

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

Anonymous Referee #2

Received and published: 27 February 2016

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.

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’ 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

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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.

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.

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.

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.

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.

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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>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-49, 2016.