Review of the modified version of **"The complex basal morphology and ice dynamics of Nansen Ice Shelf, East Antarctica"**
by Dow et al., submitted to The Cryosphere

Reviewer: Ala Khazendar

OVERVIEW

This modified version includes some improvements. These include the abandonment of blaming certain data sets for how they might be used by investigators, and the recognition that ice-shelf draft changes cannot be attributed solely to submarine melting. A T-S diagram of detected water properties is also a welcome addition.
On the other hand, I still find several issues with the methods and assumptions used to draw several of the conclusions in the manuscript. I describe these in detail below.

REVIEW in DETAIL

Issues related to the fluxgate mass loss calculations

L. 245-251:
In this discussion, the authors are considering "mass loss", yet Equation 2 calculates *volume* flux. How were the volume fluxes at the gates converted into mass fluxes?

L. 249:
On a more specific point, this paragraph states that:
"The discharge at Site 1 is 0.226 ± 0.004 km3 a-1 and at Site 3 is 0.222± 0.005 km3 a-1 demonstrating an average cross- sectional mass loss of 181 m2 a-1 as the ice flows ~22 km between these two sites."

The discharge values shown here at the two sites are the same when taking into account the errors. This means that no conclusions can be drawn on cross-sectional volume loss other than that none can be ascertained.

Issues related to ice-shelf melt rate calculations

L. 255-256:

"This translates to average vertical melt rates of 0.75 m a-1 (keel), 0.45 m a-1 (apex), 0.95 m a-1 (keel), and 0.68 m a-1 (apex) between the two sites."

How does the change in ice thickness between the sites translate into "melt rates"? Again, as I pointed out in the first round of the review, several factors can contribute to ice-shelf thickness changes and these need to be considered when attempting to isolate the signal of basal melting from ice-thickness changes.

L. 266-267:

“It is possible that mass loss is due primarily to surface melt and sublimation, which was estimated by Bromwich and Kurtz (1984) to be 0.25 m a-1 and by Bell et al. (2017) to be 0.5 m a-1.”

These numbers are then used later in this section to calculate “cumulative surface mass loss” between sites 1 and 3. This raises two questions:

- Estimating how surface processes are contributing to ice thickness change cannot only consider loss due to surface melt and sublimation, it also needs to include accumulation (precipitation, wind-blown snow, etc.). Accumulation at the surface does not seem to have been considered here.
- A “uniform” surface ablation rate was used in the cumulative surface change. There is an underlying assumption here that the values used were also uniform in time, i.e., have not changed over the several decades it takes the ice to advect between sites 3 and 1. How is this assumption justified?

L. 267-268:

“To investigate this, we integrate the basal shelf melt rates between Site 1 and Site 3 using the Cryosat-2 basal melt dataset along with ITS_LIVE 2018 ice surface velocities…”

Similar to the remark above: the basal melt and ice surface velocity values used in the integration are assumed here to have not changed over the several decades it takes the ice to advect between sites 3 and 1. How is this assumption justified?

L. 453-462:

The issues described above regarding the methods used to calculate melting rates raise doubt on the conclusions in this discussion regarding the different melting rates at the margins and at central part of the suture zone. Those conclusions, nonetheless, seem to be already supported by data from Adusumilli et al. (2020), shown in Fig. 5a of this manuscript. The authors, however, say that the Adusumilli et al. data for Nansen are “close to the noise floor (uncertainty) of the dataset for the NIS”. Yet, in other parts of the manuscript they use the same Adusumilli et al. data without any reservation about uncertainty. For example, they compare the satellite-derived basal melt rates with the ocean glider data to find “a good correspondence between the locations of meltwater near Inexpressible Island and the center of the NIS terminus, and the areas of enhanced melt from the ice shelf, L. 273-275.”
The authors need to decide how much confidence they have in the Adusumilli et al. (2020) satellite-derived data, and apply that consistently in their analyses. If they

decide that they do have sufficient confidence in those data, then the authors can rely on the satellite-derived data, especially given the problems described above in their fluxgate and point analyses. If they do not have sufficient confidence in the satellite-derived data, then their conclusions regarding the correspondence between glider observations and melting locations under the ice shelf do not hold.

Issues related to meltwater characterization

L. 473-482:

The authors put emphasis on the depths and locations at which what they call “meltwater” was detected in order to connect it to the basal topography of the ice shelf, but they do not provide any definition of what they consider to be “meltwater”. In other words, what are the temperature and salinity ranges of the “meltwater” plotted in Figs. 2b and 2e? The quotes below refer vaguely only to “cold, fresh ice shelf meltwater” or “supercooled” water.

L. 174-176: “Beneath this layer, the dense High Salinity Shelf Water (potential density > 1028 kg m-3) is presumed to drive basal melt (Rusciano et al., 2013), resulting in the formation of ice shelf meltwater below the *in situ* freezing point (potential temperature < -1.94□C), making the water supercooled (Fig. 2d).”

L. 285-286: “to determine whether the calculated basal melt in the suture zone is observable with *in situ* data, we plot the glider data representing the location of cold, fresh, ice shelf meltwater on Figures 2b, and 5.”

Issues related to the assumption of hydrostatic equilibrium

L. 345-347:

“Our *in situ* data calculations demonstrate that the mismatch between hydrostatic thickness and radar ice thickness is therefore greater closer to the grounding line. This could potentially be due to bridging stresses from the highly variable basal draft and/or from pinning of the ice shelf from valley walls or nunataks.“

Or it could be due to the well-established fact that, in the grounding zone, ice surface can lie lower than the hydrostatic equilibrium level over some distance (up to a few kilometers) downstream from the grounding line (Brunt et al., 2011). Work on the Amery ice shelf (Chuter et al., 2015) found that, while there is a mean thickness difference of 3.3% between radio echo sounding measurements and the CryoSat-2-derived thicknesses, that discrepancy rises to 4.7% near the grounding line.

Other issues

L. 421:

"Alternatively, ocean melt may be focussed on the deeper keels with frazil ice formation possible at the apexes of the basal fractures. The dampening of the amplitude of the basal features as shown in Figure 3, suggests that such differential melting and freezing may be occurring."

Several studies have already observed and modeled this phenomenon, including Khazendar and Jenkins (2003), Jordan et al. (2014), and McGrath et al. (2014). It would be appropriate to cite these previous studies in connection to what is being described here.

Throughout:

The Adusumilli et al. (2020) data are invoked frequently in the discussion, but are only explicitly cited once in the main text (L. 259) and another time in the caption of Fig. 5. Otherwise, they are referred to as just "the satellite-derived basal melt rates" or "the Cryosat-2 basal melt dataset." I think there should be more explicit mentions of the source of these satellite-derived melt rates.

Abstract:

Related to the preceding point, the Abstract states "We use a combination of airborne and ground-based radar data, satellite-derived data, and oceanographic data collected at the Nansen Ice Shelf…". This can give the incorrect impression that the satellite data are analyzed as part of this study. Again, the source of these data should be made explicit in the Abstract, or at least refer to them as "already published satellite-derived data", or similar.

Minor Issues, Typos, etc.

L. 161:
"…year-1 or less We interpolated…"
Missing punctuation.

L. 205:
"Tison and Khazendar, 2001"
This paper has more than 2 authors, so citation should be "Tison et al., 2001".

L. 333:
"…to the terminus At Site 1 and 2, further upstream, …"
Missing punctuation.

L. 421:
“focussed"
Typo.

L. 464:
“…increase to ~1 ± 0.6 m a-1 Similarly, generally…”
Missing punctuation.

L. 482:
“(Friedrichs, in review)”
The reference for this paper is missing from the References list.

REFERENCES

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