Dear Authors,

let me apologize for the long time that it has taken to get back to me. One reason was that I couldn't easily find new reviewers. The old ones declined to continue with the review, in parts because they found that initial revisions have not improved the paper to the degree required. Because of this I decided for one new reviewer and to complement this review with an editor review. This took me much longer than expected (mostly due to teaching obligations with field mapping courses). Please accept my apologies for the delay.

Unfortunately, the evaluation of the new reviewer is weak, which means that we now have five external evaluations which have raised substantial criticism. A number of points have improved (i.e. the strain rates are more convincingly displayed, and the mass budget between Site 1 and Site 3 is now closed), but take away messages still remain in parts inconclusive.

I understand that this is an unsatisfactory result, and below I mention some points which are hopefully constructive and helpful. If you decide to revise the paper, I will have to send it out to re-review again. We need at least one positive external review to move on to TC.

Kind regards, Reinhard Drews

Thank you for your comments on the paper and for the effort to find reviewers. It is unfortunate that the new reviewer didn't like the paper but we believe that was primarily a misunderstanding of what our manuscript was discussing. They were mostly interested in the calving event and fracturing at Nansen, which we agree is a good area for discussion but we published on this already (Dow et al, 2018) and so we don't wish to repeat information from that study here. We have added in a paragraph to the introduction and at the end of our results sections to provide clearer takeaways from the manuscript. Furthermore we have rearranged the 'methods' into 'datasets' and moved some of the analysis into the results which we believe streamlines the manuscript. Finally, we have moved several subfigures to the Appendix so that the remaining figures better clarify the primary outputs of the manuscript. We appreciate the opportunity to address these comments. Note that line numbers are in reference to the tracked changes version.

EC1: Derivation of basal melt rates from discharge

The comments from previous reviewers and myself have been incorporated, and the mass budget between Site 1 and Site 3 is now closed. However, I disagree with multiple statements in the paper that basal melt in the suture zone was calculated from in-situ data (e.g. l. 295; l. 447; l. 483). It wasn't. Analysis around l. 268f explains the observed thickness change with strain thinning, basal melting and surface melting. But it doesn't "derive" basal melting per se. Instead it uses existing basal melt rate estimates (Adsumuili et al.) and puts those into an ice dynamic context. Derived are maybe the surface melting rates, b/c 0.5 m/a explain the thickness change better than 0.25 m/a.

The glider data may support existing basal melt rate estimates, but they also don't derive them. Language according to this should be changed throughout the manuscript.

We have gone through the manuscript and changed the language as suggested both for the basal melt calculations and the glider data.

EC2: Interpretation of enhanced melting at the flanks vs keel.

Section 5.5. suggests that the Adsumuili data indicates that basal melting is in some parts stronger at the flanks than at the keels. This point is interesting, but not rigorously evaluated. If melting is stronger at the flanks, then the zone should widen downstream. If the Coriolis force leads to stronger melting at one flank, then the suture zone should deviate from flowlines (cf. Drews et al., 2021 -- no need to cite this paper). Is this the case? Without this kind of analysis, degree of novelty of these findings is limited as they have been mentioned in the previous studies cited in the paper.

Thank you for this suggestion. To demonstrate this more rigorously we have now included flowlines from a transect across the initial suture zone which demonstrates that the suture zone is widening more on one side than the other, strengthening our argument that Coriolis is involved. These flowlines are now included in Appendix Figure C1b and explained in the manuscript. We hope this addresses your above comment.

EC3: Hydrostatic equilibrium.

This point is technically well done, but it takes up a lot of space in section 5.2 for what is known already. It is not new that narrow surface expressions in REMA are not well represented in the basal morphology. The arguably new point (i.e. also features larger than one ice thickness show signs of bridging, l.368) appears buried. I suggest to significantly shorten this section (e.g., in order to provide more space for EC2) and to highlight this new point more clearly.

We have significantly shortened this section by amalgamating it with section 5.1 and emphasised the point about features larger than one ice thickness. This is also included in the conclusion. The reviewer asks for inclusion of REMA hydrostatic thickness analysis in this section but, following your points here we haven't added this in order to keep this section short (see below for our response to their comments and sample figures).

EC4: Integration of glider data

I now well understand that you argue that the glider data may pick up zones of localized ice-shelf melt. However, I don't find this convincing. Figure 7a detected ice-shelf water in most areas apart from two more localized (white) zones where no such water was detected. Do you suggest that that the higher basal melt rates in the suture zone (up until the ice-shelf center) are somehow flushed out localized at the front? If so, could you indicated the pathways on the Figure?

Unfortunately at the Nansen Ice Shelf front there is a large eddy (see Friedrichs et al, 2022 Fig4) so from the directionality data we can't confirm where exactly the fresh water is coming from. This is very unsatisfying so all we can really say is that there is definitely freshwater (and therefore melt) occurring at this cold cavity ice shelf. We have gone through the manuscript to clarify this and pull back on assumptions about where that melt came from. It seem a shame not to report the evidence of ISW from within the cavity since so few datasets like these exist, particularly for cold-cavity ice shelves, so we have retained it in the Appendix. We have added the following to the manuscript in Appendix B:

"However, the presence of an eddy at the terminus of Nansen Ice Shelf (Friedrichs et al, 2022) means that we cannot use directional data from the glider to determine whether the suture zone is a greater source of ISW than other regions of the sub-shelf cavity. Instead the data merely indicates that ice shelf melt is occurring at this cold cavity ice shelf."

Also, basal melting 15 km upstream from the ice-shelf front is absent and the freeze-on rates appear unrelated to the suture zone. How would that change the presence/absence of ISW in the glider data?

The basal freezing upstream implies that the recorded ISW is locally from the front 15 km of the shelf. We've removed our paragraph speculating about the source of the ISW aside from saying it's from the NIS and so we won't comment on this in the manuscript.

EC5: Detection of marine ice

From what I understand (I may not have) you suggest that the suture zone has some fractures which go through the entire ice shelf, and which are filled with marine ice. This would be a nice new finding for me. However the radar data analysis is not fully clear in that regard. It is correct that marine ice attenu-ates radar waves rapidly, but then you would still expect a marine--meteoric ice interface in the radar data (cf. Kulessa et al. -- research at the Larsen Ice Shelf). This doesn't seem to be the case. Is this be-cause the marine ice structures are vertical and not horizontal? If so, how would this imprint on the ra-dio-wave propagation? Do you see any internal signatures in the zones where the basal returns drop out, if so, how do they related to your hypothesis of vertical marine ice?

Yes we are suggesting that this is the case. We observe 'echo-free zones' on the NIS in regions within the suture zone and elsewhere where rifts may be found. In contrast to reports from other ice shelves (e.g. Larsen, Amery, etc) where a reflector is observed at depth and the pres-ence of marine ice is inferred to be below this, we find no internal reflectors in the ice below the ringing of the air/ground wave. This we interpret to be former rifts or crevasses filled with ma-rine ice and we assume that the marine ice body is vertical (but may have deformed from vertical by strain over time). To make this finding clearer, we have added more text and improved our figure 5 to show where the echo-free zone occurs in two transverse radar lines across the suture zone.

The text we have added to explain these features is in lines 235-251:

Given the abundance of clear ice base reflectors in the remainder of the radar data, it is likely that these "echo-free zones" represent areas of marine ice and/or frazil ice accumulation (Holland et al., 2009). Typically, the unconformity between meteoric ice and the subjacent horizontal layer of accumulating marine ice will reflect radar waves allowing this boundary to be identified (Fricker et al., 2001; Kulessa et al. 2014). Since marine ice is a lossy medium (Tulaczyk and Foley, 2020), no radio waves are returned below this depth and marine ice thickness can only be inferred from hydrostatic calculations. In our radargrams, there are regions where little to no signal is returned, except the signal associated with the air/ground wave near the surface (Fig 5e and f). This complete absorption of all the radar energy in these locations can only be explained by marine ice near the upper surface of the ice shelf. A healed rift or very tall/high basal crevasse filled with marine ice, would represent a vertical section where radar waves are absorbed (c.f., Hillebrand et al. 2021). On the ice surface, in the suture zone, there are many stripes of white and clear blue ice that are associated with echo-free zones (Fig. 6c). These can be traced back to the Reeves Glacier ice fall where crevasses fully fracture through the ice column, fill with sea water and refreeze (e.g., Khazendar et al., 2001, Tison et al, 2001). The filled rifts are then advected and stretched, producing visible stripes parallel to the ice flow direction along the suture zone. Khazendar et al. (2001) postulate that ice folding due to lateral compression may alter ice stratigraphy and dip angle. These processes may contribute to the blue/white striping seen in the suture zone and also imply that whatever ice is visible at the surface is not necessarily the same as the ice beneath.

EC6: Features in the transverse strain rates in the suture zone
----

The elongated red-blue stripes (transverse compression and extension) in the suture zone are a nice signal in the strain rate maps. I believe one explanation for this could be linked to secondary flow of ice into from areas of thick ice into areas of thin ice although, then we would need two zones for transverse extension which are not observed. Nevertheless, I suggest to put this observation into the context of the study from Wearing et al. (GRL, Secondary flow of ice shelf channels).

Thank you for this suggestion. We have added the following to the discussion lines 493-495:

"Alternatively, this strain pattern may be indicative of secondary ice flow into the thinnest region of the suture zone, particularly if there is basal melt occurring in this region (Wearing et al, 2022)."

Kind regards, Reinhard Drews

Minor Comments:

1. There are no results pertaining to the glider data in the abstract. This is another sign that the glider

data is not yet sufficiently connected to the analysis on the ice shelf

We have now moved the glider data to an appendix - we think it's important to demonstrate ISW is present under this cold cavity ice shelf (not a given considering the cold local ocean conditions) so it's worth stating but we agree it is not a central part of the manuscript.

l 58 What is "active basal melt"? Is there something like "passive basal melt"? Do you maybe mean "localized basal melt"?

Yes thank you, this is what me meant.

l 133 "an \rho_i is the f" ?

Strange…corrected to 'the density of ice'

l 140 ".,"

Sorry – not sure what this refers to.

l 149 "plotted in Euro? " ?

Apparently word autocorrects (e) to € …fixed now.

l. 152 another Euro

Fixed.

Figure 5: I don't get the caption (b) and (c). If both show longitudinal strain rate they should look the same. I guess (c) refers to the horizontal shear strain rates as suggested by the title of that subfigure?

Yes, (c) should have referred to Strain. Corrected now.

Equation 2: I understand what is done here, but the notation is sub-optimal. "i" is both an index and a distance. Maybe better use dl_i or something like this to indicate that you multiply with a length

Changed to dl

l. 160 "REMA hydrostatic product" better "we use the hydrostatic ice thickness derived in section 3.2" (otherwise it sounds as if the thickness product is delivered by the REMA team.)

Changed as suggested

l. 176 Instead of "high-resolution" better quantify what the resolution is in meters.

Changed as suggested

l. 180 Which are the transverse and which are the along-flow directions? I believe you define this all locally, meaning that transverse and along-flow are defined with the local velocity vector. A few words will make this clear.

Yes, we have changed to clarify: "We calculated the principal strain in the longitudinal and transverse directions, having rotated to the direction of local flow"

l 313 I find it uncommon to mention processing steps of the radar data in the figure caption.

These have been removed

In general: Always be specific which component of the strain rate tensor is shown,e. g., is the profile in 4d transverse or longitudinal? The y-label is unspecific.

We have corrected this in Figure 4d and checked the manuscript for other occurrences of this.

l 267 Much of this paragraph sounds like as if it belongs into the methods, but I don't insist on this.

We have rearranged the manuscript to better focus on the data collected vs. data other people produced vs. calculations we applied to those data. As a result we have changed the 'methods' section to 'Datasets' and the 'results' section to 'results and analysis'. While this is a less conventional approach we believe it highlights our work and findings better than our previous configuration.

l 273 v should be bold, also the divergence operator is missing, it is written as a gradient.

Thanks for catching. Corrected.

The equation near l. 273 is unnumbered. Also, in essence, this integral is an integral over space rather than time (because no datasets are available for t_1 and t_2). Wouldn't it be easier to mark this accordingly?

We use the velocity products to estimate the times for flow between site 1 and site 3 to extract dt and therefore also t1 and t2. We believe it's standard to have the flux integrated between two times to represent the flux speed – in this case we're following Das et al (2020) who we cite in this section.

l 284 Can you state what impact the smoothing has (i.e. compare the results for smoothed and unsmoothed).

In a previous version of the manuscript, we used the unsmoothed transect data to perform this analysis. Reviewers pointed out that this could lead to misleading results since, due to spreading, the apexes and troughs of the basal features might not align. We agreed with this feedback and decided to, instead, focus on a much broader-scale changes within the suture zone. This led us to remove the high frequency thickness changes along the transects to examine the suture's basal melt. We are confident that this more general examination of the suture zone shape will avoid artifacts where features which don't necessarily align from one site to the other (different locations/sizes) are subtracted from each other. To compare our current version to the unsmoothed version, please refer to Figure 5b in the second version of the manuscript.

l 304 Can you differntiate more? There is no active (do you mean localized?) melt in the suture zone near the ice shelf front. How would that imprint on the distributiion of ISW?

Following your suggestion we've decided to pull back on what we can say about the source of the ISW due to the presence of the sub meso-scale eddy (so we can't pull out non eddy-related flow directions from the data). We've added the following to this section:

"However, the presence of a sub-mesoscale eddy at the terminus of Nansen Ice Shelf (Friedrichs et al, 2022) means that we cannot use directional data from the glider to determine whether the suture zone is a greater source of ISW than other regions of the sub-shelf cavity. Instead, the data merely indicates that ice shelf melt is occurring at this cold cavity ice shelf. "

l 311 "icec)"

Corrected

l 355 Not sure what the take away of section 5.2 is. This hydrostatic thickness is a damped version fo the real thickness, but this is known. It appears that features larger than one ice thickness are also damped, but why not. From what I understand this has never been fully formalized and will be a function of the specific strain regime. I suggest to significantly shorten this section.

We have followed your suggestion above to shorten this section and focus on the features larger than the ice thickness which are damped as the primary finding. This section has now been amalgamated with section 5.1.

Which strain rates are shown in Fig 8? I guess it is transverse.

Yes, we have now added this to the caption and figure.

l 543 "marine Ice" -> "marine ice"

Corrected

Reviewer 1

In their TCD manuscript "The complex basal morphology and ice dynamics of Nansen Ice Shelf, East Antarctica" Dow et al. describe the morphology of the Nansen Ice Shelf by utilizing airborne and ground-based radar transects, remote sensing data on ice velocity and basal melt rates, and water temperature data from an ocean glider. Overall the authors present an interesting data set but I have the feeling a clear focus is missing throughout the manuscript. This review refers to the manuscript uploaded on 30 Apr 2023.

Thank you for your comments. We have now restructured the paper and added to the introduction to point more clearly towards our primary goals and findings, providing improved focus.

General remarks:

The authors need to be more clear on the accuracy of the different data products throughout the paper. For example, as shown in Zeising et al. 2022 there are flaws in the Adusumilli data set which can be traced back to the surface velocity data set applied. I, therefore, suggest carefully checking the Adusumilli data set in the study area.

Unfortunately we don't have pRES instrumentation on Nansen Ice Shelf to do a similar comparison between in situ radar melt and the satellite derived melt and we are aware that there are errors associated with the satellite-derived data, which is what we point in in lines 579-592. We have attempted to be clear about the limitations of all of our datasets and our approach is that, despite the limitations of each of these approaches they all point towards the same information i.e. basal melt. We have included the Zeising reference and expanded on the potential errors by saying:

"the satellite-derived basal melt rates signals (Adusumilli et al., 2020) are close to the noise floor (uncertainty) of the dataset for the NIS. Furthermore, the latter have been shown to differ from pRES radar measurements of basal melt at Filchner Ice Shelf due to less accurate velocity products for the satellite-based calculations (Zeising et al, 2022), and the same may be the case at NIS."

2016 calving. The 2016 calving event is mentioned several times. As a reader I would be very interested where exactly the event occurred and what was the impact on the remaining ice shelf. Is there a detectable speedup in ice flow after the event? Are there any new fractures that can be related to the calving event?

Yes we agree that the calving event is an interested aspect of the Nansen Ice Shelf evolution. There was a change in strain patterns (speed up) after the calving event and there was indeed a new fracture that appeared as a results, as stated in lines 51-56 and 498-506 of this manuscript. The reason we don't go into great detail about this is because it was extensively discussed as the subject of Dow et al (2018). We have referenced the latter paper multiple times through this manuscript.

I found the discussion section hard to follow and suggest condensing it to the most important findings, which might change during the review process.

Thank you for your suggestions. We have shortened the discussion in various sections now which will hopefully make it easier to follow.

Specific comments:

L16: please state what parameters the ocean glider is measuring.

We have moved the glider data to an appendix and so remove mention of it in the abstract.

Figure 1: I suggest showing one pre- and one post-calving Landsat scene, potentially framing the calved iceberg in the pre-calving scene. Also the profiles in panel c) could be color-coded by distance from the grounding line.

We don't focus on the calving event for this manuscript – for more details about that see Dow et al (2018). We tried color coding the profiles as suggested but the figure became too messy and we have retained our labelling method instead.

L115: so basically the authors solely employ water temperature data? Is the temperature data resolved for the whole water column? Can you distinguish the temperature of different water depths? Please be more specific here.

The ice shelf meltwater is fortunately the coldest water mass in the region (which is not true in West Antarctica), so yes we only employ water temperature and resolve those temperature data for the whole water column, and yes we can distinguish the temperature at different depths. We have moved the glider data to an Appendix, changed the glider data figure, and have expanded there on these points in order to clarify:

"This HSSW is one of the coldest water masses in the Southern Ocean (Grumbine, 1991), existing at temperatures close to the surface freezing point (~-1.9 °C, depending on the salinity; Rusciano et al., 2013; Yoon et al., 2020). Its interaction with glacial ice then produces even colder Ice Shelf Water (ISW), uniquely identifiable in the region by temperatures close to the subsurface freezing

point (<-1.9 °C, depending on pressure). We therefore take observations below the -1.94 °C isotherm (equivalent to the freezing point at a salinity of 34.7 and a depth of 50 m) as evidence of the presence ice shelf meltwater."

L119-L125: if this is not a result of the ocean glider data I suggest moving this section to the site description. Are there any more references to back up this paragraph?

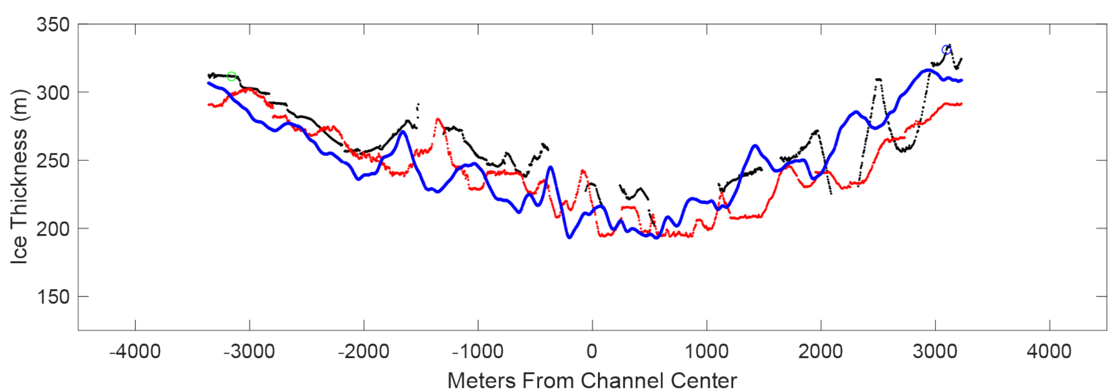
Following comments from the editor we have we have moved the glider data and description of ocean conditions to an Appendix. We have added more references as suggested.

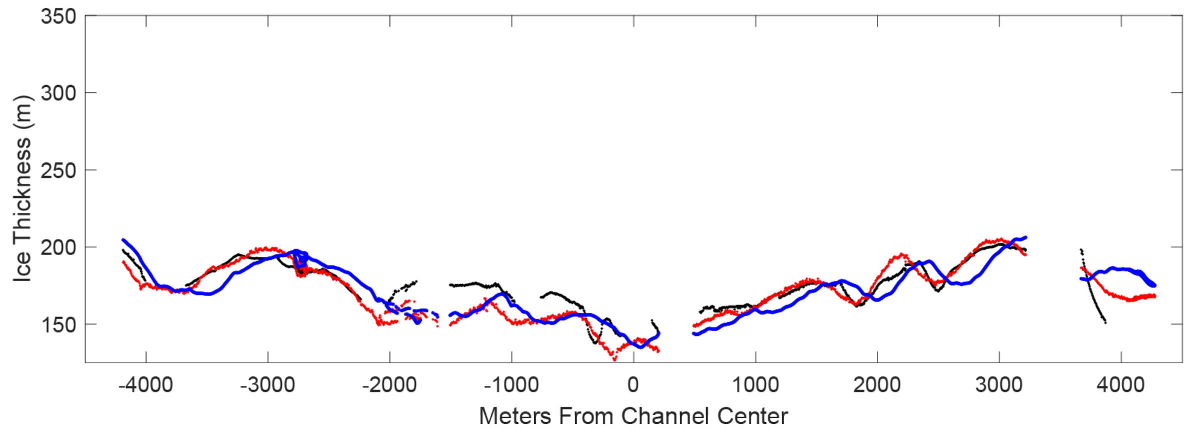
L138: this is not really true. The REMA data are used for many other purposes, e.g. plotted against the ocean glider data in panel 2e used for ice flux calculations, etc.

We have removed this sentence.

L139-L140: ok, but why are the authors not including their REMA ice thickness product in this analysis? Please see also my comment on Figure 3.

Following the suggestion of the editor we have shortened our hydrostatic analysis section and are therefore hesitant to add additional information from REMA here. We're investigating whether surface features can be used to accurately assess the basal topography, which will be more accurate with our in situ GNSS data. We tried adding in the REMA hydrostatic data here but the figures became very messy. Furthermore because the REMA data were not produced at the same time as our radar surveys there is an offset between our surface data and the REMA surface data. This can be seen in the figures below where we demonstrate the comparison and why it would be challenging to include in the manuscript.





L156-L158: why not include the site 2 profile? This would increase the spatial resolution of the boxes.

We would have like to include Site 2 but we didn't get a wide enough transect at that site to either compare to Site 1 and 3 or to cover the entire width of the channel. We now note this in the text:

"Site 2 is not included in this analysis because the radar cross-sections are not laterally extensive enough to cover the entire channel (see Fig. 2a)"

L161: The ITS_LIVE dataset is not introduced yet. I suggest moving the flux gate section to the end of the methods chapter.

We have moved this section.

L169-L170: ok, but this mass loss or gain can not be related to basal processes alone as the authors neglect smb and firn densification. Could be worth including model data here.

In these lines we don't state that the mass loss is from basal processes alone. We are just reporting change in the mass flux past the gates. In the results section we do an analysis of mass loss and take account of reported surface ablation rates. We don't include firn densification or snow accumulation because this is a blue ice zone as stated in lines 95, 144 and 340.

L178: there are more velocity fields in Figure 5.

We have clarified that the primary analysis is for the 2016 strain rates and we now state the date range for the other strain calculations in the same paragraph.

L178-180: yes, but this greatly minimizes geolocation and hence velocity errors in the dataset. For this reason, Alley et al. 2018 used a "stacked product including many overlapping pairs of Landsat scenes spanning many different time intervals, allowing for a significant reduction in both the geolocation and random velocity error." Interpreting strain rates from a noisy 32d image pair is rather difficult. I suggest showing pre- and post-calving strain rates from stacked velocity products.

*We agree it is difficult and this is why we've included multiple years in Figure 5. If we attempt stacking similar to Alley et al 2018 it removes the features of interest which are on a small scale. Unfortunately without higher resolution velocity data this is the only approach for analysing small scale topographic strain features. To clarify we now include: "It is not possible to stack multiple velocity outputs in this region to reduce noise due to the combination of ice flow speed and the narrow nature of the ice shelf morphological features. As an alternative we demonstrate the consistency of transverse strain features at NIS across m*ultiple years." Lines 202-204.

In terms of pre- and post calving, please see Dow et al, 2018 for strain calculations demonstrating the impact of the calving event.

L181-183: maybe include Bindschadler et al. 1996 here?

We have added this reference.

L189: I suggest an additional section about the reliability of the Adusumilli et al., 2020 dataset in the study region. Please see also Zeising et al., 2022 on this issue. Actually, I would go from the multi-mission Eulerian height-change rate time series as provided by Adusumilli et al., 2020 to your own basal melt products employing the ITS_LIVE or GOLIVE dataset, smb, and firn air content from modeling and ice thickness from REMA (validated at the survey sites).

We have included information about this as you suggest in the discussion":

"the satellite-derived basal melt rates signals (Adusumilli et al., 2020) are close to the noise floor (uncertainty) of the dataset for the NIS. Furthermore, the latter have been shown to differ from pRES radar measurements of basal melt at Filchner Ice Shelf due to less accurate velocity products for the satellite-based calculations (Zeising et al, 2022), and the same may be the case at NIS." Lines 580-582.

For the second part of your comment here we're not sure what you mean.

Figure 3: please include also the ice thickness from REMA along the three gates.

See above response

L192: based on REMA or IPR? How do both datasets agree?

We now clarify by saying: 'Using REMA hydrostatic thickness for the wider region and our IPR data where available" In the first iteration of this manuscript we discussed how well the REMA and IPR datasets agreed but were asked to remove it.

L268-286: this is rather methods than results. Furthermore, I doubt the reliability of this approach as already stated by the other reviewers and the editor.

We have rearranged the manuscript to better focus on the data collected vs. data other people produced vs. calculations we applied to those data. As a result we have changed the 'methods' section to 'Datasets' and the 'results' section to 'results and analysis'. While this is a less conventional approach we believe it highlights our work and findings better than our previous configuration.

Additional References:

Bindschadler R et al. (1996) Surface velocity and mass balance of Ice Streams D and E, West Antarctica. J. Glaciol., 42 (142), 461–475 (doi: 10.1017/s0022143000003452)

Zeising, O., Steinhage, D., Nicholls, K. W., Corr, H. F. J., Stewart, C. L., and Humbert, A.: Basal melt of the southern Filchner Ice Shelf, Antarctica, The Cryosphere, 16, 1469–1482, https://doi.org/10.5194/tc-16-1469-2022, 2022.