

Comment on 'Greenland Ice Mapping Project: Ice Flow Velocity Variation at sub-monthly to decadal time scales' by Joughin, Smith & Howat.

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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).

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.

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.

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.

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 into 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.

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.

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. 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 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). 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. 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).

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.

(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.

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).

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).

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*).

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.