Response to reviewer and editor comments

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

……

Thank you for submitting the revisions tc-2021-168. In the meantime, I have received two re-reviews. Both reviewers acknowledge your responses and note corresponding improvements in the revised version. I add to this, that the paper presents a strong observational dataset of airborne and ground-based IPR with much scientific potential.

Unfortunately, a number of critical comments have been raised again and the overall impression of the paper from both reviewers remains weak. I concur with the majority of the concerns raised, and below I also provide some additional thoughts from my own reading. There is a lack of clear take-away messages and some claims made in the paper are not fully substantiated by the data analysis.

> *Thank you for your comments on the manuscript. We have addressed your comments below and have altered the paper accordingly. To first address your concern listed above about the lack of a clear take-away message the primary outputs of the paper are as such:*
>
> 1. *In situ radar data provide evidence of highly complex topography at Nansen Ice Shelf.*
> 2. *This complex topography is reflected in strain rates observable at the ice surface.*
> 3. *The cause of the complex topography is a combination of fracture mechanisms at the suture zone plus active melt in the suture zone.*
> 4. *This creates a weak environment, susceptible to fracture.*
>
> *The unique nature of our dataset (particularly the combination of radar and glider ocean data with satellite data) is highly relevant to the Antarctic science community and our findings at Nansen Ice Shelf are important for examining other ice shelves, particularly for those that don't have radar data and rely only on large-scale datasets such as REMA. We have adjusted our introduction and abstract accordingly to make these aims and findings clearer.*

Hence, the manuscript is not ready for publication in The Cryosphere. Substantial revisions are required including data analysis. If you decide to address this in another round at TCD, I will send out the paper for another review including at least one new reviewer. Alternatively you may want to consider to fully restructure the paper with a clearer research focus and make it a new submission. Both options require a significant amount of work, but given the reviews and my own reading I don't see a way around this. I hope that you perceive the remarks as constructive and helpful for improvements.

> *In the response to reviewers below we detail how our plan to substantially revise our manuscript and provide additional data analysis will address your concerns and make our work publication-ready for The Cryosphere. We thank you for this opportunity.*

The comments mentioned below are in addition to the two reviews, they are not intended to replace any of the comments from the reviewers.

Lack of basal radar reflections:
I am not convinced that frazil ice accumulation is the only explanation for the lacking ice-ocean boundary in the radar data. Did you consider the basal slope? In Figure 6d I cannot estimate the slope because the x-label is given in Trace number (should be 'distance') and the y label in traveltime (should be 'depth'). Do you have an example where the lack of signal occurs in both airborne and ground-based IPR? This could be helpful to investigate effects of the different radar systems. (Fig 6a suggests that the lack of basal reflections does not occur in the spatially more extensive airborne dataset. Is this linked to the different radar systems used?).

> *We have adjusted Figure 6d so that it plots distance and depth rather than trace number and traveltime. There is also drop out in the same regions in the airborne data and we have added this to Figure 6a. This is a useful addition because it further demonstrates the link between the stripes of clear blue ice regions and the radar dropout that we discuss in relation to the ground based radar data. Given that these are different radar systems this strengthens our argument that the dropouts are due to frazil ice accumulation rather than slope because of the differences in Fresnel zone. We have also adjusted the caption of Figure 6 accordingly.*
>
> *If we were to do a basal slope analysis we would likely find a correspondence between slope and radar drop out because, as we argue in the paper, the drop-out is due to marine ice accumulation in basal crevasses or full-thickness rifts, which by definition will have steep slopes. Our argument is based on marine ice rather than basal slopes alone because we see the correspondence between ice type (the clear blue ice that can be seen to form, for example, in full ocean rifts at the base of the Reeves Ice Fall) and radar dropout. This correspondence can even be seen at the satellite level as demonstrated in Figure 6. Our new addition of the airborne radar dropout as you suggested will strengthen this argument. Finally, we do note that not all basal reflections with steep slopes have drop-out. For example, see Figure 3a – the feature with the green dot is fully visible but the feature with the red dot drops out. These have similar slopes so the slope alone is not the cause for drop-out.*

Flux gate analysis:
Why not close the gates (i.e. consider the flux into and out of a box)? Given that the flux differences between some gates are small, it seems important to exclude influx/outflux of ice between the two main gates. Neckel et al., 2012, J. Glac. would be an example for this (no need to cite this paper of which I am a co-author).

> *Thank you for this suggestion - we now use a polygon based 'closed box' flux following the methods of Neckel et al. With this method we see a volume loss of 0.0523 +/- 0.0083 km3/a, or 0.0476 Gt/year in terms of mass loss. We had an error in our flux gate code that meant we were underestimating the mass loss in our previous version of this paper.*

From thickness change to basal/surface melting:
I stumbled across section l. 252f because at first I didn't grasp the meaning of 'vertical melt rates'. I believe it is a lumped term that should include either basal or surface melting, but this was not clear to me at first and others may feel the same way.

We have removed this paragraph as stated below. We have changed the wording of 'vertical melting' to be clearer that we mean lumped basal and surface melting.

I am not convinced that the points marked in Fig. 3 belong to the same draft apexes and keels (i.e. that they are part of one feature). More radar lines would be needed to trace this coherently. Therefore, I am unsure about the inferences drawn starting l 252f.

We agree that this is too speculative and have removed from the manuscript.

Later on, I miss a number of details about how the mass budget was closed. Do you assume steady state and is that justified given current dH/dt estimates for the NIS? Very likely many of the smaller-scale features such as the head of ice-shelf channels or basal fractures are not steady-state features in an Eulerian sense an so I don't quite understand how the analysis works. I was entirely lost at the statement that "..100% of the thickness change in the suture zone center can be attributed to surface ablation processes." (l. 281). How is it possible that strain thinning plays apparently now role in the thickness change for an ice shelf?

We have rewritten this paragraph to clarify how we made the calculations (pasted below) including how the mass budget is closed in our analysis. While the basal fractures are not steady features (and therefore we can't assume that the features at site 3 were once the exact same thickness as at site 1) we address this by calculating a smoothed thickness change using a locally weighted least squares regression filter. We have improved this comparison compared to the previous version by smoothing the individual site radar thickness measurements prior to estimating the thickness change rather than smoothing after estimating the thickness change.

The point about strain thinning is well taken and we have added this to the analysis, however, the strain thinning is relatively small (on the scale of meters rather than tens of meters), which is consistent with our reported strain rates in Figure 4, and our conclusions do not change (see below). We are following the methodology of Das et al (2020) who use a similar approach and we now direct readers to their equations as well. If clearer we can move the first section below to methods rather than results. We also now acknowledge that there may be change over time of basal and surface melt rates but we have no ability to determine this information (and neither can anyone else who calculates basal melt rates with a spatial analysis unless the ice shelf is changing very rapidly such as at Pine Island). Das et al (2020) for example have the same limitation in their analysis and estimates of thickness change at Ross Ice Shelf and consider regions of flux that take 60 years. This can be compared to our estimate region of consistent melt over 120 years, which is a limitation of the analysis but likely not unreasonable for a cold cavity ice shelf. The primary purpose of this analysis is to examine the relative importance of basal melting, surface melting and strain thinning in the changing thickness of the ice shelf. The figure that we have produced demonstrates that, while thickness change in the central portion of *the suture zone could be attributed solely to surface melt, the edges of the suture zone have thinned enough to suggest basal melt is playing a role.*

We have included a reformatted figure showing the thickness change estimates from all the sources we analyse, now including strain thinning and our improved smoothed thickness change. We have changed the presentation to show thickness change in the negative (more negative = greater thinning), as this is perhaps easier to visualise for the reader. We are happy to revert to the previous presentation if preferred.

Our adapted paragraph is below:

*"*It is possible that mass loss is due primarily to surface melt and sublimation, which was estimated by Bromwich and Kurtz (1984) to be 0.25 m a^{-1} and by Bell et al. (2017) to be 0.5 m a^{-1}. To investigate this and where basal melt may be occurring at the ice-ocean interface, we extract stream lines between Site 1 and Site 2 and apply Langrangian methods as described in Das et al (2020) to estimate strain thinning, basal melt and surface melt along those streamlines:

$$\Delta H = \int_{t1}^{t2} -w_b - w_s - H\nabla.\boldsymbol{V}dt$$

where w_b is basal melt, w_s is surface melt, ∇ V is the ice velocity divergence from horizontal, and t_1 and t_2 are the times that ice passes through Site 1 and Site 3. Cumulative basal melt along the streamlines, $w_{b,}$ is calculated from the Cryosat-2 basal melt dataset (Adusumilli et al., 2020). However, the basal melt map has data up to 2 km away from the Site 3 transect (Fig. 5a) and so cumulative basal melt will be slightly underestimated using this technique. Similarly, we calculate cumulative surface mass loss, $w_{s,}$ between the two sites using a uniform surface ablation rate of 0.25 m a^{-1} and 0.5 m a^{-1}. Because the region we are examining is composed of exposed blue ice we do not take accumulation into account. Velocity divergence for strain thickness change is calculated from ITS_LIVE 2018 ice surface velocities and flow vectors (Gardner et al., 2020; Fig. 5b). This analysis assumes steady state surface melt, basal melt and ice velocity. The average time for ice to travel between Site 1 and Site 3 is 120 years, which means that some alteration of these rates may have taken place producing errors in our analysis, although with a cold cavity this is likely less of an issue than in regions of the West Antarctic for example (Das et al, 2020). We compare the values of cumulative basal and surface ablation and strain thinning with the IPR-derived thickness change between the sites. Because of the presence of basal features that are not fully aligned between the sites, we first smooth the thickness at each of Site 1 and Site 3 using a locally weighted least squares regression filter before calculating the thickness change.

The average thickness change between Site 1 and Site 3 is 80m (Fig 5b). The cumulative basal melting analysis suggests that, because of initial freezing upstream in the middle of the suture zone (see Fig. 5a), the central region of the suture zone has net zero change in basal melt. However, the steeper margins of the suture zone have sufficient basal melt to account for ~20m of thinning at the ice base between Site 1 and Site 3. If we combine this thinning from basal melt with a uniform surface ablation rate of 0.25 m a^{-1} it produces ~50 m less thinning compared to the measured thickness change. However when the thinning from basal melt is combined with a thinning due to a uniform surface ablation of 0.5 m a^{-1}, there is a good correspondence between the estimated cumulative thinning rate and the measured thinning, particularly at the northern margin of the suture zone. Thickness change due to strain thinning is limited to <13m between Site 1 and Site 3. This analysis suggests that basal melting is an important process for thinning on the slopes of the suture zone but less important in the center of the suture zone."

Derivation of the hydrostatic ice thickness:
This has been done many times and I understand the desire to not dwell on this topic for longer than needed. However, given that you compare the hydrostatic ice thickness with IPR thickness and you use it to derive basal melt rate fields, it suggest to get both relative and absolute error estimates. Your estimate of \pm 10 m (l. 114) contains only a \pm 1 error in surface elevation. There are other sources of uncertainties such as:

Our application of the hydrostatic thickness calculation is merely to compare with the IPR thickness as a matter of discussion about whether hydrostatic thickness can be misleading in the absence of radar data. We do not use hydrostatic thickness to calculate basal melt rates. Cumulative basal melt rates are calculated using the Adusumilli dataset, which are compared with the change in thickness between site 1 and site 3, both of which have thickness determined from the IPR.

The argument about the difference in thickness at site 1 between the hydrostatic and measured ice thickness is a very minor part of the manuscript. Our argument is instead focussed on the lack of highly variable basal topography observable in the site 1 hydrostatic dataset compared to the measured IPR basal topography.

As we are unable to correct for tide given the lack of a detailed sub-fortnightly tidal model in this area, and we cannot access mean dynamic topography corrections (see below), we will state in the manuscript that the offset in thickness can be corrected with an additional 2.5 m added to the surface elevation recorded at the radar and that this may be due to tides, mean dynamic topography or pinning and we are unable to extract the exact cause. We do believe that this is a real signal but it has so little impact on the manuscript arguments that we will include the above caveats and it will not be discussed further in the manuscript. We have included the following wording in section 5.2:

"At Site 1 and 2, further upstream, there was a vertical offset where the ice thickness measured using the radar was greater, on average, than the ice thickness derived from surface elevation measurements (Fig. 3). This offset can be corrected by raising the surface elevation by an additional 2.5 m and, given that we have not corrected for Mean Dynamic Topography (Griggs and Bamber, 2009) or tidal variation, some of this offset may be explained by these factors. Regardless of the mean thickness offset, the spatial pattern of hydrostatic thickness at Site 1 and 2 also does not accurately reflect the variable topography in this region."

• What about the uncertainty of ice density? A marginal difference (let's say 915 instead of 917 kg m^(-

3)) already translates into 4 m for a freeboard height of 20 m. a. s. l. . Similar for the ocean water density.

Yes, this is true but it is highly unlikely that ice density will change between our Site 1 and Site 3 locations. If we choose a different ice density for Site 1 to better match the ice profiles, the resulting profile won't match in Site 3. This is a case of using our prior knowledge about where hydrostatic balance is most likely to work i.e. as far from pinning points and the grounding line as possible and using this to calculate the best match of ice and ocean density. For the ocean water density, we use glider measurements taken near Site 3. However, as discussed above we are now not discussing the thickness difference between Site 1 and Site 3 in the manuscript.

• It appears that firn thickness at NIS increases with decreasing distance to the grounding-line. This will result in relative errors which may play into your basal melt rate calculations with similar arguments for the density as mentioned above.

As we state several times in the manuscript, the ground-based radar data are recorded on blue ice. The firn thickness may impact errors for a basal melt rate calculated using REMA near the grounding line for example, but since we don't do this and use our more accurate radar data to calculate basal melt rates and surface GPS data at the radar sites to make the hydrostatic calculations this is not an issue.

• How good approximates the GL04C sea level in this area?

In the appendices we perform an error analysis on the cross-overs between the IPR and the airborne radar. The former was corrected to GL04C and the latter was corrected based on measured sea level (as there were flight lines over the ocean). Therefore our error analysis has included possible error with the GL04C.

• Other studies (e.g., Shean et al., 2019, https://doi.org/10.5194/tc-13-2633-2019; Griggs & Bamber (2009, https://doi.org/10.3189/002214311796905659) consider effects of mean dynamic topography and inverse barometer effects. Is this irrelevant for the NIS?.

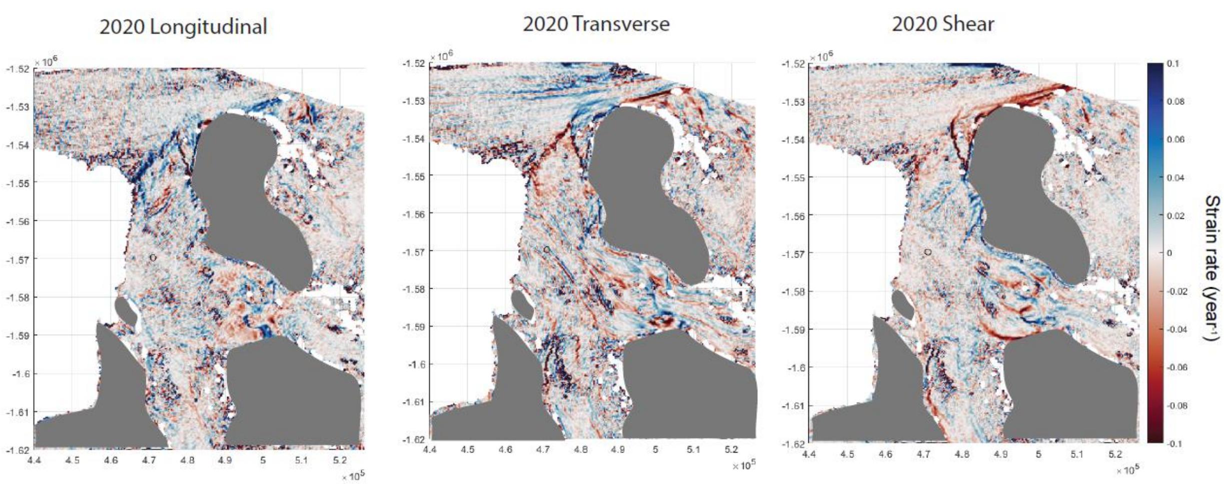
We attempted to obtain the Mean Dynamic Topography dataset that Griggs and Bamber used but it's no longer available from the datalink provided by Andersen and Knudson (2009) and we cannot find it elsewhere. We had a similar issue obtaining information about inverse barometer effects although it's unlikely to have changed over such a short time period. See above for our new addition to the discussion addressing this. We believe our cross-over analysis will cover any errors associated with these factors but, as discussed above, the difference in thickness is such a minor part of the manuscript we are willing to pull back from it.

Derivation & interpretation of strain rates:
Figs 4b,c show a fairly noisy structure (no surprise when deriving strain rates at such high spatial resolution) with some coherent bands in the along- and across-flow directions. The fast-ice/thin ice/sea

ice zone that is clearly evident in the surface elevation (Fig 4a) has surprisingly no correspondence in the strain rate fields. Fig 2c shows masked velocities in this area. Somehow I don't manage to bring this information together. I would feel more comfortable if the strain rate fields would more clearly show some first-order signatures (e.g., compression near inexpressible island) for orientation prior to going into details on scales <500 m.

We have now included a new figure in the manuscript (new Figure 5) showing transverse strain rates from multiple years with the edge of Drygalski Ice Tongue, Inexpressible Island and the fast ice zone included. We also plot longitudinal strain and shear strain from the 2020 32 day velocity output as there are fewer gaps over the ice shelf compared to 2016. This new figure shows shear and longitudinal compression at Inexpressible Island, a change in longitudinal strain at the boundary between ice shelf and fast ice and consistency in the transverse strain between years.

We've also included below a larger view image of the region with longitudinal, shear and transverse strain from 2020 that shows the clear extensional and compressional signals for the Reeves Glacier ice as it passes the grounding line, along with the Drygalski Ice Tongue lateral shear and transverse extension as is begins to pass land-based pinning points. We suggest that the figure pasted above is more appropriate for our discussion but we have included the one below to demonstrate the first-order signatures.



The statement that when calculating the strain rate fields on a 600 m grid "..produced similar results in both spatial pattern and magnitude of strain rates, but with smaller strain rate magnitudes in the suture zone on the order of ~0.08 [per] year" is confusing. 0.08 per year is almost the maximum values shown in Fig 4b,c. This seems like a substantial difference that should be investigated. How do your results compare to Antarctic wide estimates, e.g., from Alley et al., 2018 (J. Glac.)?

We mean that the strain pattern persists although the strain rates themselves change (see new Figure 5). We are not interested in the rate itself particularly but the pattern. The size of the *features producing the strain rates are around 400-500 m, therefore it is not surprising that larger length scales see less variability between strain pixels. It's essentially a smoothing exercise by increasing our length scale as detailed in Alley et al (2018) as we quote below. We now add in the reference to Alley and remove this statement about length scale tests since, on reflection, it does not add to our discussion. Instead we now present multiple Go-LIVE strain outputs from different years to demonstrate that this pattern we see is constant in time (although not in space as the strain patterns migrate with the ice). This new figure is included in an Appendix. Our strain rate values compare well with those presented in Alley et al. (2018).*

Calculating strain rates on <500 m assumes a very high quality of the surface velocities both in terms of magnitude and direction. Another figure (maybe in the Appendix) that clearly shows that the strain-rates adequately capture ice dynamic signals rather than measurement noise can strengthen your arguments. It might also be worthwhile to check if the smaller scale surface undulations could lead to errors in the satellite-derived velocities which may not have used such highly resolved DEMs (e.g., using InSAR with a more coarsely resolved DEM to correct for the topographic phase).

Our new figure of transverse strain rates over several years (new Figure 5) shows that the pattern of strain persists. In *terms of our approach, we are following the method of Alley et al (2018) who say: "This commonly used approach utilizes the highest resolution possible given the pixel size and may be appropriate for assessments of small-scale patterns of basal melt rates on ice shelves with complex basal topography. Then basal melt rates were recalculated with strain rates determined using length scales of ∼8× the ice thickness. These viscous-scale calculations might be appropriate for large-scale averages and for ice shelves with less complex basal topography". Because Nansen Ice Shelf falls in the category of having complex basal topography we argue that our approach is both standard and valid for this type of analysis. We now cite Alley et al (2018) to justify our approach:*

"Our approach of applying a length scale the same size as the pixel size follows the suggestions of Alley et al (2018) who state that such an approach is appropriate for ice shelves with highly variable basal draft topography."

Due to this variable topography and the ice flow speed of around 200 m/year (over features around 500 m wide) we are restricted to image pairs over a short time period (such as the 32 day separation of the GoLIVE images). Alternative velocity products (e.g. Measures and ITS LIVE) supply annual velocity outputs which are averaged from larger pixel sizes and therefore only capture the large-scale changes in strain rather than the smaller scale features we observe due to the basal ice draft and hydrostatic balancing. (Note: although there is supposed to be a feature available for ITS-LIVE to choose image pairs it's currently not available and we have been unable to access it.) As we understand it, steep or complex surface topography can impact velocity calculations, however Nansen Ice Shelf is flat with the complex topography on the base of the ice rather than the surface; surface undulations are on the scale of 3 or 4m over a distance of >1km. This small change in surface elevation over distance should not impact the velocity calculations, particularly when the results are consistent between years. Our findings here with extension in keels and compression in troughs fully supports other findings about ice shelf strain *characteristics and the physics of ice shelf balancing and strain (e.g. Vaughan et al. (2012)) and therefore we argue that our outputs are both realistic and valid.*

Terminology:
To me the term 'fracture' suggests a basal or surficial crack that emerged because some strain/stress limit was reached. However, some of the features discussed (e.g., the longitudinal features that are directed along-flow) could also have an oceanographic origin. Maybe consider first using a more general term such as 'basal undulations' and then more specific terms once you circled in on a possible formation mechanism.

We have changed as suggested.

Editorial remarks:

I don't see how eq (3) is actually used. Vertical strain rates are not shown. Also, is it necessary to introduce a variable (\varepsilon_1) that is always zero?

We have removed this equation.

L 185: Wrong internal reference? Fig 1c does not show the slope.

The sentence states "At the terminus, the northern flank of the thin-ice region is wider (6 km) than the southern flank (4 km) with an average ice draft slope of ~1.3˚ and ~1.1˚ at the ice shelf terminus, respectively". Fig 1c shows the ice draft across a transect at the terminus and our statement therefore stands. This includes distance and depth of the ice draft from which the slope can be estimated but we don't believe we need to physically add a slope line to this Figure as it can be computed by the data presented.

L 300: What does gain of 0.5 mean ? There are many different types of gains and I don't see how a single number would characterize this.

We have changed to: "a gain of $t^{0.5}$ was applied to the amplitude of each trace, where t represents time".

Fig 4d y label should be strain rate (not strain)

We have made this change.

Fig 5 maybe a red-blue color scale with white at 0? It is difficult to differentiate between melting and refreezing which should be a first-order outcome of this Figure.

We have changed the color scale to red-blue.

Fig 6 trace number should be distance, y label depth.

We have made this change.

Figure 6 b/c should detail viewing direction relative to ice-flow direction.

We have added arrows for ice flow direction.

(A1) should have a v^2

Thanks for pointing this out. We missed that and changed the equation.

Reviewer 1

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

Yes that is correct, we were only calculating volume loss rather than mass loss. We now use a polygon flux gate method as suggested by the editor and convert to mass in Gt.

L. 249:
On a more specific point, this paragraph states that:
"The discharge at Site 1 is 0.226 ± 0.004 km3 a-1 and at Site 3 is 0.222± 0.005 km3 a- 1 demonstrating an average cross- sectional mass loss of 181 m2 a-1 as the ice flows ~22 km between these two sites."
The discharge values shown here at the two sites are the same when taking into account the errors. This means that no conclusions can be drawn on cross-sectional volume loss other than that none can be ascertained.

We have changed our method for flux gate calculations, now using a polygon method examining flux in and out of the channel margins in addition to S1 and S3. With our error analysis included in these calculations we obtain a volume loss of 0.0523 +/- 0.0083 km^3/a. We made an error in the last iteration with the flux gate calculations so the higher mass loss rate we report here is more accurate and beyond the error margin.

Issues related to ice-shelf melt rate calculations

L. 255-256:

“This translates to average vertical melt rates of 0.75 m a-1 (keel), 0.45 m a-1 (apex), 0.95 m a-1 (keel), and 0.68 m a-1 (apex) between the two sites.” How does the change in ice thickness between the sites translate into “melt rates”? Again, as I pointed out in the first round of the review, several factors can contribute to ice-shelf thickness changes and these need to be considered when attempting to isolate the signal of basal melting from ice-thickness changes.

> *Following these comments and those from the AE, we have removed this sentence and our comparison of keels and apexes.*

L. 266-267:
“It is possible that mass loss is due primarily to surface melt and sublimation, which was estimated by Bromwich and Kurtz (1984) to be 0.25 m a-1 and by Bell et al. (2017) to be 0.5 m a-1.” These numbers are then used later in this section to calculate “cumulative surface mass loss” between sites 1 and 3. This raises two questions:

- Estimating how surface processes are contributing to ice thickness change cannot only consider loss due to surface melt and sublimation, it also needs to include accumulation (precipitation, wind-blown snow, etc.). Accumulation at the surface does not seem to have been considered here.

> *We do not have information on these processes at Nansen Ice Shelf but, as we are operating in a region of blue ice we can assume that wind-blown snow is not an issue and it is likely precipitation is not either. We now state this in the text: “*Because the region we are examining is composed of exposed blue ice we do not take accumulation into account”*.*

- A “uniform” surface ablation rate was used in the cumulative surface change. There is an underlying assumption here that the values used were also uniform in time, i.e., have not changed over the several decades it takes the ice to advect between sites 3 and 1. How is this assumption justified?

> *It is beyond the scope of this study to establish past surface ablation. We have added the following in the manuscript to point out the limitation of our analysis:*
>
> “This analysis assumes steady state surface melt, basal melt and ice velocity. The average time for ice to travel between Site 1 and Site 3 is 120 years, which means that some alteration of these rates may have taken place producing errors in our analysis, although with a cold cavity this is likely less of an issue than in regions of the West Antarctic for example (Das et al, 2020).”

L. 267-268:
“To investigate this, we integrate the basal shelf melt rates between Site 1 and Site 3 using the Cryosat-2 basal melt dataset along with ITS_LIVE 2018 ice surface velocities…”
Similar to the remark above: the basal melt and ice surface velocity values used in the integration are assumed here to have not changed over the several decades it takes the ice to advect between sites 3 and 1. How is this assumption justified?

> *The same argument can be made of anyone looking at ice shelf changes or indeed any change over time with limited data (e.g. Das et al, 2020, Lai et al, 2020). We don’t have past ice velocities of Nansen Ice Shelf and therefore we have to make the assumption that these velocities are representative. Without this, very little temporal analysis would be possible in*

regions like the Antarctic beyond the satellite record. As above, we now acknowledge that these values may have changed over time and that, with a cold cavity, decadal change is less likely an issue to our analysis compared to warm-based cavities in the West Antarctic for example.

L. 453-462:
The issues described above regarding the methods used to calculate melting rates raise doubt on the conclusions in this discussion regarding the different melting rates at the margins and at central part of the suture zone. Those conclusions, nonetheless, seem to be already supported by data from Adusumilli et al. (2020), shown in Fig. 5a of this manuscript. The authors, however, say that the Adusumilli et al. data for Nansen are "close to the noise floor (uncertainty) of the dataset for the NIS". Yet, in other parts of the manuscript they use the same Adusumilli et al. data without any reservation about uncertainty. For example, they compare the satellite-derived basal melt rates with the ocean glider data to find "a good correspondence between the locations of meltwater near Inexpressible Island and the center of the NIS terminus, and the areas of enhanced melt from the ice shelf, L. 273-275."

The authors need to decide how much confidence they have in the Adusumilli et al. (2020) satellite-derived data, and apply that consistently in their analyses. If they decide that they do have sufficient confidence in those data, then the authors can rely on the satellite-derived data, especially given the problems described above in their fluxgate and point analyses. If they do not have sufficient confidence in the satellitederived data, then their conclusions regarding the correspondence between glider observations and melting locations under the ice shelf do not hold.

We have provided the following in this manuscript:

- *Evidence from high resolution ground based radar surveys of ice mass loss*
- *Evidence from in situ ocean based records of ice mass loss*
- *Evidence from satellite based data of ice mass loss.*

This quantity of data is more than many studies are able to accumulate particularly for such an understudied region. We acknowledge in the manuscript that each of these methods has their own limitations yet the fact that they are independent methods and all point towards to the same answer gives us confidence in our arguments. Glaciology and Antarctic investigations in particular are inherently limited by the data that is available both in terms of time span and spatial sampling, and we are being very open about the limitations of the methods that we are presenting. We therefore argue that our statements and approach are fully valid and useful for the scientific community.

Issues related to meltwater characterization

L. 473-482:
The authors put emphasis on the depths and locations at which what they call "meltwater" was detected in order to connect it to the basal topography of the ice shelf, but they do not provide any definition of what they consider to be "meltwater". In other words, what are the temperature and salinity ranges of the "meltwater" plotted in Figs. 2b and 2e? The quotes below refer vaguely only to "cold, fresh ice shelf meltwater" or "supercooled" water.

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

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

> *For clarification we are changing our wording from meltwater to Ice Shelf Water and include additional details in the text:*
>
> *Lines 175-177: "... is presumed to drive basal melt (Rusciano et al., 2013), resulting in the formation of Ice Shelf Water colder than the surface freezing point (potential temperature < -1.94 degC, Fig. 2d). We therefore take the presence of this cold water as evidence of ice shelf melt."*
>
> *Line 286: "... data representing the location of Ice Shelf Water (ISW, identified by potential temperature < -1.94 degC) on Figures 2b and 5. This ISW was distributed..."*

Issues related to the assumption of hydrostatic equilibrium

L. 345-347:
“Our *in situ* data calculations demonstrate that the mismatch between hydrostatic thickness and radar ice thickness is therefore greater closer to the grounding line. This could potentially be due to bridging stresses from the highly variable basal draft and/or from pinning of the ice shelf from valley walls or nunataks.“

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

> *We did consider this effect but, in this location we are 25 km from the grounding line and we had assumed that was too far to feel the effects. However, as the AE also brought up the issue of various factors that we cannot adjust for (Mean Dynamic Topography etc.) we have pulled this statement out of the manuscript. Our primary interest is that the surface topography does not reflect the large complexity of ice draft topography and that argument stands regardless of whether there is an offset or not. We have added the following to the manuscript:*
>
> “At Site 1 and 2, further upstream, there was a vertical offset where the ice thickness measured using the radar was greater, on average, than the ice thickness derived from surface elevation

measurements (Fig. 3). This offset can be corrected by raising the surface elevation by an additional 2.5 m and, given that we have not corrected for Mean Dynamic Topography (Griggs and Bamber, 2009) or tidal variation, some of this offset may be explained by these factors. Regardless of the mean thickness offset, the spatial pattern of hydrostatic thickness at Site 1 and 2 also does not accurately reflect the variable topography in this region."

Other issues
L. 421:
"Alternatively, ocean melt may be focussed on the deeper keels with frazil ice formation possible at the apexes of the basal fractures. The dampening of the amplitude of the basal features as shown in Figure 3, suggests that such differential melting and freezing may be occurring."

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

We have added these citations.

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

We have added more citations for the Adusimilli dataset although often once datasets have been introduced in manuscripts (e.g. Measures, REMA) they are not re-cited.

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

We have made this change.

Minor Issues, Typos, etc.
L. 161:
"…year-1 or less We interpolated…"
Missing punctuation.

We have made this change.

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

We have made this change.

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

We have made this change.

Reviewer 2

Dow et. al. have improved the paper by implementing many of the suggested changes and removing the problematic interpretation of the some of the satellite data.

Nevertheless and despite of some important rephrasing being done, I think the paper still contains many parts that might indicate over-interpretation of the data or conclusions that are not supported by data. I explain my concerns in more detail below, but think that all claims related to hydrostatic balance, bridging effects and basal melt rates need to be downscaled.

• Dow et al. see discrepancies between the ice thickness from radar and the hydrostatic ice thickness from GPS especially for A-A' close to the grounding line and they relate this to bridging stress. I do not agree with this. Bridging stresses could indeed play a role for the basal structures that are narrow and tall, but cannot explain the overall offset for A-A', where the GPS hydrostatic balance seems over whole transect is almost ~10-20m off. If there is such large overall offset, I think something is wrong with the comparison anyway. A possible explanation (also indicated by Reviewer #1 and my limited understanding of marine ice radar) could be an error in the ice radar interpretation as the marine basal radar responses are potentially missing in the radar ice thickness. If the thickness measurements are based on the meteoric–marine ice interface, which could be the case in a suture zone with refreezing, it would mean the ice radar thickness measurements are underestimated. Closer to the calving front this effect this effect is as melting again dominates. In any case, based on the large offset between ice thickness from radar and the hydrostatic ice thickness from GPS, I don't think it is fair to focus only on the bridging as potential explanation and draw conclusion from it (L346-360). In my opinion the mismatch between the ice thickness from radar and the hydrostatic ice thickness from GPS is too large to draw any meaningful conclusion without investigating why it happened.

Firstly we agree that the ice thickness measurements may be underestimated if marine ice is present. However, if there is thicker ice present at Site 1 (due to marine ice accumulation) than we measure by the radar that <u>increases</u> the offset between radar and hydrostatic ice thickness rather than decreases it. However, as the AE also brought up the issue of various factors that we cannot adjust for (Mean Dynamic Topography etc.) we have pulled this statement out of the manuscript. Our primary interest is that the surface topography does not reflect the large complexity of ice draft topography and that argument stands regardless of whether there is an offset or not. We have added the following to the manuscript:

"At Site 1 and 2, further upstream, there was a vertical offset where the ice thickness measured using the radar was greater, on average, than the ice thickness derived from surface elevation measurements (Fig. 3). This offset can be corrected by raising the surface elevation by an additional 2.5m and, given that we have not corrected for Mean Dynamic Topography (Griggs and Bamber, 2009) or tidal variation, some of this offset may be explained by these factors. Regardless of the mean thickness offset, the spatial pattern of hydrostatic thickness at Site 1 and 2 also does not accurately reflect the variable topography in this region."

• Dow et al still make strong claims about basal melt rates without correcting for con-divergence of the ice or variations in melt over time. Therefore all claims (e.g. L255-265 + discussion) related to melt on specific locations (or deriving the maxima) is just hand-waiving and needs to be corrected for possible confounding factors (e.g. con-divergence) and the results and discussion should be adapted accordingly.

We now take advection and divergence of the ice into account by applying a polygon approach to the mass flux as suggested by the AE. We also now include strain thinning in our analysis of cumulative thinning between Site 1 and Site 3. In terms of variations in basal and surface melt over time, this is not something anyone can reliably calculate for melt rates in Antarctica beyond the last decade or so of available satellite data and therefore any prior work discussing basal melt over long time periods will have the same issues (e.g. Das et al, 2020). This does not negate the usefulness of the results that we present in this manuscript and we stand by our analysis. We have added the following to the manuscript to address this:

"This analysis assumes steady state surface melt, basal melt and ice velocity. The average time for ice to travel between Site 1 and Site 3 is 120 years, which means that some alteration of these rates may have taken place producing errors in our analysis, although with a cold cavity this is likely less of an issue than in regions of the West Antarctic for example (Das et al, 2020)."

However, we acknowledge that examining the change in keel depth is overstepping in the analysis due to possible differences in initial thickness and depth of basal fractures and have removed this. Our analysis of thinning drivers between Site 1 and Site 3 are compared with smoothed ice thickness values from Site 1 and Site 3 to avoid the issue of directly comparing different keels and apexes.

• Dow et al the extrapolate the Cryosat-2 melt over 123 years and add arbitrary surface melt/sublimation numbers to it to explain the thickness changes between S1 and S3. There is however so much that might have happened in 123 years that could also have affected S1 and S3, but that they don't take into account (e.g. temporal variations in basal melt rate). I therefore thing many of the conclusions drawn here are over interpreted and not supported by the data.

Melt and sublimation rates were produced by other scientific studies with robust methods, we have cited them accordingly and have no reason to reject the only available data on this key process. We performed this analysis and extrapolation after you suggested in it round 1 of *reviews and we appreciated the input. See above for our addition into the manuscript to acknowledge that melt rates may have changed over time. Beyond this we are limited by the information that we can gather today.*

• (related to previous two comments) In their response to Reviewer #1 (who asked for corrections based on divergence, advection, SMB, firn air), they rule out firn, SMB and river erosion, but don't tackle divergence or advection which both could have a significant effect on the interpretation. The flux gate analysis accounts for con- and divergence, but the rest of the spatial comparisons does not in my humble opinion.

We have now taken account of divergence and advection by applying a polygon flux gate method and strain thinning analysis and find little ice gain or loss from divergence and advection in the region examined between our radar sites (see above addition into the manuscript).

References

Das, I., Padman, L., Bell, R.E., Fricker, H.A., Tinto, K.J., Hulbe, C.L., Siddoway, C.S., Dhakal, T., Frearson, N.P., Mosbeux, C. and Cordero, S.I., 2020. Multidecadal basal melt rates and structure of the Ross Ice Shelf, Antarctica, using airborne ice penetrating radar. *Journal of Geophysical Research: Earth Surface*, *125*(3), p.e2019JF005241.

Lai, C.Y., Kingslake, J., Wearing, M.G., Chen, P.H.C., Gentine, P., Li, H., Spergel, J.J. and van Wessem, J.M., 2020. Vulnerability of Antarctica's ice shelves to meltwater-driven fracture. *Nature*, *584*(7822), pp.574-578.