Review of:

In-situ GPS records of surface mass balance and ocean-induced basal melt for Pine Island Glacier, Antarctica

By David E. Shean et al.

This paper discusses time series measurements of surface elevation change made using an array of GPS receivers set out on the floating tongue of Pine Island Glacier. These and other data are used to quantify the processes contributing to changes in ice thickness, especially the surface and basal mass balance. The analyses are described in some detail and the results are interesting. The work is therefore certainly publishable, but my feeling is that more work should be devoted to the presentation to make the results more accessible. At present there is no clear logical flow from definition of the problem, through observation and analysis of observation and ancillary data, to conclusions. The discussion jumps between observations, processes, models, supporting data, and it is difficult to keep track of what has actually been observed and what the ultimate goal of the analyses is.

A bewildering range of surface height variables are introduced, many of which are not independent but simply derivatives of the others, and this really adds to the (unnecessary) complexity of the presentation. The problems are immediately apparent in that abstract. We are told that time series of “antenna height, surface elevation and Lagrangian elevation change (Dh/Dt)” are to be presented. But without any definitions the reader is left to guess what these variables actually are and how they are distinct. Reading the paper, it seems that “height” refers to measurements relative to the instantaneous snow surface, while “elevation” refers to measurements relative to the fixed geoid. However, “elevations” are confusingly denoted by the symbol “h”, and the terminology is not used entirely consistently. The next sentence in the abstract says that “The antenna height time series show a surface elevation increase”. If the increase is relative to the geoid, the ice must the thickening, right? Apparently not. Later on we are told that “observed Dh/Dt” is negative, implying thinning. This is probably the worst example of inconsistent terminology, but the discussion could be simplified throughout by reducing the number of variables to just the independent ones and a couple of critical derived ones. To add to the current confusion, the schematic defining all the variables (figure 4) actually shows Dh/Dt as positive.

The most fundamental variable measured by the GPS is the antenna elevation. A subsidiary measurement, and a really nice addition, is the height of the antenna above the snow surface. To avoid potential confusion between elevation and height (especially if the authors are not going to be entirely rigorous about their usage), I would recommend using an entirely different name for antenna height, perhaps “ground clearance” or “exposed pole length”. Apart from these two variables, nothing is directly measured by the GPS. Everything else is a derived quantity, so why not just stick to these, unless it is absolutely essential to introduce something new. One derived variable that is worthwhile having is the snow surface elevation, since that is what the satellite stereo imagery (introduced later as an ancillary dataset) measures, but that is just the antenna elevation minus the exposed pole length. The “pole base elevation” is a useful reference level, but is just the antenna elevation, minus the fixed pole length, unless the pole angle relative to the vertical changed with time (a possibility that does not seem to have been considered). I don’t see the point of introducing the “reflector height” variable which is just the initial “antenna height” (or ground clearance/exposed pole length) minus its initial value. So trends in “antenna height” and “reflector height” are identical, apart from the sign. Arguably the “pole base depth” below the surface (or "buried pole length") is useful in that compaction rates above this level and below it ultimately need to be considered separately.

By the way, given that one pole fell over completely, can you be sure that your measurements are free of contamination from changes in the pole angle? Were there repeat measurements of pole angle? You also make the assumption that the pole base is the point that is fixed in the firn. That seems reasonable to me, but did you put an insulating stopper in the end of the pole? That would make your assumption more justifiable.

So for the GPS time series there are two measured quantities (antenna elevation and exposed pole length) and two derived quantities (surface elevation and buried pole length). I don't really see why you need any more. These can then be used to derive the surface and basal mass balance. The most consistent way of doing this would be to define a local vertical coordinate that is zero at the base of the pole and positive upwards. Then taking your equation (3) and substituting:

$$H = (z_{ia} - d_a^+) - (z_{io} + d_a^-)$$

where *ia* is the ice-atmosphere interface, *io* the ice-ocean interface and $d_a^{+/-}$ is the thickness of air above and below the origin, you get two separate equations for the surface and basal mass balance:

$$\frac{D}{Dt}(z_{ia} - d_a^+) = \dot{a} - (z_{ia} - d_a^+)\nabla.\boldsymbol{u}$$

$$-\frac{D}{Dt}(z_{io} + d_a^-) = -\dot{b} + (z_{io} + d_a^-)\nabla.\boldsymbol{u}$$

because the reference surface is assumed to be a material surface. Now, some of these terms you have implicitly ignored (probably perfectly valid, but it would have been helpful to see the process and justification of the simplifications), and of the others, some come from your GPS measurements and some from ancillary data and assumptions.

The divergence term comes from your measurements of the rate of change of the distances between GPS stations. Or does it? The methods section describes how you derive absolute positions and motion relative to fixed reference stations. But the absolute motion is unimportant here as your calculations are in a Lagrangian framework. Couldn't the inter-stake distances be derived more simply and accurately by differential positioning of one station relative to the other? No fixed base station is needed then. And given that you have all the inter-station distances as a function of time, why don't you make a direct estimate of the divergence from those data? Actually, since you have five stakes, and you only need three to do the calculation, you can do it with multiple stake combinations and get some idea about the spatial variability. Why haven't you done that? I found your ad hoc calculations based on the assumption that some stakes were oriented along flow, and that along-flow extension should dominate, to be rather unsatisfactory.

The air thickness terms come from the firn modelling, which needs a much clearer discussion of the inputs and outputs. I think it is driven by the "SMB and Temperature data" discussed in section 2.4. In which case, why is that section not a part of a larger section that discusses the firn model? As it stands, it is presented to the reader as a standalone estimate of SMB, but that is one of the quantities promised in the title from the GPS records. The point being, I assume, that you want regional estimates to drive the firn model, distinct from the point estimates that are the result promised in the title? How sensitive are your firn model results to these inputs? Basing your temperature on a measurement at Evans Knoll and an assumed lapse rate will introduce a warm bias. It is well known that a stable inversion layer can form over the flat topography of the ice shelves, so surface temperatures can be lower than those on surrounding higher ground. I doubt that ERA-Interim captures this. Could you not use remote sensing data? The outputs from the firn model are never clearly presented. A vertical velocity associated with compaction is quoted in a number of places, but the separation of compaction into the components that occur above and below the pole base (as in the above equations is never made clear). These are key components of the results, and I would have expected to see some graphical presentation of them. The surface height from the firn model is shown, but this doesn't tell the reader what was actually used in the calculations of surface and basal mass balance.

The z_{ia} term is simply the (assumed fixed) pole length minus the (measured) exposed pole length. The z_{io} term is a bit more complex, since it must come from the measurements of antenna elevation and an assumption of isostatic equilibrium:

$$-\rho_w(z_{io}+h_0)= \rho_i(z_{ia}-d_a^+)-\rho_i(z_{io}+d_a^-)$$

where h_0 is the elevation of the reference surface above the geoid, that is the measured antenna elevation minus the fixed pole length. This leads to:

$$-(z_{io}+d_a^-)=\frac{\rho_w}{\rho_w-\rho_i}(h_0-d_a^-)+\frac{\rho_i}{\rho_w-\rho_i}(z_{ia}-d_a^+)$$

which is now clearly in terms of the two directly measured quantities and two outputs from the firn model. Now the melt rate can be derived from the following equation:

$$\frac{\rho_w}{\rho_w-\rho_i}\frac{D}{Dt}(h_0-d_a^-)=-\dot{b}-\frac{\rho_i}{\rho_w-\rho_i}\dot{a}-\frac{\rho_w}{\rho_w-\rho_i}(h_0-d_a^-)\nabla.\boldsymbol{u}$$

In the above notation h_0 is the same as the authors' "pole base elevation", so I think a couple of the terms in this equation are the same as those in their equation (7), but I am not sure about the rest. I did not really follow all the assumptions made in the derivation of (7) and I would argue that the above is clearer in its relationship to the directly measured antenna elevation, the firn model output and the surface accumulation rate (itself derived above from a directly measured quantity and firn model output).

Of course, this equation and the authors' version relies on the assumption of isostatic equilibrium. Curiously the validity of this assumption is never discussed. However, the quoted thicknesses measured from radar differ from those calculated from surface elevation, so it is clear that the assumption does not hold. Herein lies the major weakness of the paper. It is well-known that over length-scales comparable with the ice thickness, vertical shear stresses can partially support the ice. This has been shown to be the case on similar channel-like features on a number of ice shelves (McGrath et al., 2012, Ann. Glaciol., 53, 10-18; Jenkins et al., 2006, J. Glaciol., 52, 325-346), including on Pine Island (Vaughan et al., 2012, J. Geophys. Res., 117, F03012). The key point is that while the ice is not freely floating (as it is not on the scale of the stake network discussed here) the regions of thinner ice will be sinking as the ice deforms under the vertical shear stress. So if you set up a GPS station in a channel you will see a surface lowering even if the ice thickness remains constant. Such a process could introduce a significant bias to the estimates of basal mass balance that does not appear to have been considered by the authors. It might, for example, explain why the surface elevation changes are rather steady despite the change in ocean conditions that have been inferred to have changed the melt rates.

Overall, my recommendation would be to restructure the paper along the following lines:

Introduction: A general introduction to the region and the problem to be addressed. Don't discuss your GPS sites in detail yet.

Method: Describe the principle of how measurements of the elevation and exposed pole length can be used to calculate the surface and basal mass balance. Introduce the reference frame and variables and show what additional information you need.

GPS data: Describe the set-up of the GPS sites, the data collected and the processing to get to the key results of antenna elevation and exposed pole length and inter-pole distances, and the uncertainties in all these numbers.

Divergence: Describe the calculations and show the horizontal variability. Discuss the uncertainties.

Firn compaction: Describe the model and the necessary input data. Show the outputs that you actually use in your calculations.

Results: Calculate surface and basal mass balance and uncertainties. Compare with other studies.

Discussion: Discuss all the other potential sources of error. Don't forget the impact of adjustment towards isostatic equilibrium. Not only is it a source of elevation change that is not related to thickness change, but it gives rise to a flow divergence that changes with depth, so your measurements of surface divergence are not representative of the depth-mean.

Conclusions: Can you say for sure that the melt rates were steady, given the uncertainties and potential biases? If it was, how do you explain that observation?

Finally, some more minor points:

I would recommend making it clearer in the title that the measurements are on the floating part of Pine Island Glacier and that the surface and basal mass balance estimates are derived from GPS measurements. The wording at present suggests that the results are directly measured by GPS.

Some of the figures could be improved. In particular, it would be much easier if the sites were labelled in Figure 2. The colour code is really hard to use. Again, label them on Figure 10.

Why did you use a tide model to remove the tides from your records? Why not do a harmonic analysis of the elevation data themselves? Your records are long enough to make reliable estimates of more tidal components than are given in the CATS output, aren't they?

In summary, while this is a long review, there are some really nice results contained in this paper. I also think the detailed discussion of the data and analyses could be a real strength, if it were presented in a more readily accessible way. The current presentation left me a little confused as to what the bottom line really is. The title promises estimates of both surface and basal mass balance, but the calculations are dependent on the results of a firn model that needs surface mass balance as an input. It is never made clear to what extent the author's regard their calculations of surface accumulation as independent results, or if the discussion is merely a demonstration that measured changes in exposed pole length are consistent with, rather than an addition to, prior knowledge. Likewise, with the basal mass balance. Do those results represent an addition to our knowledge of melting at the base of Pine Island, or are they more a demonstration of the limitations of using surface elevation data to infer melt rates at these spatial scales?