Reviewer #3, Re-review of: Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf. Friedel et al., 2017

Summary

This revised paper on Fleming Glacier is much improved from its original form. For example, vertically registering the TSX DEM's over sea ice has now been discarded in favor of using ground control points on stable mountains peaks. However, further information on the magnitude of this correction still needs to be provided. While many of the the methodological concerns have been addressed, the authors have not improved their approach to characterizing their measurement error. It is not unreasonable for a reviewer to request a spatially variable error estimate for the datasets presented, therefore the authors must calculate one before this paper is ready for publication, rather than to 'assume an error' as they have stated in their comments. As noted by the authors, the text throughout this paper, particularly in the discussion section, has been completely overhauled which has improved the clarity of the paper. Despite these significant improvements, and the quality of dense ice velocity time series, the authors have not convinced me that the ice thinning rates from the airborne data and DEM differencing are robust. The data has a huge noise range on it which is significantly greater than the stated error, and simply fitting a polynomial through it to get a plausible but unvalidated mean value, doesn't provide enough evidence to conclude with confidence that the magnitude of the thinning signal has been correctly characterized. Given that the authors acknowledge that their result contradicts the previously published estimate by Walker and Gardener (2017), the accuracy of the thinning estimate does need to be properly interrogated before this result is published.

Specific Edits

Original comment P5 L6 – The authors have responded to this comment. Although the coverage of ice velocity measurements is different for each image pair, it is possible to state % removed relative to the original pre-filtered result. The approach of only stating % removed on the fast flowing ice tongue is better than nothing, but this number will be biased low because as the authors acknowledge its harder to get good tracking results further inland. In my view, the fact that the measurements are poor in the interior, isn't a good reason for excluding this region from the filter removed stats.

Original comment P5 L16 – The authors have not responded to this important concern. The original comment was that the sensor accuracy is not an estimate of the measurement error on the ice thinning rates. As previously stated 0.2 m is the accuracy of the original point elevation measurements, not the *elevation change* measurement. The authors are also using the elevation data after re-gridding it so they need to state the accuracy of the gridded dataset instead of the accuracy of the raw point data. The elevation change error is different for different sensors, so the authors should provide formal error statistics for each dataset.

Original comment P6 L2/3 – The authors have only partially addressed this comment. Although the method for vertical registration has been completely overhauled to use stable ground control points rather than variable sea ice, the size of the vertical correction has not been stated in the paper. This is important because if the authors are adjusting the DEM heights by 10's of meters, to then measure a few meters of elevation change, then this would indicate that the raw data may not be suitable for the task. Additionally, is the vertical co-registration spatially variable, or is the mean of all ground control points used, and again if it is spatially variable the range of values should be stated. The precise method employed must be stated more clearly.

Original comment P6 L20 – The authors have not sufficiently addressed this comment. As other reviewers also pointed out, having a penetration bias account for 50% of the signal is a significant error therefore it can't be dismissed without a proper solution. The authors statement that they 'have to deal with the data they have in hand' is just not true. There are other available methods of measuring elevation change, such as using altimetry data. The TSX DEM differencing results could be inter-compared with altimetry elevation change from the same time period to establish the extent to which

the dhdt numbers can be trusted.

Original comment P6 L27 – The authors have not addressed this comment. Just because the study area is small, it doesn't mean the error is also small. Mountain glaciers and ice caps are significantly smaller than this Antarctic ice sheet drainage basin, but spatially variable error measurements are still made. It is a completely reasonable reviewer request to ask for a spatially variable error estimate, therefore the authors response to just 'assume an error', rather than to calculate one, is not acceptable.

Original comment P8 L14 – Wording still not correct English, suggest edit to: *Since 2008 the glacier tongue has not advanced seaward of the 1996 grounding line position.*

Original comment P8 L28 – yes fine.

P13, L26-27 – The authors state the following reason for the disagreement of their elevation change result compared with Walker and Gardener (2017), as: 'Their approach of averaging elevation change in 5 km intervals probably filtered out the positive trend that is most prominent on the lowest 3 km of the profile.' It would be trivial for the authors to test this hypothesis, so I suggest they do this rather than postulate, as this new result conflicts with the published literature.

Figure 6 - The wording 'Likely recent grounding line', for the authors new grounding line is not good. It either is a measurement of the new grounding line position, or its not. A time stamp for the new line should also be provided. This lack of commitment to the measurement limits usefulness of the information.

Figure S8 - This figure is incorrect. In 1) there is no dynamic thinning yet because the ice shelf is still providing buttressing force, and the ice velocities are stable. If the ice was dynamically thinning, then the ice velocity would have accelerated. There should be dynamic thinning in 2). And there should also be dynamic thinning in 3) if the ice velocities have continued to accelerate. This schematic is incorrect as it stands, and doesn't really add anything to the discussion so I would just remove the figure.