Modelling GNSS-observed seasonal velocity changes of the Ross Ice Shelf, Antarctica, using the Ice-sheet and Sea-level System Model (ISSM)
– Response to reviewers –

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Firstly, we would like to thank the editor and all three reviewers for their constructive and detailed comments. We agree with many points that were raised, especially the lack of discussion of our results in the context of recently published papers (e.g., *Klein et al.* [2020] & *Mosbeux et al.* [2023]). We have responded to each reviewer's comments below.
All three reviewers' main comments included the need for additional discussion and consideration of *Klein et al.* [2020] and *Mosbeux et al.* [2023]. This is a good point that we will address throughout a revised manuscript, as we realize that we did not adequately motivate and contextualise our study, and these previous works deserved more recognition.

Novel contributions

Several reviewers questioned the novelty of aspects of our study. Here we briefly summarise aspects of our study which we believe are novel contributions.

- We present new GNSS time series from the Ross Ice Shelf that have not previously been published. These include sites near the calving front, near a significant pinning point, and in the deep interior of the ice shelf near the grounding line.
- Notably, our Site 2 is close to the calving front near the Ross Island region, which has been identified as observing high basal melt rates on a seasonal timescale (Stewart et al., 2019).
- We show that these measurements consistently show 2 peaks in ice shelf velocity every year (for Sites 1, 2 and 4, the newly collected datasets), contrary to previous measurements presented in *Klein et al.* [2020] and *Mosbeux et al.* [2023].
- We suggest that the seasonal variability of SSH (i.e., yearly cycle) may not be able to reproduce our GNSS seasonal velocity variability (i.e., semi-annual).
- We therefore turned to the potential role of basal melt and wanted to test *what it would take* to match velocity variations by changing the forcing as little as possible.
- Our approach of combining Automatic Differentiation and weekly MITgcm basal melt rates ([*Klein et al.*, 2020] used monthly basal melt rates) is also novel.

Sea surface height (SSH)

An area where the two previous studies should be discussed more in our manuscript is in regards to what other factors that could be driving the observed velocity variations on the ice shelf. [*Mosbeux et al.*, 2023] nicely shows that the seasonal variability of SSH can explain their observed seasonal variability of ice velocity.

To take this into account, we will rerun our simulations with the same SSH forcings implemented in [*Mosbeux et al.*, 2023] to consider this factor. However, we expect that the seasonal variability of SSH cannot explain our two-peaked seasonal velocity variability, as mentioned above. In our revised manuscript, we will also discuss other possible factors (tides, sea ice buttressing etc), that may also be good candidates to explain our new GNSS observations.

Basal melt rates

Melt rates are difficult to model and properly constrain, especially close to grounding lines, despite their critical role on ice dynamics. All reviewers commented on the realism of the basal melt rates. We agree that these basal melt rate perturbations we use are extremely high for the Ross Ice Shelf, today and in the future. However, this paper focuses on asking whether perturbations in basal melt rates *can* reproduce a similar velocity variability as observed by the GNSS units. We acknowledge that our contribution is a proof of concept, not a definitive answer to the question, and we will do our best to make this clear in the revision.

Multiple peaks in melt rate perturbation

Several reviewers questioned our use of multiple peaks in melt rate perturbation. Here we clarify our motivation for doing so. The baseline weekly MITgcm basal melt rates include a clear peak in the austral summer, and multiple other (much smaller) peaks throughout the year, highlighting that the basal melt rates have more variability than presented in [*Klein et al.*, 2020]. We also refer to [*Stewart et al.*, 2019] basal melt observations in our discussion, highlighting that they observe the largest peak in the austral summer, but also smaller peaks in the austral winter.

Klein et al. [2020] suggest that the actual total summer increase in the heat content of the AASW layer near the ice front is likely to be larger than the modelled increase, and the seasonal enhancement of the basal melting will continue further into autumn than in their model. [*Klein et al.*, 2020] extended the late melt period to April and found that it also shifted the timing of maximum velocity a month later, showing that a longer or later melt period at the front could align the modelled and observed velocity phases.

Our approach is to use multiple basal melt peaks as the basis for our phasing of the basal melt forcing, and we apply perturbations on this forcing until we reproduce a similar velocity variability to the GNSS observations. Through this, we can highlight that seasonal basal melt rates can reproduce the GNSS velocity variability on an interannual timescale for XX of the sites. We do not state or intend to imply that these perturbed basal melt rates are realistic for the Ross Ice Shelf. Our study instead serves as a proof of concept, motivated by Klein et al. "as-yet-unidentified seasonal processes". This overall aim will be clarified in the revised manuscript.

1 Reviewer 1

1.1 General comments

As stated in the submission guidelines for The Cryosphere, I strongly encourage the authors to deposit all scripts and configuration files for setting up and running the ISSM simulations in a FAIR-aligned repository, such as Zenodo.

The scripts and configuration files for setting up and running the ISSM simulations can be found here: https://doi.org/10.5281/zenodo.11098089.

Add to the introduction paragraph on lines 80-88 a brief description of how this present study differs from [*Klein et al.*, 2020] and [*Mosbeux et al.*, 2023] which, as described in the preceding paragraph, provide an explanation for the intra-annual velocity variations for the RIS.

This is a great point, see summary above.

Additionally, please add text in the Discussion that addresses why the conclusions of this present study differ from [*Klein et al.*, 2020], which claim that seasonal velocity variations are not driven by basal melt rate variability.

Again, this is a good point, hopefully addressed in the summary above.

Use "intraannual" throughout the text to refer to monthly to seasonal variability. For example, line 73 refers to monthly to seasonal variability as "interannual" but this should be changed to "intraannual". Please check the entire manuscript for other cases of this.

Done, this has been edited throughout the manuscript.

Add a map of observed velocities of the ice shelf to Figure 1.

Figure 1 shows the modelled surface velocity results after initialisation, we think that displaying the modelled surface velocity results is sufficient since ISSM used observed velocity from the MEaSUREs dataset to calibrate the model (modeled and observed velocity therefore look similar).

Remove Figure 3 because Figure 5 shows the same data.

We think that Figure 3 should still be included as it shows the raw *observed* velocities directly from the GNSS sites, whereas Figure 5 highlights the velocity variations. The observed velocities are important to show as they highlight at which sites the velocities are flowing faster, and the seasonal changes in these velocities.

It is not clear to me whether including both Figure 2 and Figure 4 is necessary and how the interpretation differs for the results shown in these two Figures. My understanding is that Figure 2 shows the sensitivity of the final velocity for a 6-month simulation and Figure 4 shows the sensitivity of the final velocity for a 2-year simulation. I also see that Figure 2 shows sensitivities that are above the selected threshold, whereas Figure 4 shows the full range of sensitivities. However, it seems like the text in Section 3.2, which describes the results in Figure 4, could also apply to the results in Figure 2. I may be wrong, in which case please feel free to disagree. If it is decided to keep both figures in the manuscript, please add text to Section 3.2 that explains why the results in the two figures are different and what additional information for interpretation is provided by Figure 4 that isn't already provided by Figure 2.

We agree with the reviewer that Figure 2 is not necessary, it will be removed.

Add a figure showing absolute modelled and observed velocities at each GNSS site to Supplementary Materials and reference this figure on line 266.

This figure will be added to the Supplementary Materials, although we are focusing on velocity variations, not absolute velocities.

The paragraph on lines 353-360 hypothesizes that perturbing the melt rate at the KIS grounding zone could modify driving stress at Site 4, through a modification of basal friction. Couldn't you use the ice sheet model to test this proposed process? ISSM simulates changes in driving stress and the corresponding change in basal friction due to the simulated melt rate perturbations. You could analyze the changes in the force balance at Site 4 to address this. Please either add this analysis to the paper or provide text explaining why this is not possible with your model configuration.

We will model the driving stress through time at Site 4, and analyse the results. If we think they are appropriate to include in the updated manuscript, we will add a figure and explanation in the discussion. We found that it was not appropriate to include the driving stress results, as there was less than a 2% change in driving stress at Site 4. We have also removed the story around the driving stress change in lines 353-360.

Wherever possible, begin each paragraph in the Discussion section with a topic sentence that describes the main result that is being discussed in the paragraph. For example, on line 312, change the topic sentence to: "We model a seasonal signal in velocity variability that is similar in phasing and magnitude at GNSS Sites 1 and 2 but not Sites 3 and 4." Another example is on line 362, where the topic sentence could be changed to: "The melt rate perturbations used in our modelling experiments are realistic for Sites 1 and 2 but less realistic for Sites 3 and 4." Please go through the Discussion to find other opportunities to make changes to topic sentences to clarify the result being discussed in the paragraph.

This is a great suggestion, and we will go through the discussion and begin each paragraph with a topic sentence in the updated manuscript.

1.2 Specific comments

[line 13] The word ”today” seems out of place here. Can it be removed?

Done.

[line 126] I suggest adding a reference to https://doi.org/10.5623/geomat-2005-0004 to cite the CSRS-PPP specifically.

Done.

[line 175] Add a sentence to Section 2.4 stating that one set of simulations was run in which the basal melt rates were perturbed at locations where there was sensitivity in the velocities for any of the GNSS locations (as opposed to separate simulations where the melt rates were perturbed for each individual GNSS location).

We did not do this, and we apologize for the confusion. We only ran *one* set of simulations in which the basal melt rates were perturbed at locations identified as highly sensitive (using our threshold) for at least one GNSS site. We will clarify this in Section 2.4.

[line 181] Replace ”raw” with ”unperturbed”.

Done.

[Figures 2 and 4] Is the black line showing the grounding line? If so, state that in the caption and change the passive ice outline from black to a different colour.

The caption has been edited to state that the black line is the grounding line. These figures will be edited in the updated manuscript, with a different colour for the passive ice outline.

[Figure 4] Add labels and arrows showing the locations of Roosevelt Island, Crary Ice Rise, Steershead Ice Rise, the Shirase Coast Ice Rumplus, Byrd Glacier and any other locations that are refered to in the text when describing this figure.

Thank you for this suggestion, we will add labels to Figure 1 to identify the locations that are referred to in the text in the updated manuscript.

[Figure 5] Color each dotted black line using the same colors as the solid lines to denote the melt rate perturbation magnitudes.

We tried to implement this, but using different colours made it difficult for the reader to identify each line. We decided it was best to keep it as a black dotted line.

[line 264] The text states that ”use of the lower sensitivity value did not significantly affect the final modelled velocity variations” and Figure 5 shows that is indeed correct for sites 1-3 but for site 4, it looks like the velocity peaks are about 30 percent larger for the highest perturbation magnitude. I suggest quantifying the differences between the two simulations that used different sensitivity thresholds.

While we agree with the reviewer that this statement should be toned down, we are a bit resistant to play with this threshold too much as the aim of this paper is not to try and match the GNSS and modelled velocities, but rather we are trying to determine if it is possible to reproduce similar velocity variations from basal melt alone, at least for some GNSS sites.

[lines 340-341] This sentence needs to be reworded: ”Figure 2 highlights that basal melt rates are perturbed at the Ross Island region for Sites 1, 2 and 3.” to something like ”Figure 2 shows that velocities at Sites 1, 2, and 3 are most sensitive to basal melt rate perturbations at the Ross Island region.”

Done.

[lines 350-352] This sentence is repetitive and states the same thing as the previous paragraph. Please delete this.

Done.

[line 361] Change this section heading to: "Comparison to observed basal melt rates beneath the Ross Ice Shelf".

Done.

[line 384] Please define what "short-term" basal melt rates are. This is the first mention of this term and it is not clear how this is defined.

The term "short-term" has been removed.

[Figure 6] Similar to the previous comment, please define the timespans that "short-term" and "mean" are covering in the figure caption.

The term "short-term" has been removed and the caption has been edited to replace "mean" with "average".

[lines 419-423] Are these "additional" experiments ones that are already described in the paper? If not, the configuration and results from these additional experiments need to be included in paper. They can be added to Supplementary Materials or an Appendix but please add figures of (1) the locations where melt rates were perturbed and (2) the resulting velocity variations.

These additional experiments are now included in the methodology and results sections.

2 Reviewer 2

2.1 General comments

The findings of Klein et al., 2020 and Mosbeux et al., 2023 are referenced but then appear to be largely disregarded until the discussion. In particular, the potential influence of changes in sea surface height and ocean tides is completely overlooked. Tides are known to cause substantial variations in velocity over short periods (e.g. Anandakrishnan et al., 2003; Doake et al., 2002), and also potentially over long periods of up to a year (e.g. Murray et al., 2007). The GNSS processing smooths out short-term tidal effects, but I expect daily variability is large, being previously observed nearby at up to 100 percent of the mean (e.g. Brunt et al., 2010). It is not inconceivable that small, solar annual or semi-annual tides could drive the remaining ¡1 percent semi-annual variations in velocity shown in Figure 3 and it needs to be explained why they can be ignored..

Hopefully our summary above addresses this important and fair comment. In the revision, we will re-run our model experiments with the seasonal variability in SSHs and discuss the potential influence of tides in more detail.

Secondly, it is not clear to me why a more realistic melt forcing was not used? The forcing used here (and required to match behaviour at sites 1 and 2) is symmetric with two peaks, when observations from Stewart et al., 2019 show only one dominant peak in the northwest region near Ross Island. Also, the peak melting observed by Stewart et al., 2019 occurs consistently in February while here melt peaks are applied in April and October, and the magnitude of the melt perturbations appears to be substantially higher than observations, with 20 m/a additional melt required, presumably on top of a baseline rate?

Again, this is a fair point. We realize that we have not made the objective of our study clear. Our experiments try to quantify how high the melt rates need to be to explain the variations in ice speed. We highlight that these perturbed basal melt rates are unrealistic for Sites 3 and 4, and suggest that they may be more realistic for Sites 1 and 2 and are potentially likely to occur in the future due to global warming. Stewart et al., 2019 found maximum summer basal melt rates of 52 m/a at the calving front near Site 2, highlighting that our basal melt magnitudes are not that unrealistic compared to the current maximum summer basal melt rates. Please refer to the summary provided above for a more detailed response to this comment. We will also clarify that these perturbed basal melt rates are on top of the baseline rate provided by MITgcm, and we will provide a figure of these baseline basal melt rates in the Appendix.

My fundamental issue with the paper is that without an understanding or attempt to account for other more dominant factors that are driving variability, I don't think it can be concluded with any confidence that seasonal melt is driving (or even influencing) seasonal velocity variability at any of the sites.

Hopefully, our responses above clarify this point, we realise now that we did not take into account other studies in our current manuscript, and this will be updated significantly. Additionally, other possible dominant factors will be discussed in more detail in the updated manuscript and we will make sure to account for changes in SSH, following *Mosbeux et al.* [2023].

2.2 Specific comments

Abstract: 'sensitive regions' are mentioned several times before it can be determined that this means regions where ice velocities are sensitive to basal melt

The sensitive regions being discussed here are related to *Baldacchino et al.* [2022]; *Fürst et al.* [2016]; *Gudmundsson et al.* [2019]; *Reese et al.* [2018]. We will clarify this in the updated manuscript.

Line 12: "We suggest that... velocity variations... could be partly.... driven by melt". This is a very tentative conclusion and implies that you don't believe this to be the case either.

This is a sentence where we tried to clarify the scope of the study, which is to investigate whether basal melt *CAN* explain GNSS' data, but we are not providing a definite answer.

Line 55: sp. Siple Coast ice streams

Done.

Line 70: I would argue that GNSS doesn't provide a unique opportunity to measure seasonal variations in velocity – satellite methods can also do this – although GNSS does have better resolution and accuracy.

We would argue that the high temporal resolution of the GNSS instruments that are deployed for multiple years on the ice shelf still provide this unique opportunity. Furthermore, GNSS allows us to capture wintertime velocities, when visible-band remote sensing is not possible (due to 24hr darkness in Antarctica). This alone makes the data unique.

Line 70: sp. MacAyeal

Done.

Line 80: While you discuss the work of Klein et al., and Mosbeux et al. in the previous paragraph, the aims and approach taken in this paper do not follow from this discussion.

This section will be edited so that the aims and approach in this manuscript follow on from the discussion.

Line 101-104: These two sentences contradict each other.

This sentence has been removed "However, thinning of this region or changes in the ice-front location (i.e., via iceberg calving) will alter the stress balance and velocities of the ice shelf (Gudmundsson et al., 2019; Klein et al., 2020)."

Line 129: What is meant by 'data processing was iterated'? How are initial positions updated and until what criteria are met?

Because initial position precision wraps into final position precision, updating the initial position in a GNSS Rinex file with an estimate closer to the actual initial position improves the final estimate. To achieve this, the data is processed the first time, this first estimate of position is then used as the initial position for a second processing round. This is accomplished by creating a new rinex GNSS data file with the updated position and processing. This is typically only done once and removes unrealistic steps between position solutions.

Line 132: What is the 'reported processing uncertainty'?

The formal position uncertainty is estimated by the NRCAN CSRS-PPP service.

Line 133: More detail is required here to understand the processing steps and error propagation.

We think this is clear. The gradient in the x direction, dx/dt, provides the velocity in the x direction. The uncertainty in this gradient provides the uncertainty in v_x. The same goes for the gradient in the y direction. We propagate the uncertainties by adding percentage uncertainties when determining the total velocity magnitude and direction by adding the percentage uncertainties when multiplying and dividing.

Line 136: Is aliasing the correct word here?

We think aliasing is fine here.

Line 141: Where is this presented?

Figures 3 and 5, this has been edited in the updated manuscript.

Line 159: Is melt varied across the whole domain or locally? Assuming these units are m/a (velocity) per m3/a (melt), it is not clear over what area of the ice shelf the melt is integrated.

Yes the melt perturbation $\delta\dot{M}_b$ is spatially variable (hence the use of a directional derivative, and Automatic Differentiation), and is expressed in m/a. The integral is over Ω the entire model domain (although the gradient will be 0 over grounded ice since melt is not applied there).

Line 181: It would be useful to know what the baseline MITgcm basal melt rates are and how / if they vary seasonally before the perturbation is applied.

A figure showing the baseline MITgcm basal melt rates will be added to the Appendix of the updated manuscript. In this figure, we can see that the basal melt rates do vary seasonally, with a peak observed in the austral summer. However, we also see that there is a lot of noise in the MITgcm basal melt rates throughout the year, with multiple smaller peaks shown in the austral winter.

Fig 2 caption: The whole of the RIS and islands are also outlined in black so it is not clear which bit is 'passive ice'.

Passive ice will be identified using a different colour in the updated manuscript. The caption will be edited accordingly.

Fig 3. What is meant by 'errors'?

The errors are described in Section 2.2 as uncertainties. This will be edited in the Figure 3 caption.

Line 247: I don't see evidence that 'local changes in basal melt influence the velocities at Site 4'? This also seems to contradict the conclusions (Line 486).

In Figure 4 Site 4 we see high sensitivity at the KIS grounding zone to changes to basal melt. Here we are highlighting that local changes in basal melt *AT* the KIS grounding zone impact the velocities at Site 4. In the conclusions, we suggest that these local changes in perturbed basal melt rates *AT* the KIS grounding zone are unrealistic and thus changes in basal melt do not influence the velocities at Site 4. We can clarify this in the text in the updated manuscript.

Line 265: What is the difference? It could be important to note here.

The aim of this paper is not to try and match the GNSS and modelled velocities, but rather we are trying to determine whether it is possible to reproduce similar velocity variations from basal melt. We want to provide a proof of concept and test our hypothesis that perturbations (both in magnitude and phase) in basal melting at the identified sensitive regions could reproduce a similar behaviour in velocities to the GNSS observations. Therefore we will not quantify the difference, as we deem this is outside our aims of the study.

Figure 6: Is this the baseline dataset (i.e. perturbation of 0 m/a)? In which case why is there no velocity variability in the 0 m/a perturbation scenario (Fig 5). The mean and max imply seasonality in this baseline dataset.

Yes, this is the baseline dataset, and a time series of the baseline dataset will be included in the updated manuscript. We observed no velocity variability in the 0 m/a perturbation scenario, and we suggest this is due to the high seasonal basal melt values shown in Figure 5 not occurring at the GNSS sites locations, but close by (especially for Site 2). Hence, why the perturbations in basal melt rates were needed in our experiments.

Line 422: I don't think it is justified to say that "the majority of Site 2's intra-annual velocity variability is driven by seasonal changes in melting".

This has been changed to "Site 2's intra-annual velocity variability could partly be driven by seasonal changes in melting".

Line 449: Duplicate reference of Liu and Miller, 1979.

This has been removed.

Line 456: What is the mechanism for a short period of surface melting to lead to a velocity increase in the centre of the ice shelf, far from any shear zone?

This paragraph will be reworded, and sentence removed, as it does seem unlikely that a short period of surface melting in the centre of the ice shelf can lead to a velocity increase. The impact of the El Nino event on the basal melt rates will instead be focused on. [*Klein et al.*, 2020] state that the surface heat fluxes over the ocean during this surface melt event in January 2016 may have been substantially different than those used to drive the ocean model that provided the basal melt rates used to force their ice-flow model. This will also be further discussed in our updated manuscript.

Line 460: This is a misrepresentation of what is stated in Mosbeux et al., (2023) who do tentatively attribute the 6-monthly signal to tides.

This sentence will be edited, and further discussion will be provided on the findings of [*Mosbeux et al.*, 2023] in the updated version of the manuscript.

3 Reviewer 3

3.1 General comments

I therefore have several concerns regarding the realism of the modeled melt rates and the conclusions of the paper. Furthermore, the paper overlooks the potential influence of other factors such as sea surface height variations and tidal effects, which have been shown to significantly impact ice flow dynamics in previous research. Even focusing solely on basal melt rates, seasonal melt close to the grounding line where ocean models usually struggle to correctly model high melt rates (e.g., the melt under Pine Island ice shelf in Dutrieux et al., 2013) and their effect on the grounding zone, could have been explored by the authors

Thank you for this comment. Firstly, we are aware now that we did not discuss the potential influence of other factors in enough detail. This will be added in the updated manuscript, as well as discussing the [*Klein et al.*, 2020] and [*Mosbeux et al.*, 2023] studies in more detail. We ran additional model simulations to take into account the seasonal variations in SSH (shown in Figure 4), and added text in Section 2.5 to explain the methodology of these additional model simulations. As detailed in our summary response above, we will take into account sea surface height variations and discuss tidal effects, when concluding the influence of basal melting on the observed velocity variations. Regarding the realism of the modelled melt rates and the conclusion of the paper, please also refer to our summary response above, and Section 4.2.

3.2 Specific comments

Figure 1: To me, this figure could be reworked and made cleaner. Why drawing null velocities in the ocean? It only decreases the readability

Figure 1 will be reworked and made cleaner in the updated manuscript.

On site 3, which is the main site used by [*Klein et al.*, 2020], the data derivation from displacement to velocities gives you a minimum in April.

Yes, this is correct. These are the velocities we obtained using our processing steps as outlined in the manuscript, and our results compare well with [*Klein et al.*, 2020]. We show similar seasonal variability in the GNSS-derived velocities at Site 3, however, we do observe a minimum in April and a maximum in August, which are offset by 1 month compared to results presented in [*Klein et al.*, 2020] (minimum in March and maximum in July).

Figure 2. The figure really looks like a draft and not a publishable figure. The grounding line and the safety bands are both plotted in black. There is no metrics on the x and y that are used and written. The southern part of the grounding line is cutoff without specific reasons

We agree with the reviewer that this figure needs work, but following the recommendation from Reviewer 1, we will instead remove this figure in the updated manuscript.

To me, if the MITgcm modelling shows a seasonality in melt rates, this seasonality should be explored, even if it does not give the correct phasing on the ice flow velocities. The MITgcm melt rates should be shown with maps of melt rates at different period of the years, or at least with a timeseries of the integrated melt rates over the ice shelf. For example, the model melt rates in [*Klein et al.*, 2020] shows only one peak melt rate in February (see their Figure 7a or the maps in Fig. 8). Why building a twice peaking melt rate if it is not realistic or backed by any modelling or observation?

A figure showing the timeseries of the baseline MITgcm basal melt rates will be added to the updated manuscript. As detailed in our above responses, we are using these perturbations in basal melt rates to understand whether basal melting *CAN* reproduce the GNSS observed velocity variations, assuming that MITgcm basal melt rates are imperfect and may not include all possible variability (especially in the vicinity of grounding lines).

Figure 5: Looking at the pattern of your observed velocity variations, it seems that ice flow reaches a minimum velocity in March and a second one in August. My understanding is that this is the reason why the authors apply two peak melt rates in your idealized sinusoidal melt. However, such a semi-annual cycle caused by something different like a semi-annual variability in tidal amplitudes and affecting the grounding zone of the ice shelf, as suggested in [*Mosbeux et al.*, 2023] conclusions. This could be seen as a process similar to the nonlinear response of the ice shelf (and the ice sheet) to the diurnal tide (e.g. Gudmnundsson, 2011; Rosier et al., 2020). Site 3 semi-annual cycle does not seem as clean as on other sites but still visible with a sharp drop in velocity in November followed by plateau from early January to March, a second drop in March-April before a reversal with a speed up until August, ending with a second Plateau from August to November. From the detrended displacement in Figure A4, we do not see any sharp change in displacement in November. How do you explain such result? Also, the strong direction changes before January 2016, does not really reflect in the detrended x and y displacement. Looking at [*Klein et al.*, 2020], the velocity trend looks a bit different. It would be good to investigate the reasons for this.

This is an interesting point, we will include further discussion about other potential factors (SSH and tides in Sections 4.1 and 4.3) that could be driving the observed variability in velocities in the updated manuscript. The differences between our velocity trend and [*Klein et al.*, 2020] velocities will be discussed in more detail in the updated manuscript (in Section 3.1 and 3.3). The difference could be due to [*Klein et al.*, 2020] using T-TIDE analyses to remove the tidal signals in the dataset, and could also be due to the time window used. In this paper, we use a time window of 8 weeks to smooth the short-term tidal effects and to identify seasonal changes.

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