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Interactive comment on "High accuracy UAV photogrammetry of ice sheet dynamics with no ground control" *by* Thomas R. Chudley et al.

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Unmanned Aerial Vehicles (UAV) photogrammetry has become a very valuable tool to generate high-resolution ortho-images and Digital Elevation Models (DEMs) for geo-scientific studies at relative low cost. UAV are of special interest in glaciology, as UAVs can survey remote and hardly accessible glaciers. Repeat UAV surveying, SfM-MVS photogrammetry together with feature-tracking method can be used to track changes of the glacial terrain over time, as for instance due to ice flow motion, melt or calving. However, the inter-comparison of UAV-based photogrammetrical products requires very accurate geo-referencing. So far, it was mandatory to install Ground Control Points (GCPs) next to zones of interest to obtain a sufficient level of accuracy. This need of GCPs was therefore the Achille heel of UAV photogrammetry for glacier surveying

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as it is difficult to install a network GCPs in such an extreme environments. Direct-georeferencing (not based on GCPs, but on camera locations) is key to overcome this issue, but this requires (at least) accurate geotagging of aerial pictures – a level of precision that can not be achieved by standard GPS (SPS).

This study solves this issue by the means of miniature, low-cost and cm-accurate differential GPS inboard the UAV. The added-value of this technique is illustrated by the first-ever velocity field derived by UAV of the interior of an ice sheet (i.e. without any immobile margin serving as reference, and without using any GCPs).

This is an important contribution as it introduces a promising and affordable technology for the surveying of glaciers, and their changes over time, with significant improvements compared to former methods. Beside this, the paper contains a lot of technical details that are necessary for replication.

I have one major comment (see below) about the uncertainty assessment – which is a central question in this paper. I also have others specific and minor comments that might help to improve the paper.

Major comment

- My main concern is about the uncertainty assessment in the horizontal direction. The authors claim that they get a 0.07 m horizontal accuracy. However, this number is based on the assumption that feature-tracking error contributes by 0.5 pixel of the uncertainty. It means that 0.07 is obtained by subtracting 0.10 (or estimated feature tracking error) to 0.17 of RMSE. Is it not a bit shortcoming? Assume the feature-tracking performs better than 0.5, it means that the uncertainty your are trying to estimate is larger. From your results, we can reasonably say from your result that 0.17 is the combined uncertainty of the two possible sources of error – this is already a very strong result! Yet, if one wants to clearly evaluate the contribution of each, this can only be done separately. For Feature-tracking (or template matching), this might be difficult. Yet for SfM photogrammetry only, have you tried to perform surveys of an accessible terrain with GCPs for error assessment? This can be done anywhere, not necessarily in Greenland.

Specific comments

- What about giving the uncertainty in pixel rather than in meter (here you have sub-pixel accuracy)? If you were flying twice higher with twice more spaced lines, would you get the same sub-pixel accuracy (but the pixel would be twice bigger)? If yes, your result given in pixel would be more general. Of course, this only if the accuracy scales with the resolution (or equivalently the flight altitude). Additionally, you do not provide any estimate of the inaccuracy of Emlid Reach of the camera location (about 1 cm horizontally and vertically from my experience, not millimeter). Does the final inaccuracy of the DEM and orthophoto partly result from this small-but-existing error of the camera location or other uncertainties in the SfM-MVS (lens calibration, ...)? If both, can you quantity the contribution of each? This is related to my previous point: if the small but existing inaccuracy of the position given by Emlid Reach receiver plays no role (because it is overridden by other sources of errors), then the final accuracy of the SfM might broadly scales with the resolution (GSD). In that case, I think the result inaccuracy is better given in pixel.
- It would be good to add non-glaciological references in the discussion, e.g. studies that assess the accuracy of PPK UAV SfM photogrammetry. Of course, the results always depend on many settings that depend on the UAV equipment and type of mission flown (altitude, overlap, ...). Yet, numbers of recently release commercial products (Sensefly, Wingtra, ...) equipped with similar technology offer sub-pixel accuracy. How does your result uncertainty compare with other non-glaciological studies applying SfM UAV photogrammetry and GNSS-AT?

- It would be interesting to deepen the gain of performing multi-level flights and quantify the reduction of the bowling effect (l 22-26 p 4), e.g. by performing the SfM photogrammetry twice i) once with one flight level ii) once with two levels, and differentiate the 2 DEMs.
- I find the calving event you captured (l 26-30 p 8) very interesting, especially because you get a clear discontinuity in the velocity field indicating that a large iceberg is about to collapse. There is interesting material for discussion (influence of the plume, can this crevasses be propagating by hydro-fracturing, or similarity with the proceeding reported in (Jouvet, 2017, TC)?). However, your text is mostly factual, and does not discuss at all what processes might be responsible for these observations. Discussing these observations (in the discussion) and making a parallel with similar ones reported in the literature would be a true added-value.
- It would be more logical to move section 4.3 (Future direction) after the conclusion.

Minor comments

l 15 p 2 "0.09 m per m^{-1}" should be "0.09 m per m"?

l 35 p 2 "Here, we show that ... is used to geolocate imagery acquired over a large Greenlandic outlet glacier with a fixed-wing UAV." You could keep this statement more general (removing "Greenlandic outlet" and "fixed-wing") as your method is inherent to Greenland and type of UAV.

l 8 p 4 1 Kg looks overestimated for a Sonny alpha 6000 unless the lens is really heavy?

l 8 p 4 1500 $ sounds quiet low (my owns cost more than twice when summing the costs of all components). Does it include only the cost of the frame only? and anything else you need to fly it (RC, Second Elmid Reach, ...).

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l 14 p 4 "Artupilot's Mission Panner": I would remove "Artupilot's"

l 18 p 4 GSD of $\sim$ 11 cm

l 18 p 4 "Out camera" should be "Our camera"

l 20 p 4 Did you process raw images in PhotoScan? or JPEG. I understand you processed raw otherwise why recording them in raw? If not, please, clarify.

l 10-12 p 5 "... equivalent to 2-3 mm" I'm not sure to understand these numbers. Do they refer to measure of the carrier phase? the accuracy? Maybe this sentence could be a bit more explained for novices in GNSS systems.

l 15-20 p 6 The unnecessary usage of RTK can be summarized more efficiently in a single sentence: "Whilst the Emlid Reach GNSS receiver is capable of RTK, we used instead PPK positioning for simplicity as the RTK brings additional technical constrains and is not more accurate than PPK"

l 9 p 7 DEMs always have coarser resolution than ortho-images after being produced by SfM-MSV. If you have GSD of 11 cm, 15 cm is logical resolution to output orthoimage. However, is 20 cm optimal for the DEM? Do you really get that resolution?

Section 2.5 As said before, I think it is dangerous to subtract an unknown error on the feature-tracking to s_{RMSE}. I'd simply use s_{RMSE} as an upper bound, mentioning that this estimate might be sub-optimal as it might contain a contribution external to SfM.

l 16 p 8 I can see crevasses on Fig 4b, but I can not really observe crevasse opening (i.e. positive strain rate) on this figure. Either reconsider the figure or rephrase.

l 20 p 8 Fig. 3c, 4a and 5

l 22 p 8 "Our method is sensitive to small changes occurring at the calving front": I don't understand this sentence. Is the method or the calving front sensitive to small change?

4, caption A **large-scale** calving event occurs between ... of July.

l 14 p 11 "track displacement of <1m" would be better given in pixel.

l 4-6 p 13 This sentence seems redundant with one sentence of the section before. Maybe this is a way to avoid this redundancy.

9, caption Specify along what profile these velocity fields were taken.

13-15 p 14 It might be good to add that it worked as water of supra-glacial lake was clear/transparent. Of course, this would not work with turbid water (unlikely to be met on supra-glacial lake).

16-19 p 15 "radial error", radial with respect to what? the lens? "can be reduced significantly with the introduction of accurate aerial georeferencing (James et al., 2017)" ⇒ "can be reduced significantly with the introduction of GNSS-AT (James et al., 2017)" if this is the case, better use always the same terminology for clarity.

l 22 p 15 We actually had more than 2 GCPs on each side, but instead about 10 GCPs on each side, plus a few moving with the ice.

Fig 2b is too small, would require either more explanations or can be removed as well as the chain of GPS is sufficiently well explained in the text.

10, caption split last word 'lakebed'

18-20 p 15 It would be interesting to quantify the reduction of the bowing effect.

l 15 p 16 high-resolution **in-situ** GNSS measurements

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l 5 p 17 "had negated cross-correlation 10" ???

l 7 p 17 velocity variation due to lake drainage or precipitation events are not necessarily expected right under the suprglacial lake, but could occur far away (dependent on the un/efficiency of the subglacial hydrological system). I'm wondering whether there is a confusion between 2 different things: i) mapping of supraglacial lake (which might be useful to track possible drainage) ii) consequence of lake drainage on the ice velocity. But the two are most likely not to be observed in the same region.

l 21 p 17 Is full DSO geolocation already available at affordable price, or very expensive, or still in research development? What gain would be expected using full DSO (compared to GNSS-AT)? Could you add this information?

32-33 p 17 "Firstly, the integration of these systems allows for the realisation of a truly low-cost, GCP-free UAV-MVS workflow for glaciological applications." This sentence looks unnecessary to me (and even somewhat harming your work). If I understand well, the gain of using dual-frequency GNSS receiver (instead of single) is to obtain **absolutely** accurate locations, and not being constrained by the 10 km max distance separation between rover rand base? Therefore, this would relax the constrain of the chain GNSS receiver (to determine the absolute position of the Emlid Reach base station) you implemented for getting accurate absolute georeferencing. I don't think you expect an improved relative geo-location with dual-frequency receiver, and then a reduced uncertainty?

tion, Fig 3 'seperation' should be 'separation'

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