Das et al response:
Both reviewers raise some interesting questions that has improved the manuscript. Thank you. We address the major and common questions here. Our response is in blue.

Significant change in this version:
To address Reviewer 2's comment about other datasets becoming available in ITS_LIVE database, we added the 2022 velocity field to our analysis. The resolution of the dataset also changed from 240 m to 120 m. The current analysis is completed with 2022 and 2013 velocity datasets. Please note that this meant we had to redo everything in an already expansive manuscript. Given the scope of federal funding, this was a huge time effort. 2022 is the most recent dataset we had at the time of redoing this analysis.

Here we provide major figures to show how our analysis went, including the errors. For the velocity data, both reviewer 1 and 2 expressed concern for limiting our data based on errors > 15 m a-1. In this version, we have included all values in the velocity dataset and have quantified the errors separately. This figure will be included in the main paper as Figure 2. If the reviewers are happy with this version, we will include the velocity difference streamlines as a fourth panel in this figure, which will be similar to Figure 2a in the current manuscript.

As in the previous analysis, the velocity data shows a coherent increase in the southern sector from 2013 to 2022. The northern sector shows a deceleration during these years. Our fundamental result from the last manuscript that predicted a more vulnerable southern sector is still valid. Recent calving of A84 from the southern sector is an example (the analysis and satellite images of A84 are part of another analysis and we will not include this in the already expanded current manuscript).

A supplementary text is included in this iteration. Some key figures are included in this response.

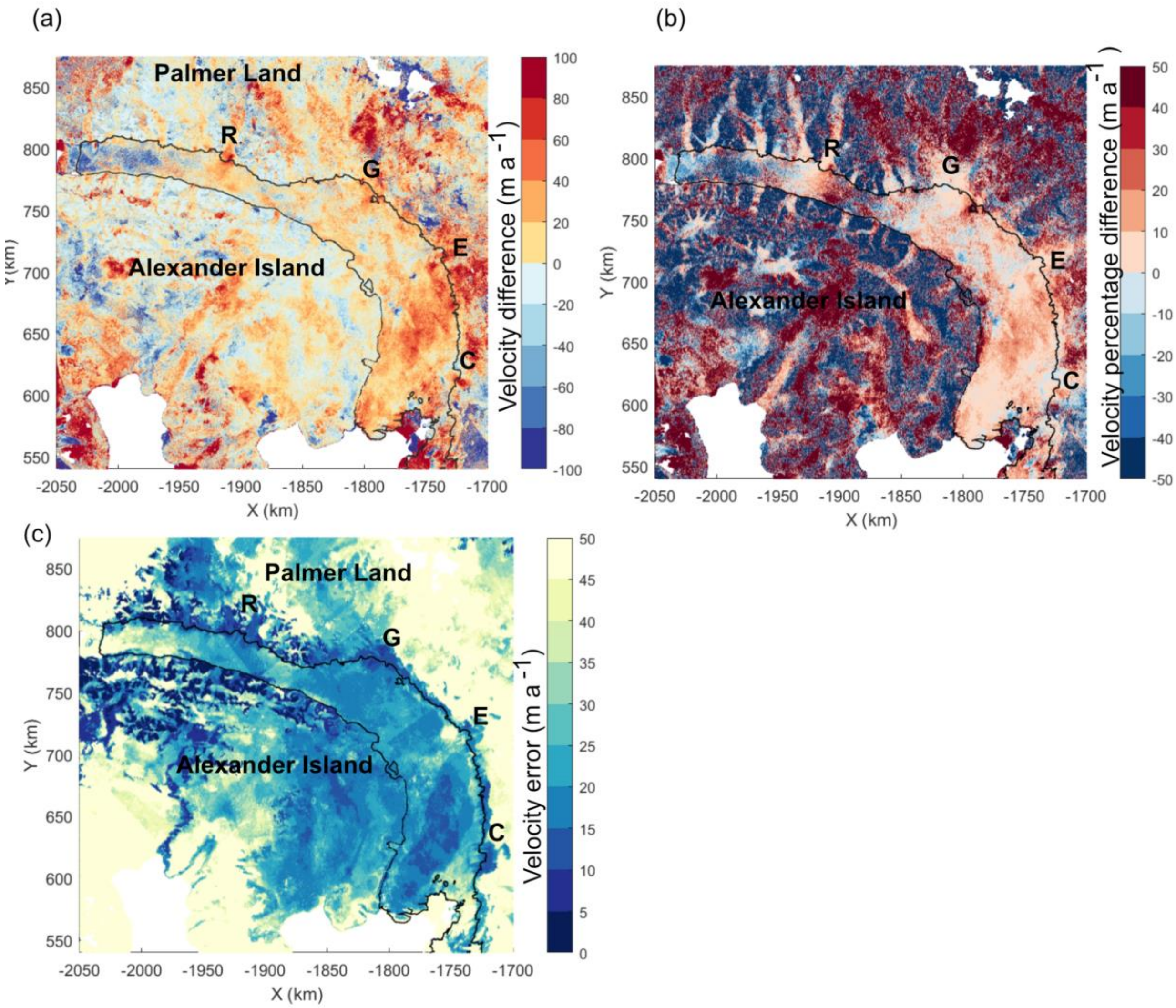

Figure 2 in the manuscript: (a) ITS_LIVE (Gardner et al., 2018) velocity differences from 2022 and 2013 (v2022-v2013), R indicates R, G, E, C are Ryder Glacier, Goodenough Glacier, ERS Ice Stream and Cryosat Ice Stream; (b) Percentage difference in velocity. The southern sector shows an increase in velocity compared to the northern sector and is largely driven by the major outlet glaciers and ice streams; © Mean velocity error between the two years. The velocity errors are provided in the ITS_LIVE database.

Other uncertainties: Uncertainties in dynamic thinning and ice flux were already calculated in the previous manuscript (Figure 6). As per reviewer's comment, we will include a separate section in methodology for error estimates. The errors are calculated via standard error propagation method. I have used the same technique I use in Das et al., 2020, JGR (Basal melt paper that also uses dynamic thickness change).

Most of the dynamic thickness change equations of this paper comes from my previous Das et al., 2020 paper, and I have referred that paper. Some expansions of the equations are from standard mathematics text book that do not require any references.

Updated Figure 3 with strain rates from the modeled and observed, and the strain rate differences between 2022 and 2013 are also provided below. This addresses the results of our new dataset, as well as reviewer comments on the modeling section. The modeling section comments are also addressed in detail in the General comments.

Differences in strain rates (2022-2013) are provided as a separate panel in Figure 3 below. This figure will replace the original strain rate figure in the manuscript.

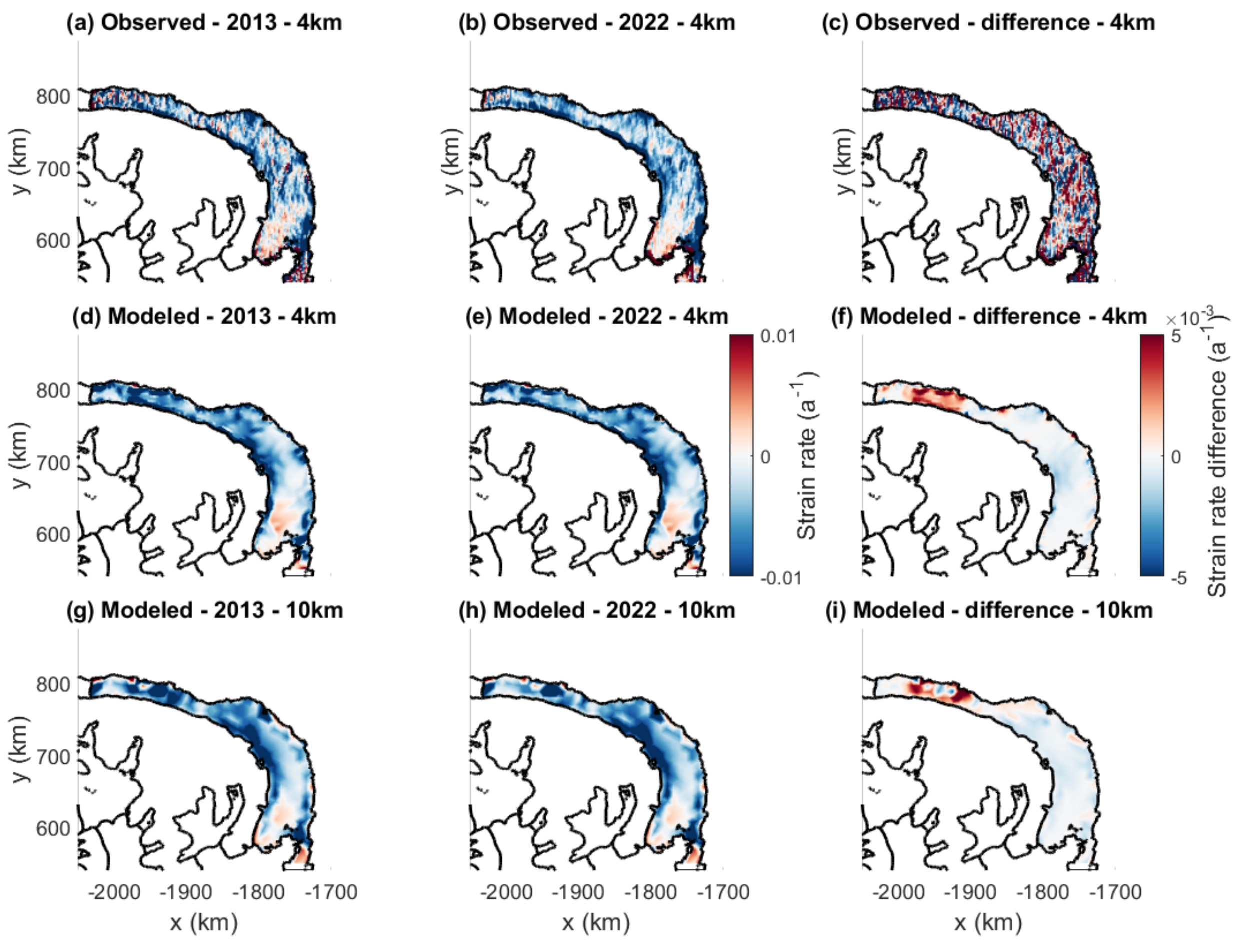

Figure 3 (main text): Strain rates from 2022 and 2013 observed (a-c) and modeled (d-i). The modeled strain rates are over 4 and 10 km. to demonstrate the importance of length scales for a smaller ice shelf such as GVIIS. The differences in the strain rates are also provided.

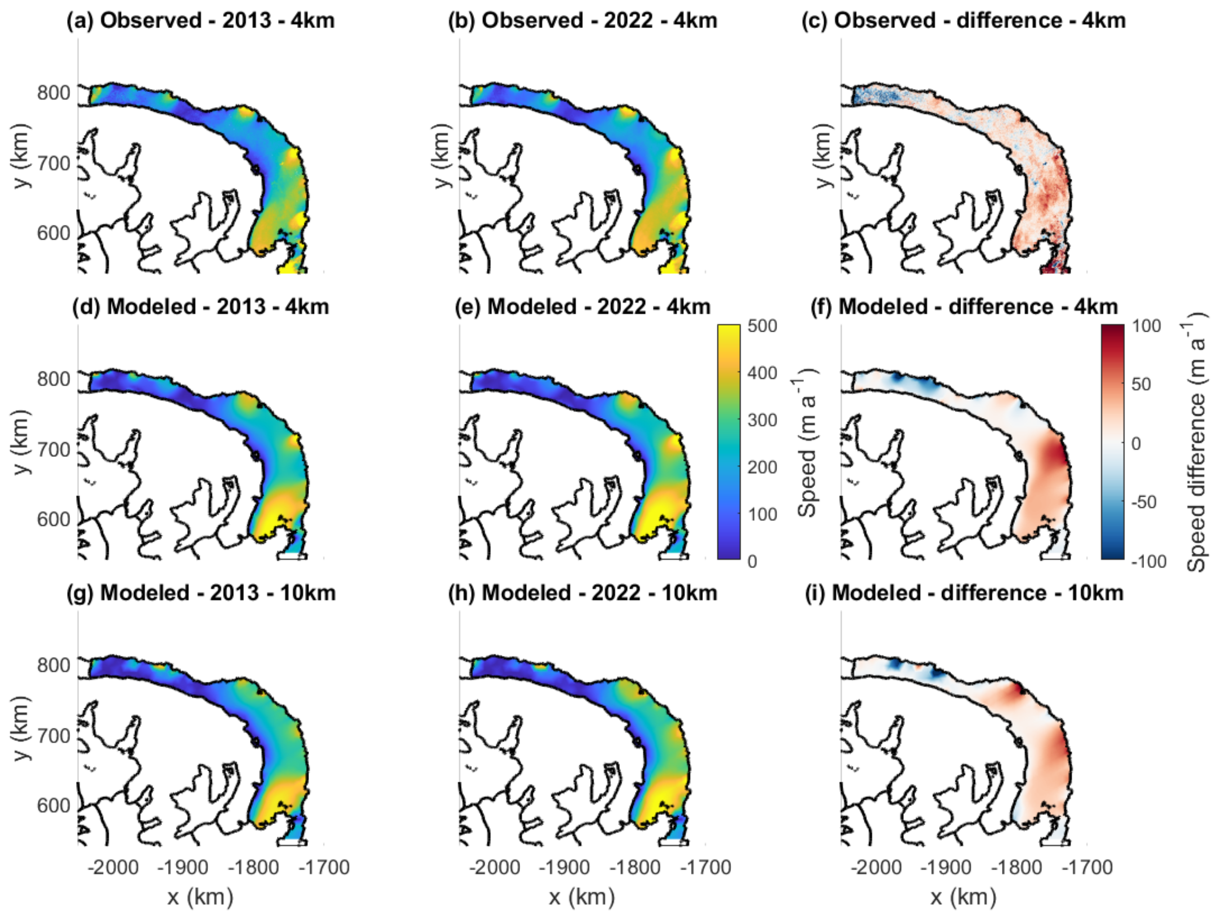

Figure Supplementary: Observed and modeled ice velocity for 4 and 10 km (left and middle panels), differences in the velocity from 2022 and 2013 (right panel).

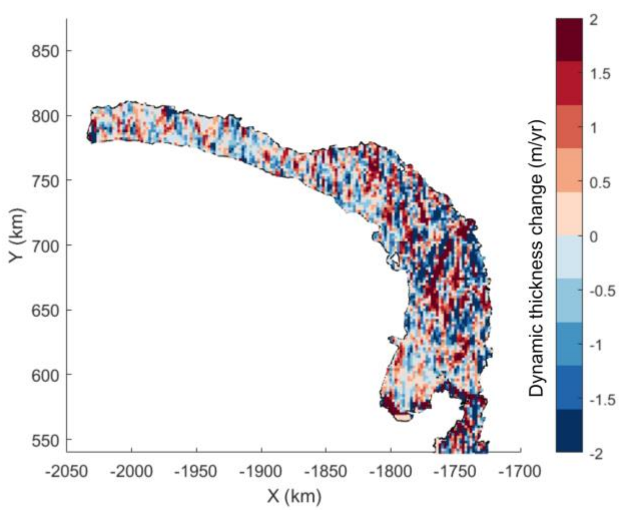

Figure Supplementary: Total strain-induced dynamic thickness change (m/yr) from 2013 to 2022.

The streamline version will be in the main text similar to the previous manuscript.

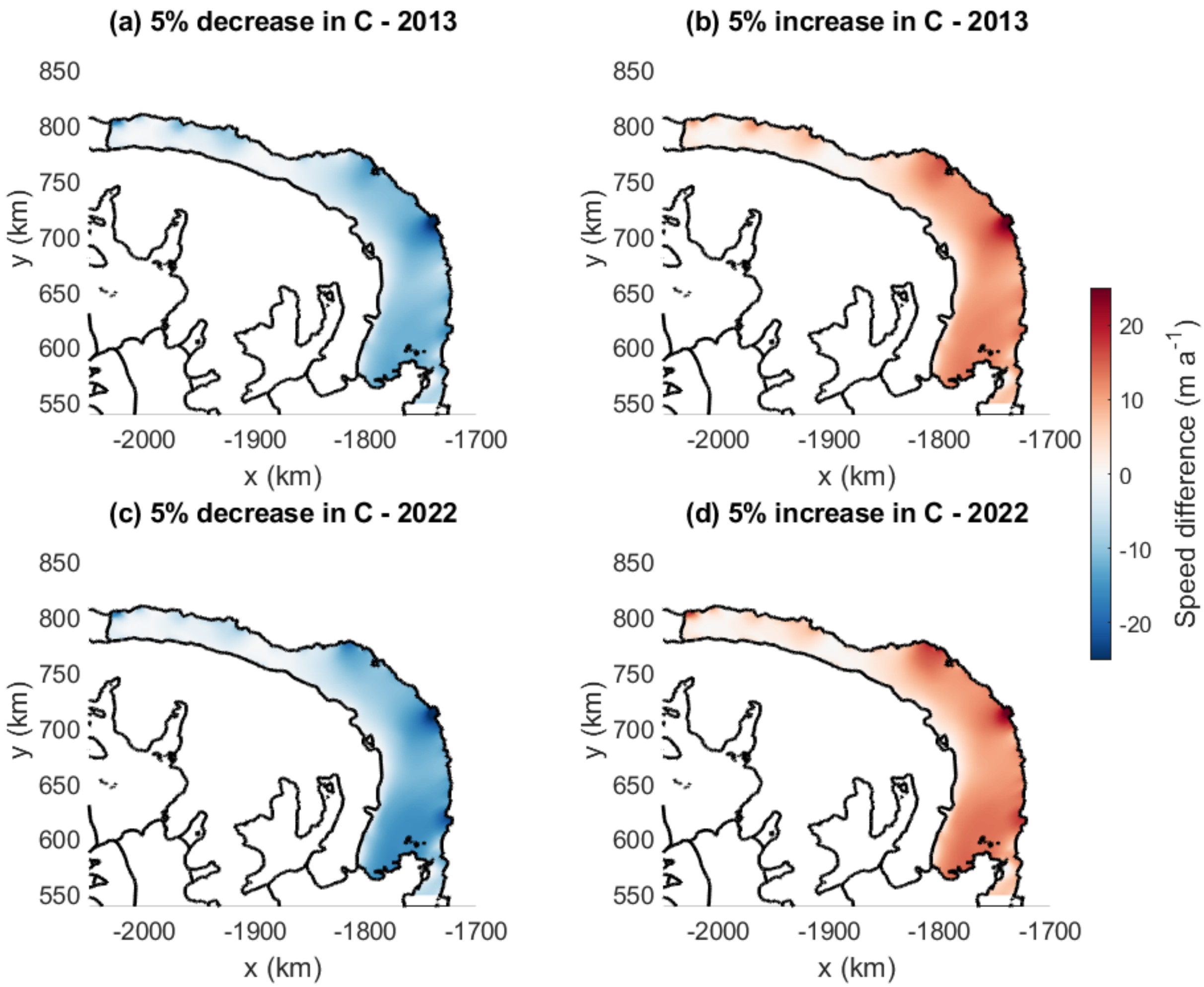

Figure Supplementary: Modeled speed difference as C is changed.

This figure will be included in the Supplementary and will address reviewer question, please see general section for further explanation.

Errors:
Velocity error:
As shown in Figure 1 of this response document, the velocity errors are calculated as follows:

$$\mathrm{v}_{\mathrm{error}} = mean(verror_{2013}, verror_{2022})$$

V_{error} is provided in the ITS_LIVE database and the total velocity error is shown in Figure 1c.

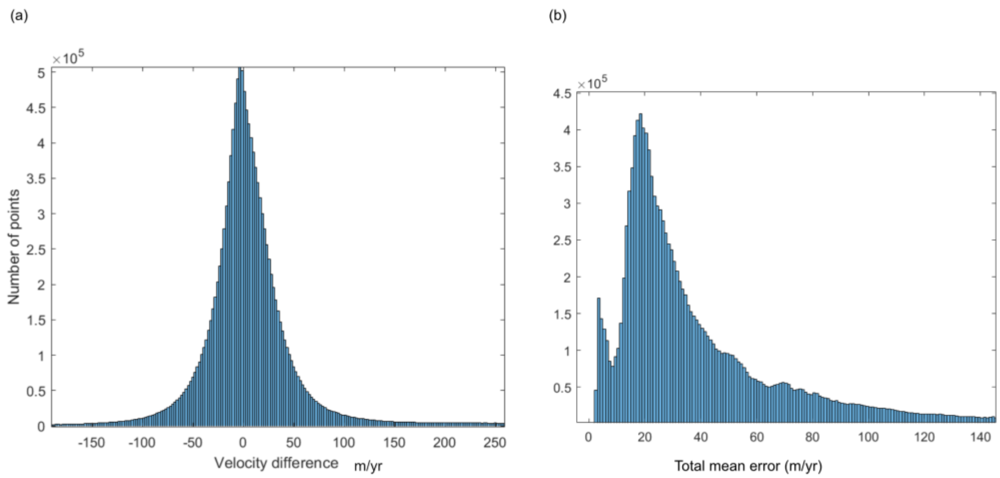

Figure Supplementary: (a) Histogram of velocity difference (m a^{-1}); (b) total mean error calculated using the equation above.

We had included detailed error estimates in the previous manuscript. This is what we will follow this time around as well. These equations use standard error propagation technique. The errors from ice flux and dynamic thinning was included in Figure 6 of the previous manuscript which will be similar to ours this time as well. From the previous manuscript:

Error for ice flux:

The error estimates are calculated separately for the strain thinning and ice flux as follows:

$$\epsilon_{Flux} = \Delta F_{mean}\sqrt{\left[\left(\frac{\epsilon_{velocity2013}}{M_{velocity2013}}\right)^2 + \left(\frac{\epsilon_{velocity2018}}{M_{velocity2018}}\right)^2 + \left(\frac{\epsilon_{IceThickness}}{M_{IceThickness}}\right)^2\right]} \qquad (9)$$

where ϵ_{Flux} is the error in ice flux in Gt km^{-1} a^{-1}, ΔF_{mean} is the mean flux change within the 20×10 km boxes, ϵ is the standard deviations of the various components, M is the mean of the various components used in calculating the ice flux.

Error for dynamic thinning:

The errors in dynamic thinning are calculated for each component of velocity for both 2013 and 2022. *The error in ice thickness is counted twice because of the two years used in dynamic thinning calculations.* We will also use mean values of two years this time to see if anything changes.

$$\epsilon_{DT} = \sqrt{D_{diff}{}^{2}\left[\left(\frac{\epsilon_{Xvelocity2013}}{M_{Xvelocity2013}}\right)^{2} + \left(\frac{\epsilon_{Yvelocity2013}}{M_{Yvelocity2013}}\right)^{2} + 2\left(\frac{\epsilon_{IceThickness}}{M_{IceThickness}}\right)^{2} + \left(\frac{\epsilon_{Xvelocity2018}}{M_{Xvelocity2018}}\right)^{2} + \left(\frac{\epsilon_{Yvelocity2018}}{M_{Yvelocity2018}}\right)^{2}\right]}$$

(10)

where ϵ_{DT} is the error in dynamic thinning (m a^{-1}), D_{diff} is the difference in dynamic thinning between 2013 and 2022, ϵ terms within the square root are the standard deviations of the various components, and the M components are the average values of those components.

General comments

- The introduction is too long. Consider removing some unnecessary information.

We have worked on this part of the manuscript.

- Consider reducing and merging Sections 2.1 with introduction and 2.2 with methods.

We have improved this section.

- Section 3 (Methods) is confusing. Please, see my suggestions on the specific comments.

Done.

- The majority of the results are presented without uncertainty. There are some small changes that an uncertainty is really needed. Furthermore, a histogram of all the velocity changes is desired with error bars.

Velocity changes are included in the figure 1 added to this manuscript. The errors are provided by ITS_LIVE and now plotted in the Figure 1 c. These are mean errors of the two datasets. Their errors are high, and therefore ours are too. The histogram is also provided.

There is not much we can do about the velocity errors. Everyone in the community will be using these datasets. We have a 9-year gap between datasets now. The errors will be high for everyone who will use this dataset.

We have included velocity errors and velocity change errors now in the supplement.

- The section of discussion includes the method of error calculation. This part should go to its specific section.

This is now moved to Methods section. It is just a style issue, but because we have included the error estimates for velocity as well, we have now created a separate section in Methods for this.

Specific comments

L56: I think citing Cook et al. (2016) is important.

Yes, we agree. It is now included, along with all the other suggested references.

L63: This is the first time that the acronym GVIIS is used (excluding the abstract). Define it here instead of defining on the next line.

Yes, done.

L65: I would change the order and remove the redundant definition of the acronym: "In terms of ice thickness, George VI Ice Shelf (Fig. 1), the largest ice shelf on the western AP, appears to be…". I think it will improve the reading flow.

Done

L72: Calling a 0.5 increase of 78.5 is too strong. furthermore, the precision of 79 is lower than 78.5, which would be rounded to 79. You also need to take into account the error. I would avoid calling this change an increase.

Yes, agreed.

L82: Do you think citing a specific figure from the Schannwell publication is necessary? I checked their figure and it did not explain more. Instead, consider adding a citation to your previous claim (namely "..have distinct structure, topographic settings and connectivity to the Bellingshausen Sea").

Maybe not. We have removed it.

L93: I did not understand the sentence "This complexity has led to uncertainty in the greatest risks to GVIIS". Please, rephrase.

Okay.

L107 – 111: Consider removing this information: "with the onset of retreat... limited paleo proxies exist south of GVIIS"

We have kept tis sentence because we want to make a point that paleo observations do not exist south of GVIIS, and studies use just limited observations in north to make a claim. We do need more paleo data in the South. That was the point of that sentence. We will, however, try to rewrite it so that the point comes across clearly. Thank you.

L142: I think a sentence must be added to explain why and how the flow lines of the middle cross the whole shelf. Does all the ice mass disappear through melting?

I have REMA DEM strips which shows extensive pressure ridges at the edge of GVIIS on the Alexander Island side. It appears that ice from Palmer Land dominates as they are from larger outlet glaciers and pushes the ice from the Alexander Island side. I do not know what happens after that. There are many melt ponds on the pressure ridges, so there is some melting. One is not sure where the water drains to. It is a complicated area.

L144: Are the "black boxes" actually yellow?

Yes, sorry about missing that.

L144: I would change "20x10" to "around 20x10", since they are not always the same.

Okay.

L144: "km extent" should go inside the parentheses.

We decided to keep it as is. It’s a style thing.

L168: I would put the years in subscript.

This looks better to me actually. The subscript makes it difficult to read sometimes.

L173: Clarify why you are not interested in velocities too slow or too fast.
In this version, we have included all velocities.

L174: Change "because of" to "due to".
It is style, we decided to not change this.

186: This section is missing references, and it is confusing. You should better explain why equation 2 is important for your work. It should also be combined with the information of the second invariant of the strain rate tensor, because then you can use Equation 2 to invert from observations to strain rate. In general, this subsection must be rewritten.

Equation 2 is just the mathematical expansion of the dynamic thickness change term. We
We have used these terms in Das et al., 2020 to calculate dynamic thickness change. This is a good representation of strain-induced dynamic thickness change. We will reword this section to clarify.

L189: Add a reference to the vertically-integrated mass-conservation equation.
done
L190: H is thickness, not thickness change.
Yes. Corrected.
L191: Justify why you can assume that they are the same. e.g. fast flowing regions near the terminus are dominated by basal sliding.
We have to assume by necessity, because we do not have the vertical velocity profile. We have clarified this in the manuscript.
L202: The word "change" is underlined.
Corrected
L205: How did you generate the streamlines?
In MATLAB. They have built in functions.
L210: Please, make a reference for the ice density, This value is for pure ice. Most of the discharge calculations assume 900 kg m^{-3}, since some air is always trapped in the ice.
We have used 900 kg m^{-3} this time. However, there is a lot of values that can be used between this and the one we have used.

L211: Is it the first time that this method is used? If not, provide a reference. I am wondering how precise it is. Averaging the thickness and the velocity loses a lot of information. Why you did not use the flux gate method (e.g. Shahateet et al. 2023) instead? You have all the required information for that.
We are calculating the ice discharge along the streamlines. The 20x10 km boxes are also aligned along the streamlines to show where the ice packet is originating from. We acknowledge Shahateet et al, but our method is also correct, as the boxes are small and aligned along the streamlines. In any case, larger glaciers have multiple boxes oriented along the streamlines, and we capture most of the information.

L212: I think years in subscript would be better.
Subscript becomes too small to read clearly.
L225: The original Shallow-Shelf Approximation (MacAyeal, 1989) assumes no basal drag. Please, clarify how you introduce the basal drag or provide the reference for that. Bueler and Brown (2019) can be helpful.
We are actually using the Shallow-Stream Approximation, which does include a drag term. The text has been corrected to reflect this. We have also added a further reference to a paper which includes a derivation of the equations behind Úa (Gudmundsson, 2008).

L241: Bigger integral symbols.
Done
L278: 8% increase is not much. An uncertainty estimation would be nice to compare the signal with the noise.
done
L291: There is no legend for the colors of the lines. Figure 2c is not well explained. The grounding line is at the same distance for 3(2?) different glacier?

It is the same grounding line, but it curves around. So the GL appears at different places on different glaciers. Figure 2 c shows how three streamlines from the same ERS Ice Stream can have different velocity changes, and if being buttressed and freely floating can influence the profiles. We will explain it more in the new manuscript thusly.

L331: This section is more methodological.
Yes. It will be moved appropriately.

L348: How did you calculate the error? Its method is in discussion, and I was wondering for a long time how you calculated the error, since it is expected to be in methods. Furthermore, a sentence discussing the high values of the uncertainty compared to the total change of F is desirable.
Equations 9 and 10 of the previous manuscript. It was in the discussions, now we have moved it to the methods section. We use standard deviation which varies widely because of velocity changes within the boxes. This is why we have large errors.

L353: Again, the uncertainty of the "2 m a^{-1}" is important.
Yes, we have included error estimates for this number in the new manuscript. 30% error in this value.
L354: Please, cite the "previous studies" you are referring to.
Adusumilli et al., 2018. Although their rates of basal melt are slightly higher in absolute values.

L356: A legend on top of the scale bars would be helpful. For example, "point colors", "field colors".
Not sure what you mean by that. We decided to keep it as it is.

L392: Please, add uncertainty.

okay
L397: I would expect the uncertainty calculation to be presented in methods, not here. Also, add a reference to the equation.
Standard way of calculating errors using propagation of uncertainty

L402: A legend on top of the scale bars would be helpful. Otherwise, the legend of the figure is incomplete.
Not sure what the reviewer means. I will take a closer look.

L411: Idem L397.
Not sure what the reviewer meant here.

L426: I would make clear that the simulation was made by you, starting the sentence with: "Our simulation experiment shows that…"
Done
L438: Is this "2 m a^{-1}" a mean value? If so, say it. Also, present the uncertainty.
Done. It is an approximate value. Because you ask for uncertainty we will need to take a region for our calculation of this value.

All references will be added in. Thanks for providing these.

References of my comments:
Bueler E, and Brown J (2019), Shallow shelf approximation as a “sliding law” in a thermomechanically coupled ice sheet model, J. Geophys. Res., 114, F03008, doi:10.1029/2008JF001179.
Cook AJ, Holland PR, Meredith MP, Murray T, Luckman A and Vaughan DG (2016) Ocean forcing of glacier retreat in the western Antarctic Peninsula. Science, 353(6296), 283–286, ISSN 10959203 (doi: 10.1126/science.aae0017)
Shahateet K, Navarro F, Seehaus T, Fürst JJ, Braun M. Estimating ice discharge of the Antarctic Peninsula using different ice-thickness datasets. Annals of Glaciology. 2023;64(91):121-132. doi:10.1017/aog.2023.67
Citation: https://doi.org/10.5194/egusphere-2024-1564-RC1

• RC2: 'Comment on egusphere-2024-1564', Anonymous Referee #2, 03 Oct 2024
Reviewer1:

1. Ice velocity data: The authors should explain their choice and processing of ice velocity data in more detail to justify why it is suitable for this study. In particular, no detail is given on how the ITS_LIVE velocity mosaics the study uses are processed and produced. I know that this is detailed by the Gardiner et al. 2018 paper, however I think it is important to provide some level of detail in the manuscript for the reader, because since that paper came out in 2018, there are a number of new ITS_LIVE products available (Lei et al., 2022). I assume the authors have used the LandSat mosaics from the 2018 paper, not other products with SAR data included, but

this is not clear, so more detail is needed. Did the authors consider other ice velocity products for Antarctica, for example the MeASUREs annual mosaics (Mouginot et al., 2017) or monthly mosaics from ENVEO for the ESA CCI project (https://cryoportal.enveo.at/data/)? The authors must also justify their choices to exclude velocities below 1 m/a and above 2000 m/a and where the error is > 15 m/a. What is the impact of these exclusions, what % of data points does it remove?

We have addressed this concern at the beginning of this response document. Both ITS_LIVE and MeASUREs InSAR are widely used in the Cryospheric community. We just used ITS_LIVE instead of InSAR and it is a valid choice of dataset.

For ice discharge calculations, the authors have not attempted to account for firn air content changes, nor ice thickness changes between 2013 and 2018. Any potential ice thickness change could be evaluated from publicly available altimetry datasets. I think it is important to consider ice thickness change, because a significant focus of this paper is dynamic thinning. At the very least, the authors should justify their choice not to account for these terms.

This is an important point. We did consider dh/dt and firn thickness. But the scope of this paper is strain-induce dynamic thickness change. We calculated those terms to isolate their scope and study them. We assume that the ice thickness would not change much in steady state. To add or subtract dh/dt on a dynamic ice shelf will not be trivial. For this paper, it is better to focus on one parameter and the rest can be a future scope. Firn air content is notorious to constrain. However, the reviewer's concerns are well founded, and we will try to add a sentence in the manuscript stating why we opted out. It would be too much for the current scope of the paper.

2. Comparison between observations and modelling: Overall in this manuscript, I feel that the comparison between observations and modelling could be significantly improved and the links between the two made more explicit. In particular, it would be beneficial to report the difference in C between the 2013 and 2018 Ua inversions. This would provide context for changing the parameter C by 5%. Currently, I am not sure if 5% variance is a large amount or a small amount for this model setup.

The 5% change to C is a small perturbation. In equation 5, C is raised to the power of -1/3, so changing it by 5% causes ~1.7% change to the basal stress.

A direct comparison of C fields between the two years is unfortunately not a particularly insightful exercise, as they have come from two entirely separate inversion calculations. The inverse problem is ill-posed by nature, so carrying out different inversions, even using similar inputs, can lead to very different solutions in the C-field while still producing velocities close to observations. In our case, the C fields for 2013 and 2018 were calculated using velocity datasets which differ in their spatial coverage and measurement error (due to differences in available data for each year), causing variation in the values of C. Differences between C fields calculated from different data cannot be easily interpreted in a physically meaningful way, as there are so many variables in the inversion process.

When the C field has been chosen and the velocities calculated using it, small perturbations to the value can then provide useful insights into the system by looking at their effects on the velocities. This perturbation of the C-field is done in a controlled way which allows changes seen in the velocities to be interpreted, in a way which differences between the C fields cannot.

We have updated Fig. 5 to show the effect on strain rates of changing C in both years (2013 and 2022 using the new data), rather than just one. The new version of the figure is available at the bottom of this document.

I am further concerned that the modelled strain rates for 2013 and 2018 (Figure 3c & 3d) appear to contradict the conclusions of the paper. This plot shows a positive change in strain rate between 2013 and 2018 for the northern GVIIS around Ryder glacier ie a decrease in compression, however there was an increase in velocity and discharge in the observations in this period (Figure 4, line 300). Can the authors explain how this is consistent with their conclusion than increased ice discharge increases compressive stress for the NGVIIS? Additional plots of ice velocity and ice velocity change between Ua runs may help to clarify this point.

Due to this and the previous point, we took another look at our inversion process and found that data gaps in the measurements used from 2013 were causing abnormally large velocities on a section of the western coast opposite Ryder Glacier, at around -1950km on the x-axis. These large velocities were an artifact of the numerical procedure filling in the data gaps, and were not present in the 2018 velocities. Hence, in this section the model was actually producing a large and unrealistic decrease in ice discharge. The new version uses a more recent dataset, and efforts have been made to reduce this anomaly. However, the observed velocity changes between years are within the error margins of the data, and within the magnitude of the velocity misfit in the inversion outputs. We still see a decrease in velocity between 2013 and 2022 on the northern section of the ice shelf in the model outputs. Extra discussion is being added in the manuscript to emphasize the limitations of the modelling, and where the model results are most useful (i.e., the effects of increasing/decreasing C and how the strain rates respond to changes in velocity when forced in this way are the more robust modelling result, rather than the details of the strain rate changes between years). We have also updated Fig. 3 to include new panels showing the differences between strain rates, to make the figure easier to interpret. A figure showing ice speed and differences from the Úa simulations will be added to the supplementary material. All new/updated figures are available at the bottom of this document.

3. Errors and uncertainties: Throughout the paper, the presentation of errors and uncertainties is lacking and inconsistent. Quantities are often quoted without an accompanying

uncertainty, including in parts of the manuscript where these values are important to the conclusions. See line-by-line comments below.

We did calculate the errors for the parameters (equations 9 and 10 of the last manuscript). They are included in the error estimate figure (ice flux and strain-induced dynamic thinning). Please see our detailed explanation at the beginning of this response. We have now moved the error calculation in the methods section.

4. Presentation: The quality of the figures in this manuscript is somewhat disappointing, with missing or incorrect keys and subplot labels. Additionally, the referencing throughout the manuscript falls well below the standard I would expect for The Cryosphere. In numerous places, significant statements are made without justification or supporting references. The authors must address this for the manuscript to be suitable for publication in The Cryosphere.

Figure 1 subplot has been labelled now; Figure 3 has been updated. Thanks for pointing it out. It was an oversight as the correct figure was left out and a previous version erroneously uploaded.

Line-by-line comments:
55: I think you also need to cite: 'Ocean forcing of glacier retreat in the western Antarctic Peninsula' Cook et al. 2016.

Yes. We have included this in the current version.

55: This sentence and the previous one together are a bit confusing. Does the retreat refer to tidewater glaciers, ice shelves, or both? Consider clarifying. The authors should also discuss how atmospheric warming has also been linked to retreat here. For example the fact that the collapse of ice shelves on the AP was primary linked to atmospheric warming, melt ponding and hydrofracture (Rack and Rott, 2004; Rignot et al., 2004; Vaughan and Doake, 1996).

Reviewer 2 caught a fundamental omission in the first sentence. It is retreat in the "western" Antarctic Peninsula that we refer. However, in light of your suggestion, we will include how the AP glaciers retreat generally by atmospheric warming and then on the western side there is also warmer ocean water. I will include the references suggested.

56: I'm not sure that the Hogg and Gudmundsson paper cited here is the right paper, because it's about the calving of a giant iceberg from the Larsen-C ice shelf. Did the authors mean to cite: 'Increased ice flow in Western Palmer Land linked to ocean melting' by Hogg et al. 2017?
Yes. Thanks for pointing that out.

57: Other useful references for the ocean induced retreat and acceleration of tidewater glaciers on the west AP: Ocean forcing of glacier retreat in the western Antarctic Peninsula, Cook et al. 2016. Ocean warming drives rapid dynamic activation of marine terminating glacier on the west

Antarctic Peninsula, Wallis et al. 2023. Widespread increase in discharge from west Antarctic Peninsula glaciers since 2018, Davison et al. 2024.
Yes, thank you.

63: The authors might also consider citing the references above here and also: 'Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf, Antarctic Peninsula', Friedl et al. 2018.
Yes, thank you.

63: The Wallis paper referenced here is about seasonal ice speed variations in the west AP, rather than widespread acceleration. A better reference might be: 'Widespread increase in discharge from west Antarctic Peninsula glaciers since 2018' Davison et al. 2024.
Thank you. This will be included.

70: Again, I think this is the wrong Hogg citation.

We will carefully look at appropriate Hogg references here and elsewhere in the manuscript.

72: These discharge figures should be quoted with an error. Also, I don't think it's fair to call this an increase when 0.5 Gt is likely well within the uncertainty of these measurements.
The discharge figure had errors calculated in the second panel. Agree on not calling it an increase when it is within the uncertainty. However, these days we have gotten better at constraining uncertainties and most datasets are accompanied by large uncertainties. Our velocity data is a good example.
In this work, if the values consistently increase over the 9 years in various datasets, we consider them an increase. However, we will carefully frame our increase here and elsewhere in the manuscript.

85: Please reference a paper or bed elevation dataset for these statements about the height of the glacier beds.
BedMachine. We will include the reference.

87-91: The sentence 'Strong gradients…' needs substantial references to back it up, otherwise it is too vague. The authors could consider citing: 'Drivers of Seasonal Land-Ice-Flow Variability in the Antarctic Peninsula' Boxall et al. 2024, but I think more references than just this will be needed to back up this statement.
Okay.

119: Substantially more references are needed to back up these statements on upstream processes, for example no reference is given for enhanced lubrication of the bed by surface meltwater penetration.
This is a very well-accepted phenomenon. It is observed in Antarctica and also very widely in Greenland. Many papers through decades have worked on this problem.

131: This sentence about the thinning of the GVIIS needs expanding. The authors say that measurements suggest a net thinning, but it’s within the range of uncertainty. Likewise, on line 65 in the intro the authors say GVIIS appears to be thinning, but do not mention the range of uncertainty. I think it would be much clearer for the reader if the authors directly quoted the thinning rates and uncertainty measured by previous studies and discussed the spatial distribution of melt rates. This would allow the reader to draw a more informed conclusion about the significance of observed melt rates.
Done.
140: This statement about MISI should be supported by a reference.
Done.
142 Figure 1: It would be beneficial to also show the whole ITS_LIVE velocity field that these streamlines are extracted from. This could be done in a supplementary figure.
Figure included.

142 Figure 1: The coordinates for this figure are not useful without saying which coordinate reference system is being used. I assume that for this plot it is EPSG:3031. This should be explicitly stated, as other polar stereographic CRSs are available, or the authors should provide a Lat/Lon grid overlay.
Generally, it is understood that it is EPSG 3031, the grid that everyone uses and all datasets ate referenced to. However, I will include this in the manuscript.

142 Figure 1: The coastline and grounding line data used for this figure should be referenced in the caption, here and in other figures.
Okay.
152: This section needs to be significantly expanded, see general comments.
Yes, agreed.
161: This statement about which dynamic components are most likely to affect the stability of an ice shelf should be justified with references.
All dynamic components affect stability in various degrees. People have not studied dynamic components separately much. But we reword this and include references where appropriate.

167: See general comment about ice velocity data used.
We have elaborately addressed this.

183: The authors should provide a more robust justification for their decision to exclude glaciers from Alexander Island. For example, by quantifying the difference in ice discharge or velocity.

These glaciers are very small as the velocity figures show. The ice thickness if also not very high. We have included an ice thickness map in the supplementary and have a paragraph there why they may not be important for GVIIS. We have not done any extra calculations. I think this is sufficient.

189 equation 1: a is not defined.
Accumulation rate. Will include in the manuscript.

190: Surely H is the ice thickness, not the change in ice thickness through time?
Yes, H is the ice thickness. Sorry about the typo.
208: These boxes are yellow in Figure 1.
Yes.
278: All these speed changes should be quoted with an uncertainty.
Yes. Corrected in the current version. Velocity errors provided in Figure 1 c of this response document.
279: It would be good to show the absolute speed change on a map, too. This could be a supplementary figure.
Included. Please see Figure 1
292 Figure 2: What do the different colors mean in panel b/c?
Different profiles/streamlines on the same glacier.

292 Figure 2: the GL marker on panel c shouldn't cross all the axis like this, it should match panel b.
Okay.
307: This sentence is confusing, because it mentions the velocity increase for ERS and glaciers south, but then talks about meltwater at Ryder glacier. Please clarify.
We will reword this and clarify. Because there is surface melt on the ice shelf where Ryder drains, we speculated that water could be draining in. However, in this version we are contemplating removing the speculation.

309: I don't think this reference to Pedley et al. supports the statement that meltwater may reach the bed at Ryder Glacier. This paper is about meltwater drainage from the surface of the ice shelf into the ocean at the shelf margin.

No, it doesn't, however it works on GVIIS and maybe there are other areas. we were hinting at a possibility. It's a speculation. We will reword this.

326 Figure 3: Subfigures are not labelled.
Sorry about that. Now corrected.

331: This paragraph repeats points from section 3.6. Consider merging these.
Yes, done.
352: the figure of 2 m/a must be quoted with an uncertainty.
Yes, please see our response to reviewer 1 as well.

354: Which previous studies? This must be referenced appropriately.
Yes, please see our response to reviewer 1.

357 Figure 4: The colormap chosen for ice flux change is confusing, because at first glace it appears to be divergent, like the colormap for thickness change, but actually it's 0 to 30. Consider changing it to a non-diverging colormap.

Finding an appropriate colormap is exhausting when we have so many unique/independent datasets and parameters. Point well taken, we will try to accommodate this comment, although we have tried before as well.

382 Figure 5: It would be beneficial to also show ice velocity change due to modifying C.
Please see this figure at the beginning of this response. This figure has been added to the supplementary material to show the change in speed, and is available at the bottom of this document. In combination with Fig. 5, it clearly shows the relationship between increasing ice discharge and lowering strain rates very clearly.

388: This comparison is written in a confusing way and should be clarified. Are you saying that the seasonal velocity fluctuations observed by Boxall et al. are comparable to the overall acceleration measured in this paper between 2013 and 2018? Is this a fair comparison?

While this is not a fair comparison, it is probably well understood that velocity changes are more likely to happen in the summer than the winter, unless a subglacial channel triggered in the winter. We will reword this to clarify.
396: Details of error calculations should be in the methods or a supplement.
In methods now. It is just another style of writing.

400: Does this standard deviation refer to the standard deviation within the averaging boxes? If yes, then using the error provided with the ice velocity or bed elevation products would be more suitable and those should be used if they are available.
We have used standard deviations within the boxes in all our published works. The deviations themselves are very large, I think this is another way of calculating errors. Please note that in the velocity change estimates, we have used the error provided in the dataset as well. In addition, the value of density used in flux calculation will not have an error values associated with it.

434: Please explicitly quantify the change in strain rates for your experiment here. The conclusion of the paper relies on it.
Yes, we will try to make it clearer. The figure is included at the beginning of this response document.
451: The sentence 'Similar warming…' must be justified with refences.
Banwell et al., 2021. Banwell, A. F., Datta, R. T., Dell, R. L., Moussavi, M., Brucker, L., Picard, G., Shuman, C. A., and Stevens, L. A.: The 32-year record-high surface melt in 2019/2020 on the northern George VI Ice Shelf, Antarctic Peninsula, The Cryosphere, 15, 909–925, https://doi.org/10.5194/tc-15-909-2021, 2021.

Thanks for the references.

Our references:

Gudmundsson, G. H.: Analytical solutions for the surface response to small amplitude perturbations in boundary data in the shallow-ice-stream approximation. The Cryosphere, 2, 77-93, https://doi.org/10.5194/tc-2-77-2008, 2008.