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.

Anonymous Referee #3

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This paper seeks to define a methodology for retrieving pertinent river variables from time lapse imagery. At its core, this methodology relies on site selection to be effective: the authors were quite clever to notice different image regions and exploit these for filtering. This is a great idea that could be adopted to other studies. The authors also rely heavily on a 'nearly vertical' boulder for stage and width measurements. This is also rather clever, but acts as a double edged sword: What if no boulder can be found with a flat face orthogonal to the camera plane? The authors need to elaborate, at paragraph length or longer, the considerations of site selection for this method. Is it serendipity that the image regions and boulders emerged, or were they chosen? In addition, the authors MUST quantify their method much more accurately- descriptions

of quantities are too vague and not replicable. If I want to apply this method and need to find classification regions, what distributions of intensity should I seek? How should I define my different image regions, or will this method only work at this one site with this unique assemblage of morphologies? Therein lies my main concern- the clever filtering techniques that are an improvement on Gleason et al may not work everywhere. The authors need to more clearly define their threshold values, and I think distributions of intensity in each of the eight regions should be shown at multiple flow levels to prove their utility beyond taking the authors' word for it. Precise values are given in Table 1, but not in the text, and no distributions are shown. The reported wave issues are also troubling for the site selection issues discussed above. How much of an issue would these be in a different site? The photogrammetry here is effective, and I do not quibble with the empirical functions for stage and width. However, these do require calibration that makes this procedure time consuming. Could the authors comment on the amount of time needed to make measurements for sufficient calibration? i.e. could I set up 50 of these stations, all well-calibrated, in a summer in remote terrain? This is an important discussion left out of the manuscript that should be added, as the authors propose that this method should be adopted for such streams. The use of precise rotation matrix measurements and classic photogrammetry would obviate the need for this calibration, as would establishing several cameras in stereo. Overall, I recommend this paper for publication, provided the authors write several new sections detailing the concerns discussed above- all of which relate to the role of site selection in this method. Some minor comments: The English writing is sloppy at times- needless plurals, .jgp instead of .jpg, backward brackets, etc. This needs to be amended and made more professional. Page 4, line 13: What is this standard deviation? It is insufficient to say 'very low.' Also, this is a filter, correct? It should be identified as such. Page 4 -> page 5: This writing is redundant- section 3.1 should be eliminated and combined into section 3.2 Page 5, line 15+: again, what is the threshold of SD? Also, this writing is unclear- words like 'high' and 'drops' and 'smoother' are used, which are imprecise. Since this paper proposes an algorithm for installation and monitoring, it must be specific so as

to be reproduced.

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