Dunse et al, The Cryosphere. Seasonal speed-up of two outlet glaciers of Austfonna, Svalbard, inferred from continuous GPS measurements

Summary

This paper reports on some very interesting ice-dynamics data from two tidewater glaciers in Austfonna, Svalbard. It uses data collected from single frequency GPS observations along two flowlines over the course of two years. The data are analysed and interpreted with reference to surface melt-conditions inferred from air temperature data and also compared with ice-dynamics data from the mid-1990s. The data reveal substantial variations in ice motion over seasonal and inter-annual timescales which are explained by links to surface melt-rates. There are also substantial differences between the ice velocities observed in the 2008-10 study period and those from the1990s. The data have a valuable contribution to make to the literature concerned with investigating the mechanisms driving tidewater glacier dynamics. However, there are several areas which need developing further and these are outlined below. In particular, the findings must be put in to the context of the voluminous literature on this interesting topic. This is especially important given that the explanations for all the observed seasonal behaviour are based on surface melt-driven processes with no consideration of any oceanic forcing mechanisms.

Substantive issues

1) **Continuity of the velocity records**. There are places where there are gaps in the data (e.g. July/Aug 2009 at Basin-3 and Duve #1). However, I think that the GPS position is known at the start and end of these gaps. Assuming this IS the case, it would help clarify the annual dynamics by adding the mean velocity to the plots during that 2-3 month period as a dashed line at each site. If the positions at the start and end of the current velocity gap are not known, then I cannot see how annual velocities can be calculated.

2) **Timing issues for the upglacier propagation of speed-up events**. Two aspects of the data suggest there are potential problems with the detailed analysis of the timing of the upglacier propagation of speed-ups. Firstly, the seven day averaging procedure for ice motion ensures that there are considerable problems with precise daily speed-up prescription. Secondly, even if this procedure is accepted, eyeballing of the data suggests that the method for defining the onset of summer speed-up as "the day where velocities are in excess of three standard deviations from the pre-summer mean" may be pragmatic but is not appropriate. For example, the very clear step change in velocity gradient at all three sites (Duve #2-4) in Fig. 6a would suggest that all three sped up on the same day, not in the staggered fashion inferred by the prescribed onset protocol. The same issue could be levelled at the onset of speed-up dates chosen for Basin 3 #1-5 in Figure 5a where the change in velocity gradient suggests the speed-up lag from BS #1 to #5 is much shorter than 11 days suggested. Given the discussion on the timing of meltwater inputs and longitudinal coupling that drive these "onset-delays", this analysis needs more thought.

3) **Timing of "concurrent" peaks**. On P3434, it is argued that in 2008, "the summer speed-up was characterised by 3 distinct peaks, concurrent at all locations". From the plots, the second speed up at Basin-3 and Duve appear to be offset by ~7 days. This may relate to the issue above but suggests that either the timings are not concurrent, with implications for the suggested consistent forcing mechanism, or that the resolution of the data simply doesn't enable you to look at timings at less than ~1 week resolution.

4) **Hydrology-dynamics explanation/discussion**. The upglacier propagation of the 2009 Duve speed-up is explained by the delayed onset of surface melt at higher elevations "together with longitudinal coupling". Longitudinal coupling is an instant process (for e.g., an acceleration downglacier pulls the ice located upglacier) and therefore is not an appropriate process for the delay in the response of the upglacier ice regions.

The faster flow in Basin-3 in 2009/10 compared with 2008/9 merits debate beyond the suggestion that it "could be explained by a general shift in basal water pressure to higher values". This is of course true but can any sensible reasons for such a shift be postulated and in particular, could marine mechanisms be a possible control?

5) **Discussion and referencing**. In the introduction, the authors mention a number of papers, in particular concerning links between hydrology and dynamics and fast glacier flow (P3425-3426). However, reference is not made to a single piece of literature in the whole >2 page discussion on surface-velocity fluctuations (5.1). This makes the value of this section rather limited. The findings must be put into context – there is a very large literature on hydrology/dynamics links, in Alpine and polythermal glaciers and more recently from the Greenland Ice Sheet. You must therefore place your discussion/findings in the context of this work since most of what you say (e.g. onset delays, hydrological evolution, longitudinal coupling etc) has already been postulated for other systems. The same issue is true for section 5.2 regarding "implications for iceberg calving" when you only refer to the two Dowdeswell et al (1999, 2008) papers. There is a lot of literature on tidewater glacier dynamics, especially from Greenland and you must link your observations and explanations to these. This is especially the case since much of the tidewater glacier literature argues for most dynamics being driven from the grounding line (i.e. marine forcing) yet you argue throughout for the importance of surface melt-driven dynamic perturbations.

6) **Implications of dynamic changes for iceberg calving.** The data presented suggest a substantial increase in calving fluxes compared with the Feb 1992 and Jan '94 Dowdeswell et al (2008) InSAR-derived velocities. These increases (2x and 4x at Duvebreen and Basin 3 respectively) merit more discussion. If correct, these are very dramatic increases in ice flux and it would be good to see some attempt at explaining the very different dynamic regimes. The seasonal dynamics observed in this paper are all explained by surface-melt driven subglacial hydrology-basal sliding mechanisms (as opposed to marine forcing). If the change between the early 90's and the present day is also driven by the same mechanism, there should be something in the temperature (PDD) record to support this. If in fact the air temperatures are similar, then a different mechanism may need considering. Such a

discussion should refer to some of the copious Greenland literature (e.g. Rignot and Kanagaratnam, 2006) where similar increases in flux have been observed at many tidewater glaciers although oceanic forcing mechanisms have usually been postulated as the cause. In addition, the introduction on Austfonna (p3427-9) discusses the significance of surge glaciers on the ice cap and specifically with reference to Basin 3 (p3429). Could this reduce the importance of the observed speed-ups from a mass-balance perspective if the glaciers are in part of a surge ‘cycle’?

On the same issue, a little more caution is perhaps required for the inferred 2x and 4x speed-ups. Dowdeswell et al only had data in Feb ’92 and Jan ‘94 and the variability within your data suggests different 1-2 month ‘averaging’ windows would give very different annual mean velocities. This isn’t to suggest that the velocity changes aren’t substantial but I think you need to be more circumspect. In particular, you shouldn’t say in the abstract that “the observed annual mean velocities …. exceed those from the mid-1990s by factors two and four….” since the Dowdeswell data were very clearly not mean annual velocities.

Typos/minor issues

p3424, L23 spatially limited FAST-flow units….
p3424, L26 implications FOR glacier…
p3425, L2 and the CHANGE IN POSITION of the…
p3425, l10 delete “or near” – basal sliding can occur in cold ice but I don’t know any work that suggests fast glacier flow can occur without basal temperature at PMP.
p3425, l18 the Price reference is not really appropriate here as it was looking at land terminating ice margins. It should however come in later in the discussion when you are talking about longitudinal coupling and coupling lengths.
p3425, l19 not clear what you mean by excessive “charge” of the subglacial drainage system
p3426, l2 start new paragraph at sentence beginning “The relationship….”
p3426, l10 can be DERIVED FROM satellite…
p3427, l2 in parallel WITH the
p3427, l4 and flow dynamics. WE ALSO compare the…
p3427, l17 the suggestion that this ice-surface velocity pattern “is typical for a slow moving Arctic ice cap” needs a reference to support it.
p3427, l23 “in the past; Etonbreen and ….”
P3428, l1-4 The observed interior thickening and marginal thinning are explained with reference to timing within a surge cycle. However, could this behaviour be indicative of a warming climate signal with enhanced accumulation inland and enhanced melting at the margins? (along the same argument as recent observations from Greenland e.g. Pritchard et al, 2009).
P3428, l25 delete “down”
P3428, l27 earlier you say that Basin-3 surged between 1850-70 so this reference to some years prior to 1873 is rather odd.
P3428, l28 does this need to say “from its maximum SURGE or LIA extent”?

P3429, l1 delete “since”
P3430, l6-7 should really refer to the work of Gudmundsson here in terms of the effect of bed topography on the surface expression of ice.
P3433, l17-18 at Duve 1 and 2, velocities appear to stay considerably above the pre-summer values for several months, not just a few weeks as stated i.e. at Duve 1 pre value is ~175m, post value is ~200m for 4 months and at Duve 2, pre value is ~135m and post value is ~150m for ~ 6 months.
P3434, l1 “quasi-stationary” is the wrong term, approximately constant or invariant perhaps better.

Tables – Many of the columns in the tables have too many decimal places – either this level of detail is not needed (e.g. azimuth) or not appropriate given the associated errors (e.g ice thickness/bed elevation) so space could be saved here. Similarly, using ’08 and ’09 instead of 2008 and 2009 would save space.

Fig 2. Add distance to the calving front terminus to the caption for both glaciers.
Fig 3b. The x axis cannot be interpreted. Can you make it easier to make sense of with better labelling and clearer hash marks for the named months.