



EGUsphere, referee comment RC4
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Comment on egusphere-2022-735

Anonymous Referee #3

Referee comment on "Adaptively monitoring streamflow using a stereo computer vision system" by Nicholas Reece Hutley et al., EGU sphere,
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General

This manuscript introduces a new stereo computer vision stream gauging (CVSG) system for monitoring streamflow in rivers. Compared to existing systems, the added value of such a contactless streamgauge measuring both water level and surface flow velocity comes from the camera calibration without ground control points and the adaptive estimation of the rating curve. While the originality of some features of the system is real, several important methods are not described in enough detail so they could be understood and reproduced, which in my opinion cannot be accepted in a scientific publication. Even with this lack of information, some concerns arise about some methods involved, especially the velocity distribution model, and the rating curve model and estimation.

Specific points

L47-50: the text here suggests that rating curves could not be established with acceptable uncertainty in natural waterways without artificial controls. This is not true, as many hydrometric stations demonstrate.

L61: you should also mention surface velocity radars as a technology affordable for contactless streamflow monitoring stations (e.g. Khan et al. 2021, Uncertainty in Remote Sensing of Streams using Noncontact Radars, J of Hydrology). This s an efficient alternative to image-based systems that should be compared and discussed.

L71: Other commercial image-based stations exist, e.g. the product sold by Tenevia, France.

L87: 'initial surveying and calibration of new sites' is not a strong limitation for monitoring

stations, as it is a limited additional effort compared to the installation of the system. Autocalibration would be more decisive for portable streamgauging systems (eg smartphone applications) for which surveying is a problem.

L118: 40 m is a limited range for stage measurements in medium to large rivers. Then L155, a range of 2 to 10 m is mentioned, which is very limited. What range is the right one?

Also, using an IMU may be too expensive for just the initial survey of a station with a fixed angle and position... what is the additional cost and weight/size of an IMU?

Fig 1 is a good summary of the system but there is not enough information in the text (L179-193 especially) to understand the methods in a reproducible way. At least the principles should be explained and underlying equations provided so that the manuscript can be published as a research paper.

Eq. 1: what is the physical justification (or reference) of this velocity distribution model? Why not using existing models, eg the Froude-based models, cf. Fulford and Sauer (1986)? This exponential model does not look very physical.

L220: are the alpha values in Hauet et al. (2018) local or cross-sectional averages? Large differences between local and average values have been reported by Welber et al. (2016, WRR) for instance. How do your values compare with their empirical values? And with theoretical models, cf. eg LeCoz et al. (2010)?

L236: again, equations are needed here, but the sentence suggests that a single power equation $Q=a(H-b)^c$ is used for modelling the rating curve. At most streamgages, a single segment is not enough to build a rating curve due to multiple controls. You should review and use more relevant rating curve models and estimation methods, in particular refer to the comparison of 7 methods by Kiang et al. (2018, WRR) and explain how your method compares with the methods recently proposed by several research groups, some of them being publicly available.

L350: was the system placed too low due to its limited sensing range? This is a very problematic limitation, in practice.

L370-378: this paragraph belongs to conclusions, not to results. Please move it to Conclusions or remove.

Fig. 3b: STIV and DischargeLab velocity measurements are much higher than reference (ADCP) velocities (and than CVSG velocities) in the irrigation canal case. What is the cause for such large, unusual errors? L423: what are the HydroSTIV 'ambiguous results'? Should manual determinations of the STI slopes be used, as often done in practice? Is there some operator effect? This should be clarified.

This case also shows that the velocity distribution model is inaccurate for such complex case. Then, what is the value of fitting such a model instead of using the high-resolution velocity measurements? Why not using the model only for interpolating missing data in unmeasured areas? The CVSG error with model fit (+55% in table 3) is clearly unacceptable and calls for not using such a model fit.

L461-464: this argument is weak because rating shifts may have occurred during such a long period of time. Also, the huge scatter in Fig 4 may be due to the same cause (rating shifts).

L507-508 and L520: my conclusion is that image-based stage measurement is a failure. Modern contactless gauges such as radar gauges are a much better option in terms of cost and accuracy. And they also work at night and in the fog, rain, etc.

L548-550: measurement improvement through real-time learning seems to hide some error compensation, since stage measurements are affected by substantial errors. This is a problem, as a wrong rating curve is certainly established to cope with stage errors specific to the CVSG system. Such biased rating curve could not be used with conventional, accurate stage records...

L587-589: acquiring measurements much faster than conventional streamgauging techniques is indeed a critical advantage of such image-based (or radar-based) velocimetry monitoring systems. However, the advantage is not specific to the CVSG system proposed here.

L592-594: this argument can be discussed depending on the rating model assumed. Unlike the vague description of the rating method before, here it is suggested that several (piecewise?) power segments are used to compute the rating curve... Details and equations are definitely needed for clarification. And the 'smoother fit of the gauging station rating curve' is not necessarily less accurate than a more flexible rating curve model because it usually rely on physically-based considerations, ensuring a better extrapolation for high flows, for instance.

More generally, it is a pity that no uncertainty intervals around the rating curve estimates are presented, whereas methods are now available for this (cf. Kiang et al. 2018 and the associated methods). Accounting for the variable uncertainty of discharge measurements

is especially important for surface velocity methods like the CVSG.

The Section 4 'Discussion' needs to be more formally organized around precise questions to be more precisely related to the methods and results of the paper. Also, more references should be used, in particular on surface velocity radar stations and index velocity methods (as an alternative to image-based streamflow monitoring stations), rating curve estimation methods (including the modern data assimilation methods already mentioned, cf. Kiang et al.), other image-based monitoring solutions (e.g. Tenevia video stations, and Stumpf et al. (2016, WRR) is a needed reference on stereo cameras L668).

The Section 5 'Conclusions' does not provide a real summary of the results, including success and failure of the attempts. It thus fails to present perspectives for improving or extending the system. The first sentence (L693) is highly questionable as the study does not demonstrate the 'successful development' of the system since at least some parts of the methods have failed or could not be tested, including the stage measurements, the velocity distribution model, the night measurements, etc.

Minor points

Abstract L21: 'error margins of 5-15%', what do you mean precisely? Is this the uncertainty at a given probability level? Or what?

L46: Doppler

L526: true dischargeS

L527: 'somewhat overestimated': this is vague, by how much?

Fig 6 caption: 'and gauge water levels', remove 'and'