‘We suggest this occurs when water ponds…’

Line 471: New sentence after ‘dam’. ‘Therefore, it is more likely to be the result…’

Line 497: hyphenate snow/firn-covered

Line 500: add ‘firn’ into ‘available pore space’

Line 547: hyphenate ‘large scale’

Line 863: Western and Eastern System are the wrong way round in the figure caption. Wrong dates in legends?