

***Interactive comment on “Stand-alone
single-frequency GPS ice velocity observations on
Nordenskiöldbreen, Svalbard” by
M. A. G. den Ouden et al.***

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The authors present an analysis of their low-power/low-cost single frequency GPS system deployed at an ever growing list of glacier/ice sheet sites worldwide. Dual frequency GPS is designed to remove the ionospheric effect to first order using signals in the L1 and L2 bands, but with an increase in power and cost over a single frequency (L1) receiver. Hence single frequency receivers are desirable, but one must accept a substantially reduced precision/accuracy - the very thing the authors attempt to present in this work. The wide use of their system is reason enough to warrant a publication on the system and its accuracy/precision.

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The paper employs multi-year data from multiple GPS on one glacier in Svalbard and reveals some interesting signal in their GPS solutions. The long duration of the record is a strength in the paper, but I return to this below. The uncertainty (repeatability) of the positions are assessed and the experiment is well designed. I would expect the results from the glacier will be useful and citeable. The figures are well drawn.

However, I don't consider the present manuscript is sufficient in testing their technique, as I explain below.

1. The principal limitation of single frequency receivers compared to dual frequency is the effect of the ionosphere. The issue of the ionosphere is critical since it varies with time. Total electron content varies diurnally, with the Sun's 27-day rotational period, seasonally and according to the 11-year solar cycle. This is not made clearly in the paper. The tests described in the paper are made with data from 2006-9 which is Solar ionospheric minimum. At ionospheric maximum the delay on the signal will be on average a factor of 3-4 greater, with peaks during high solar activity being 10 times greater for daily averages. This delay can be ± 100 m in some situations. The ionospheric model available in the GPS broadcast orbit accounts for up to $\sim 50\%$ of this, but substantial error remains. This means that the error determined in the paper is a minimum error and will not be representative of the error in coming years (or back in 2000-2003). This poses a serious problem to the authors as their analysis is incomplete without considering this. Double differencing the observations with a base station's observations helps cancel this over a few km, but that approach cannot be used in this system where the coordinates are computed on the receivers (I think).

2. There is a general lack of detail about how the GPS coordinates are computed, perhaps due to an opacity about what the receiver is doing to compute the positions - do the authors have access to this information even? The authors do not explain the strategy for positioning and what corrections or models are applied. For instance, a model of tropospheric delay may be applied and reduce tropospheric errors to a 10-30cm, or maybe its not, and hence ~ 2.0 - 2.3 m of error is present. I presume the system

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does between-satellite differencing to difference satellite clock errors. I presume that the broadcast orbits are used and hence the derived positions are in a WGS84 reference frame. (By the way, the broadcast orbit positions are now accurate to $\sim 0.5\text{m}$ most of the time through comparison with IGS precise orbits). What observation weighting was used - uniform or elevation dependent? Was the ionospheric model from the navigation message applied? The actual GPS observable used is not given, but I presume it is C1 or P1 (which?) alone and carrier phase L1 data is not used (you can get an ionosphere free observable from single frequency data by combining carrier and code as $L1+P1$). If all this is done, the dominant errors are ionosphere, orbit errors and code multipath (a few decimetres to metres). The first two will be common to all sites, the last somewhat site-specific (at least there should be significant differences between rock and ice sites). Solid earth tides are very small at high latitudes.

3. More could be done to investigate the origins of the periodic signals the authors identify. First, I'd suggest expressing the amplitude of the signals, not the power - it's impossible to tell what is important and what's not - the 15day signal may be just a few cm. A Lomb Periodogram would help in this regard. Reporting the period of the peaks would also help - is the $\sim 15\text{day}$ one 14.76 or 13.66 days and hence of tidal origin? Differencing coordinates of NBRef with the other stations will remove almost all satellite propagation effects and solid earth tides and just leave relative multipath (there will be some small error since the two receivers will see slightly different satellites at times). If the 15day signal disappears then it's probably to do with the orbits (since nothing in iono is 15 days) - the receiver computes the satellite orbits using an algorithm which will likely be simplified; tidal earth rotation corrections tend to be excluded. The 15 days signal is erroneously suggested as being from satellite repeat periods (p993) after having been correctly given as 12h56m earlier. Is the 15day signal parameterised (or removed beforehand) in computing the velocities?

4. it would be worth showing time series of high frequency variability to fully demonstrate that these are not reliable (ie, an unsmoothed version of Figure 6 but for just 2-3

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days)

More minor issues: P981L3: also steady state dynamics

P984L5: replace "uncertainty" with "error"

P985L12-13: the 6m value proved difficult for me to track down. In the 2008 version of the cited document, I think it is in "B.3.1.8 Computing the SAT Values" -correct? the authors would need to read it carefully if this applies to high iono conditions also? P985L18: I'd suggest introducing dual frequency here, explaining its disadvantages. the next para needs to explain what is happening in the receiver (as per 2. above)

P986L1: I don't think this is correct - patch antennas have reception below the antenna, although with reduced power compared to those above P986L7: introduce the power requirements of this system P986L17: use $t, t+1$ in notation of differences. P986L23: Earth curvature is only 1mm/km, so this is pretty negligible here.

P987L13: DGPS terminology is confusing in the literature - some mean differenced pseudorange, some mean just differenced, some mean differenced carrier phase. Please clarify and I'd suggest not using the term.

P989L12: the two are, of course, correlated, having been derived in a XYZ system.

P989L21: presume elevation should be +60 only

P990L2: have the authors studied the optimal sampling interval - given they need to average, could they go to lower frequency measurements? P990L17: these harmonics will be partly multipath (which repeats every $\sim 23\text{h}56\text{m}$ but is not a pure sinusoid). Some contribution from diurnal ionosphere is likely. Tropo is much smaller than these. P990L22-: this paragraph needs revision based on the previous comment

Section 4.2.1: the add the NBref vel, which is essentially zero in truth.

P992L3: these differences could be due to ionospheric activity P992L16: 1.6m applies only to this period P992L20: "period of the variations"? Do you mean the detectable

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periods? P999L25: Is the winter data clearer due to lack of summer variability in the glacier? Figure 6: I'd like to see the NBref velocity added on another panel.

P993L17: this discussion is wrong and needs revision

P996L4: revisit after revision P996L18-19: delete

Table 3&4: I'd add height here for completeness, even if they are largely unuseable (although I note their use in Fig 7).

Fig 7: are the units on the RHS m/yr rather than mm/yr?

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