Response to comment by Tedstone et al.

We have read the above discussion paper with interest; the paper demonstrates the tremendous improvements and additions to the GIMP data archive and their potential to enhance records of ice sheet dynamics and the processes controlling dynamic change. However, we wish to raise some concerns regarding the results and discussion of inter-annual velocity trends in the south-west sector of the ice sheet, much of which arises from comparison with our own study on the same topic (Tedstone, Nienow, Gourmelen, Dehecq, Goldberg and Hanna, 'Decadal slowdown of a land- terminating sector of the Greenland Ice Sheet despite warming', *Nature*, 2015; hereafter T2015).

We appreciate the comments on the data and address the concerns below.

On the south-west GrIS sector, Joughin et al (hereafter J2018) conclude that 'the trends Tedstone et al (2015) observe may be statistical artefacts, resulting from some combination noise [sic] and a shorter-duration (after 2000) record' (P16,L23-24). Broadly, we suggest that, rather than the results from the two studies disagreeing, the differences are likely due to methodological differences in the derivation of the data-sets used and that there are flaws with the current methodology used by J2018 to derive their 'winter' velocity time series. As such, we believe that the broad conclusions from this section of the paper are not currently robust and the specific conclusion that the results from T2015 'may be statistical artefacts' is not justified based on the data presented and should therefore be removed unless considerable further evidence is presented to back up this assertion, including the explicit details of the derivation of the 'winter' time series.

We address a similar question as T2015, which focuses on whether there is a trend in speed in western Greenland from approximately 2000 onward. In summary, our findings using different data, differing seasonal coverage, and covering a slightly different period show virtually no trend, which contrasts with the results presented by T2015. We have done what we can do rule out methodological differences with our data. We were quite careful not simply say "we are right, they are wrong." Instead we tried to go through the possible differences carefully and try to offer explanations based on our understanding of the data. The operative word in the statement cited above is "may", which according to a the first google definition means "expressing possibility."

The error bars on the T2015 data are not particularly small (60 m/yr), so it is not unreasonable that noise could be a factor. We also allow for the extra couple of years in our time series to be a contributing factor. We discuss the annual sampling window length below, which we believe can introduce larger biases that T2015 assume.

In explaining our concerns in more detail, the rest of this comment is in two parts: (1) an examination of the methodological differences used to derive the respective data sets, and (2) a comparison of the results presented.

(1) Potential methodological differences

T2015 mapped the decadal trend in ice motion (their Fig. 1) by differencing two multi-year time periods: 1985-1994 and 2007-2014. Each of these multi-year periods was computed from annual feature-tracked image pairs with a baseline of 352-400 days. In contrast, J2018 use 'winter' velocity mosaics (dataset: NSIDC-0478), which are available for the winters of 2000-01, 2005-06 and 2006- 07 onwards; note here that 'winter' is assumed to be any data collected in the nine months from Sept through May and is not uniformly sampled. We understand that the mosaics preceding 2014- 15 are predominantly composed of InSAR Campaign-mode data, which was only acquired for a subset of the 9-month winter period. Whilst J2018 have treated these winter mosaics as indicative of net winter ice flow, previous studies show that ice flow varies considerably through winter, which we shall now expand upon.

> ***We were also careful to express there were sampling issues with both data sets. We have directly compared several winters where the SAR data did not include the full 9 months with full-winter GPS estimates and found good agreement, suggesting relatively little bias due to different sampling periods (see Fig. 8 and further discussion of this issue below).***

Variable ice velocity during winter becomes a substantial issue once the degree of variability is considered and if one is trying to characterise a net winter velocity from a temporal subset of winter values. Detailed GPS data presented in Joughin et al (2008, their Fig. 2) show winter velocities increasing from ~55 to ~80 (GPS V_{NS}) and ~70 to >95 (GPS V_{SS2}) m/yr between September and April respectively; this overwinter velocity change thus represents a ~45% and ~35% increase in velocity between the early winter minimum and late winter maximum. Examples of the same phenomena are also shown clearly in Colgan et al (2012, J. Glac, Fig. 2), Sole et al. (2013, GRL, Fig. 2) and elsewhere where winter GPS velocity records exist. As such, the precise period in winter when velocities are sampled will have an enormous impact on any 'winter' time series and subsequent trend analysis undertaken.

> ***Since we have the raw data, we can be more precise about some of the numbers referenced above. There seems to have been some issue with reading numbers off plots in J2008. The range directly from the data (Sept 1 2006 to May 31 2007) is 69 to 88 at NL, with about 10 m/yr of that rise occurring in September. Over the rest of the winter there is a small increasing trend in velocity. All of the radar data at these sites begins in early October or later and ends in April (i.e., the slowest and fastest months are excluded). For the years where we do have coincident GPS and full 9-month estimates, we have good agreement between the "partial" winter estimates and "full" winter estimates.***

To evaluate the sensitivity the sensitivity to our winter sampling interval, estimated the winter velocity for the North Lake GPS data from Joughin et al 2008 (a full winter of weekly average velocities). We used a 3-month window (roughly the length of the radar campaigns) and evaluated the results with the window starting on the first of each month, starting in October, which approximates the various radar campaign periods. The corresponding differences between the 9-month and 3-month averages are:

Oct-Jan :	*0.3 m/yr*
Nov-Feb:	*1.2 m/yr*
Dec-Mar	*2.0 m/yr*
Jan-April	*2.6 m/yr*

The last sampling window ends in April since we did not include May data for this region. These biases are much smaller than the worst-case biases mentioned in the comment, because we sample the part of the winter when change in speed are relatively muted, and are largely representative of the full 9-month average.

Note the biases just mentioned are with respect to the full 9-month winter averages. If every one of our data sets were collected over the same 3-month period each winter, then the biases would have no effect on the trend since they would be approximately the same each winter. Most of the campaign data are from ~Dec-Feb, so we re-ran our analysis with the Sentinel estimates computed just for the Dec-Feb period, which should more closely match the other campaign data. This change made virtually no difference, other than reducing the number of significant trends detected slightly (possibly due to reduced bias or more noisy data), which is opposite to the effect claimed by the T2015 authors in this comment. Since the differences are small, we opt to keep the less noisy 9-month Sentinel estimates.

T2015 evaluated their sampling strategy using a simulated time series, which involved a simple 2-value model with one value for the summer and one value for the winter. Had we used same simulation as T2015, there would be no bias due to the constant winter value they used. To evaluate their sampling scheme with more realistic data, we cyclically (annually) repeated the NL GPS data and evaluated annual velocity estimates with window durations ranging from 350 to 400 days, and window start dates from 150 to 226 days. With this test, we obtained annual velocities ranging from 90.4 to 98 m/yr. In other words, the T2015 sampling scheme applied to these GPS data produces annual estimates of speed that deviates from the true value by a range 3x greater than for our winter estimates (7.6 vs 2.6 m/yr).

In either case (T2015 or J2018), these biases would have to occur a consistently increasing or decreasing way to greatly change the trend, because if randomly distributed in time they would simply contribute additional noise. In a worst-case situation, our trends could be off by 0.16 m/yr^2 (2.6 (m/y)/16y) while the T2015 results could have an error of up to 0.54 m/yr^2 (7.6 (m/yr)/14 y).

The velocities in Colgan et al are pretty stable during the winter, which wouldn't change our conclusion. Neither would Sole et al data which shows pretty flat velocities in the winter. To the extent there are small spikes, these would be smoothed out by our typical 3 -month window for winter velocity determination.

J2018 note that "many of these earlier GIMP winter-velocity maps use campaign-mode data and are hence derived from acquisitions spanning only a few months" (P5,L31-P6,L1) but do not take the implications of this in to account in the subsequent analyses. For example, the only time period where J2018 explicitly distinguish the 'sub-winter' period of sampling in the manuscript text is for winter 2014-15 when the 'winter' map was "produced largely from data collected toward the latter half of the 2014/2015 winter" (P6, L3). This sampling period would therefore be expected to produce a 'winter' velocity that is considerably enhanced (>~15-20%?) relative to the actual winter velocity, reducing the likelihood of finding an inter-annual slow-down trend. J2018 does not provide any indication of when the 2001 'winter' time-series was collected, just that "early results in the time series were derived from only a few image pairs" (P16, L28-29), but the failure to capture the full winter velocity ensures that the subsequent trend analysis is flawed, especially given the dependence of the inter-annual trend analysis on this sample point at the very start of the time series five years prior to the next sample.

We fully acknowledged that these data are from a few-month periods and explicitly state that we are assuming these data representative of winter speeds. To evaluate the impact of this assumption, we compare several years of such radar winter estimates with GPS data and find good agreement (Figure 8). As we indicated for the comment above, any influence on the trend is likely to small.

Because the periods when the data were collected are published with the data online at NISDC, we made a point of not wasting valuable journal space with a table of dates. Given the concerns raised here, however, we will make it clear in the text that there are no September or May data in campaign data for the area covered by Figure 7 (there may be such data elsewhere on the ice sheet).

Given the numbers presented above and the good agreement with the GPS results shown in the text, we have no more "failed to capture the winter velocity" than T2015 have failed to capture the annual velocity by not using a sampling window of exactly one year, which, as shown above, can lead to larger biases than are present in our winter estimates.

The sampling issue highlighted above is a more significant problem when considering relatively small absolute changes in velocity. The intra-winter velocity range of ~25-30 m/yr reported in Joughin et al. (2008) (and Colgan et al., 2012) is of the same magnitude as the overall annual velocity decrease reported in T2015; as such, any failure to correctly estimate winter motion in the present study will have considerable implications for a trend analysis.

This statement seems to be repeating earlier arguments – the 25-30 m/yr is a red herring as noted above. The GPS example above indicates small biases.

The data underlying the J2018 trend analysis thus fail to capture net ice flow over annual and longer time scales, instead providing sub-annual snapshots of observation periods which vary in their acquisition time, both in length and period during the winter, from one winter to the next.

As indicated above any such biases are small, which we will augment the manuscript to indicate.

They therefore have the potential to incorporate considerable variability in each of their derived 'winter' velocities, depending on the precise period of time that was sampled/available to derive their velocity estimate. We ask that the authors provide considerably more detail of their different winter 'snapshots', beyond the existing explanation at P16, L2-3 and without simply directing readers to the underlying NSIDC dataset metadata.

We will add clarification to the revision to provide more detail about the timing for this specific area, but we see no need to repeat metadata that are freely available (to the best of our knowledge the data presented in T2015 are not publically available).

We note that J2018 provide a comparison between their radar derived NL data collected over "only a few months of each winter" and NL-GPS data reported in Stevens et al. (2016, GRL). They conclude that "most of the GPS points agree well with the radar-derived speeds" (P11, L17-18) and subsequently suggest that their "results are not unduly biased by seasonal variability" (P16, L5-6). We estimate, taking the data from Fig. 8., that while 'most' GPS do agree well, some comparisons are poor (e.g. 2008 where the SAR data looks to be ~10 m/yr too high, possibly due to seasonal bias).

In the above statement "some" actually means one. The point is about 9 m/yr faster than the GPS, or roughly 2 sigma (given the formal errors are not perfect this isn't bad). As noted above, ~2 m/yr of the difference might be due to seasonal bias, and the rest we expect is noise.

While the comparison gives confidence that the radar is performing reasonably well in terms of absolute GPS displacement (+/- ~5 m/yr s.d.), such an error can introduce considerable variance in velocity trends when the trends are small in absolute terms.

> *Yes, but from what we can tell T2015 are estimating trends (Fig. 2) using 1-sigma errors ranging from about 5 to 15 m/yr and differences of maps mosaicked from results with errors of up to 60 m/yr, so why is that more valid than our results, which have considerably lower errors ? We also note that quite a bit of the area in the T2015 paper actually appears to have little or no change in speed (i.e., there is quite a lot of white area in Figure 1).*

As a result, the data as currently presented do not provide compelling evidence that InSAR is generating 'winter' velocities at the requisite temporal resolution that can ensure that the results "are not unduly biased by seasonal variability", especially when investigating trends in areas where the ice is moving slowly (~ 100 m/yr).

> *Would not such a statement also apply to the T2015 data, which have considerably larger errors and potential biases (as indicated above)? We feel the GPS results above (which we will fold into the revised discussion) provide a sound demonstration that our winter sampling is valid (the analysis is similar to the T2015, except we used actual velocities rather than idealized seasonal velocities in the T2015 paper, which can understate sampling biases).*

Last, J2018 criticise the T2015 choice of baseline period as opening the potential for seasonal variability to be aliased (J2016, P16, L6-9). However, we note that T2015 investigated this possibility in some detail (see Materials and Methods - 'Impact of varying baseline durations on annual velocity' and Fig. S1). J2018 do not make any reference to this analysis in their critique of T2015. To summarise, T2015 found that longer baseline periods beginning/ending in summer are likely to lead to a small artificial increase in ice motion, which is in the opposite direction to the decadal slowdown signal that is found and reported in the T2015 study area. In line with the T2015 baseline sensitivity analysis, we therefore ask that the authors demonstrate statistically that their own sub-sampling methodology has not impacted their results. Such an analysis should be robust for the whole SW sector analysed if the current conclusions are to be justified.

> *As requested we produced such analysis. We applied both our sampling strategy and the T2015 strategy to actual GPS data. The results suggest small trend biases for both, although such errors are 3x larger with the T2015 sampling strategy.*

(2) Comparison of the results presented in J2018 and T2015

In J2018 Fig. 7, the units are metres per year. If the aim of J2018 is to undertake a valid and direct comparison with T2015, the authors should use the same units,

namely percentage change relative to a reference period. In principle, we assume this would be 2000-01, although given the issues associated with the 'winter' sampling in 2000-1 (and the large error bars associated with this time period as shown in Fig. 8), this would likely be problematic because this earlier reference period may not be representative of net winter motion.

> ***It is important to keep in mind that the purpose of this paper was not a redo of the T2015 results. Rather our purpose was to describe the data and demonstrate the quality of the data through several case studies (chosen for both demonstration purposes and scientific value). We also took a different, but equally valid perspective to the analysis (fitting trends to the data at each point, rather than a simple difference of two multi-year epochs).***
>
> **There is an overall trend presented in T2015 is -1.5 m/yr2 in Figure 2b, hence it is fully appropriate that we include our trends in these units. We did not provide a difference comparison to match that of T2015 because such a comparison as it beyond the scope of our paper (we also don't have 1985-94 base map to make such a comparison).**

On P11,L7-9, J2018 states that 'In the T2015 region (see black rectangle in Figure 7), we find some indication of slowdown, but the trends are less than those indicated by Tedstone et al. (2015)'. We request that the authors are more precise and provide, for example, an average velocity and
average % change for the region. This will ease comparison with both T2015 and the GPS measurements presented in the study. This is especially important given that J2018 find statistically significant evidence for a slowdown (Fig. 7 and P11, L7-9) but conclude later in the manuscript that it is due to aliasing of seasonal variability (P16, L6-7).

> ***Our Figure 7 does present a speed map and one can reference the trend relative to that. With respect to a percentage change, T2015 differ an 80s/90s map with a 2007-14 map. Since we have no such prior basemap, it is not really possible to do such a comparison. What we said in the referenced section was "find some indication of slowdown, but the trends are less than those estimated by Tedstone et al. (2015)" What we said is simply a statement of fact (you might not believe our numbers, but we believe this to be an accurate statement of the differences). Our statement regarding trends was meant to be relative to the 1.5 m/yr2 value given in Figure 2. We will investigate revising the text to make this comparison clearer.***

The approach chosen for trend analysis (as opposed to differencing two time periods as per T2015) requires clearer explanation. For example, does the analysis take the formal velocity uncertainty at each pixel each year into account, and do they exclude potential outliers (robust linear regression)? What is the estimated error of the computed trends (i.e. computed from the covariance matrix)? Are the pixels used for

computing the trend analysis present in every single mosaic or are some pixels missing some time points? Furthermore, we note that a trend of 0 m/yr is a valid result that should not be excluded, yet it appears that these are not shown due to the filtering applied (Fig. 7 caption).

Although there is some variation from year to year, the quality of all of the data is reasonably consistent over time. Thus, we did a simple linear regression with no weighting for errors (scipy.stans.linregress). Because there are some gaps in the data, we only computed fits were there were at least 6 points. Omission of the endpoints could have the biggest impact, especially for 2000. Only about 1% of these points have no 2000 data (mostly along the very far right of the region shown in figure 7) and there are valid data for all of the winter 2016 points (i.e., to the extent points are missing for some estimates, its mostly toward the middle of the interval where their omission should have the least effect)

To the extent possible, outliers have been rejected prior to the regression in the culling process that is part of the overall QA in creating the velocity maps.

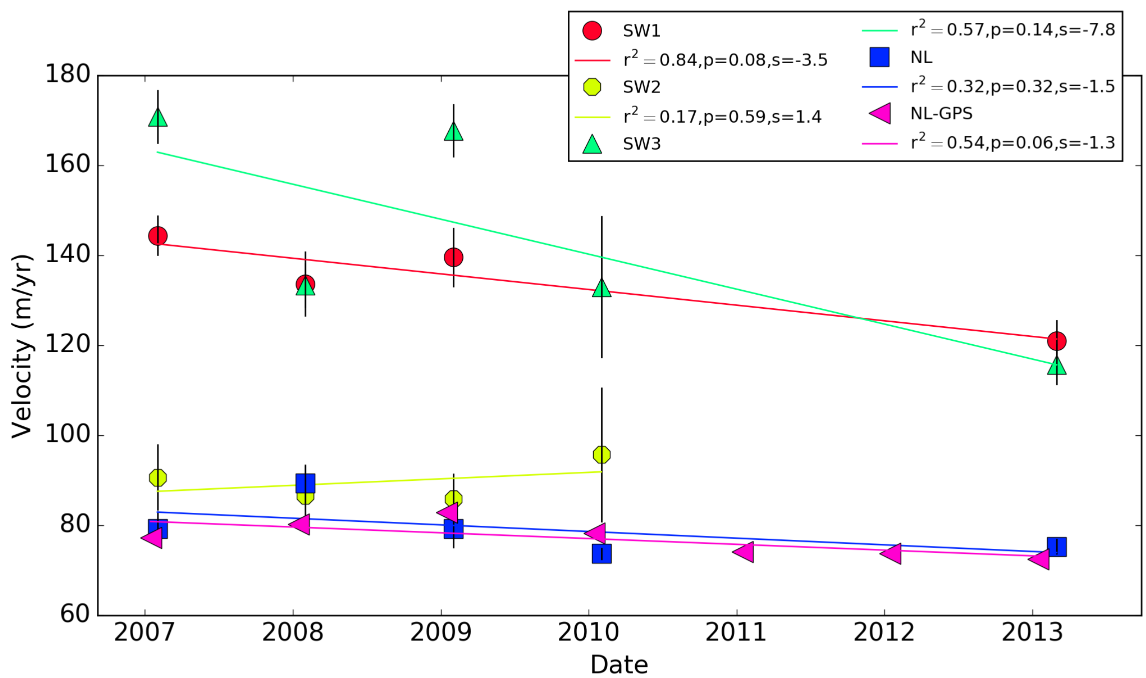
Of the 27592 points where we evaluated trends, only 132 were rejected due to lack of data (we will amend the caption to indicate this point, but in general a lack of color in the figure means there was no statistically significant trend).

It's not a matter of us excluding statistically significant results of 0 m/yr (a trend that is not significantly different than 0 is indicated by a lack of color). We only excluded results that were statistically insignificant. Essentially if the null hypothesis is "that there is no trend in speed of the ice sheet," then the alternate hypothesis that there is a trend is validated once a trend is identified that meets the test for significance (we picked the very common p=0.05). As noted there are some 27000 pts, and a significance level of 0.05 implies if the null hypothesis were true, then we would expect 27592*0.05=1379 points that incorrectly pass the test. In fact, 4137 pass (~15%), but many of these occur on fast moving areas where a trend is expected (e.g., edge of Jakobshavn and glacier at the lower end of the box). Thus, if we exclude these areas where there are clear trends, the number that pass the test on much of the ice sheet area approaches what we would expect if there were no trend (or a trend for a small part of the area).

While a null hypothesis such as that above ("there is no trend in speed of the ice sheet") cannot be proven by an analysis such as ours, we can more conclusively state that "given the level of noise in our measurements, there is no detectable trend in speed of the ice sheet." It always possible that there is a weak trend that simply is not detectable. A long record or less noisy data might eventually reveal such a trend. That said, we do not feel that the T2015 data meet this criterion (slightly shorter post 2000 record, and larger uncertainty in the velocity data).

We also note that the GPS observations presented show good agreement with T2015. We presume that, unlike the velocity estimates obtained from InSAR, the GPS dataset records net winter and annual ice motion as opposed to shorter temporal snapshots. NL-GPS (within the T2015 study area) has a computed slow-down of 1.3 m/yr (p=0.06) over the period 2007-2013 (Fig. 8), compared with the T2015 region-average of 1.5 m/yr during 2002-2014 (T2015 Fig. 2 and text). Meanwhile, J2018 fail to reproduce the GPS trend or T2015 trend with their InSAR observations (Fig. 8, NL timeseries). Similarly, a long-term decrease (1990-2012) in annual ice motion in this region has been measured by GPS – at the K-transect (van de Wal et al., 2015, The Cryosphere).

The GPS observations show a short-period time series, so one has to be careful about saying it agrees with the T2015 (the T2015 data also show do not show much change at this elevation either). And in fact, in terms of the calculated trend, the InSAR data produce a very similar trend, albeit with no significance (p=0.36) – see Figure below.



This plot indicates two points:

1) ***Noise in the data can obscure a trend that less noisy data will find. For any given data set, the level at which a trend can be detected is noise dependent. While for satellite data it remains a challenge to detect such trends, we feel our data (and similarly derived insar results) offer the best performance. We certainly would not be surprised if longer and/or more accurate data reveal a long-term trend.***

2) *As with many time series, it is easy to find a trend in a short section of the data that is not sustained over the long run (when we more than double the period the trend disappears). There are a variety of statistically insignificant signals in these data; for example, the GPS speeds increase from 2007 to 2009, then decrease afterwards.*

We haven't analyzed the van de Wal data, but certainly their Figure 8 does not suggest much of slowdown trend from 2000 to 2012, which agrees well with our results for this time period. To the extent any slowdown is apparent, it is from 1990 to 2000, before any change in melt, which is in opposition to the T2015 finding.

Last, we note that the discussion about summer ice motion at P16,L11-24 could be improved through stronger grounding in existing hydro-dynamic-coupling literature. For instance, studies such as Sole et al. (2013, GRL) show that summer velocities are faster than winter velocities, so proposing summer slow-down to below winter velocities as a possible explanation for T2015 but then immediately 'disproving' it (P16, L14-15) is confusing and has the potential to mis-lead. Similar datasets and discussion can be found in e.g. lead-authored work by Doyle, Sole, Bartholomew, Tedstone, Hoffman, Stevens. Moreover, given that the discussion makes comparison with T2015, it should also directly address T2015's hypothesis, namely that the processes responsible for the slow-down occur following the cessation of melting, i.e. early winter (e.g. T2015, p694, paragraph 1), *not* during summer.

Such a discussion is beyond the scope of the text, and completely unnecessary as what we presented was a simple argument to answer the question "could the difference between winter an and annual velocities be explained by differences in summer velocities." We simply say if this were the case, then what is the logical conclusion (a huge summer slowdown), which turns out to be completely inconsistent with observations. Hence, the difference cannot be explained by changes in the summer velocities, which our data do not sample. In fact, the last point above ("not during summer") is the exact point we were making (if it is in the annual, it has to be in the winter only too).

As noted above, such a literature review involves issues that are beyond the scope of the paper. For example, rather than supporting the hypothesis of the responsible processes, Stevens et al 2016 show that the correlation on which the hypothesis is founded is likely a statistical artifact : "Thus, the improved correlation observed when multiple years of runoff are included is an expected outcome of analyzing two variables with long-term temporal trends, even if the mechanism generating these trends is unrelated to the annual variability."

Summary: Both sets of data have issues with sampling, which we have tried to acknowledge in the original manuscript. Based on the comments, we note that in the revision we need to a) provide clarification on the sampling period b) to *fold in the analysis of the GPS data discussed above to make clear sampling biases are small, and c) to provide additional detail on how the trends were computed (i.e., how many points were used).*

Many of the issues brought up in this comment have been shown to be unsubstantiated (for example, our analysis suggests our winter velocities should have an order of magnitude less bias than has been suggested, and in fact, less bias than the T2015 annual velocities).

We will add a sentence or two acknowledging that it is difficult to pull out such trends from data and we may be operating near the margin of what's achievable. That said, we note the errors in our data are considerably (order of magnitude) smaller than the T2015 velocity data, so we are inclined to believe that until shown otherwise by longer or cleaner data sets that there is no widespread trend in deceleration on the ice sheet in response to increased melting. To the extent that there may be some slowdown in the region examined by T2015, it does not extend other regions of the ice sheet with similar flow patterns and similar melt forcing. Thus, even if some slowdown occurs in their study region, it cannot be generalized to nearby regions with similar melt forcing (i.e., perhaps it's a localized influence – e.g., more water captured by the adjacent Jakobshavn basin, where slopes steepened greatly over the last decade).