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Earth Surface Dynamics Discussions

Interactive comment on "Evaluating the Potential of PPK Direct Georeferencing for UAV-SfM Photogrammetry and Precise Topographic Mapping" *by* He Zhang et al.

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General comments: The paper presents analyses of topographic data acquired by UAV and SfM-MVS photogrammetry using a PPK-GNSS direct georeferencing approach. This is a technique with broad relevance to a wide range of disciplines because the method will become increasingly widespread. Nevertheless, novel findings within the work are difficult to identify clearly and I haven't found the methods section sufficiently detailed to fully understand what has been done. The contribution of the work would be much clearer if existing similar work was evaluated more critically to provide a detailed context, and the aims and outcomes more concisely defined. Drawing more

deeply on published work should allow statements of well-established principles (such as "camera properties . . . have an impact on the accuracy") to be removed from the key sections such as abstract, discussion and conclusions so that the new findings can be more clearly communicated. The work is of interest but insufficiently described and, currently, the paper is somewhat challenging to assimilate. Overall, my suggestions below are aimed at highlighting the most transferrable new results from the work, by downplaying areas that have been previously covered and extending discussion to explore the underlying concepts further.

Specific comments: With the paper focussed on PPK direct georeferencing for UAV surveys, the introduction would be well served by focussing on this. With UAV-SfM approaches not being so new, substantial regions of text (e.g. up to P 3), which introduce the broader aspects and uses of UAV photogrammetry could be condensed into a few sentences or a single paragraph. The introduction would be strengthened by incorporating Table A1 into the main text and critically evaluating the progress of PPK-controlled UAV surveys so far. Consideration of established use of this approach for crewed aircraft could be covered briefly. Inclusion of the recent PPP work by Grayson et al. (2018; DOI: 10.1111/phor.12259) – and references included within it – will also strengthen this section.

One aspect of the work is an exploration of predictors for survey repeatability. The rationale for some of the selections could be strengthened here, and the utility of tie point density (which is shown to explain <50% of the variance for one camera) more critically considered. How useful is this, given that the analysis only appears to work for one camera and requires deployment of GCPs to determine the relationship? The number of tie points retained per image is usually a software setting that can be varied. Consequently, any parameterisation of repeatability would be software and UAV system dependent. Furthermore, other more important parameters are not considered. Within the bundle adjustment, measurement precision for a tie point is related to the number and angles of observations – how do these vary? The authors cite James et

al. (2017) who show how point coordinate precision varies spatially and can be linked directly to these photogrammetric factors and other georeferencing factors within the adjustment. Consequently, maps of 3D point precision can be determined without any GCPs. The work here would be strengthened by discussing the authors 'spatialised error' approach in context with the 3D precision maps of James et al. (2017). The authors could also consider the findings of Mosbrucker et al (2017; DOI: 10.1002/esp.4066) within the discussion (or introduction).

Comparison of results from different cameras (S4.2; particularly the last paragraph) dominantly reflects established relationships between camera/flight parameters and conventional survey design principles. In my view, this material should form the rationale behind the survey design, and be given within the introduction or methods sections. Placing this within the discussion detracts from the newer aspects of the work (the PPK). Throughout, when discussing results from different cameras, I suggest that a dimensionless approach based on pixels (or ground sampling distance) is also used. This could be used to assess the quality of the photogrammetric networks achieved, and to generate insight – again, see previous work, including that of Mosbrucker et al. (2017). I would actually see a much more detailed assessment of the PPK performance as providing the most useful (i.e. transferrable) insight.

I have annotated the ms with areas where I have been unclear about the methods. Unfortunately, this means I may not have fully understood all the aspects of the results. It would be good to see some more details to support the photogrammetric processing though – e.g. what were the rms image residual magnitudes? Did they vary image-to-image in any way that would help understanding of the repeatability? The clear image overlap outlines shown in Fig 6a suggest that camera positions may have been over-constrained in at least one survey (e.g. see a similar effect in Fig 1 of James et al. 2017a - http://dx.doi.org/10.1016/j.geomorph.2016.11.021, resulting from over-weighting the GCPs in that case). Details of the a priori assigned camera position precisions used in the adjustments need to be provided and, given that they are often

optimistically estimated, the effects of diluting the estimates could be explored.

Technical corrections, typing errors, etc.: Most of the detailed suggestions have been annotated on the manuscript, with a few additional points below. The text does contain typos and errors in English that I have not had the opportunity to individually correct.

Fig 2 – Are photographs of the GPS system etc. really needed (c, d)? Much more valuable would be examples of the imagery processed (i.e. the underpinning data on which the work relies), with enlarged excerpts to illustrate the image quality and show how the GCPs have been imaged.

All figures need to be checked for readability of the text labels. In particular, all map figures have scale and other labels which are far too small to be readable, and font sizes vary substantially across the figures. Labels must be readable: more consistent font sizes, of at least 9 point, will help.

Fig 4 Rephrase 'error of detection' for clarity.

DoDs – represent image overlaps etc for the DSLR but not for the action camera.

Fig 7b LoD before/after lines indistinguishable – needs more careful visualisation.

Fig. 8 Colour scale given to four decimal places could be tidied up.

Table 1 The caption mentions three flights but I can only see data from two (i.e. one with DLSR, one with action camera). Which flights are these? Where are the results from the others?

Table 2 The link to Table 1 is unclear. 05 April DLSR results are the same as in Table 1, but no similar repetition for Action camera. Maybe I haven't understood what Table 1 is?

Fig A1. I am not convinced how useful these visualisations are – it is difficult to extract much from them. I would suggest that a more informative plot would be as an XY map of discrepancy vectors, with symbols to indicate the check point position and Z-

discrepancy showed by symbol colour. This way, any spatial systematics (which would be concealed in the current plots) would be clear.

Table A2 This information is critical to a reader's understanding; it needs to be early in the main manuscript, not in the appendices. Why were some flights repeated? Where are these repeated data, and what did they show?

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2019-2/esurf-2019-2-RC1-supplement.pdf

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