Response to reviewer comments

We have included our response to the reviewer comments below, indented in italics.

REVIEWER 1

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 and we have rearranged the manuscript which we believe improves the flow of the manuscript.*

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 section (which has now been moved to the discussion) we have clarified to say (line 325-331):

"Although the general features of basal fractures can be examined using REMA, ice shelf draft estimations that are extrapolated using hydrostatic balance calculations do not take bridging stresses or pinning points into account and therefore may not fully represent the ice thickness and basal ice morphology (Drews et al. 2015; Gladish et al., 2012; Mankoff et al., 2012; Vaughan et al., 2012). 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 surface elevation data to test this at the NIS."

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 5.2, 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 our offsets between surface GPS measurements and ice thickness are significantly more than 9m. In this version of the manuscript we removed this paragraph because we think it was misleading for our analysis with in situ GPS measurements vs. the ice thickness measurements.

- 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 (lines 345-350):

"Our in situ data calculations demonstrate that the mismatch between hydrostatic thickness and radar ice thickness is therefore less precise 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. However, the nearest pinning point is Inexpressible Island, 10 km to the north of 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 modeling 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 focused 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, in the ice melt section in the discussion (new section 5.3) and in the conclusions; we now suggest that basal melt is occurring 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 section 4.3. 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 now 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 have expanded on these various points in the text along with substantial alterations to Section 4.3 following the new 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 and have improved our analysis to take surface ablation into account.

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 2d 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 2b by adding arrows as suggested by reviewer 2. We also clarify where we mentioned 'lined up' by changing the wording to say (lines 477-481):

"Some of the meltwater observed by the glider underneath and directly offshore of the middle of the NIS calving front is at a depth that could be linked to the thinner draft of the NIS in the suture zone (150-190 m), where enhanced melt is observed both from our IPR analysis and the satellite derived basal melt rates. However, it is more challenging to determine exactly where the deeper meltwater recorded by the glider originated from (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 (lines 518-522):

"Our analysis of changes in ice morphology, flux gate volume, oceanographic data indicating the presence of meltwater, and satellite-derived ice shelf basal melt suggest that active oceanographic melt is occurring within the suture zone. Basal melt is greatest on the steep margins of the suture zone whereas surface ablation dominates mass change in the thinnest central region. In situ sampling is required to determine the location and rates of melt, for example whether it is focussed only on the keels between basal fractures."

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 moved this section to merge with the 4.1 Nansen Ice Shelf morphology to improve the flow of the paper. Elsewhere in the manuscript we have changed '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 4.1 by saying (lines 201-208):

"Comparing these regions with the REMA hydrostatic thickness map shows they are associated with the thinnest region of ice in the suture zone and with the basal fractures on both sides of the suture zone. Given the abundance of clear ice base signals in the remainder of the radar data, it is likely that these "echo-free zones" represent marine ice formation and/or frazil ice accumulation (Holland et al, 2009). The lack of radar echos in these regions are likely due to the

presence of unconsolidated frazil ice at the base of meteoric ice (Fricker et al, 2001; Tison and Khazendar, 2001). 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 (Fig. 6c). 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 (lines 486-487):

"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 ice shelf cavity walls due to 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 483-486 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 located 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. However, we have rephrased the conclusion to say that (lines 523-519):

"… it may have played a role in active fracture in a higher melt region within the suture zone of the Nansen Ice Shelf"

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 terminus.

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 and the suture zone in Figure 2a. 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 (line 70).

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.

REVIEWER 2

Dow and colleagues present a multi-source dataset consisting of satellite data, airborne and ground-based radar and oceanographic data to describe the spatial variations in ice shelf draft, strain rates and relate them to sub-ice shelf melt and ice rheology. Finally, they make statements on the applicability of the remote sensing data sets to derive conclusions about the presented results.

GENERAL COMMENTS

The topics of understanding ice shelves by analyzing the spatial variations in draft and strain rates is a highly relevant topic with interesting results and the presentation of the new measurements is highly valuable for the research community. Yet, I think the current version of the manuscript shows still some major shortcomings (see detailed description below) that should be tackled.

In my opinion the main issue of the paper is the following:

The main conclusion of the authors is that they stress the limitations of the high-resolution data sets for deriving conclusions about ice shelf characteristics and I do not agree with this conclusion at all. It is true that their analyses show limitations, but this is might not be caused by the data itself but instead by the assumptions of the authors when using the data. For both the hydrostatic balance assumptions for REMA and the strain rates I can identify potential methodological inconsistencies that could affect the conclusions drawn in the paper (see more detailed comments below). Addressing this main comment will result probably in a highly changed paper where many of the results and conclusions need to be adapted.

Thank you for your constructive comments on this manuscript. Reviewer 1 also pointed out that we misrepresent issues with usage of data as problems with the data itself rather than its application. 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 new discussion section focused on hydrostatic equilibrium (section 5.2), the discussion on strain to section 5.3 (ice shelf fractures and strain rates), and the discussion on melt to (the new) section 5.5 (ice shelf melt). All of these parts of former section 5.5 have been slightly adjusted to fit in *with their new sections and discussion focused on interpretation of the data rather than issues with the datasets themselves.*

We have directly addressed your comments on our usage of REMA and strain rates below. In short, we very minimally use REMA in this manuscript, with our hydrostatic analysis focused on in situ surface elevation data from GPS collected simultaneously with our ice penetrating radar thickness data. We were not sufficiently clear about this in the manuscript and have corrected this. However, this removes concerns about directly analysing REMA data for hydrostatic calculations. In terms of strain data we agree that the datasets have associated errors but we have expanded upon our explanation of the application demonstrating that the patterns we observe are consistent across multiple years. We are also interested in the pattern rather than the magnitude of strain for this manuscript and therefore we suggest the consistency in strain patterns over multiple years supports our arguments.

SPECIFIC COMMENTS

* It is often difficult to follow the structure of the paper with several sections that seem diluted between methods, results and discussion. Therefore, it is difficult to distil take-home messages from the paper and/or get an overview of the impact (e.g. I find it very difficult to summarize the paper). I think that by re-organizing the method, results, discussion in single sections on morphology, strain, melt and rheology would improve the readability a lot. Now for example, for the ice morphology section it is not very clear and rather arbitrary where the site description ends, where the results start and what the discussion is. The switch between results and discussion often feels very arbitrary and makes it difficult to follow as the flow is interrupted.

Thank you for your suggestion. We are following standard Cryosphere guidelines for paper layout, beginning with introduction, study area, methods, followed by results and the discussion. It would also be challenging to separate these entirely as some of the analysis for each method is included in the same discussion section. However, we acknowledge that were unclear in some areas about how different sections connected together, and also had some overlap between results and discussion. Therefore, we have significantly reorganised the paper and we believe the new layout makes it easier to follow.

In the results section, we add the marine ice section to the ice morphology section (4.1) and move some of the former ice morphology section to the discussion (section 5.1). For example, we no longer talk about the reasons for thin ice in the suture zone in the results section but reserve this for the discussion. Also in the results, we've moved the Oceanography data section to the ice shelf melt section (4.3) as they are addressing the same issue. Finally, we have moved the hydrostatic equilibrium section from the results to the discussion (new section 5.2) to avoid discursive analysis within the results section.

* The abstract seems to read as a loose collection of individual sentences, which make it difficult (for me) to follow. I think that using some bridging words/terminology between the sentences would

increase the readability. *Note: these are a subjective comments (my apologies) and I do see that R1 did make a more positive comment on the writing, so do not see it as hard advice. Nevertheless, it might also be an indication that it might be difficult for others to follow.

We have examined the abstract within the space constraints and believe that our current approach combines sufficient information and the main points of the paper. Given the support of reviewer 1, and your subsequent comment that your suggestions were facultative, we did not rewrite the Abstract. We have altered it slightly, however, to account for our move away from examining the applicability of satellite datasets.

Many of the results paragraphs are very descriptive paragraphs with results that are difficult to "see" in the figures. I think it would be beneficial if the figures would use direct labelling instead of looking them up in the caption which requires zigzagging between the figure and label. By using text, arrows etc. to indicate where to look, it will be way easier to interpret the figures.

We have added text and arrows into Figures 2 and 4 to aid with this including labelling the fracture and the river location. We have also added a legend to Figure 1.

L51-54 "*Around the time of this event, the ice surface strain patterns changed from extensional across-ice to extensional down-ice within ~8 km of the calving front and drove the formation of a new fracture over the thinnest region of the central NIS*"-> Not clear if this a new result or part of Dow et. al. (2018). The terms across-ice and down-ice are also very difficult to interpret as I would guess this is about the flow and not the ice. Perhaps use across-flow and down-flow?

Yes, this is part of Dow et al (2018) and to clarify we have added in the reference again at the end of the sentence. We have changed the wording to across-flow and down-flow as suggested.

L59-60 "*we make recommendations of where and when satellite data is sufficient to analyse ice shelf properties without in situ data.*" I do not agree with these later recommendations (see my later comments) as this is not about the data but about the methodologies and assumptions behind the methodologies. This is therefore largely a data handling problem and not a data problem.

We agree with your comments and have removed discussion of this and have deleted this sentence from the introduction (see our response in the general comment section).

L74-76: "A 30-km long depression + a river was observed + transverse fracture" -> if these features are important, they should be drawn and labelled (directly) on the map in Fig.1.

As Figure 1 is to introduce the location of the NIS and the radar survey lines, we have instead added the rive, suture zone,r and fracture labels to Figure 2, which includes the ice surface and thickness information and have referenced Fig 2a in this sentence. We experimented with adding these features to Figure 1 but it became too crowded.

Fig.1: perhaps direct labelling instead of the caption as it requires the reader to unnecessary zigzag between figure and caption .

We have added a legend to describe the radar lines in Figure 1.

The site description could be integrated with the section 4.1 to join similar information together.

We experimented with this but struggled with this setup because the hydrostatic calculations have to be introduced before we can discuss the ice morphological features. We also feel like we can't move the site description entirely as this is part of the introduction to the region. As such we have retained our study site section.

L102 "*it is sufficiently sparse that DEMs interpolated from these data have significant errors in regions that lack high data density*" -> complicated sentence for simple thing, perhaps rephrase to "*it is too sparse to accurately interpolate*"

We have changed as suggested.

L103 If REMA strips (and not the mosaic) are used, then they lack bias and tilt corrections (which could be significant), tide corrections, inverse barometer effects etc. All these corrections are necessary to draw any conclusion on the ice draft etc. Just assuming the strips are correct is methodological not-correct (which can explain many of the later critique on the REMA data) as these raw strips were never provided to be used without the necessary corrections and without them the absolute elevations (and hence drafts) cannot be interpreted

We chose to use the strips rather than the mosaic because they span a shorter time scale than the mosaic. However, given your advice, we have changed to the mosaic. As mentioned above, the DEM and ice thickness information from REMA is used very minimally in this manuscript. All analysis that we do for hydrostatic corrections are from our in situ surface GPS and radar thickness data. We do use the REMA thickness DEM 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. As such, any error from using the REMA strips rather than the mosaic would have only been observable in Figure 2 and would not have any impact on the remainder of the manuscript. We realise that we were not clear about this in the manuscript and have adjusted accordingly. In the methodology we now discuss hydrostatic correction more generally than only application to REMA. In the hydrostatic equilibrium discussion section we have clarified to say (lines 326-331):

"Although the general features of basal fractures can be examined using REMA, ice shelf draft estimations that are extrapolated using hydrostatic balance calculations do not take bridging

stresses or pinning points into account and therefore may not fully represent the ice thickness and basal ice morphology (Drews et al. 2015; Gladish et al., 2012; Mankoff et al., 2012; Vaughan et al., 2012). 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 surface elevation data to test this at the NIS."

We have also further clarified in the new version of discussion section 5.2 where we state that we are using in situ surface elevation data when we are discussing hydrostatic calculations.

L105: What was the offset between the strips? How was it corrected?

As we have switched to the REMA mosaic as suggested we don't address this.

L109-114: In my opinion the assumption of pure blue ice is a wrong assumption as the ice shelf locally (especially in the channels) seem to contain snow/firn cover. When this snow/firn is not taken into account any conclusion on bridging stresses is potentially wrong (see also later).

As we state in the manuscript, firn is present on the ice shelf closer towards the Reeves ice fall and towards Inexpressible Island. However, where we do the hydrostatic analysis at sites 1-3 there is no firn because of the katabatic winds that strip any accumulation. We can confirm this because we ran these radar lines on foot so we were able to directly observe the surface conditions. This can also be seen in the photos in Figure 6. The whiter areas are solid, but bubbly (meteoric) ice, whereas the very blue areas are solid refrozen ice. We discuss this in lines 77-79 where we say:

"This polynya plays an important role in sea ice production for the Ross Sea (Stevens et al., 2017) and is formed by katabatic winds, which also strips the NIS of much of its snow and firn cover leaving a significant portion of the surface as blue ice (Kurtz and Bromwich, 1983)."

and also in lines 110-112 where we say:

"The zone of firn-free blue ice covers the regions of ground-based radar survey but firn is present towards the grounding line of Reeves Glacier and Inexpressible Island, therefore in these regions the hydrostatic calculations are less accurate."

Fig.2: perhaps use direct labelling for the green/yellow circle and lines instead of the caption as it requires the reader to unnecessary zigzag between figure and caption.

As we now label the fracture following your above suggestion, we have removed the yellow circle. The green circle is relevant to aspects that we discuss in the text but would be too long to explain in the caption and so we retain this. We do, however, add that the text description of the green circle can be found in section 5.1 of the manuscript.

Fig.2: it took me some time to understand that the red-dashed line was moved to overlap draft with meltwater. It could be helpful to indicate that in the figure (e.g. with arrows)

Thanks for this suggestion. We have made this change.

Fig.3: the alignment for REMA is potentially problematic if the proper strip corrections are not applied

As explained above, these are radar lines and hydrostatic thickness from our surface GPS records that we collected during the radar surveys. We do not use REMA here. See above for where we have clarified this in the manuscript.

L150: what about the spatial and overall accuracy of GOLive data? Small inconsistencies in the velocity data and/or geometric accuracies could have large impacts on the interpretation of the strain rates and I think this should be accounted for in the later analyses. Just assuming the velocity (which may be a wrong assumption) are correct can result in the observed misrepresentations (see also later comment)

While GOLive has inherent error, as does any derived velocity product, the correspondence between the topography and the strain data which follows known ice shelf strain physics gives us confidence that this is a realistic representation and does not impact our analysis. We have discussed a pixel size sensitivity test to determine that the strain signals are not corresponding to one pixel which could cause error. We now include more information about the error that this could create in the magnitude of the strain values. We also point out that these patterns in strain are visible over multiple years and multiple image pairs as we state (lines 221-223):

"We note that the patterns of strain presented here are also visible in NIS strain rate maps from multiple years, at different times of the year, and therefore appear to persist over time (e.g. see Fig. 5 in Dow et al, 2018)."

In this manuscript we are interpreting only the pattern of strain for our discussion rather than the magnitude and we therefore believe that this is sufficient evidence to support our arguments.

L160-161 " which produced similar results in both spatial pattern and magnitude of strain rates": what is similar? What are the differences? Perhaps quantify etc.

We have now clarified by stating the error in strain produced between the 300m and 600m pixel test (lines 159-161):

"We ran a sensitivity test with a length scale of 600 m, which produced similar results in the spatial pattern of strain, but with the magnitude of strain rates smaller in the suture zone of NIS by ~0.08 year^{-1} or less."

These are minor changes in the strain compared to the contrast between extensional and compressional strain in this region and we are primarily interested in the pattern of strain for this study as discussed above.

Section 3.5: these are very interesting data and should be elaborated further. How was the meltwater classification done?

We have now added a temperature-salinity plot into Figure 2 to demonstrate how the meltwater classification was completed for this study. This plot shows practical salinity versus potential temperature for the data presented in Fig. 2c. On this plot, we note the isotherm beneath which water is classified as Ice Shelf Meltwater and reference this in the text where we first discuss the classification.

We also further clarify in the ocean glider data section in the methods (section 3.5) where we say (lines 174-177):

"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℃), making the water supercooled (Fig. 2d). We therefore take the presence of this supercooled water as evidence of ice shelf melt."

Section 4.1: this reads very much as a continuation of the site description and could perhaps be integrated

This is the only part of the manuscript where we utilize REMA hydrostatic calculations and we do this to examine larger scale changes on the scale of multiple km. As such, errors inherent in REMA hydrostatic inversions will not impact this analysis. The section however, is discussing larger-scale variations in ice thickness which would not be possible without the hydrostatic calculations. Since we have to introduce how we apply hydrostatic calculations prior to discussion of these results, we therefore retain this section in the results part of the manuscript.

L175-185: these features are difficult to find and see on the map for a non-experienced reader. Would be a good idea to help the reader and indicate all the described features on the map.

We have added labels showing parallel and oblique features to Figure 2 to address this.

L197-199: "*Along the center of the suture zone there is an alternating region of horizontal compression (red) on the northern side and extension (blue) on the southern side: both regions have widths of ~800 m. When compared with the ice shelf draft, the switch between compression and extension occurs at the apex of the thin-ice suture zone region (Fig. 4d)*". I do not agree with these statements as I think it is almost impossible to interpret the strain locations relative to the radar given i) that the strain rate is only calculated every 300m and ii) the potential (spatial) uncertainty in the velocity data. For example, shifting the compression peak 150m to the right (which seems well within the strain uncertainty) would result in a compression peak that is nicely aligned with the apex (which would also make more sense given the discussion later):

Yes, it is certainly possible that the strain data are not exactly aligned with our radar data, although the excellent correspondence between smaller extensional and compressional regions with the topography (e.g. at -500m and 1.5km) suggest that the alignment is generally very good. We now discuss the potential offset by saying (lines 225-227):

"When compared with the ice shelf draft, the switch between compression and extension occurs at the apex of the thin-ice suture zone region (Fig. 4d), although with a strain pixel size of 300 m there may be some error with the location of this transition."

L199-200: "*the switch between compression and extension … is limited horizontally*" This switch is by definition always localized to a point (as there is either compression or extension) so it seems a strange statement

We agree that this was not a well-phrased statement. We have removed it to avoid confusion.

L200-206: any of the conclusions based on the location of patterns of strain vs. apex-keels are debatable as these patterns can be easily (mis-)aligned when using small shifts (which seem within the uncertainty of the data). I therefore do not think statements on alignment can be made and the assumption that it can be derived from satellite velocities is potentially overambitious.

As we include in the discussion, there are physical reasons to back up the observations here along with records at other ice shelves. Namely, surface depressions coincident with thinner ice/higher basal draft tend to have compressional regimes and surface hills coincident with thicker ice/deeper basal draft are extensional (following, for example, Vaughan et al 2012). We also present the strain in 2D (Fig 4b) to demonstrate that the larger scale pattern follows the surface topography (Fig 4a). We have added in a caveat about the pixel size for the transition zone between compression and extension in the thinnest region of the suture zone; here we find that applying a shift in the strain plotting by +- 300m can impact whether this apex is primarily compressional, extensional, or both (see above comment for our text adjustment). However, with an error of +- 300 m the remainder of the strain results that we report along this transect are consistent due to the larger scale variation of the basal draft.

L224-227: "*The northern side of the suture zone has minimal change in relative basal draft but approaching the apex of the thin ice region, there is substantially more ice loss. The greatest mass loss is in the highest apexes of the central suture zone and the basal fractures on the southern side, with the keels of the latter relatively unchanged compared to the spatially-constant background melt rate."* It might be due to my misunderstanding of the methodology of alignment, but I do not necessarily agree. If you align the draft for the apex (instead of the edges (see example below)), you could conclude that the largest changes occurred at the edges and not at the apex.

This is an excellent point and we completely agree; thank you for pointing this out. This was a mistake on our part in the application of the relative change calculations. The change could be on the margins compared to the central suture zone and our method did not address this. We have removed the plot of the relative basal draft change from Figure 5b and the related sentences in section 4.3. Instead we now focus on the change in thickness between the sites, which does not demonstrate that the central suture zone has such significant change in shape as we previously argued but instead shows the steeper margins of the suture zone is where the most active thickness change is taking place. Following your suggestion we have further analysed this using the Cryosat-2 basal melt data along with estimations of surface ablation, which supports this new argument (see our more detailed response below). We have also therefore altered our statements about where melt is most likely taking place in the results, the discussion and the conclusion.

Fig.5. It is very difficult to interpret the relative basal draft (what is it, how was it calculated/quantified)

See above. We have removed this plot and the related discussion.

L239-246: the analysis of the melt rates derived from Cryosat-2 is potentially very interesting and should be elaborated on further. What would be the total melt of transect of C-C' was advected (with the velocity) to A-A'? Would this integrated melt show a similar (smoothed) pattern as the simple observed difference between Site1.

We have performed this analysis as suggested with integration of melt rates between A-A' and C-C'. 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 latter 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 discussing the role of ocean melt. We have also replaced the subfigure in Figure 5b with one showing the integrated basal melt, surface ablation, total melt and measured thickness change. Thank you for this suggestion for additional analysis as we believe it has strengthened the manuscript.*

L249-251: "*Mapping these regions on top of the REMA 2016 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*" Where can I see this? It would be useful to replace Fig.6a with this overlay over REMA

We have changed the wording to 'comparing these regions with the REMA hydrostatic thickness map'. We tried many methods to combine the two datasets for clear visual comparison by the reader but the results were very messy and difficult to analyse. This is why we include the surface elevation map of REMA in Figure 4a to allow visual comparison between the elevation (and therefore ice thickness) and our strain plots in 4b and 4c.

Section 4.5: although the data are very interesting I do find it very difficult to see any conclusion, take home message from this section. Here again it would be beneficial if integrated with the discussion section to remove the fragmentation and increase the impact

We have removed this section and added it into the ice shelf melt section (4.3) as it directly relates to our analysis of ocean vs. surface ablation for mass change in the suture zone and therefore has more impact when included in this section.

Section 4.6: One of the main potential errors in the hydrostatic assumption is that the ice shelf is completely snow/firn free, whereas for example Sentinel-2 data shows that there is snow deposition/firn over transect A-A' which could provide an explanation for the lack of /muted channels in the hydrostatic REMA approximation. Especially as the snow/firn cover seems stronger in the south where larger offsets in the Dow analysis occur.

As above, we don't use REMA for hydrostatic analysis in this manuscript. We know that the sites that we do these calculations (Sites 1-3) are snow/firn free because we are using radar and surface data that we collected on foot in these locations.

In addition, if there was the presence of firn/snow in the southern region, it would act to reduce the total density of the column, resulting in thinner ice for a given surface elevation. As shown in

Figure 3, our offset issue is that measured ice thickness is greater than hydrostatically calculated ice thickness and therefore if firn were present it would exacerbate rather than reduce this offset.

L296-304: I do not agree with any of this paragraph as (also indicated earlier) i) the REMA strips require several corrections (tides, tilt+offset effects, barometric effect) before they allow to convert to draft ii) the snow/firn could result in local biases as well. Both these forgotten corrections makes the interpretation of the hydrostatic figures very much dependent on potentially wrong assumptions as both offsets+snow could result in similar results. Therefore the conclusion of bridging stresses is not necessarily supported here.

See above points. We do not use REMA – this was our fault for not being clear in the manuscript but we have now corrected this.

Section 5.1 reads very much as a continuation of the results of 4.1 and perhaps it should be considered to be integrated.

See above response. We have moved some of the results in 4.1 to this section to more clearly distinguish between results and discussion.

L335-339: many of the statements (e.g. alignment, bridging stress) are not necessarily supported by the results (see my earlier comments) and therefore I doubt the correct interpretation of this paragraph

See our above responses to your comments. We are confident in our results and therefore in this Discussion section.

L435 " *we find that some of the ice shelf properties are not well represented in the satellite-derived data sets*.": see my earlier comments, but again I do not think it is a fair comment to blame the data. These mis-representations are either the potential result of wrong assumptions (e.g. for hydrostatic balance) or by using the data (e.g. strain rate) without accounting for inherent (spatial) uncertainties that should be accounted for.

We have deleted this paragraph and section and have moved the discussion to other, more relevant sections.

L436-451: I do not agree (see my earlier comments)

See above responses.

L455 "*If only longitudinal strain had been calculated, these features would have been missed*" Yes, but why would you only calculate longitudinal strain and neglect transverse strain? This is again not a problem of the data, but of a potential wrong assumption (fracture dynamics rely only on longitudinal strain).

> *In previous publications (e.g. Lai et al, 2020) only the longitudinal strain was used for analysis and here we agree that using transverse strain is useful, and therefore important to point out. We have moved the components of this section to their respective discussions so the focus is no longer on the datasets, but instead on their applicability.*

TECHNICAL COMMENTS

L20 "*Nansen Ice Shelf has a highly variable* morphology"

> *Done*

L41-45: "*The increasing variability of … opens up possibilities*"

> *Done*

Caption Fig.2 "*The green circle highlights an area referenced in the text*" This is a rather non-helpful caption as I now still don't know what I am looking at (and why?) and requires me to go and search the paper

> *See above response to this comment.*

Fig.2 and later figures: I do find it confusing that S1-S2-S3 and A-A', B-B', C-C' are used interchangeably. Would be clearer with consistency throughout all figures etc.

> *We retain our labelling because A-A' etc only show one radar transect at each site. As we have multiple radar transects at the sites and may wish to refer to them in future publications we keep the site names and specify individual transects within each site using the capital letters.*

Line 166: difficult for non-experts to see this suture zone. Perhaps direct label the suture zone on Fig.2?

> *We have added labels to Figure 2 to direct readers to the location of the suture zone.*

Fig.4: it would be beneficial if the strain rates were overlaid (semi-transparent in color) over the REMA DEM (e.g. in grey) as it would allow to link the strain to the DEM. Now it is basically impossible to see direct linkages between the different panels.

See above response to this comment

Fig.4 colorbar: it would be beneficial if compression and extension is directly labelled on the colorbar as it would make things clearer without the need to read the entire caption.

We have added 'extension' and 'compression' on the colorbar to clarify.

L194: "*The extent of the region in Fig. 4b is shown in Fig. 2*". Sentence is obsolete in the main text (can be part of caption) and breaks the flow.

Moved to caption as suggested.

Fig. 5b: perhaps add transect B-B' (S2) to allow the reader to check for temporal consistency etc?

We have removed this subfigure (see above responses).