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 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 slightly adjusted to fit in with their new sections and discussion focussed 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. We have adjusted our discussion and conclusion following suggestions from both yourself and reviewer 1, but our larger scale arguments remain the same.*

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. However, 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. As such, we have kept the layout as it is. However, we have changed the wording in the introduction to state that we are examining each of these areas of research, which we hope makes the paper clearer .

"We analyze and compare several datasets including a satellite-derived digital elevation model (DEM), ground-based and airborne ice-penetrating radar (IPR), ice velocity-derived strain rate data, and oceanographic data. These data sets are used to examine the morphology of NIS and how this interacts with ice strain, basal melt and ice, all important factors in shelf fracturing"

* 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 river and fracture labels to Figure 2 which includes the ice surface and thickness information and have referenced Fig 2a in this sentence.

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 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. 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 results we have clarified to say:

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

We have also clarified in the new version of discussion section 5.1 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 77-78 where we say:

"The katabatic winds that allow persistence of a significant polynya at the terminus of the NIS also strips the ice shelf of much of its snow and firn cover leaving a significant portion of the surface as blue ice"

and also in lines 109-111 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, so 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:

"We note that the patterns of strain discussed 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:

"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 ~0.1 day^{-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.

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:

"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 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 and now suggest that the primary role is on the slopes of the suture zone rather than channelised in the thinnest region of the suture zone. We have also replaced the subfigure in Figure 5b with one showing the integrated basal melt, surface ablation, total melt and measured thickness change. 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 without access to the original Matlab plots. 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

These are results and therefore, at this stage of the manuscript there are no conclusions or take-home messages. In the Discussion we amalgamate different results to discuss the system on a larger scale. We feel this interpretation should be conducted in the Discussion, not the Results section, which is one of the reasons (in addition to The Cryosphere's instructions to authors) why we are retaining our layout.

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 also now expanded this section by moving some of the discussion of hydrostatic calculations from the former section 5.5 here.

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, as we cite in the manuscript) 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 surface river, which in the middle of the suture zone. This should clarify the location of the suture zone. We tried also outlining the suture zone on the figure but it became too messy and the river location was the clearest option.

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' alongside 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).