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Interactive comment on "Observing river stages using unmanned aerial vehicles" *by* T. Niedzielski et al.

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[REVIEWER #2] Firstly, this paper is at times difficult to understand due to the poor use of English language. I have made a few suggestions for improvement on the first page, however I think the text should be professionally edited before publication.

[AUTHORS' RESPONSE] We accept the criticism and thank the Reviewer #2 for spotting linguistic problems. We are ready to correct the text according to the marked suggestions and edit the manuscript before publication.

[REVIEWER #2] I think the use of aerial photos taken from UAVs to observe river stage could be a useful practice. I also think that this paper is not doing the idea sufficient justice. The aim/tested hypothesis is too basic to provide results of any impact. The tested hypothesis 'meaningful changes in river stages are observable using the UAV'

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is most likely going to be accepted as long as your data is of sufficiently high resolution and the channel morphology such that an increase in stage results in a larger water surface area. The first of these two requirements is solely dependent on the combination of UAV flying height and camera resolution, so can be adjusted until sufficient ground resolution has been achieved. The latter requirement is of course dependent on the channel under observation. In most cases some increase will be visible with increasing stage and certainly in channels with less steep banks surface area will vary quite strongly. In any case this variability means that results found in this study are not necessarily transferable to other river systems with different channel cross sections.

[AUTHORS' RESPONSE] We thank the Reviewer #2 for finding our concept of reconstructing river stages using UAV-taken photographs a useful practice. We accept the criticism, but we believe that – after implementing the suggestions kindly offered by two Referees – the revised manuscript is likely to do the above-mentioned idea sufficient justice. We hereby response to two key comments (height/resolution issues and channel properties) in the itemized points below.

– Indeed, flight altitude (and the resulting ground resolution) influences the water surface area observations. Our flight characteristics were kept uniform over the five observational campaigns, hence the resolution is also stable over the exercises. The similar comment has been also offered by the Reviewer #1. We double checked the flight logs and confirmed the comparability of UAV height parameters. The statistics calculated from the log data are juxtaposed in the following table (also included in the response to comments raised by the Reviewer #1).

A	B	C	D	E	F	G	H	I
27/11/2012 (1)	109.0	296.4	113.6	109.2	410.0	405.6	451.0	447.5
27/11/2012 (2)	109.0	296.5	112.4	109.1	408.9	405.7	453.5	447.9
13/05/2013 (1)	109.0	297.7	116.0	108.9	413.7	406.6	456.2	449.9

13/05/2013 (2)	109.0	299.8	118.1	109.0	417.9	408.8	458.6	450.4
21/08/2013 (1)	109.0	301.1	117.0	108.2	418.2	409.3	458.8	450.6
21/08/2013 (2)	109.0	295.2	115.0	108.9	410.2	404.1	450.0	444.8
27/09/2013 (1)	109.0	294.5	114.8	108.8	409.3	403.4	452.7	455.5
27/09/2013 (2)	109.0	295.5	114.5	109.1	410.0	404.6	454.3	446.4
02/06/2014 (1)	109.0	305.3	115.3	108.3	420.7	413.6	452.7	461.0
02/06/2014 (2)	109.0	294.3	114.3	108.9	408.6	403.1	451.1	445.4

A – Date and number of flight

B – Planned height above takeoff location [m]

C – Takeoff altitude [m a.s.l.]

D – Maximum height [m]

E – Mean height [m]

F – Maximum altitude [m a.s.l.]

G – Mean altitude [m a.s.l.]

H – Maximum altitude WGS84 [m]

I – Mean altitude WGS84 [m]

Since an extensive data set of UAV flight data is available, we are ready to include a new table that juxtaposes the characteristics in question. We believe that – given the fact that altitudes (and hence resolutions) were kept comparable – the properties of flight parameters did not undermine our inference.

– We are grateful the Reviewer #2 for mentioning the influence of channel morphology, mainly the slope of banks, on the observation of water surface area with the UAV. We agree that such a relationship exists. We also entirely accept the comment that the results, prepared for a specific river in the SW Poland, are not transferable to other rivers with different cross-sectional parameters. In fact this has been already pointed out in the context of the relationship between water surface areas and river stages by Smith (1997) who argues that "[. . .] Until additional empirical rating curves relating inundation area to ground measurements of stage or discharge are made, it is difficult to assess their potential for extrapolation to other rivers of similar morphology. However, it seems likely that such curves will vary significantly between rivers and therefore must be constructed for each site. [. . .]" [Smith L.C, 1997. Satellite remote sensing of river inundation area, stage, and discharge: a review. Hydrological Processes 11, 1427–1439]. However, our approach is centred on a statistical analysis of water surface areas, not river stages themselves. In fact, we quantitatively infer on statistically meaningful changes in water surface area (this is a key part of our procedure) and only qualitatively, through the existence of a relationship between water surface areas and river stages published by Usachev (1983) [Usachev V.F., 1983. Evaluation of food plain inundations by remote sensing methods. In: Proceedings of the Hamburg Symposium, IAHS Publ. 145, 475–482], extrapolate our results into changes in river stages. We believe that our quantitative approach (recall that this concerns seeking changes in water surface areas in the orthophotomaps produced from the UAV-taken photographs) forms a general method that – under several conditions clearly identified in the manuscript – may be applied in other regions. However, the use of the approach to infer on river stages should be made with caution, since such an extrapolation requires a knowledge about the relationship between water surface areas and river stages (and the characteristics of this relation are vulnerable to sites-specific river morphology, especially bank slopes). We think that such constraints should be included in the revised manuscript, in particular in the discussion or conclusions.

[REVIEWER #2] The justification of the absence of GCPs is not fully clear to me. Regardless I think it is impossible to accurately draw polygons at the same location in images taken at different points in time, if these images cannot be georeferenced through a set of gcps visible in each image. You suggest each image is linked to a Lidar dataset, from which exact polygon locations may have been mapped on to the images, but again the explanation of this procedure is not clearly described. Line 28/29 p. 6 mentions that 'the procedure to determine the edges of water extent should be well-documented to enable its repetition', but there is no further mention of this procedure.

[AUTHORS' RESPONSE] Yes, we are aware of possible problems that may be associated with measuring the area of polygons that are generated on a basis of orthophotomaps produced without GCPs. We also know that a procedure for delineating boundaries of water surface area needs to be clarified. The similar remarks have been also offered by the Reviewer #1, and the responses below use the arguments which we raised when replying to the comments provided by the Referee #1. In the two itemized points, we focus on: (1) area computation when GCPs are unavailable, (2) delineating edges of water surface areas.

– In line 5 on page 11 of our HESS Discussion Paper we argue that a presence of a potential shift between two spatial data sets does not cause meaningful changes in area of the considered objects. The arguments that support this statement can be found in a recent paper by Mesas-Carrascosa et al. (2014) [Mesas-Carrascosa F.J., Notario-García M.D., Meroño de Larriva J.E., Sánchez de la Orden M., García-Ferrer A., 2014. Validation of measurements of land plot area using UAV imagery. International Journal of Applied Earth Observation and Geoinformation 33, 270–279]. These authors argue that "[. . .] Other shortcomings include the lack of vertical adjustment of the aerial camera and the unknown or variable interior orientation of the camera. These factors affect point position accuracy but do not necessarily decrease the accuracy of area measurements. [. . .]". Having justified a stability of area measurements in case of smaller point position accuracy, i.e. also in case of shift of orthophotomaps produced without GCPs, we hereby describe the spline-based procedure that fixes all orthophotomaps to a single LIDAR data. We identified characteristic features in the LIDAR DTM which were evenly distributed and possible to identify in the orthophotomap. These features comprise: crossings of bounds, crossings of drainage ditches, and centres of bridges or passages (crossings of streams and roads). More than 10 points were used to perform georeferencing, as the spline method requires. A spline function allowed us to precisely georeference the control points (i.e. the aforementioned mutual features) and transform raster dataset with continuity and smoothness, such as the rubber sheeting method.

– In lines 27-31 on page 6 and the subsequent part of Subsection 2.2 in the HESS Discussion Paper we listed three criteria labelled as (1), (2) and (3). While the latter two issues are associated with GIS methods (the same scale should be kept when carrying out a vectorization procedure and a cartographic projection as well as reference system should be unified before measuring areas), the first one is strongly related to environmental factors, mainly vegetation. This first statement has been explicitly highlighted by the Reviewer #2 as an element that needs to be clarified. Mapping vegetation with UAVs becomes popular as a recent paper by Husson et al. (2014) shows [Husson E., Hagner O., Ecke F., 2014. Unmanned aircraft systems help to map aquatic vegetation. Applied Vegetation Science 17, 567–577]. These authors focus on delineating edges between water and non-submerged aquatic as well as riparian species. They write that "[. . .] In practise delineation was done by hand on paper printouts [. . .]" and "Vegetation mapping, i.e. digitizing the UAS orthoimages, was performed manually by a human interpreter in a GIS using ArcGIS software". Although we concentrate on a fluvial environment, the idea behind our manual expert-based vectorization remains similar to what Husson et al. (2014) propose. It is worth noting that our vectorization was practically carried out by two experts (GIS specialist + fluvial geomorphologist). Given this introduction, we unequivocally reply that the procedure met the assumed criteria (this is attained by the expert-based vectorization). We also believe that the accuracy of the produced water surface area is acceptable. However, it was our intention to include Fig. 4 and Fig. 5 which help the reader to identify potential sources of errors.

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[REVIEWER #2] A further issue with the data is the independence of the observations. You did a test to test for the independence your samples, but I'm not sure this is appropriate (the test is in any case meant for time series not spatial dependence). Since the polygons were taken along the same river at relatively short distance, the measurements are to some extent going to be affected by spatial autocorrelation. The difference measured at the one location is going to be very similar to that measured at a nearby location. A t-test requires independence of observations, a requirement which is therefore not met.

[AUTHORS' RESPONSE] All assumptions of the Student's t-test have been checked using: the Ljung-Box test (independence), the Shapiro-Wilk test (normality), the D'Agostino test (symmetry as a feature of the Gaussian distribution), the Anscombe-Glynn test (mesokurticity as a feature of the Gaussian distribution). The tests, performed with the significance level of 0.01, suggest that each sample (we analyze 5 samples corresponding to five dates) is "internally" independent is normally distributed. In particular, the Ljung-Box test provides arguments for independence since p-values are equal to 0.059, 0.092, 0.444, 0.713, 0.828 (Tab. 3 in our HESS Discussion Paper), for five consecutive dates. Hence, from a definition of statistical independency we infer that they cannot reveal autocorrelation. We would like to take this opportunity and put an emphasis on spatial independence which has not been investigated in our work. In addition, variances of paired data sets have been found to be similar (Tab. 4 in the HESS Discussion Paper).

[REVIEWER #2] As far as I'm aware it is not appropriate to do a series of t-tests on the data, as you increase your probability of making a Type I error. Instead an ANOVA style test should be done, with post-hoc tests to identify differences in means.

[AUTHORS' RESPONSE] Indeed, the ANOVA test is a generalization of the t-test to more than two groups. However, although we process five samples our intention is to allow pairwise comparisons. In other words, our approach is targeted at solving a simple operational problem: we have two sets of UAV-acquired observations carried out on two different dates, and would like to know if water surface area increased in comparison to the preceding observation. In our manuscript we simply have 5 observations which allows us to carry out many tests to make the inference more evident. However, pairwise comparison is a fundamental feature of our procedure.

[REVIEWER #2] I think the study area description is too extensive, as a lot of the information relates to quite an extensive area while the study only focusses on a short section of a small stream in that area. Individual measurement site descriptions also take up a large part of the paper text, while the detail given is not overly relevant for the analysis.

[AUTHORS' RESPONSE] We entirely agree with the Reviewer #2 that the description of the study area is too detailed. In the revised manuscript, the information on study area will be significantly shortened and condensed so that the reader focuses on the method and its application.

[REVIEWER #2] Please also note the supplement to this comment: http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-49/hess-2016-49-RC2- supplement.pdf

[AUTHORS' RESPONSE] We are grateful to the Reviewer #2 for offering us the remarks in the supplement. They will be conscientiously considered when we are allowed to work towards a revised manuscript.

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