Review of **"The complex basal morphology and ice dynamics of Nansen Ice Shelf, East Antarctica"**
by Dow et al., submitted to The Cryosphere, July 2021

Reviewer: Ala Khazendar

OVERVIEW

In this manuscript, the authors analyze several data sets from the Nansen Ice Shelf in the Ross Sea, with focus on a suture zone at the confluence of two tributary glaciers. The authors reach conclusions on the morphology of the suture zone and its possible relationship to basal melting and channelized flow; and the suitability of the use of certain data sets.

The authors address a subject, suture zones, that is of increasing importance to the question of ice-shelf stability and evolution. Two of the data sets they present would be of much interest to the community, namely, ice-penetrating radar profiles across the suture zone and elsewhere on the ice shelf, and hydrographic glider measurements.

On the other hand, I found that some of the main conclusions drawn by the authors were not clearly supported by the available evidence, that some of the observations that could help in evaluating their arguments were not presented, or that their analyses did not take into consideration factors that could lead to different interpretations of the data. I believe that these issues need the attention of the authors, and I discuss them in detail below.

The manuscript is generally well written, making it possible to follow the arguments on the many aspects that the authors address. Some sections on related themes could potentially be combined to make the text flow better.

REVIEW in DETAIL

<u>Issues Related to the Hydrostatic Assumption and Data Use</u>

L. 280 and elsewhere: The discussion of hydrostatic equilibrium
The authors write: "…Satellite-derived estimations of ice shelf draft data are limited by assumptions of hydrostatic balance, which do not take bridging stresses or pinning points into account."
This statement, and the ensuring discussion, raise several issues:

- What are the uncertainties of the REMA surface elevation data? These seem not to be considered in the analysis. Each 1 m of surface elevation uncertainty would translate into ~9 m of draft uncertainty, which would account for at least part of the difference between REMA-inferred ice-shelf drafts and those from IPR.

- A similar argument applies to tidal effects. The authors dismiss those on L. 299 as “less than a meter in this region (Padman et al, 2002).” But, again, each meter of surface elevation variability due to tidal movement translates to ~9 m of draft estimates.
- The authors invoke “bridging stresses or pinning points (L. 282 and elsewhere)” to explain the limitations of the deriving ice-shelf draft from surface elevations. That might well be the case in parts of the ice shelf. But, are the authors implying that this is the case over all of the ice shelf? Pinning points are specific locations where the ice shelf is intermittently or permanently grounded on bathymetric features—a phenomenon distinct from bridging effects. Perhaps the authors could avoid lumping these two together, and clarify where they apply in the ice shelf.
- As a suggestion, what I think is a valuable discussion of features at the bottom of an ice shelf and their expression (or its absence) at the top surface can be found in Nicholls et al. (2006).

L. 432, Section 5.5 (Data applicability)
The authors write: “With the availability of high-resolution datasets such as REMA and GoLIVE, large-scale analyses of ice shelf characteristics can be made. However, from our application of multiple data sets including in situ data we find that some of the ice shelf properties are not well represented in the satellite-derived data sets.
The primary limitation of REMA is that hydrostatic calculations do not take into account bridging stresses and variability of ice rheology.”
This is a theme that is emphasized by the authors. I fully agree with them that investigators should be careful in how they use satellite-derived data sets. The problem with the authors’ stance as expressed and emphasized in the manuscript is that they appear to lay the blame with the data sets themselves. Statements such as the “primary limitation of REMA …” imply that this is an inherent defect of the data set. I disagree. The problem lies in the use of the data sets without paying the necessary attention to their limitations, which is a different matter.

Issues Related to Ice-Shelf Basal Melting

L. 218, Section 4.3 on ice shelf melt rates
The authors infer basal melting rates by a direct comparison of ice-shelf draft changes between two sites. Yet, ice-shelf thickness changes could result from several processes (e.g., Moholdt et al., 2014) including:

- velocity divergence as the ice shelf flows and spreads under gravity;
- downstream advection of thickness gradients;
- changes in surface mass balance;
- changes in firn air content that affect the density of the snow-firn-ice column;
- in addition to basal melting.

The firn air content might not be an issue due to the intensity of katabatic wind on NIS, as the authors point out. On the other hand, I think it would be helpful if the authors could clarify how they accounted for the other processes in inferring basal melting rates. For example, as they point out, “a river was observed in the surface depression as early as 1974 and annually from 2014-2016 (Bell et al., 2017).” The presence of this river in the surface depression that runs along the suture zone suggests that surface mass balance probably plays an important role in how the ice-shelf draft changes, not only basal melting.

L. 382–394, L. 410 and elsewhere: The discussion of how the change in the shape of the suture zone suggest ocean-driven melting
As an example that summarizes the authors argument, they write on L. 410 “The higher ice loss rates in the middle of the suture zone compared to the thicker edges do, however, suggest an active melt component and that the suture zone is acting to channel water. “
Again, as discussed in the previous point, these arguments assume that all change in ice-shelf draft is due to the presumed ocean-induced basal melting, ignoring the others processes described above that could also modify the draft.

L. 267, Section 4.5 (Oceanography data)
The hydrographic glider measurements discussed by the authors could be one of the more interesting aspects of this work. It is therefore surprising that there are no figures presenting these data (other than the locations where fresher, cooler water was detected). Such figures could show:

- temperature and salinity profiles with depth, and/or be T-S diagrams encompassing all relevant measurements in front of NIS;
- the speeds and directions of the flow currents being discussed.

It is difficult to assess the authors’ arguments and conclusions of this topic without such figures and the information they would convey.

L. 412–422: Discussion of melt water emergence from the suture zone.
In light of the absence of the hydrographic data described in the previous point, the statement by the authors that “meltwater observed by the glider underneath and directly offshore of the middle of the NIS calving front lines up with the region of enhanced melt within the suture zone and the thinner ice in the suture zone” is difficult to assess. What does “lines up” mean in this case? How is the origin of the observed melt water in the suture zone demonstrated? The authors do address some of the difficulty of establishing the connection between the suture zone and the melt water in L. 418-422.

L. 486, the “Conclusions” section:
The authors write “Our analysis of changes in ice morphology, flux gate volume, oceanographic data of freshwater and satellite-derived ice shelf melt all point toward active channelized melt within the suture zone.”

This conclusion does not seem justified in light of the ambiguities of the analyses of ice-shelf morphology and oceanography discussed above.

Issues Related to Marine Ice

L. 246: The section entitled "Ice rheology" does not discuss rheology. The section mentions the two types of ice that might compose the ice shelf (meteoric and marine), but no rheological differences are discussed. For example, the early stages of marine ice formation could still be permeable (see next point below), making its temperature closer to the freezing point, hence contributing to a distinct rheology from that of the colder meteoric ice.

L. 248: The authors write "…it is likely that these "echo-free zones" represent marine ice accumulation (Holland et al, 2009)."
This is a misunderstanding of Holland et al., 2009. When those authors, and others, state that marine ice basal returns are rarely detectable, they are referring to the *bottom* of the marine ice layer, not the *meteoric-marine ice interface*, which is often detectable by IPR. In other words, the absence of radar returns is likely not due to the presence of consolidated marine ice, as the interface between meteoric ice and any consolidated marine ice beneath it would have been detectable. For further background, the literature on marine ice in the Filchner-Ronne and Amery ice shelves (e.g., Fricker et al., 2001, which is already cited in the manuscript) is instructive.
A plausible hypothesis to explain the absence of radar returns is the presence of *unconsolidated* layers of a mushy mixture of frazil ice crystals and seawater and/or layers of slushy ice that have not yet fully consolidated to form solid marine ice. Such mushy or slushy layers could result in the attenuation of radar signals without creating a clear interface where dielectric properties change abruptly, hence potentially accounting for the echo-free zones. Evidence for the possible presence of such unconsolidated frazil ice in the Nansen Ice Shelf itself can be found in Tison et al. (2001, I am a co-author of that work), and in Jansen et al. (2013) on the suture zones of the Larsen C Ice Shelf.

Other Issues

L. 426: "…However, as this strain transition occurred in the same region as the maximum melt it is potentially linked to the higher thinning rates of this region compared to further upstream."
There are a couple of issues with this statement:

- It is not clear how the strain transition is linked to the higher thinning rates. Could you please explain what you think are the mechanism(s) underlying such a link.
- This link, as presented here, appears to be mostly conjecture, yet on L. 488-490 (in the Conclusions section) it is reported as a more concrete finding of the work, which I do not think is justified, unless supported by further discussion/analyses.

L. 138 (Figure 3): I apologize if a I missed something, but this figure does not seem to be correct. For example, it shows the suture zone to be thicker in the middle, unlike all the other figures.

L. 64-73: Regarding the flow speeds and thicknesses of the two tributary glaciers, why do the authors cite observations that are two decades old, rather than use the recent data available to them (e.g., GoLIVE; IPR)?

L. 65-72: A map of ice-shelf and tributary glacier flow speeds would be helpful in illustrating several features described here, including glacier speeds, the suture zone, and the flow around Teall Nunatak and Inexpressible Island. There are two figures showing surface elevation (2a and 4a), but none showing flow speeds.

Minor Issues, Typos, etc.

Figure 1: The label "Priestley Glacier" is difficult to read. Please consider other colors.

L. 50, and elsewhere; L. 127; L. 158; and L. 266: instead of "ice shelf terminus", "ice shelf edge", "ice shelf calving front" or "calving edge", consider more consistently using the same terminology, for example, "ice-shelf front".

L. 73-74: It might be helpful in Fig. 1a to point to where this surface-depression is.

L. 223-224: "…we compare the horizontal difference in basal draft…"
The word "horizontal" here is confusing. Perhaps "cross-sectional" or similar would be clearer.

L. 260: "… resulting in the formation of frigid ice shelf meltwater (potential temperature < -1.94°C) at or below the point of supercooling."
Please consider rephrasing to "… below the in situ freezing point, making the water supercooled" or similar, which is a bit clearer.

L. 265: "We plot the glider data representing the location of cold, fresh, ice shelf water on Figs. 2b, 5 and 1."
This might be a typo as Figure 1 does not show glider data.

REFERENCES

Jansen, D., Luckman, A., Kulessa, B., Holland, P. R., and King, E. C. (2013), Marine ice formation in a suture zone on the Larsen C Ice Shelf and its influence on ice shelf dynamics, *J. Geophys. Res. Earth Surf.*, 118, 1628– 1640, doi:10.1002/jgrf.20120.

Moholdt, G., Padman, L., and Fricker, H. A. (2014), Basal mass budget of Ross and Filchner-Ronne ice shelves, Antarctica, derived from Lagrangian analysis of ICESat altimetry, *J. Geophys. Res. Earth Surf.*, 119, 2361– 2380, doi:10.1002/2014JF003171.

Nicholls, K. W., et al. (2006), Measurements beneath an Antarctic ice shelf using an autonomous underwater vehicle, *Geophys. Res. Lett.*, 33, L08612, doi:10.1029/2006GL025998.

Tison, J.-L., Khazendar, A., and Roulin, E. (2001), A two-phase approach to the simulation of the combined isotope/salinity signal of marine ice, *J. Geophys. Res.*, 106(C12), 31387– 31401, doi:10.1029/2000JC000207.