Seasonal land ice-flow variability in the Antarctic Peninsula (tc-2022-55)

Author Response to Reviewers (13/07/22)

Dear Dr. Berthier,

We thank both reviewers for their detailed, constructive feedback on our manuscript and were pleased to read that they deem it to be "*well structured and … performed"* (Reviewer 1) and one that "*breaks new ground on sensitive detection of seasonal velocity signals*" (Reviewer 2). We were also especially pleased to read that Reviewer 2, Dr. Ted Scambos, believes the manuscript will provide "*a significant contribution to Antarctic glaciology*" when published.

Below, we address in full each of the points raised by the reviewers, and have revised the manuscript accordingly. As requested by both reviewers, the most significant change has been to provide a more balanced evaluation of the relative roles of surface and ocean forcing for explaining the observed seasonal speedup signals at GVIIS.

In the following response document, we first present a detailed summary of the major change detailed above (hereafter referred to as **Major Change 1**), and then provide point-by-point responses to the reviewers' detailed comments. We have included the numbered reviewers' comments (*italicised in blue*), our responses (black text) and amendments to the original text (*italicised in grey*). Unless otherwise stated, line and figure numbers referred to below are in line with those in the original manuscript, available at https://doi.org/10.5194/tc-2022-55.

Kind regards,

Karla Boxall

(on behalf of the co-author team)

Author Response to both reviewers

Major Change 1 – Balanced evaluation of surface and ocean forcing

Both reviewers raised concerns regarding our attribution of the observed seasonal velocity variation to oceanic forcing, suggesting that the role of surface meltwater should not be dismissed as a potential forcing mechanism. Having reflected on this, we are inclined to agree with the reviewers, with the proviso that there is currently a dearth of surface and ocean in situ records with which to verify the exact mechanism(s) responsible for driving the observed signals. In this regard, we have overhauled the manuscript (Section 5 (Discussion) especially) as suggested by the reviewers to provide a more balanced evaluation (pros and cons) of the relative importance of surface and oceanic forcing.

Specifically, we have made the following revisions:

a) We have rewritten Lines 16-18 of the abstract to remove the assertion that the ocean is the key driver of the observed seasonality, and instead offer a more balanced statement.
The sentences now read:
"Both surface and oceanic forcing mechanisms are outlined as potential controls on this seasonality. Ultimately, our findings imply that similar surface and/or ocean forcing mechanisms may be driving seasonal accelerations at the grounding lines of other vulnerable outlet glaciers around Antarctica."

b) The last clause of Line 42 has been edited to remove the claim that oceanic forcing is the favoured mechanism.
Sentence now reads:
"We then evaluate the potential mechanisms responsible for driving the observed seasonal ice-flow signals upstream of George VI Ice Shelf."

c) As outlined above, the Discussion (Section 5; Lines 309-356) has been overhauled to tone down the assertion that oceanic forcing is the sole potential driver of the observed seasonal variability. Similarly, statements proposing that surface meltwater cannot be the trigger of the observed seasonality have been removed. Instead, the Discussion now provides a much more balanced (and nuanced) evaluation of both potential forcing mechanisms.

The revised paragraphs discussing surface forcing (Section 5.1) now note that the timing of our seasonal signal coincides with the peak in annual surface melt rates, and that the greatest speedups (2016/17 and 2019/20) occurred during years in which the melting was particularly intense (Banwell et al., 2021) (on the basis of the comments raised by both reviewers). Reviewer 2 also states that "*It is not essential that the water be visible on the surface as pools*" in relation to previous work that has reported a lack of rapid surface drainage events over the observational era to date (e.g. Rott et al., 2020). We interpret this to refer to the possible presence of firn aquifers not detectable via microwave (and other) remote sensing, a discussion of which – together with the feasibility of such aquifer formation/persistence – is also now included.

Section 5.2 has been similarly revised to tone down the importance of ocean forcing as the sole driving mechanism, and first discusses the existing state of knowledge regarding sub-GVIIS CDW presence and circulation as constrained from temporally limited in situ observations (following the initial version of the manuscript). Addressing reviewer 2's concerns (Comments 31-33), a discussion of the limitations of these in situ records for ascertaining any seasonal cycle in CDW forcing is then presented. This is followed by a discussion of two potential sea-ice driven mechanisms through which modelling studies suggest sea ice variability could alter the depth of CDW presence/influx over seasonal timescales. Finally, a discussion of the cavity throughflow rates needed to drive the seasonal signals is included (in line with Reviewer 2's Comments 3 and 32) as both a 'pro' and 'con' in favour of ocean forcing, motivating the requirement for detailed future in situ data collection within this region of the Southern Ocean for verification purposes.

The revised discussion now reads:

5.1 Surface forcing

The role of surface-sourced meltwater in stimulating seasonal accelerations in land ice flow is well established on valley glaciers and the Greenland Ice Sheet (Iken et al, 1983; Hooke et al, 1989; Zwally et al., 2002; Moon et al., 2014; Kraaijenbrink et al., 2016). At both locations, early summertime surface water inputs to a subglacial drainage system drive near-instantaneous reductions in effective pressure, increased basal sliding and the acceleration of the entire ice column (Iken, 1981; Schoof, 2010). Enabled primarily through the mass drainage of supraglacial meltwater via surface-visible 'moulins' (which are themselves formed through the sustained meltwater-driven erosion of the ice column over one or more summers), such velocity accelerations are typically short-lived as the subglacial hydrological drainage system becomes more efficient through time, thereby increasing effective pressure and arresting flow (Bartholomew et al., 2010). Near the surface of the Greenland Ice Sheet, the drainage of perennial, summer meltwater-fuelled 'firn aquifers' may also deliver large quantities of meltwater to the bed via crevasses and/or other englacial pathways (Harper et al., 2012; Koenig et al., 2013), instigating similar, transient accelerations in ice velocity (Schoof et al., 2010).

In Antarctica, the clear velocity signals we observe inland of GVIIS' grounding line (Figs. 4 and 5) emulate closely the summertime accelerations observed in valley glaciers and the Greenland Ice Sheet, implying that they may be driven by similar surface meltwater-related processes. The relatively short-lived (~1-2 months duration) velocity maxima followed by sharp deceleration trends at most glaciers lend credence to this interpretation (Figs. 4, 5 and A1), with the latter resembling a 'Greenland-style' late- to post-summertime switch towards more efficient subglacial drainage. A current lack of in situ observations, however, makes this hypothesis difficult to verify. Nonetheless, we note further that for 75% of the outlet glaciers nourishing GVIIS, the greatest instances of ice-flow speedup occurred during either the austral summertime of 2016/17 or 2019/20 (Fig. A1), which correspond to years characterised by exceptional surface melting on the ice shelf (Banwell et al., 2021).

Despite the seemingly close correspondence between surface-meltwater forcing and the seasonal signals we observe at GVIIS' glaciers, satellite observations show that supraglacial meltwater presence and persistence is limited inland of Antarctica's grounding zone (Dirscherl et al., 2021; Johnson et al., 2022), and no obvious regional contrasts in melt exists near the

grounding line of Alexander Island and Palmer Land (Trusel et al., 2013; Bell et al., 2018). At GVIIS, routine satellite observations have also revealed minor trends of decreasing meltwater presence over most of the 21st Century (Johnson et al., 2022), which is consistent with a previously documented, pervasive cooling of the Antarctic Peninsula from the late 1990s onwards (Turner et al., 2016; Adusumilli et al., 2018). Inland, we expect that melt rates will have similarly decreased but at a greater rate given the lapse rate associated with the Antarctic Peninsula's mountainous terrain. Together with the variable thicknesses of the glaciers spanning GVIIS' perimeter (Morlighem et al., 2020), which would presumably require differing amounts of meltwater flux to form surface-to-bed moulins and enhance basal sliding, these findings suggest that rapid surface meltwater drainage events alone are unlikely to explain the region-wide, year-by-year seasonal speedup signals we observe near the grounding line. Summertime coherence in both the interferograms used to locate the position of the grounding line in 2018 (cf. Section 3.1; Mohajerani et al., 2021) and our offset-tracking-derived velocity estimates (cf. Section 3.2), alongside a previously documented absence of any such rapid meltwater drainage events in the Antarctic Peninsula (Rott et al., 2020), further support this assertion.

At depth (i.e., beyond that detectable by microwave sensors, whose radiation penetrates typically a few metres into the firn column), Greenland-style firn aquifer-related drainage events could also be involved in driving the seasonal velocity signals we observe. Previous in situ campaigns have confirmed the presence of such aquifers on Wilkins Ice Shelf to the north (Fig. 1), and these have been implicated as potential drivers of Wilkins' past disintegration events (Montgomery et al., 2020). We note, however, that the formation and persistence of firn aquifers requires high levels of surface melt and accumulation (Harper et al., 2012; Koenig et al., 2012; Montgomery et al., 2020): phenomena which appear improbable inland of GVIIS during most of the Sentinel-1 era given the pervasive cooling of the Antarctic Peninsula over approximately the past two decades noted above (cf. Turner et al., 2016). Notwithstanding this cooling, long-term model outputs suggest that the spatial distribution of firn aquifers in the Antarctic Peninsula is limited largely to the northern reaches of Wilkins Ice Shelf, and that no aquifers reside inland of GVIIS (van Wessem et al., 2021) — patterns which follow closely the thermal limit of ice-sheet viability over this region of Antarctica (Cook & Vaughan, 2010). While we acknowledge that the model estimates of, for example, van Wessem et al. (2021) may underestimate firn aquifer presence, the rapidly steepening topography inland of GVIIS' grounding line (which often exceeds 1° of slope; Howat et al., 2019; cf. Fig. 1) is presumably also unconducive to their formation and persistence compared with the relatively flat Wilkins Ice Shelf. We would expect any subsurface meltwater to perpetually flow towards the grounding line due to gravity.

5.2 Ocean forcing

The seasonal velocity signals we observe at and proximal to GVIIS' grounding line (Figs. 3, 4, 5 and A1) may also be diagnostic of seasonal fluctuations in ocean forcing. As discussed in Section 2, this interpretation is supported firstly by in situ oceanographic observations revealing the widespread inflow and flooding of relatively warm circumpolar deep water (CDW) to GVIIS' cavity, which is sourced from the continental shelf via a net northwards throughflow from Ronne Entrance (Jenkins and Jacobs, 2008). There, the strongest inflows of CDW have been observed to occur underneath its northern margin proximal to Alexander Island (red arrow in Fig. 1; after Jenkins and Jacobs, 2008) which may, by extension, explain the observed, earlier onset of summertime speedup at and proximal to the grounding line

along that stretch of coastline relative to Palmer Land (Fig. 6). On the basis of these earlier observations, we further expect that enhanced CDW upwelling in the cavity, enabled by the buoyancy driven advection of ice-shelf meltwater entrained within the northerly throughflow and deflected towards Alexander Island due to Coriolis forcing, may have also maximised this contrasting regional melting effect. This hypothesis is consistent with in situ- and modelling-based estimates of GVIIS' sub-shelf circulation (Jenkins and Jacobs, 2008; Holland et al., 2010) and, more broadly, with inferred patterns of melting observed recently along the Coriolis-favoured flank of Dotson Ice Shelf, West Antarctica (Gourmelen et al., 2017).

It is important to note, however, that the findings of Jenkins and Jacobs (2008) do not present any evidence for seasonality in CDW presence and/or depth owing to the limited timeframe in which these in situ observations were collected (less than two days' worth of continuous measurements in March 1994). Nonetheless, recent research has revealed two possible mechanisms through which sea ice conditions offshore from GVIIS may control CDW draft in its sub-shelf cavity. First, modelling experiments, emulating closely the observational records of Jenkins & Jacobs (2008), have suggested a process of wintertime sea-ice growth, brine rejection and resulting convection throughout the mixed layer that leads to a thickening of the underlying CDW (Holland et al., 2010; Petty et al., 2014). These processes drive a seasonal cycle in melt rate at GVIIS' grounding line (Holland et al., 2010), greatest around mid-to-late wintertime, which would precede the resulting summertime accelerations in ice flow we observe by ~3-5 months. Several ice-sheet modelling studies have suggested a lag time of several weeks to months from the onset of ocean-induced melt to surface ice acceleration at the grounding line (Vieli and Nick, 2011; Joughin et al., 2012b), suggesting that this timescale may be plausible. Second, in situ observations from the neighbouring Amundsen Sea Sector have revealed that sea-ice growth and associated brine rejection can alternatively result in a destratification of the water column and thus restrict CDW inflow during austral wintertime (Webber et al., 2017). This mechanism would, by implication, facilitate enhanced summertime melt at the grounding line more in-phase with the accelerations in ice flow we observe.

Ultimately, a historical dearth of oceanographic observations in the Bellingshausen Sea hinders our ability to ascertain which mechanism is the dominant control on CDW influx to GVIIS' sub-shelf cavity, justifying the future collection of detailed oceanographic data in this region. Such data would also yield high-resolution (and potentially more representative) insights into the nature of oceanic circulation beneath GVIIS. Indeed, we estimate that an ~8-16 cm s⁻¹ northwards throughflow of CDW would be required over the course of ~1-2 months to induce the relatively narrow summertime speedup windows observed along the entirety of GVIIS' 420 km long cavity (Figs. 4, 5 and A1), assuming laminar and linear flow. These rates are up to almost an order of magnitude greater than the observationally constrained estimates reported in Jenkins & Jacobs (2008; ~2.5 cm s⁻¹), although the latter, which were collected over ~30 hours, may not necessarily be representative of seasonally averaged rates of flow. Elsewhere in Antarctica, longer-term in situ observations have revealed much greater rates of sub-ice shelf CDW circulation (~8-20 cm s⁻¹; Jacobs et al., 2013; Jenkins et al., 2018), suggesting that similar speeds may, in fact, be plausible underneath GVIIS.

Finally, we note that beyond relatively local-scale ocean forcing, modelling experiments suggest that CDW influx to GVIIS' cavity is relatively insensitive to far-field intra-annual-to-decadal-scale atmosphere-ocean variability (Holland et al., 2010). This lies in contrast to the atmosphere-ocean processes controlling CDW transmission to the Amundsen and wider Bellingshausen Sea coastal margins (i.e., wind-driven Ekman-transport; cf. Steig et al., 2012;

Dutrieux et al., 2014; Christie et al., 2018; Jenkins et al., 2018; Paolo et al. 2018), further implicating relatively local-scale, sea-ice induced oceanographic modification as a key potential control on the seasonal ice velocity signals we observe at GVIIS.

d) Finally, we have reworked Lines 361-367 in the Summary and Implications (Section 6) to give a more neutral conclusion regarding our expected explanation of the observed seasonality. This section now reads:

"… Both surface- and oceanic-forcing mechanisms are evaluated as potential controls on this seasonality, although insufficient observational evidence currently exists with which to verify the relative importance of each. Elucidating the precise surface and/or ocean mechanisms governing GVIIS' outlet glacier flow variability is therefore a critical area for future research…"

1. *The authors present a comprehensive analysis of the glacier flow variability of the GVIIS tributaries. The analysis relies on Sentinel-1 data and is backed up with independent Landsat measurements. Overall the paper is well structured and most sections of the analysis are well performed. However, there are some issues that must be addressed:*

   Most important, the authors state that surface meltwater cannot be the trigger of the observed seasonal variations. However, I am not convinced by the presented justification. Recent publications indicate the warming and also increased surface melt on the AP (e.g. Carrasco et al. 2021, Banwell et al. 2021). So, the authors should also consider surface meltwater in the discussion of their findings or provide evidence that surface meltwater can be neglected as a potential driver. (see also comments below, abstract, discussion, and conclusions need to be adjusted accordingly)

We thank the reviewer for their positive and insightful review, and were pleased to read that they assess the manuscript to be "*well structured*" and that "*most sections of the analysis are well performed*".

Having reflected on both reviewers' comments on the manuscript, we agree that at present, surface forcing should not be dismissed as a possible key forcing mechanism (and, likewise, that there is not enough observationally constrained evidence to implicate the primary influence of ocean forcing). With this in mind, we have overhauled the Discussion (Section 5) to offer a more balanced discussion (pros and cons) on the possible surface and ocean forcing mechanisms at work, and have also edited the abstract, introduction (Sections 1 & 2) and Summary and Implications Section (Section 6) to reflect this. These changes are detailed in **Major Change 1** above.

Regarding the papers by Carrasco and Banwell, see also our response to Comment 33 below.

2. *Moreover, the description of the methodology has some shortcomings. Please provide here more precise information and be always clear on which region (spatial extent), i.e. whole glacier or just the 10km² areas, is your analysis and interpretation based. Please justify the interpolation in Fig.6 and explain the applied approach. The error analysis should be also extended. See detailed comments below for some specific issues.*

As stipulated in Sections 3.3.1 and 3.3.2 of the manuscript, the analysis presented in Section 4.2.1 is implemented on the entire drainage basin of GVIIS, whereas Section 4.2.2 is executed using the 10 km^2 regions immediately inland of the grounding line at each flowline. We have updated the text in Section 3.3.1 to provide further clarification of this point.

Line 152 of Section 3.3.1 now reads:

"Following Fig. 2, for each year spanning 2014 – 2019, we first constrained the month in which observed velocities were greatest on a pixelwise basis throughout the entire domain, v_{time}."

In Section 3.3.2, Line 170 of the original text states that "we calculated mean monthly velocity and standard error within a 10 km^2 region located directly upstream of the grounding line between 2014 and 2020". As such, we believe no further clarification is required in this section.

Please see our responses to the detailed comments below regarding Fig. 6 (Comments 30-32) and the error analysis (Comment 27).

In Fig. 3a, the colour scheme shown was adopted to emphasise the spatial clustering of summertime (DJF) speedup immediately inland of the grounding line. The raw data used to produce this figure (Boxall et al., 2022b), however, show that a spatially coherent pattern in the timing of speedup does not exist inland of the grounding line, and that the month of maximum velocity is distributed randomly across the twelve months. This random distribution is also reflected in the dataset used to produce Fig. 3b. Therefore, the dominance of seemingly large swathes of grey should not be interpreted as a coherent and/or significant speedup between March and November.

We realise, however, that this was not explicitly stated anywhere in the original manuscript, so we have revised Line 244 to clarify this point. Sentence now reads:

"Notably, no coherent seasonal signal exists beyond ~10-20 km of the grounding line, where v_{time} is randomly distributed across all 12 months and $v_{anom_{SAR}}$ shows no associated clustering."

4. *Why is the speedup only visible close to the GL. Why is there* [no] *speed up further up?*

In our opinion, this is an interesting finding which implicates the role of oceanic forcing, since basal melting at the grounding line would elicit acceleration in flow at the grounding line prior to farther inland. Qualitatively, this is similar to the multi-decadal acceleration trends witnessed both here (see, for example, Hogg et al. (2017)) and in places such as Pine Island and Thwaites, albeit on a much smaller (i.e seasonal) temporal scale. On the assumption that surface melt will be greatest at lower altitudes due to lapse rate effects, the grounding line is where one might equally expect the manifestation of surface drainage-related acceleration to first occur, similar to the seasonal signatures witnessed in e.g. Greenland and Svalbard. However, this mechanism is presumably unlikely to drive such coherent, clustered acceleration events along the *entirety* of the grounding line, as the glaciers which reside there exhibit, for example, highly disparate thicknesses which would require different amounts of surface melting and drainage to enhance basal sliding. These points are now included in our revised discussion (see our comments on **Major Change 1** above).

5. *Is there any correlation of speedup with altitude (either the area affected by the speed up or the general hypsometric profile or hypsometric index of the glaciers)? Difference Alexander Island vs. AP?*

We thank the reviewer for this interesting question. Fig. 3 shows that the coherent speedup signals we report are restricted to the region at and immediately inland of the grounding line. There, REMA DEM (Howat et al., 2019) also reveals no discernible differences in elevation profile between Alexander Island and Palmer Land. In light of this finding (and that of our related response to **Major Change 1** and Comment 4 above), no further changes have been made to the text.

6. *Tides are also affected by the season. Could the seasonal changes of the tides affect the glacier, in particular the GL? E.g. stronger tides lead to a wider grounding zone.*

To locate the grounding line in this study, we used the most landward limit of tidally induced vertical ice-shelf flexure observed (see discussion in Sections 3.1 and 4.1). All analyses were carried out inland of this grounding line and thus include grounded ice only, not freely floating ice or the grounding zone.

Detailed comments:

While Seehaus et al. (2015; 2016) focus primarily on the long-term (1993-2014) velocity speedup of grounded outlet glaciers in response to the collapse of Larsen A and Prince Gustav ice shelves, respectively, both studies do also note short-term summertime accelerations of the outlet glaciers. They attribute this phenomenon to the fragmentation of ice mélange, and the associated reduction in buttressing. We have therefore changed the text as follows to account for the findings of these studies.

Line 10 now reads:

"…, observations of similar seasonal signals are limited in Antarctica."

8. L39: Why is it vulnerable?

We have removed the phrase 'climatically vulnerable' from the text.

9. L53: Does the velocity field represent the long-term average?

The velocity field displayed in Fig. 1 represents the mean velocity between September 2019 and August 2020. The caption has been updated to reflect this.

The caption of Fig. 1 now reads:

"Mean ice flow of George VI Ice Shelf…"

10. L56: Source of flowlines?

The flowlines follow the centreline of fastest flow according to the mean ice velocity displayed in Fig. 1. The caption of Fig. 1 has been updated to include this information.

The following text has been added to the caption of Fig. 1:

"Numbered flowlines delimit the centreline of the fastest-flowing outlet glaciers draining to GVIIS (named according to the UK Antarctic Place-names Committee) …"

11. L69: You list publications regarding meltwater lakes from 2017 onwards and say that such studies lead to the identification of GVIIS as a potential site for future ice shelf disintegration, identified in a study from 2013. That's somehow inconsistent

Thank you for this comment. For chronological consistency, the reference citing GVIIS as a potential site for future disintegration has been updated from Holt et al. (2013) to Holt and Glasser (2022).

12. L85: please explain "seaward extent". The glaciers are flowing into an ice shelf.

The Southern Ocean extends beneath GVIIS. We would therefore prefer to keep this term in the text.

13. L98: Did you apply any multi-looking or filtering? What about the coregistration of the images? Some more technical information would be nice.

We have added additional information on the co-registration and geocoding of the SLC images and interferograms (cf. Comment 17 below) to this section, as well as the inclusion of a reference to Christie et al. (2022) which provides further technical details. We also note that further information is provided in the metadata/README files associated with the grounding line datasets accompanying

this paper (Boxall et al., 2022a). Beyond the use of REMA DEM at the co-registration, DInSAR and geocoding steps in this study, all other processing parameters are virtually identical to those cited in-text, so for brevity we would prefer not to re-list these here.

Revised text reads:

"To recover the location of GVIIS' modern-day grounding line, we employed double-differential interferometric SAR (DInSAR) processing techniques to all consecutive 6-day repeat-pass Sentinel-1A/B Interferometric Wide (IW) single look complex (SLC) images acquired during extended austral wintertime (May-October) 2020. Extended austral wintertime imagery was used to maximise phase coherence between successive image pairs which may be degraded due to the attenuation of radar waves by summertime supraglacial water presence (cf. Sect. 2). Similar to earlier work (Park et al., 2013; Rignot et al., 2014; Christie et al., 2016; 2022), we co-registered each successive image pair and removed the topographical component of phase from all subsequently generated interferograms, using the Reference Elevation Model of Antarctica (REMA; Howat et al., 2019). Assuming ice creep to be common between each SAR image, we then differenced all successive interferograms to locate the limit of tidally induced vertical ice-shelf flexure. This limit is represented as the landward extent of a band of closely spaced fringes on double-differenced interferograms (Rignot et al., 2011b), and is an accurate proxy for the true grounding line which cannot be recovered directly by satellite-based imaging techniques (Fricker et al., 2009; Friedl et al., 2020). Finally, all interferograms were geocoded to EPSG:3031 (Antarctic Polar Stereographic projection) using REMA."

14. L100: Could you please provide an overview of the used imagery

Every available Sentinel 1A/B IW SLC image available between 1st May 2020 and 31st October 2020 was processed to delineate the grounding line location. This is stated in Lines 98-99 in the text, and so no further changes have been made to the text.

15. L103: Did you prove this assumption? You should use your velocity measurements to prove it.

This assumption is part of a standard technique reported commonly in the literature. We would therefore prefer to keep this sentence as originally found in the text. We appreciate, however, that velocities will not be entirely common over these periods (Rack et al., 2017) and that this can induce grounding line errors of up to one ice thickness. These errors, however, fall inside the total range of grounding line positions imaged (Fig. B1; Boxall et al., 2022a).

16. L119: Please describe here briefly how the uncertainty was estimated and what is a "valid pixel". This would be beneficial for the reader

As indicated in the text (Line 119), uncertainty was estimated as one standard deviation (1σ) from the mean of all valid pixels. We therefore believe no further action is required on this point.

Regarding valid pixel count, we have now clarified this on Line 119. Revised text reads:

"… associated grids of uncertainty (1σ) and valid pixel count (the number of non-NaN observations used in the production of each monthly estimate)."

17. L122: This information should be provided in section 3.1. and here you can refer to 3.1.

Done. See also our response to Comment 13 above which includes details of the revised text.

Reference to Sect. 3.1 has been added to Line 123.

Thank you for this suggestion. This reference has been added to the text.

19. L130: what is sigma? The average of all pixels?

This is a similar question as that posed in Comment 16 above. Sigma (σ) is the standard notation used to denote standard deviation. This is stated explicitly in Line 131.

20. L136ff: Unclear explanation. You are using intensity tracking, thus you measure also displacements in azimuth direction and not only in range (LOS) direction. For sure, the shifts in the phase center depth can affect your measurements. But please rephrase this section to be more clear. Did you account for this shift in LOS direction? How much would it be? Any suggestion on how to estimate the bias? A brief statement would be nice at the end of this section.

We thank the reviewer for highlighting this small typo (LOS pertains to the slant range of the sensor, and not the two (range/azimuth) components required to estimate horizontal displacement) which the second reviewer (Ted Scambos) also remarked upon (see his Comment 24 below). For clarity, we have reworked this section of the text to read:

"We averaged velocities over monthly timescales to minimise contamination associated with ionospheric and tropospheric delay between successive (6/12-day repeat pass) Sentinel-1 image acquisitions (Rosen et al., 2000; Selley et al., 2021). Moreover, across the Antarctic Peninsula, Sentinel-1A/B acquisitions are currently acquired in descending mode only (ESA, 2022), meaning that the velocity data utilised in this study represent the relative displacement of point targets from a single look angle only. Recent work has shown that below sub-monthly resolution, these velocities can be subject to bias owing to radar penetration differences associated with the freezing state of the snow-firn-ice interface between image acquisitions (Rott et al., 2020). This phenomenon can induce shifts in the radar line-of-sight (LOS) distance to target by up to several meters (Joughin et al., 2018; Rott et al., 2020), resulting in either an under- or overestimation of velocities depending on the flow direction of the ice relative to LOS (Rott et al., 2020). At present, reliable, sub-monthly estimates of the magnitude of this bias over the western Antarctic Peninsula are difficult to model owing to a lack of detailed in-situ information on the temporally variable composition of the firn layer; for these reasons, monthly composites were also utilised to dampen the effect of this bias."

Regarding firn-related phase shifts, we believe our initial writing made it implicit that the use of monthly composites was intended to minimise sub-monthly bias(es), although for clarity we have now provided more detail on the difficulties associated with trying to model/correct for this potential phenomenon in the last sentence above.

21. L152: On which spatial scales did you apply the analysis. Throughout the whole glacier area? Only for the 10km² areas next to the GL? Please clarify

This comment is addressed in our response to Comment 2 above.

22. L153: Is this analysis based on the monthly mosaics or single velocity fields?

As stated on Line 118, all analyses were carried out using the monthly mosaics. Monthly mosaics were used instead of the single velocity fields to reduce atmospheric contamination (Lines 133-134) and minimise the effect of the bias caused by the shift in the LOS distance (added to Line 141 in response to Comment 20 above). As such, we believe no further action is required on this point.

Pixels with no coverage for a specific month (i.e. a valid pixel count of 0) were removed. The text has been updated to clarify this.

Line 165 now reads:

"… pixels without continuous monthly data coverage throughout the entire year were culled from our analyses."

We believe this comment pertains to Section 3.3.2 as a whole. In short, yes, we have discarded very slow-flowing regions. We have reworded this section's header to clarify this point, noting that an examination of slow-flowing regions would be illogical in the case of the present study given the lack of any coherent seasonal signal in the slower flowing regions. New heading reads:

"3.3.2 Flow variability near the grounding line of GVIIS' fast-flowing outlet glaciers"

Thank you. The typo has been corrected.

The spatial pattern displayed in Fig. 3 is noisy because it represents a complex series of geophysical processes. Per SAR image acquisition, many such processes contribute to speckle returns on a per pixel basis, including (but not limited to): firn effects, snow blow and surface temperature, height slope change and, of course, ice dynamical processes. Changes in speckle typically reduce coherence between successive scenes, which will ultimately propagate into our monthly averaged velocity estimates. While we could have performed filtering and/or smoothing of these grids to reduce the noise in the figures, we were eager to avoid manipulating the "raw" observations as much as possible so as to circumvent the presentation of potentially biased and/or unrepresentative seasonal signals.

The outlet glaciers have been numbered in Fig. 3, as suggested.

To address the first element of this comment, we have revised Line 170 in the text. The sentence now refers to Sect. 3.2 which explains how the error was calculated (see Equation 1), and clarifies how the mean monthly velocity was calculated.

Line 170 now reads:

"To do this, we calculated the mean velocity and standard error (cf. Sect. 3.2) at monthly intervals within each 10 km^2 region located directly upstream of the grounding line between 2014 and 2020."

Regarding the second element of this comment, standard error is reported specifically for the reason identified in the reviewer's comment, since it implicitly accounts for variability in data availability as

determined from the valid pixel count (Sect. 3.2, Equation 1). This is a standard method in which to report error associated with velocity measurements (see, for example, Rignot et al. (2016) and Greene et al. (2018)), so we believe no further justification of the technique is required in the text. On a related note, despite the lack of observations at some glaciers, there are still obvious summertime speedup signals at those locations which are completely consistent with the timing and magnitude of behaviour exhibited elsewhere. This, by implication, verifies the behaviour of the more poorly observed glaciers (and vice-versa).

> *28. L262: Maybe there was a switch between effective and ineffective subglacial drainage. This might explain the late-summer slowdown. At some other glaciers, a late summer or even March/April minima is also visible.*
> *And the late winter slowdown might be caused by a lack of bed lubrication at all. Well, that is just pure speculation from my side. Some studies at Columbia Glacier or also in Greenland revealed similar patterns. (e.g. Moon et al 2014, Vijay and Braun 2017…)*

The authors are aware of the meltwater-controlled seasonal velocity variations observed on the Greenland Ice Sheet (e.g. Moon et al., 2014) and mountain glaciers (e.g. Vijay and Braun, 2017) that are attributed to the switching between inefficient and efficient subglacial drainage networks. Having taken on board the comments made by both reviewers regarding the possible importance of such surface forcing mechanisms, we have now discussed the pros and cons of (amongst others) evolving drainage efficiency in the Discussion section. Please see our responses to **Major Change 1** for further details.

> *29. *L271: Maybe surface melt onset is earlier on Alexander Island as compared to the glacier's origination from the AP. Any correlation with average glacier altitude or surface melt data from climate modeling data?*

This is a good question, although as stated in our revised Section 5.1 (see **Major Change 1** for further details), passive microwave records show no obvious differences in total summertime melt rate between Alexander Island and Palmer Land (cf. Trusel et al., 2013; Bell et al., 2018; Johnson et al., 2022). While this of course may not *always* be true owing to, for example, very short-lived passing storms/atmospheric rivers, it is consistent with the Rossby radius of atmospheric deformation at that latitude (~1000-1500 km wide). In other words, the climate and any associated melting would, over seasonal (and longer) timescales, be expected to be largely homogeneous at Palmer Land and Alexander Island inland of the GL, which are <70 km from one another.

The suggestion that there may be a correlation with altitude has been addressed our response to Comment 5.

> *30. L287: Why did you apply any interpolation? Just show the pure data.*

We thank the reviewer for allowing us the opportunity to revise the language used here. We realise that 'interpolation' is probably the incorrect choice of word in this instance, given that mathematical interpolation was not actually applied in the production of Fig. 6 (now Fig. 7). Instead, Figure 6 (now Fig. 7) merely represents a gridded statistical summary of the density of valid pixels, and does not manipulate the underlying data in any way. Ultimately, this visualisation was chosen to better highlight high density regions given the noisy nature of the original data (see also our response to Comment 26).

The text has therefore been reworded to better articulate this fact. Lines 286-291 now read:

"Upon examination of the velocity field (Fig. 1) in conjunction with the phenomena observed in Fig. 7, we infer the clustered regions of agreement farther inland (~40-150+ km from the grounding line) to be falsely identified regions of speedup falling close to combined sensor error limits (cf. Sect. 3.3.3). There, clustering resides mostly over areas of near-stagnant flow unlikely to have experienced significant seasonal variability (~10 m yr⁻¹)."

Note that Figure 6 is now Figure 7 following the insertion of Figure A1 to the main text (now Figure 4).

> *31. Fig.6: Please use different colors or line styles to illustrate the SAR derived average velocity contours. How did you generate the heat-map? Please provide more information on how you computed the density. Please do not interpolate the density, if the interpolation is causing such strong artifacts (see comment above).*

We have revised Fig. 6 (now Fig. 7) to use different line styles for the SAR-derived velocity contours.

As described in our response to Comment 30, the densities displayed in Fig. 6 (now Fig. 7) are not interpolated, but reflect the densities of unmanipulated observations within a prescribed search radius (10 km). Since we mask all observations seaward of the grounding line, the search radius used to calculate density is effectively halved relative to further inland. Mathematically, this explains the circular-like signatures observed inland, which extend beyond the spatial dimensions of fast flow and are therefore believed to be unrealistic (see revised text in our response to Comment 30).

For reasons of data transparency, we would prefer to retain the figure as is (rather than, for example, generating a new figure where these obvious blunders are clipped out). We would, however, be happy to reconsider should the reviewer and/or editor feel strongly in support of this option as an alternative.

> *32. L291ff: Please show at least one example in Fig.6. Otherwise, it is difficult to figure out this issue.*

Done. A few symbols have been added to Fig. 6 (now Fig. 7) to highlight examples of the circular-like artefacts mentioned in the (revised) text, and the caption has been adjusted accordingly.

Caption reads:

"Red crosses represent examples of areas of clustering not representative of a true geophysical signal."

> *33. L320: You should also mention the more recent warming on the AP which overlaps strongly with your observation period (reported by Carrasco et al. 2021). This should be considered in your discussion. There is also a strong surface melt anomaly in 2019/2020 reported by Banwell et al. 2021 on Alexander Island and at least close to the GL next to the AP. So you should consider also the option of surface meltwater as a driver for seasonal fluctuations*

We thank the reviewer for raising these points, and reiterate that the majority of this comment has now been addressed in **Major Change 1**.

Regarding the recent findings of Carrasco et al. (2021) and Banwell et al. (2021), this is a similar comment to that posed in Comment 1 above. In short, we would prefer not to include Carrasco et al. in the references given the inconclusive evidence presented in that study for the possible return to relatively warmer conditions over the Antarctic Peninsula. The basis of their argument pertains to the one or two anomalous warming years seen in recent years (2016/7 and 2019/20) although, from a climatological perspective, it is simply too early to ascertain whether this is representative of a statistically significant trend.

Regarding the study by Banwell et al. (2021), the anomalous warming the reviewer notes is restricted largely to the Wilkins Ice Shelf region (i.e., the other side of the ice divide from our region of interest); this is consistent with the climatological limit of ice shelf viability as noted in our revised Discussion (cf. Section 5.1 and references therein). Unfortunately, no data exists across most of Palmer Land in that study, so any detailed comparison between 2019/20 island vs. mainland melt would furthermore be scientifically unrobust. We do, however, now make note of the seemingly strong correspondence between the magnitude of our accelerations and the 2019/20 melt event documented by Banwell et al. (2021) in the revised discussion.

34. L324ff: Please revise and account also for potential surface meltwater availability (see comment above)

We thank the reviewer for raising this point, which we have addressed in our response to **Major Change 1** above.

35. Fig. A1: On many panels, the glacier names are covered by black lines.

Thank you for this comment. The label positions have been adjusted so that they no longer overlay the data.

36. Fig. B1: Could you please include the central flowlines of the glaciers and glacier numbers.

Thank you for this suggestion. Numbered flowlines have been added to Fig. B1.

The flowlines have also been numbered in Fig. 3 and Appendix C, for consistency.

37. Fig.C…: what about 2019-2020?

The SAR-derived velocity observations extend from October 2014 to August 2020. For the calculation of v_{time} and $v_{anom_{SAR}}$, a year was considered to span from December to November (i.e., four complete seasons). Therefore, the calculation of these metrics for the period encompassing December 2019 – November 2020 was not possible because the processed velocity time series only extends to August 2020.

The following sentence has been added to Line 166 to clarify this:

"Notably, the calculation of either metric was not possible for 2019/20 given the lack of velocity data beyond August 2020 (Sect. 3.2)."

38. Table D1: Could you also include the most dominant frequency

Done.

1. *The paper uses a combination of Sentinel 1 A/B velocity data and ITS_LIVE Landsat-8 velocity data, along with a careful mapping of the grounding line, to assess the scale and extent of a clear seasonal variation in flow speed for glaciers inflowing on both sides of the George VI ice shelf. The paper makes a strong case for the validity of the signal they see, and the seasonality is quite sharp and clear, albeit not large. The authors attribute this to variation in ocean forcing. This is a well-written, well illustrated and described study that breaks new ground on sensitive detection of seasonal velocity signals (a -few- other studies are out there now for some other regions).*

We thank the reviewer, Ted Scambos, for his detailed and constructive review and were particularly pleased to read his endorsement that our study is *"well-written, well illustrated"* and *"breaks new ground on sensitive detection of seasonal velocity signals"*.

2. *However, the attribution of this signal to ocean forcing in untenable. While this means that the paper absolutely needs to be revised, in fact 80% of the paper is ready to go. It is necessary that the paper revise the attribution to discuss the pros and cons of ocean forcing and surface melt percolation to the bed equally. That is, if the following considerations do not convince the authors that surface melting has in fact a far stronger case for this speed-up. I would like to point out that such a conclusion, or preferred but qualified causal process (surface meltwater reaching the glacier bed), would still make this paper a significant contribution to Antarctic glaciology.*

This is a similar verdict to that expressed by Reviewer 1, which has formed the basis of our **Major Change 1** above. As detailed in that section, we have adhered to both reviewer's suggestions and have overhauled the manuscript (Discussion (Section 5) especially) to provide a balanced discussion of the pros and cons of both surface and ocean forcing. Ultimately, we hope the reviewer will concur that these are underpinned by a lack of in situ observational evidence needed to unambiguously ascertain the leading driver(s) of these signals, which is also now reflected in our revised manuscript.

3. *The sharpness and regularity of the signal, spanning the entire GVIIS cavity within one or two months (Figure 4 and 5), is the first indication that this is related to summertime melt rather than ocean flow. Peaks in ice flow in December and January are timed closely with peaks in surface melting. Moreover, these timings occur sharply year after year (Figure A1). Nearly all of the glaciers showed a significant spike in 2019-2020, a major melt year for the region. As the paper notes, the -potential- for surface water to induce glacier acceleration is well-proven. It is not essential that the water be visible on the surface as pools (see papers by Harper, Humphrey, Pfeffer; by Koenig, O. Miller, Miégè, Forster.)*

   On the other hand, oceanographic signals along the Antarctic coast are rarely so sharply seasonal. The cited papers do not (-can- not) discuss seasonal variations in cavity currents or changes in the depth of the CDW layer. The authors infer and favor ocean forcing, but don't discuss how it would occur – would it be related to sea ice losses? (far more variable and uncertain than the surface melt season) Or wind patterns moving the polar water layer and changing isopycnals in that fashion? (also not reliable enough to provide a signal like Flowlines 2, 3, 4, 5, 17, 18, 19, 20, and 21 in Figure A1). Note that if the change in CDW depth or flux is related to a south-to-north current, the speed required would be an order of magnitude faster than that discussed in Jenkins and Jacobs, 2008 (and it would have to be a continuous laminar flow or wave in the isopycnal).

We thank the reviewer for these comments, most of which are also reflected (and addressed) in the reviewer's subsequent comments below and in our responses to **Major Change 1**.

In short, most of the points raised here regarding surface melt vs. oceanographic forcing have now been incorporated into our revised discussion. We wish to reiterate here, however, that while we appreciate the sentiments expressed regarding ocean forcing, we are less convinced by the reviewers' comments on the lack of sharp seasonality in ocean state (see, for example, the papers on strong ocean seasonality along the Antarctic coast by Holland et al. (2010) and Webber et al. (2017) referenced in the Discussion (Section 5)). Our initial version of the manuscript also included suggestions for the processes hypothesised to be driving these signals, making explicit reference to these papers (Lines 336-356). It further occurs to us that the strong velocity signals observed may indeed be forced by the ocean, but only 'sharp' in nature due to the competing influence of across shelf glaciers (cf. Comment 5 below). That said, following our response to Comment 2, a lack of in situ data exists to verify this speculation (and the relative importance of both surface and ocean forcing processes in general), resulting to our decision to opt for a more balanced (pros/cons) approach in the revised discussion.

> 4. *At the very least the authors need to discuss the two possibilities as equally likely. Personally, the case for summer melt influence is far stronger in my view. However, there are data that might save the ocean discussion: instrumented seals. Data collected by instrumented seals and analyzed by, e.g., Lori Padman or Lars Boheme, might be able to show strong seasonal ocean variations. Have a look at Padman et al., 2012, JGR-Oceans – perhaps in the data used in that paper there is an indication of seasonality (but I don't think it is mentioned in the paper). The sharp downturns in the ice velocity just before, or just after, the seasonal speed-up pulse are not easily explained in the ocean scenario.*

We are grateful for these suggestions and, following **Major Change 1** and our responses to the reviewer's Comments 2, 3, 6 and 33, we have now overhauled the discussion as suggested. The use of seal data is also a good suggestion and one which we are already examining for a follow-up piece of research going beyond the 'proof-of-concept', satellite-based remit of this study.

> 5. *Also – the authors missed something really cool in the data shown in Figure A1. Look carefully at the signal of Flowline 3 and Flowline 21, and their geographic position. These glaciers are influencing each other across the ice shelf. The earlier acceleration of Flowline 21 (Grotto Glacier, west side, Alexander Island) -slows- the outflow of Flowline 3; then Flowline 3 (Millet Glacier, east side, with a later melt-season peak, perhaps?) accelerates and forces Flowline 21 to slow down. You can see a similar but less clear influence in Flowline 2 and 4, and then Flowline 20 and 19. Like an angry uncle at Thanksgiving, one glacier is shoving the ice shelf table, turkey and all, towards the unsuspecting nephew; the nephew then makes his final point, and shoves the table back toward the uncle. (I suppose it stars with the aunt dumping her drink, meltwater, on each of their heads in succession.)*

We thank the reviewer for this metaphor which, as Europeans, was entertaining. Family feuds aside, this is a highly astute and interesting observation that we have now included in the revised text in Section 4.2.2. Lines read:

"Spatially, our results are also suggestive of an apparent regional contrast in the timing of summertime speedup … Within this trend, Figs. 4 and 5 also present clear evidence of the ability of GVIIS' outlet glaciers to influence each other across the ice shelf, whereby the earlier acceleration of Alexander Island's glaciers initially arrest those flowing from Palmer Land, delaying the onset of their acceleration

until the late summertime (compare, for example, the timing of peak velocity at the geographically opposite Flowlines 21 and 3, and Flowlines 20 and 4-5). Upon late-summertime acceleration, Palmer Land's glaciers then arrest the flow of those on Alexander Island in a similar manner. "

On the basis of this observation, we have also now decided to place Fig. A1 into the main text as the new Fig. 4, and have updated all other figure numbers accordingly.

> 6. *The last parts of the paper should be re-written with these considerations in mind, but the majority of the paper is publishable as is. I suggest moving some of the figures to the appendices or supplemental information, but overall this is a very well done study, that needs to revise the attribution to a wider perspective at least; if not outright favor surface melt-driven acceleration.*
>
> *I would like to review the revised paper.*
> *Also, the authors are invited to Thanksgiving at my house.*

Thank you for the invite! We very much look forward to joining you in November!

Detailed Comments

> 7. *L10 – Change to …within intra-annual timescales…*

Thank you for this suggestion. This edit has been implemented.

Line 10 now reads:

"Unlike the Greenland Ice Sheet, where historical, high temporal resolution satellite and in situ observations have revealed distinct changes in land ice flow within intra-annual timescales, …".

> 8. *L16 – using what satellite or data set?*

This is a good suggestion. We have named the satellite used to derive the independent optically derived velocity observations.

Line 16 now reads:

"These findings are corroborated by independent, optically derived velocity observations obtained from Landsat 8 imagery."

> 9. *L18 – Can you add a short statement about the basis for this statement? Beyond contrasts in onsets and overall timing – something about the facts that led you to this?*

These sentences have been revised following **Major Change 1** to provide a more neutral discussion of the relative roles of surface and ocean forcing. New lines read:

"Both surface and ocean forcing mechanisms are outlined as potential controls on this seasonality. Ultimately, our findings imply that similar surface and/or ocean forcing mechanisms may be driving seasonal accelerations at the grounding lines of other vulnerable outlet glaciers around Antarctica …".

> 10. *L22 – demise is too strong a word… for now. Decline?*

Thank you for this comment. We have replaced the word demise.

Line 22 now reads:

"Three decades of routine Earth observation have revealed the progressive decay of the Antarctic Ice Sheet, …"

> 11. *L25 – add something from surface melting and fracturing here – Rignot et al., 2004; Cook and Vaughan*

Thank you. Both studies have been added to the cited literature here.

> 12. *L30 – a general dearth…*

Thanks. We have replaced 'historic' with 'overall'.

> 13. *L34 – need to note that the signals are much larger for these more northern ice masses – or we'd have seen them already*

Line 32 has been edited to highlight the large magnitude of the seasonal signals observed on mid-latitude and Arctic ice masses.

"…; this is in contrast to mid-latitude and Arctic ice masses, where the timing and large magnitude of seasonal ice-flow variability is now well observed."

> 14. *L37 – change to: In* [this] *study, we find evidence of…*

Done.

> 15. *L37 – remove climatically – you're claiming that it's being impacted mostly by the ocean*

Thank you for this comment, which is in line with Comment 8 from Reviewer 1. The phrase 'climatically vulnerable' has been removed from the text.

> 16. *L42 – Suggest you remove this –the case is convincing for ocean versus surface melting.*

As described in our response to the **Major Change 1**, we have revised the manuscript to provide a more balanced discussion of the role of both surface and ocean forcing. This clause has been edited to reflect this. New sentence reads:

"We then evaluate the potential mechanisms responsible for driving the observed seasonal ice-flow signals upstream of George VI Ice Shelf".

> 17. *L52 – very nice map, thank you*

Thank you.

> 18. *L67 – Amplification is not the right word. Rapid increase.*

Done.

> 19. *L77 – unusual word choice. Mimicking.*

The word 'aping' has been replaced with '*similar to*'.

> 20. *L82 – Add text along these lines: However, the George VI also has a long record of intense surface melting and surface ponding of melt, primarily in the central sector. Melt season duration is one of the longest on the continent at XXX (ref) and the adjacent Wilkins Ice Shelf has experienced episodes of hydrofracture-driven retreat (Braun Humbert or Scambos ref). Both processes will be examined as possible causes of the seasonal speed-up.*

Thank you for this suggestion. We have assimilated most of these points into a now reworked Lines 61-72 where we think it is better placed given the surface forcing context. Lines read:

"… These events have been attributed primarily to the surface warming-induced presence of supraglacial meltwater lakes (Dirscherl et al., 2021), which are surmised to have instigated a process of rapid ice-shelf hydrofracture and collapse such as that observed most recently at Wilkins Ice Shelf (Fig. 1; cf. Scambos et al., 2000; 2009; Banwell et al., 2013; Leeson et al., 2020). These phenomena have, in turn, been linked to an Antarctic Peninsula-wide increase in surface temperatures over most of the observational record (Vaughan et al., 2003). At GVIIS, intense supraglacial meltwater presence has been observed over the ice shelf since routine satellite observations began (Kingslake et al., 2017; Bell et al., 2018; Banwell et al., 2021), and melt season duration is one of the longest on the continent. This has led to the identification of GVIIS as a potential site for future ice-shelf disintegration (Holt and Glasser, 2022); recent modelling studies suggest that resultant land ice losses associated with such an event would contribute an ~8 mm rise in global sea-level by 2100 (Schannwell et al., 2018)."

> 21. *L92 – both ICESat-2 and MOA have mapped the grounding line – here is the new reference: Li, T., Dawson, G.J., Chuter, S.J. and Bamber, J.L. 2022. A high-resolution Antarctic grounding zone product from ICESat-2 laser altimetry. Earth System Science Data, 14(2), pp.535-557.*

We thank the reviewer for pointing us to this new dataset and that of MOA. Ultimately, the point we were trying to convey was the need for systematic, spatially continuous, and high-precision observations of the grounding line (or more technically: the limit of tidal flexure), since this location is vital for ensuring velocity signals do not fall either within or seaward of the grounding zone (see Rott et al. (2020) and references therein for the main motivation here). As the reviewer is aware, the Li et al. (2022) dataset relies upon (spatially discontinuous) repeat-track techniques, whereas MOA is limited by its moderate resolution compared to SAR imaging and its inability to reliably image the absolute limit of tidal flexure (although the authors nonetheless appreciate the value of this extremely useful dataset for other applications). As such, we have slightly rephrased this sentence for clarity:

"… In comparison to the detailed knowledge of grounding-line migration within these sectors, however, there have been no high-resolution, spatially complete grounding line surveys at GVIIS since the mid-1990s (Rignot et al., 2016). Accurate and updated knowledge of GVIIS' grounding line is therefore of paramount importance for distinguishing precisely between grounded and floating ice."

> 22. *L95 – Remove after ice, -- the last part of this sentence should be in the intro of not used here*

Thank you for this suggestion. We have removed the last clause of the sentence.

> 23. *L112 – See the MAO data sets available from NSIDC, for 2004-5 and 2008-9 and I think there's a grounding line for 2014-15.*

Thanks. See our response to Comment 21 above.

> 24. *L136 - … in one line of sight direction only.*

Good spot. We have rephrased this sentence for clarity as recommended here and by Reviewer 1. Please see our response to Reviewer 1's Comment 20 for more detail.

> 25. *L166 – outside of ? standard error bounds*

We would prefer to keep the text as written. Pixels falling *within* standard error bounds were discarded because, given an error of ±1.8 m yr^{-1}, a seasonal signal between -1.8 m yr^{-1} and 1.8 m yr^{-1} cannot be trusted to be a true signal.

> *26. Line 222 – no comma*

Thank you. This comma has been removed from the text.

> 27. In this area, the glaciers are talking to one another.

Thanks, this is an astute and interesting observation. As detailed in our response to Comment 5, we now discuss this phenomena in Section 4.2.2.

> *28. Fig.6 - Consider moving this figure to supplemental information. It's a very nice confirmation but mostly in the direction of data quality and data issues.*

Following the clarifications and edits made in response to Reviewer 1 regarding this figure (Reviewer 1, Comments 30-32) and its description in Section 4.2.3, we would prefer to retain it as a main figure (Fig. 7 in the revised manuscript). We believe it serves as important confirmation on the reliability of our observed seasonal signal, and further highlights (in addition to Fig. 3) the spatial distribution of the speedup along the coast.

> *29. L316 - …but they are persistent in the areas that they occur. It's not 'rare' in areas where there is a significant melt season.*
> *30. L318 – Well, yes – no one has done as thorough an analysis as you. How would they ever be identified if precedent (not seen before…) is the justification for not considering the possibility?*
> *31. L321 – Banwell notes abundant melt ponds on the GVIIS in several years. It is wrong to imply that there is not significant surface melting in this grounded ice area! GVIIS has one of the longest melt seasons on the continent*
> *32. L324 – This is ad hoc, -you- are making the best case in this work, this is how one discovers that water is involved in grounded-ice seasonal variations.*

Thank you for these comments. As stipulated in **Major Change 1**, we have overhauled the discussion to present a more balanced evaluation of the relative roles of surface meltwater and ocean forcing, which we trust directly addresses each of these.

> *33. L327 – these papers say nothing about a strong seasonality in flow speed or CDW depth.*

We agree that neither Jenkins and Jacobs (2008) nor Meredith et al. (2010) reference any seasonality with regards to CDW flow speed or depth. Discussion of these papers in the original text was intended merely to first highlight the existence of CDW in the cavity beneath GVIIS.

In our revised Discussion (cf. **Major Change 1**) we have now stipulated this point for clarity (sentence opening the second paragraph of text in Section 5.2). This addition also serves to better allude to the following series of arguments on the mechanisms through which seasonal CDW influx variability may occur (see also our response to Comment 3 and 35).

Revised lines read:

"It is important to note, however, that the findings of Jenkins and Jacobs (2008) do not present any evidence for seasonality in CDW presence and/or depth owing to the limited timeframe in which their in situ observations were collected (less than two days' worth of continuous measurements in March

1994). Nonetheless, recent research has revealed two possible mechanisms through which sea ice conditions offshore from GVIIS may control CDW draft in its sub-shelf cavity. First, …".

> 34. *L333 – The JandJ 2008 paper indicates flow speeds of ~2cm/sec. A monthly transit of the 500 km-long GVIIS cavity –which is what you are implying to create the narrow summer speed-up –would require speed near 20 cm/s, and that is if the water moved laminarly and linearly.*

This is an interesting thought exercise which we now initially elaborate upon in the revised discussion as a potential ocean forcing 'con' (Revised Section 5.2). (Note here that we have re-performed the calculation using more precise measurements of the cavity's long axis (420 km) and a presumed transit time ranging between 1-2 months which is potentially more realistic of the duration needed to yield the speedup trends observed – this gives a speed ranging between ~8-16 cm s^{-1}).

In the context of our new, wider discussion on a lack of ocean observational constraints, we also compare the 2 cm s^{-1} value estimated by Jenkins and Jacobs (2008) (which was estimated from only ~30 hours of observations in 1994) to values observed elsewhere in Antarctica over much longer timescales. These observations reveal speeds of up to 8-20 cm s^{-1} (see Jacobs et al., 2013; Jenkins et al., 2018), suggesting that our calculated GVIIS rate of 8-16 cm/s may not be entirely unreasonable. As such, this finding is also presented as a 'pro' in favour of ocean forcing, all the while providing the motivation/need for much more ocean research in this area of the world.

This new section reads:

"Ultimately, a historical dearth of oceanographic observations in the Bellingshausen Sea hinders our ability to ascertain which mechanism is the dominant control on CDW influx to GVIIS' sub-shelf cavity, justifying the future collection of detailed oceanographic data in this region. Such data would also yield high-resolution (and potentially more representative) insights into the nature of oceanic circulation beneath GVIIS. Indeed, we estimate that an ~8-16 cm s^{-1} northwards throughflow of CDW would be required over the course of ~1-2 months to induce the relatively narrow summertime speedup windows observed along the entirety of GVIIS' 420 km long cavity (Figs. 4, 5 and A1), assuming laminar and linear flow. These rates are up to almost an order of magnitude greater than the observationally constrained estimates reported in Jenkins & Jacobs (2008; ~2.5 cm s^{-1}), although the latter, which were collected over ~30 hours, may not necessarily be representative of seasonally averaged rates of flow. Elsewhere in Antarctica, longer-term in situ observations have revealed much greater rates of sub-ice shelf CDW circulation (~8-20 cm s^{-1}; Jacobs et al., 2013; Jenkins et al., 2018), suggesting that similar speeds may, in fact, be plausible underneath GVIIS."

> 35. *L336 – I am suggesting a complete re-write of this section, and the summary.*

We presume the reviewer's motivation for this comment arises from Comments 3 and 9, which make reference to a lack of any discussion pertaining to the mechanism by which seasonality in CDW influx may occur (which we interpret to be a possible oversight on the part of the reviewer given Lines 336-356 of our initial manuscript).

Nonetheless, we have overhauled the discussion as advised (see also our responses to **Major Change 1** and Comments 2-4 and 33-34) which we hope will address the reviewer's concerns. Similar to the abstract, we have also reworded the summary to offer a more balanced conclusion on the relative roles of both surface and ocean forcing.

> 36. Figure A1:

- Flowline 3 – this signal, and many of the others, is inconsistent with ocean forcing for the seasonal speed-up.
- Flowline 5 – compare this pattern with flowline 20: again, opposite seasonal phasing
- Flowline 20 – [compare] with flowline 5, Ryder
- Flowline 21 – This glacier is on the opposite side of the GVI, with an opposite pattern of seasonal variation.

We thank the reviewer for his insightful comments on this figure. These have now been addressed in response to Comments 3 and 5 above.

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