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Interactive comment on "Technical note: Stage and water width measurement of a mountain stream using a simple time-lapse camera" *by* Pauline Leduc et al.

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This paper builds on previous work to further demonstrate the value of an inexpensive, portable and easily-sited time-lapse camera for making frequent (potentially continuous) measurements of stage and water width. The system is applied to a mountain stream with an irregular bed, where other technologies might be difficult to deploy. Particular contributions of this paper include: (a) the validation of an image-based system against a pressure-based system for measuring stage; (b) automation of detection of the water edge in the images; (c) observations of the detailed relationship between stage as measured on the rock surface and as measured by pressure; (d) a demon-

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stration of the successful deployment of this novel type of system in a new site.

The work is closely related to that reported in Young et al. (2015). (I am the first author of that paper.) Our paper lacked an independent measure for comparison, and this paper addresses this problem and provides interesting (and reassuring) results. The image-based approach has thus been validated using a separate measurement, which we were unable to do. The authors take the data further, observing some phenomena such as differences depending on whether the flow is increasing or decreasing, which merit further investigation in the future.

I have two relatively minor criticisms. The first is that the significance of automation of water edge detection is a little overstated; the second is that stage and width measurements are treated as separate problems, rather than integrated into a single model.

The approach depends on detection of the water/rock boundary in the images. It is important to automate this if large amounts of data are to be processed (and one can imagine a study in which many cameras are deployed over an extended period, all collecting many images per hour). However, Leduc et al.'s method depends on having a large rock surface such that the water surface lies across it for all stages of interest, and where the water/rock boundary exhibits the highest contrast (or second-highest in certain lighting conditions). Given such a situation, our method could also have been fully automated - we used manual intervention to select the correct edge in a much more cluttered image, using a selection of smaller rocks. Leduc et al.'s images show strong contrast between the water and the rock (the water is very bright in the images that appear in the paper) and it is not clear that their method will work well in situations where the water is flowing more smoothly, or the river channel is more complex. Their image analysis algorithm has some ad hoc elements which may not generalise well.

It would be worth noting that selection of the maximum grey-level gradient along a vertical profile is almost the same as Canny edge detection in a narrow strip - the only real difference is that Canny uses some Gaussian smoothing to reduce noise. Thus

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both this paper and ours use (not surprisingly) rather similar image analysis techniques.

I think it would be useful to include some more discussion of the tradeoff between automatic analysis and the need for careful site selection, and also of the limitations of using a single surface for the stage measurement.

Our approach of combining measurements on multiple surfaces (both near-vertical and near-horizontal) allows for a larger range of stages to be covered and for estimates to be made of the consistency and statistical reliability of the measurements, as well as providing a rough estimate of the channel geometry. Leduc et al.'s approach relies on a more carefully chosen camera position and a suitable large rock, which allows automated image processing and a simpler analysis of the image measurements. In my view, the merits of the two approaches are complementary, and future work should draw on both.

The water width measurement is interesting, but again I am concerned about how well this would generalise to other settings. A little more needs to be said about how the nonlinear fit in Eq 2 was arrived at - is this a purely empirical equation or is there a model behind it? The final paragraph of section 3.7 does, however, provide a good summary of the issues.

Section 3.9 is novel and interesting, going significantly beyond our work, and reveals how the technique can give results that would not be available in any other way.

I feel that section 4 of the paper raises interesting questions, and I agree strongly with the conclusions reached in section 5.

Overall, I think this is a very strong contribution and I am happy to recommend publication.

Technical comments:

p 1, l 17: Extra parentheses in reference (cf l 21) p 2, l 6: We didn’t know the channel geometry - we assumed a V-shaped section and estimated the slopes from the data.

p 2, l 12: boulder -> bouldery

p 2, l 24: too many ’oblique’s

p 4, Fig 3, and subsequent figures: can the figures be made bigger? When the paper is printed, the details are hard to see. This is particularly the case for Figs 9, 10 and 11, and the central element of Fig 1.

p 6, l 1-14: This section could be a little and more explicit. I didn’t understand how the re-sizing operation worked, or what its basis is.

Table 1: the values for SDs assume, I think, a brightness range of 0-255. This ought to be stated explicitly (it’s common, but not universal).

p 10, l 2: Should "board stage" be "stage board" for the calibration? If not, I don’t understand what is meant.

p 14, l 5: are much higher -> is much higher

p 14, l 14: First bracket backward

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