Review of: Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf. Friedel et al., 2017

Summary

The topic of this paper, Fleming Glacier, is extremely interesting given the significant changes reported by previous publications, so I was very keen to read these new results. The authors present new ice velocity, elevation change and grounding line position data acquired from a range of airborne and satellite based instruments. The ice velocity results are nice, and I believe constitute the most complete time series of velocity measurements over Fleming glacier which provides new insight into the timing of ice speedup on this sector.

However, after reading the manuscript there are a number of major flaws with the methods employed to derive surface elevation change and to measure the grounding line retreat. As it stands, the problem with the techniques make it highly likely that both the magnitude of the elevation change signal, and the grounding line retreat, may not be correctly reported in this paper. For example, cross calibrating the DEM elevations over sea ice, which varies annually and can range in thickness from 0-5m, is extremely unsatisfactory. Moreover, even if this correction is accepted, the authors estimate that known X-band penetration bias can account for ~50% of the dh/dt signal across the basin. Even if the estimate of elevation change is accepted, the associated error measurements do not reflect spatial variability in the data quality, and are unrealistically small given the spread of the raw data. For example, it is stated that the CAMS-ATM dh/dt data has an error of 0.2m/yr in regions where the point measurements at the same location range from -0.5 to -7 m/yr. These errors must therefore be revised. Regarding the estimate of grounding line retreat, the technique is unproven and is un-validated in this paper. Even if the authors demonstrate that the technique can be trusted, as it stands the results presented in this paper are contradictory because regions measured to be grounded are located within the new floating ice shelf area.

On top of these technical issues, I have a few more minor concerns about the use of scientific terminology throughout the paper, and the overly simplistic nature of the analysis and discussion sections. The specifics of these and other concerns are documented in detail below.

My criticisms of the methods and results presented in the paper are major, and will be time consuming and require a significant effort to properly address. The implications of these concerns is that I believe the magnitude and spatial pattern of the elevation change and grounding line retreat data may be incorrectly reported in this paper. This is significant as I do not have confidence in two of the three core datasets presented, therefore, it is my recommendation that this paper is not suitable for publication in its current form.

Specific Edits

P1 L21 – Edit 'far upstream of the glacier', using this as a location is ambiguous, better use a fixed reference location, like the calving front in 'x' year.
P1 L26 – Edit paper to quantify 'much larger ice masses'.
P2 L4 – Edit missing to 'be'.
P2 L7 – Edit ice 'shelf' tributaries.

P2 L30 – Edit 'explain' to 'investigate'. I'd argue at this point the authors haven't demonstrated they can explain the observed signal.
P3 L23 – Poor wording, edit out 'got' both times.
P4 L2 – Edit 'dynamic' and check use throughout paper. Previous publications (e.g. Rignot et al 2005) have demonstrated that Fleming exhibited dynamic imbalance during their study period, but as the authors state this paper spans a longer time period, and at this point it hasn't been proven that Fleming is dynamically imbalanced for the full duration of their study period.
P5 L6 – More informative to state what % of each velocity image is removed during the filtering process as this should be an indicator of the quality of the tracking output. The filtering should remove 100% of the unreasonable results, otherwise its not a very good filter!
P5 L16 – 0.2 m is the accuracy of the original point measurements; the authors are using the dataset after re-gridding it so state the accuracy of this dataset instead or as well as the accuracy of the raw data. The accuracy is also different for different sensors, so the authors should provide statistics for each dataset.
P6 L2/3 – This is correction extremely unsatisfactory. Sea ice is a complex parameter, and is certainly not a stable/constant reference surface for precise cross calibration of elevation measurements. In the Antarctic, sea ice can range from 0 to 5 m thickness with very large spatial and temporal variability, snow depth on sea ice is not routinely measured but can account for half the thickness retrieval, and ocean height varies with tides, atmospheric pressure etc. When deriving the correction, the authors have not attempted to account for inter-annual variability in sea ice thickness so this must be addressed before any confidence can be had in the elevation change measurements. This is critical because the range of thickness variability is the same order of magnitude as the dh/dt signal calculated from the DEM differencing. The authors must revise the manuscript to characterise the temporal variability of the sea ice over which they are cross calibrating the DEM's, and to rule out any influence from this factor on the end elevation change. If this effect can be proven to be negligible, the authors should also state the size of the correction, and which DEM was adjusted, in the manuscript. Having said all this, I suggest the authors dont cross calibrate the DEM's over sea ice at all as it hugely reduces the confidence that I believe we can have in these results.
P6 L20 – The authors calculation that up to 2 m/a of the thinning rate can be attributed to TSX DEM penetration bias. This is a huge error which accounts for ~50% of the dhdt signal present across the majority of the basin. The ICESat and ATM tracks that this error was calculated from have extremely limited coverage, and don't pass through the region with the highest thinning rates, therefore its possible that this number might even be an underestimate. For example, other studies have shown that the penetration bias in DEM's derived from TSX/TDM data over snow covered terrain can be as large as 4m (e.g. Dehecq et al 2016), which is the same magnitude as the dh/dt signal presented in figure 4. The large size of the known errors relative to the size of the signal, combined with the limited data that has been used to characterise the error makes it very difficult to have confidence in the thinning rates presented here. Other auxiliary datasets such as atmospheric temperature data, or SAR backscatter images might also be used to characterise the onset and spatial pattern of melt in the study area. The authors description of how they have accounted for this source of error is cursory given its size relative to the dh/dt signal in the study area. As surface melt and therefore penetration is known to have large spatial variability, I recommend that the authors revise their approach to account for this spatial variation across the basin, as a polynomial fit derived from a single track of airborne data will definitely not capture the magnitude or pattern of this effect across the study area.
P6 L27 – Assuming an error of 0.2m/a just because its one order of magnitude higher than the direct inter-comparison with the ATM data the dh/dt was calibrated against isn't satisfactory. Errors are spatially variable, so the authors should revise their approach.

P6 L33 – Why use a 35 m buffer if the ICESat footprint is known to be 70 m? I recommend the authors use the same footprint size as the aim is to do the most direct comparison possible.
P7 L15 – Cite a reference for the source of the firn density correction variable.
P7 L25 – As far as I'm aware, this method of detecting grounding line position has not been proven in peer reviewed literature. Although the logic behind it is reasonable, (i.e. if the ice is in hydrostatic equilibrium it must be floating), factors such as the spatial resolution and error on each input dataset will severely limit the sensitivity of the technique for detecting grounding line position, let alone change in grounding line position. To be convinced that the technique works, I recommend grounding line retreat from this method is evaluated against known retreat rates, in the Amundsen sea for example. If suitable data isn't available to validate this technique in another area, then alternatively a proven technique can be employed to evaluate the hydrostatic technique in this study area. For example, ERS-2 SAR data with a 3-day temporal baseline was acquired in this area in 2011, so if coherent, this should be used to produce a grounding line estimate from the proven quadruple difference interferometry technique (Rignot et al, 1998). At a minimum the authors must state the error on their estimate of grounding line position from hydrostatic height anomaly, and it follows that if the uncertainty on the measurement is greater than the change in position assumed, then the method is not viable.
P8 L3 – The Figure 2 z-scope is not easy to interpret. I suggest the authors re-plot this information as a standard x/y line plot of the time series of flow line ice speeds, with an inset showing change in calving front position.
P8 L14 – Poor sentence wording. Edit.
P8 L28 – 8 to 14 km upstream of which location. Edit sentence to be more precise.
P8 L30 – Edit text to quantify 'lower parts'.
P8 L31 – Edit manuscript to remove all 'could be detected' wording. You are stating what results you have observed, so it 'has been detected', not the less affirmative 'could be'.
P9 L4 – The scatter on figure 5a is very large and must be addressed given that it is significantly greater than the previously stated errors. I recommend that a) distance markers are annotated onto the ATM and ICESat track locations on Figure 4 so we can see how this corresponds to the x axis distance scale in Figure 5. My interpretation of figure 5a is that the elevation change measurements are unusable between 0 and 20 km of the grounding line, which looks like its about up to the 'g' on the Fleming annotation on figure 4. This is the key area of interest, so vastly limits the usefulness of these datasets. B) state the method used to calculate the lines of best fit, e.g. moving average, polynomial fit? How has the clearly erroneous data been removed? C)
P9 L10 – Based on figure 5a, stating that the CAMS-ATM show elevation change of 4.1 ± 0.2 m/a is not credible. The raw data shown in figure 5a shows that at this location the elevation change ranges between -0.5 to 7 m/yr, so the error of 0.2m/yr is effectively meaningless. Please revise the error estimate here, and throughout the rest of the results paragraph.
P9 L12 – The fact that Fleming is thinning between 04-08, doesn't prove that the catchment hasn't reached an equilibrium since shelf collapse in 1989. The two effects may be entirely uncorrelated, so although its possible, without a continuous dataset I don't think it can be proven one way or the other. I recommend the authors revise this wording. Changing 'shows' to 'might suggest' would be more factually correct.
P9 L22 - Although I don't like the method, it's clear how the hydrostatic equilibrium has been calculated along the airborne tracks. Can the authors clarify what method they have used to draw the grounding line connecting the dots in Figure 6? For example, according to their own data, a section of grounded ice on track 'c)' is included in the now 'floating' area. I recommend the authors revise the line as their data shows it isn't correct.
P9 L30 – Based on the above comment, the number stated for the area of the floating shelf

will also need to recalculated.
P9 L33 – Quantify 'several km'.
P10 L25 – Although Turner et al 2016 shows that the long term air temperatures are decreasing the situation may be more complicated than that, and Sundal et al (2011) showed that a simple linear relationship between melt water vs lubrication is not currect, as melt induced speed-up can be offset by drainage efficiencies. I'd revise this text to avoid oversimplifying these relationships.
P11 L4 – Remove sentence about basal melt. This hasn't been measured in this study so is just a generic assumption, and no reference provided to previous study evidencing statement.
P11 L8 – Same statement as above re basal melt inference.
P11 L11 – Really poor sentence wording. Edit to be more diligent with regards to terminology. Stability is a specific process, i.e. 'unstoppable' retreat that will continue to propagate even if environmental conditions returned to their original state. Glacier imbalance and grounding line retreat can occur stably. I haven't seen evidence presented in this paper of about the likely future instability of Fleming, so tighten up language.
P11 L12 – edit Thwaites
P11 L18 – Again I feel the analysis here is overly simplistic. Wouters et al were the first to present the rapid thinning rates and mass loss from the Western Palmer Land region, but subsequent publications (Hogg et al 2017) have shown that only ~30% of this should be attributed to ocean induced dynamic imbalance. Revise text to reflect known complexity.
Discussion general – the authors have stated results from other regions of WAIS/AP, however this really isn't tied very coherently into how this impacts on the results they have presented on Fleming.
P12 L10 – as previously stated I do not think the authors have proven dynamic instability on Fleming. Imbalance maybe, but instability, no. Equally, attributing the signal to ocean induced dynamic forcing without properly evaluating any oceanographic or atmospheric data is poor. These interlinked processes are very complex, and really hard to disentangle. Although its entirely plausible that ocean forcing is responsible, I do not think the analysis presented in this paper has proven it.
P12 L18 – 'ocean forcing is likely to continue'. Do the authors present any evidence to support this statement, or is it just a guess?