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.

> *Thank you for your helpful comments on this manuscript. We have addressed your concerns listed above in the response below.*

REVIEW in DETAIL

Issues Related to the Hydrostatic Assumption and Data Use

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.

> *We realise now following these comments and those from reviewer 2 that we were not clear enough about how and why we conducted our analysis of hydrostatic balance. In Figure 2 we do apply hydrostatic analysis to REMA but this is essentially for visual purposes only and we don't*

use it for small-scale analysis within the manuscript. We do use it to discuss general thickness trends in section 4.1 but this is a large-scale (multiple kilometer) analysis and therefore smaller scale errors are not relevant. Instead, for all small-scale comparisons we used our in situ GPS surface data along with radar thickness data collected concurrently on the ice shelf. We have clarified this throughout the manuscript and in particular in the methodology where we now discuss hydrostatic correction more generally rather than only in application to REMA. In the hydrostatic equilibrium results we have clarified to say:

"Ice shelf draft estimations that are extrapolated using hydrostatic balance calculations do not take bridging stresses or pinning points into account and therefore may contain errors. Our in situ ground-based and airborne radar thickness data provide high-resolution data of ice shelf draft features and can be compared with hydrostatically-derived thickness using simultaneously collected GPS surface elevation data."

We have moved away from assessing the satellite datasets and instead focus on applicability of the datasets as suggested later in the review (also see our response to that comment below). As such we have removed section 5.5 'Dataset applicability' and moved the relevant parts of that section to other areas of the Discussion. Addressing the above comment, we have also clarified our approach to hydrostatic calculations in the new version of Discussion section 5.1, where we state that we are using in situ surface elevation data..

- 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.

Yes this is the case, but we continue in this section to explain that the surveys were conducted within a week of each other and checked with a cross-over analysis (of both the surface elevation and ice thickness). Now that we have clarified that we are discussing in situ dGPS measurements of surface elevation rather than REMA surface elevation we hope that this is satisfactory. We also point out that the differences in measured ice thickness and thickness assumed using hydrostatic calculations is on the order of 10s of meters rather than <10m.

- 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.

This is a good point and we have rephrased to clarify:

"This suggests the difference in thickness at Site 1 and 2 between measured and calculated could instead be due to bridging stresses from the highly variable basal draft and/or from pinning of the ice shelf from valley walls or nunataks. However, the nearest pinning points from Site 1 are the Teal Nunatak at the Reeves Glacier grounding line and Inexpressible Island, both ~15 km from Site 1. Given this distance, it is more likely that bridging stresses are impacting the narrower basal draft features in the suture zone of the ice shelf (McGrath et al, 2012; Bassis and Ma, 2015), although this would have to be confirmed using modelling approaches."

- 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).

Thank you for this suggestion. We have added this reference into our discussion of the differences between surface depressions and basal topography.

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.

This is a good point, and one also made by reviewer 2. In the new version of the manuscript we no longer discuss the limitations of the datasets but instead focus on how the data are utilized, as suggested. We have removed reference to this in the introduction and we have removed section 5.5 order to address this. Instead the discussion on hydrostatic balance has been moved to the discussion section on complex ice morphology (section 5.1), the discussion on strain to section 5.2 (ice shelf fractures and strain rates), and the discussion on melt to section 5.4 (ice shelf melt). All of these parts of former section 5.5 have been adjusted to fit in with their new sections and discussion focussed on interpretation of the data rather than issues with the datasets themselves.

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 surfac mass balance probably plays an important role in how the ice-shelf draft changes, not only basal melting.

Following this comment and a similar comment by reviewer 2 we re-examined our calculations of basal draft change and found that our technique was not correct and was misleading in terms of the location of most melt. We have therefore removed the basal draft relative change comparison from Figure 5b and the related discussion in section 4.3 (ice shelf melt rates). Instead of this prior focus on change in basal draft, we now examine changes in ice thickness between the sites. We have also now used the Cryosat-2 basal melt data to calculate cumulative melt rates between Site 1 and Site 3. We then compared the melt over this distance with the thickness change between the two transects at Site 1 and 3. We find that the basal melt from the Cryosat-2 analysis can explain approximately 50% of the thickness change. To investigate the remaining thickness change we run the same cumulative melt integration between these two transects for a surface melt rate of 0.25m/year (following Bromwich and Kurtz (1984), and also 0.5 m/year (following Bell et al, 2017). Adding this surface melt rate to the Cryosat-2 basal melt rate produces a thickness change within a reasonable range of the thickness change from the Site 1 and 3 radar transects. This shows the steeper sides of the suture zone are melting by ~120 m between these two transects, but only by 60 m in the center. The change in ice thickness at the suture zone center is primarily due to surface ablation but as the ice thickens towards the side of the suture zone basal melt from oceanic processes is also occurring. In general the influence of *the basal and surface melting appears to be 50/50. Following from our error with the relative basal draft calculations this new analysis has provided more accurate information about where active melt is taking place. We have adjusted the manuscript accordingly when presenting the results in Section 4.3, when discussing the role of ocean melt, and in the conclusions; we now suggest that the primary role is on the slopes of the suture zone rather than channelised in the thinnest region of the suture zone. We have also replaced the subfigure in Figure 5b with one showing the integrated basal melt, surface ablation, total melt and measured thickness change.*

We also agree many aspects could impact mass change and we have covered these in lines 219-229. By using multiple techniques to assess whether the mass change is as a result of dynamic thinning or due to melting we are narrowing the options, although it is still difficult to definitively state exactly how much melting occurs without in situ measurement by drilling into the ice shelf, for example. We directly address the potential for velocity divergence, downstream thickness advection and surface mass balance, and we believe our new analysis as described above has strengthened our arguments. Some of the causes can be discounted, such as changes in the firn air content as we are in a blue ice zone and there is no firn. And the surface river may erode some surface ice, but this is on the range of ~2m and limited to a width of <200m. It therefore cannot explain the larger scale change in mass that we observe between the sites. We don't have an analysis of longitudinal extension that may thin the ice although the longitudinal strain map in Figure 4c doesn't indicate consistent extensional strain (with instead stripes of extension and compression) which would usually be associated with enhanced longitudinal thinning. We have expanded on these various points in the text along with substantial alterations to Section 4.3 following the analysis described in the paragraph above.

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.

See above response. We have now removed these sentences from the manuscript.

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.

> *We are glad that these glider data are of interest. We are, unfortunately, limited in how much data that we can present within this manuscript as they are under review and embargoed for a different journal with focus on the oceanography conditions. A velocity instrument was not present on the glider so we are unable to present data on the speed and direction of the flow currents. We have, however, added a T-S diagram to Figure 2 demonstrating the water column characteristics for the region where meltwater was detected. We also direct the reader towards Friedrichs et al (in review) for additional data.*

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.

> *Our comparison between the location of the water and the suture zone is achieved by examining the thickness of the ice shelf upstream of the freshwater signals. We have clarified this in Figure 2 by adding arrows as suggested by reviewer 2. We also clarify where we mentioned 'lined up' by changing the wording to say:*
>
> *"The meltwater observed by the glider underneath and directly offshore of the middle of the NIS calving front appears to be spatially aligned with the suture zone. However, the range of depth in the meltwater recorded by the glider means that it is hard to determine where that water originated from, although some is close to the ice draft depth of 150-190 m in the central region (Fig. 2c)."*
>
> *The caveat in the second sentence and our change in wording now better reflects that there is some uncertainty in the origin given that the meltwater wasn't all measured directly beneath the shelf.*

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.

> *We have adjusted this sentence to indicate uncertainties in the measurements, our removal of the relative change in basal draft and replacement with the thickness change and cumulative Cryosat-2 melt analysis. It now reads:*
>
> *"Our analysis of changes in ice morphology, flux gate volume, oceanographic data of freshwater, and satellite-derived ice shelf melt suggest that active oceanographic melt may be occurring within the suture zone focused toward the thicker edge of the suture zone rather than the center, with the caveat that in situ sampling is required to fully determine the location and rates of melt."*

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.

> *Thank you for pointing this out. We have retitled that section (4.4) to say 'Marine Ice' and elsewhere change 'ice rheology' to 'ice provenance' to clarify that we are discussing how the ice was formed.*

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.

> *We have now clarified this in section 4.4 by changing "it is likely that these "echo-free zones" represent marine ice accumulation" to "it is likely that these "echo-free zones" represent marine ice formation and/or the presence of frazil ice". We then continue by clarifying following your suggestions above by stating:*
>
> *"Mapping these regions on top of the REMA hydrostatic thickness map shows they are all associated with thinner regions of ice and, in particular, with basal fractures on both sides of the suture zone along with the thinnest portion of ice in the suture zone. The lack of radar echos in these regions are likely due to the presence of unconsolidated frazil ice accumulating at the base of the ice shelf (Tison and Khazendar, 2001; Jansen et al, 2013)"*
>
> *Some of our echo free zones are full-thickness rifts originating from the Reeves grounding line (following your own work in the region) and as such, there is no meteoric-marine transition because there is no meteoric ice present. We clarify this in this section by saying:*
>
> *"On the ice surface, in the suture zone, there are many stripes of clear blue ice between larger regions of white aerated ice which also produce no radar echos. These can be traced back to the Reeves Glacier ice fall where crevasses fully fracture through the ice column, fill with sea water and refreeze (Khazendar et al, 2001)."*

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.

We agree that our meaning here was not clear and have changed to say:

"However, as this strain transition occurred in the same region as the maximum melt, this region may be associated with a reduction in buttressing on the valley walls as a result of thinning ice, compared to further upstream"

The longitudinal extensional strain regime only reached from the terminus to partially up the ice shelf and therefore it is likely inhibited by stronger buttressing further upstream.

The link between the change in strain direction (from transverse to longitudinal) and the location of the new 2016 fracture was discussed in Dow et al (2018) and is referenced in lines 416-419 where we say:

"The formation of this fracture was argued to be due to an alteration of the strain regime from transverse to longitudinal extension between 2014 and 2015, linked closely to the expansion and the calving of a fracture much closer to the ice front (Dow et al, 2018)."

As such we argue that the strain regime of the ice shelf does play a significant role in fracturing of the ice shelf and suggest that it should remain in the conclusion.

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.

Our y-axes have the thicker ice at the top and thinner ice at the bottom, therefore the suture zone is still thinner. We have added the axes labels to all three subfigures to make it clearer.

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)?

Good point! We've changed this to use the Go-Live and IPR data.

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.

We have added an extra panel to Figure 2 to show the ice velocity in the region.

Minor Issues, Typos, etc.

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

We have altered the Priestley Glacier labels and adjusted the colors to make the figure clearer.

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”.

We have changed the manuscript to refer only to ice-shelf front.

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

We now add in a label showing the location of the river in Figure 2a which will indicate the location of the surface depression. We looked at also adding an indicator of the surface depression to the satellite image on Figure 1a but it was difficult to fit with the scale of the image and the labels so in this line we now direct readers to the elevation map of Figure 2 to see the surface depression.

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.

We have changed this to ‘cross-sectional’ as suggested.

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.

We have changed to “resulting in the formation of ice shelf meltwater below the in situ freezing point (potential temperature < -1.94°C), making the water supercooled”

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.

Thanks for catching this. Corrected.