SUMMARY

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.

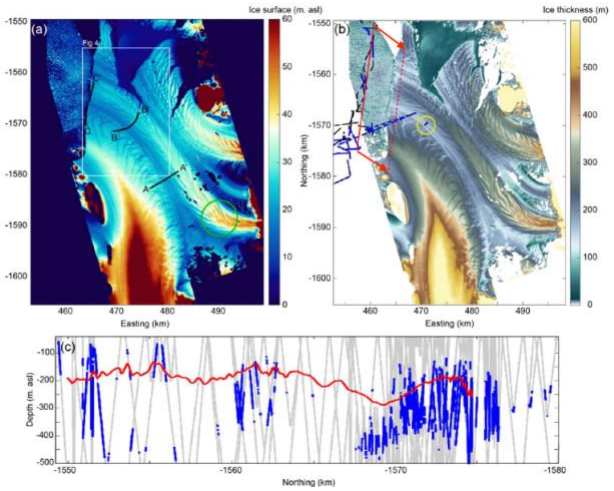
In my opinion the main issue of the paper is the following:
The main conclusion of the authors is that they stress the limitations of the high-resolution data sets for deriving conclusions about ice shelf characteristics and I do not agree with this conclusion at all. It is true that their analyses show 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.

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.
- * 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.
- 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

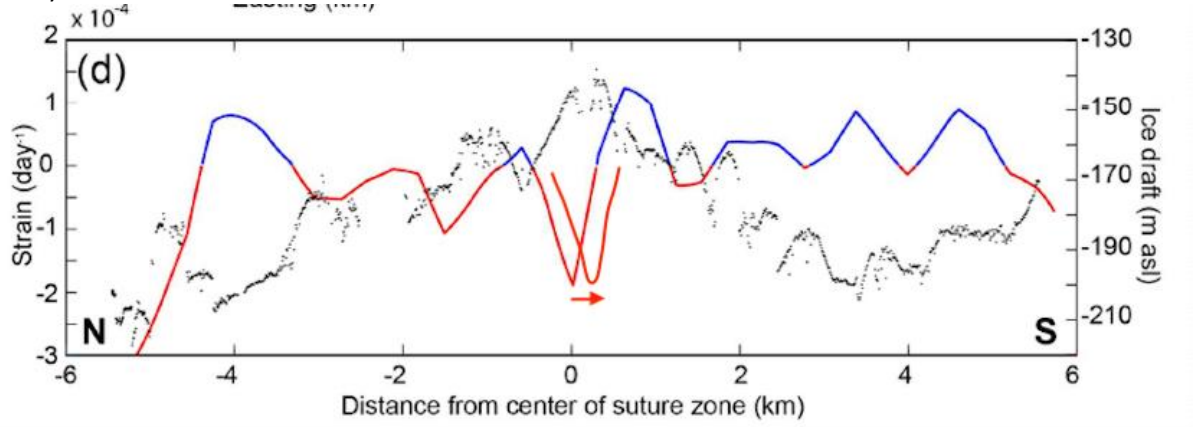
between the figure and label. By using text, arrows etc. to indicate where to look, it will be way easier to interpret the figures.

- 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?
- 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.
- 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.
- Fig.1: perhaps direct labelling instead of the caption as it requires the reader to unnecessary zigzag between figure and caption .
- The site description could be integrated with the section 4.1 to join similar information together.
- 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*"
- 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
- L105: What was the offset between the strips? How was it corrected?
- 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).
- 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.
- 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)

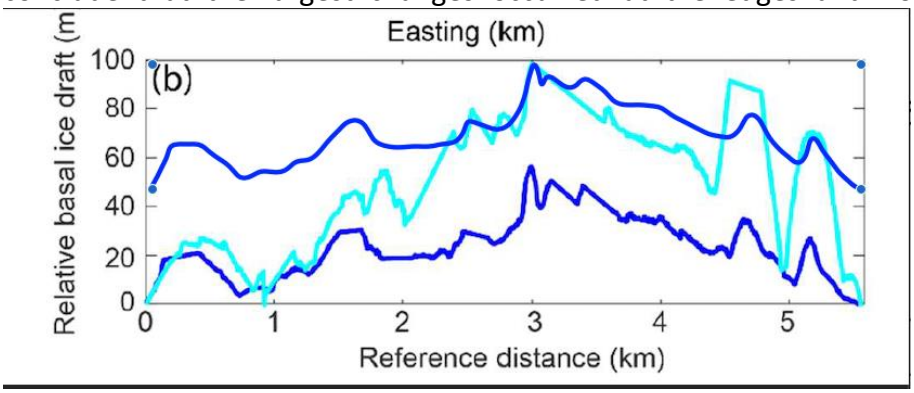


- Fig.3: the alignment for REMA is potentially problematic if the proper strip corrections are not applied
- L150: what about the spatial and overall accuracy of GOLive data? Small inconsistencies in the velocity data and/or geometric accuracies could have large impacts on the interpretation of the strain 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)
- 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.
- Section 3.5: these are very interesting data and should be elaborated further. How was the meltwater classification done?
- Section 4.1: this reads very much as a continuation of the site description and could perhaps be integrated
- 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.
- 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):



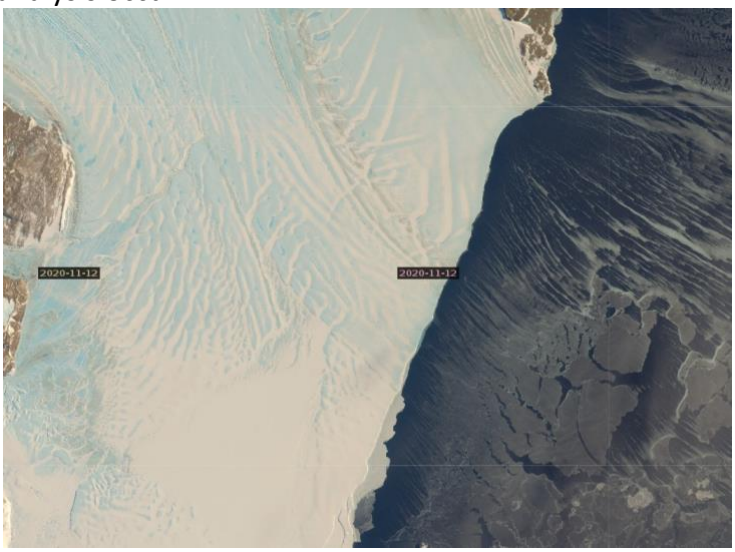
- L199-200: "*the switch between compression and extension … is limited horizontally*" This switch is by definition always localized to a point (as there is either compression or extension) so it seems a strange statement
- L200-206: any of the conclusions based on the location of patterns of strain 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.
- 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.



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

- 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
- 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.



- 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.
- Section 5.1 reads very much as a continuation of the results of 4.1 and perhaps it should be considered to be integrated.
- 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
- 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.
- L436-451: I do not agree (see my earlier comments)
- 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).

TECHNICAL COMMENTS

- L20 "*Nansen Ice Shelf has a highly variable* morphology"
- L41-45: "*The increasing variability of … opens up possibilities*"
- 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
- 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.
- Line 166: difficult for non-experts to see this suture zone. Perhaps direct label the suture zone on Fig.2?
- 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.
- 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.
- 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.
- Fig. 5b: perhaps add transect B-B' (S2) to allow the reader to check for temporal consistency etc?