

## ***Interactive comment on “Flow velocity and discharge measurement in rivers using terrestrial and UAV imagery” by A. Eltner et al.***

**Anonymous Referee #2**

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This article presents a procedure to extract surface flow velocities and flow discharge from images captured either with permanent cameras or with drones in natural water systems. The methodology includes i) vibration removal from captured images, ii) feature identification with the GFTT algorithm, iii) feature tracking and trajectory development with normalized cross-correlation, iv) trajectory filtering based on a set of predetermined rules, and v) velocity estimation. Flow discharge can be estimated by reconstructing the bathymetry with structure-from-motion techniques and utilizing a velocity coefficient to estimate depth-averaged velocity.

The manuscript does not represent a substantial contribution to scientific progress by introducing new concepts, ideas, methods or data. Most of the algorithms applied in the procedure are well known and share similarities with existing literature (see,

for instance, Perks et al., 2016, Cao et al., 2018 and Tauro et al., 2018). Flow discharge measurements have already been demonstrated from Unmanned Aerial Vehicles (UAVs), see, for instance, Detert et al., 2017. Finally, several tools exist for flow calculation (see, for instance, Fudaa-LSPIV and RIVeR by Patalano et al.).

However, I acknowledge that a limitation to the implementation of image-based measurements is the lack of user-friendly and widely shared toolboxes. In this vein, the manuscript addresses the relevant scientific issue of establishing a procedure that can guide the users from image acquisition to flow discharge calculation. In this respect, the manuscript may be appreciable to the HESS readership as a technical note, and provided the focus of the article is targeted on the presentation of the tool and on its performance. Regarding the scientific quality and validity of the applied methods, many details are missing and, in its current form, it is difficult to evaluate the scientific soundness of the work. Additional experimental and analytical justification and, sometimes, data would be mandatory to establish a novel procedure. Finally, the presentation of the work should be improved as well as several figures.

In the following, I report major comments.

1. An important flaw of the work is that the computational tool is barely presented and recalled to during the manuscript. Since the focus of the paper is the introduction of a new procedure, the work should clearly state the underlying assumptions of the algorithms, required data and expected outputs. Some of these points are only mentioned briefly in the supplementary material and they are not given the right visibility. For instance, I believe it should be made clear that the water level is an input to the procedure, as well as a decent number of ground control points. The sentence “the provided velocity tracking tool allows for a contact-less measurement of spatially distributed velocity fields and to estimate river discharge in previously ungauged and unmeasured regions” should, therefore, be properly edited. Another important point regards the limitations of the procedure with respect to required inputs. For instance, it looks like images need to capture river banks in order for image co-registration to be

effective. This is a remarkable limitation and it should be clearly stated for users and readers.

2. Since the Authors claim that a new procedure is being introduced, a motivation on the selection of the specific sites should be provided. If the sites mostly differ in the morphology of their river bed, the bathymetry of both of them should have been independently (that is, not with images) measured and considered as a benchmark for structure-from-motion results.

3. Details on the ADCP benchmark measurements are missing. For instance, it is not clear how surface flow velocities were extrapolated from a range of 14 cm near the water surface. Given the rather shallow depth of both streams, it is surprising the Authors did not try to reconstruct the full velocity profile with the ADCP. Wind effects are not mentioned as well as alternative possible sources of noise in the data.

4. The description of the optical experimental setup is also unclear. The orientation angles of the optical axes of the cameras are not provided. Also, in case of experiments on the Wesenitz, even if three terrestrial cameras are installed along the cross-section, none of them captures the entire width of the stream. Using diverse optical parameters for the cameras could have been interesting if results had been better discussed and referred to such settings.

5. Most of the presented algorithms share common traits with already published material. However, some of them introduce novel aspects whose accuracy is not adequately assessed in the manuscript. Was the co-registration tested elsewhere before? Was it tested in windy conditions, under different camera orientations/frequencies/resolutions? What about the feature search area and pose estimation? What are the parameters such procedure is sensitive to? Was it validated in diverse conditions? If the method was only tested in the two case studies reported in the paper, then how can this tool be regarded as a robust alternative to thoroughly tested and used ones?

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6. Some of the velocimetry phases require the definition of threshold values. It is not clear if they can be edited based on the specific case study. Even if this is possible, I believe the Authors should provide some guidance for the selection of appropriate values. For instance, what are nearest neighbor area dimensions that allow to find strong clusters of particles? Or which is a suitable number of particles? I believe such parameters are highly dependent on the specific experimental conditions, and automatic ways of computing them may be developed rather than asking for an intensive visual inspection of images by the users. Similarly, are search area dimensions pre-defined or inputs to the workflow? Introducing search area dimensions automatically poses constraints on the admissible frequencies and, therefore, flow velocities to be observed. In the track filtering, the criterion of the minimum number of frames across which the features have to be traceable also causes a constraint on measurable flow velocities and camera frequencies. Again the users should be aware of these implications and guided towards a sound selection.

7. The velocimetry procedure involved multiple filtering of particles and trajectories. This may be inefficient as compared to alternative approaches that perform the filtering only once. However, nothing is mentioned on the efficiency of the procedure. What are computational times related to image frequency and resolution? In several instances the Authors recommend to capture adequately long videos. Nonetheless, this can be time consuming and introduce additional variability due, for instance, to the occurrence of unevenly spaced tracers.

8. Transformation of trajectories to rasterized cells is not clear.

9. How was the velocity coefficient estimated? This is generally an approximate methodology that is not adequate in case of irregular sections. Since water level is an input to the procedure and the bathymetry of the stream reach is reconstructed, why weren't alternative approaches be considered and integrated for flow discharge computation?

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10. Figures should be improved. For instance, in Fig.1 panels a and b are misplaced. Also, it would be nice, for each case study, to overlap the field of view captured by each camera to facilitate velocity comparison (same difficulty in Fig.9). In Fig.3a, all points are colored, it is unclear what the Authors are referring to. In Figs. 3 and 4 it would be nice to see the influence of the various steps of the filtering. In Fig.7, points that are far from the center of images do not necessarily display higher standard deviation. This should be commented and motivated in the manuscript.

11. It would be nice to see the tracks that fall within 1 m from the ADCP measurements in a figure. In some cases the computation is done on a very different number of trajectories regardless of the cluster-based filtering. Were values in Table 3 weighed by the number of track counts?

12. Even if the manuscript is mostly well readable, several typos and sentences should be improved. Some units are wrong. The sentence at lines 4 to 6 on page 7 is unclear.

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