Reviewer 3

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.

Reply: *The authors thank the reviewer for their general feedback and comments about the real added value and originality offered by this work. We understand that some clarification was required within the manuscript in order for the reader to more clearly understand the procedure of the methods and study being presented, and have made changes in order to address areas of confusion or lacking in clarity. We believe that the concerns raised about some of the methods involved were resulting from this confusion that was evident in the specific points that were noted.*

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.

Reply: *We thank the reviewer for their perspective on these lines, but we do not agree that these lines make this suggestion, while the following lines do describe the usefulness of the development of discharge rating curves in natural waterways.*

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.

Reply: *The authors thank the reviewer for this valuable suggestion for improving the manuscript's provision of relevant contextual background information to the reader.* *As such, we have added a line and suggested citation to this paragraph at L61 strengthening the introduction.*

Modification from L61: "Therefore, non-contact and affordable solutions such as radar (Rahman Khan et al., 2021) or optical, offer the potential to overcome these challenges by measuring velocity and stage without in-situ sensors. Similar to one of the oldest manual methods to measure velocities in a waterway by measuring the displacements of surface floats over time, the passive optical measurement of surface velocities using relatively inexpensive camera systems has been an attractive approach to stream gauging (Dobriyal et al., 2017)."

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

Reply: *We thank the reviewer for raising the Tenevia example that we are aware of, however we do not preclude the existence of other commercial image-based stations in the text, and we do not feel* *it necessary or appropriate to list all image-based stations that have been developed (this is not a review paper). The authors did not find peer-reviewed research detailing the methods, application, or evaluation of the Tenevia product, and hence we have opted for mentioning and citing the details of another commercially available product as a referenced example.*

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.

***Reply:** The authors thank the reviewer for their comment, however the perspective of the 'initial surveying and calibration of new sites' being of limited effort compared to the installation of the system is not shared by the authors. The installation of the system presented is not a significant effort, and the necessary additional equipment, cost, expertise, errors, and time required for initial surveying and calibration is a strong motivation for this research and development. Furthermore, the stream gauging solution presented is a portable stream gauging system in its own right (as it can be moved between established sites similarly to existing smartphone applications mentioned by the reviewer).*

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?

***Reply:** We thank the reviewer for their comment, and note that this confusion was also evidenced in a previous reviewer's comment. The 40 m mentioned on L118 is relative to the position of the camera and referring to the system hardware limitation for estimating water levels using stereophotogrammetry requiring the water near edge to exist within the vision of the camera within a 40 m range from the camera's physical location, while the 10 m on L156 is describing the region of the water surface used for stereophotogrammetry water level estimation relative to the near bank interception with the water edge.*

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?

***Reply:** The authors thank the reviewer for their concern regarding the cost, weight, and size of an IMU, however we there is no additional expense and negligible weight/size required as we use the IMU embedded in the ZED 2/2i stereo camera (Stereolabs Inc., San Francisco, CA, USA). With the embedded IMU, we also note that, the CVSG system does not just use this hardware for initial surveying with fixed angle and position, but recalculates this orientation with reference to the previously sensed orientation, and measures the stability of the camera with each video recording analysed.*

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.

***Reply:** We thank the reviewer for their compliment on the graphical summary of the system provided by Figure 1, and critical feedback about the level of detail provided in the text towards reproducing the methods.* *<u>While we believe that every component of the methods described in the identified text is now an accurate and clear description of the simple arithmetic operations performed on the results of the optical flow field estimation, the underlying principles of the equations and procedure have</u>*

been explained in great detail with the optical flow solution utilised from the cited optical flow literature now additionally added to the manuscript in this section.

Modification from L171: "Shi et al. (2020) compared three established and widely applied optical flow techniques to breaking surges, noting the advantages of the Farneback algorithm for its relatively high accuracy and dense flow fields, as well as a lower sensitivity to noise with the converging iterative solution for the displacement vector, d, between a pair of images using quadratic polynomials following Eq. (1):

$$d(\mathrm{X_{im}}) = \left(\sum_{\Delta X_{im} \in I_{local}} wA^T A\right)^{-1} \sum_{\Delta X_{im} \in I_{local}} wA^T \Delta b_f \ , \qquad (1)$$

where I is the greyscale image with local neighbourhood regions denoted by I_{local} using the image coordinates x_{im} and y_{im} to form $X_{im} = \begin{bmatrix} x_{im} \\ y_{im} \end{bmatrix}$, where the change in brightness between the corresponding pixels in the pairs of images are denoted ΔX_{im}. Furthermore, w is a weighting function over the local neighbourhood regions, while the polynomials are defined by $f(x_{im}, y_{im}) \cong a_1 + a_2 x_{im} + a_3 y_{im} + a_4 x_{im}^2 + a_5 y_{im}^2 + a_6 x_{im} y_{im}$ with $A = \begin{bmatrix} a_4 & \frac{a^6}{2} \\ \frac{a^6}{2} & a_5 \end{bmatrix}$, $b_f = \begin{bmatrix} a_2 \\ a_3 \end{bmatrix}$, and $c = a_1$.

The approach is a variational method combining the assumptions of local neighbourhood brightness intensity variation between frames the minimisation of an energy function assuming a slowly varying displacement field for locally smooth velocity gradients (Shah and Xuezhi, 2021)."

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.

Reply: *The authors appreciate the reviewer's questions and commentary; however, the physical justification and motivation is described in the text explaining Eq. (1) (now Eq. (2)). The exponential relationship from the boundary distance factor to the surface velocity profile is just a simple logarithmic relationship of the rearranged form -bx = ln(1-V_s/V_infty). Many alternative relationships and boundary distance factor transformations were tested against optically estimated surface velocity distribution observations at different sites, but the form presented here was the best generically fitting to the data. While the authors appreciate the suggestion of the reviewer, the models suggested cannot be used to simply and reliably fit surface velocity measurements in the transformed boundary distance domain of generic cross-sections.*

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)?

Reply: *While the alpha values are applied locally (naturally lending more weight in estimating depth average velocities from the surface velocities in deeper flows above the defined threshold), the overall effect is to smoothly transition the effective global alpha value from the lower bound to the upper bound dependent on the distribution of water depth across the cross-section. The default values we have applied (derived from the empirical work of Hauet et al. (2018)) fall within the reported interquartile range of Welber et al. (2016). Their local alpha estimations are supportive of a transition below (the default) 2 m water depth, where the spread in the local alpha estimations was seen to increase dramatically. However, Welber et al. (2016) also note that care must be taken with the pairing of the ADCP results that were applied in the local estimations. The relation with the* *theoretical models of LeCoz et al. (2010) were considered already in Hauet et al. (2018), and agrees with the range of default values applied in this work using the conclusions of Hauet et al. (2018).*

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.

Reply: *The authors thank the reviewer for their suggestion and acknowledge that the reviewer has become confused by the wording of these lines, resulting in the reviewer coming to the wrong understanding of how the discharge ratings are fitted.* *As such, we have improved the clarity of the details provided from L234, particularly noting that the power law weighted fitting method has not been applied in this work. Instead, the more preferable (and default) configuration for the CVSG system has been utilised, effectively applying a standard signal filter to the range of discharge estimations calculated across the range of water levels observed in the learning surface velocity distribution.* *The outcome of this method is a linearly locally fit discharge rating, which is an approach supported by the systematic arguments presented in Fenton (2018).*

Modification from L234: "The learning discharge rating can be configured to either be generated from the range of discharge estimates by directly applying a locally fitted Savitzky-Golay signal filter (Savitzky and Golay, 1964) (using a filter window size of 0.05 m vertically with nearest boundaries and linear fitting) or fitting a power law weighted by the number of observations and the optical flow coverage measured at each 0.01 m water level increment. The latter power law weighted fitting method has not been applied here, as the Savitzky-Golay signal filter is chosen instead for the results presented in this work (considered by the authors to be the preferred default configuration for general application following the arguments of Fenton (2018))."

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

Reply: *It is not anticipated in the experience of the authors that stream gauging infrastructure can always be immune to all possible flooding levels. The system was placed above the historic record flood levels, however the authors did not and could not have reasonably predicted that the new record highest flood would occur during the first twelve months of site deployment. The authors estimate that the camera could have been secured higher on the pole on site (which would be expected to either slightly improve or reduce the quality of the stereophotogrammetry estimated water levels and optical flow estimations to some degree). In this view we do not see how this is a very problematic limitation, in practice, relative to the benefits of affording more monitoring sites without the large infrastructure required to tentatively guarantee equipment survival.*

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

Reply: *We thank the reviewer for this suggestion.* *We agree that we can move this paragraph (representing an overview of the results section) to a results overview section at the end of the results, but we do not agree that this paragraph is a good fit for the conclusions.*

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, unsual 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.

Reply: *The authors thank the reviewer for this feedback, and agree that the manuscript could be improved with explicit outlining of the STI slopes in Figure 3 that were required to be adjusted in order to reduce the automatically overestimated velocities resulting from the higher frequency surface wave patterns or underestimated tracer-poor search lines.* <u>This modification has been added to Figure 3 to clarify the determination method that was able to be used for each of the STI slopes across the cross-sections with this additional detail added to the figure caption.</u> *Advice was sought from the official support provider for the Hydro-STIV software package where they kindly provided their own distortion correction ensuring that the camera was correctly calibrated. They provided advice confirming the camera location would need to be moved to a more consistent section of the channel in order to measure the flow using Hydro-STIV in a more stable manner.*

Modification of Figure 3:

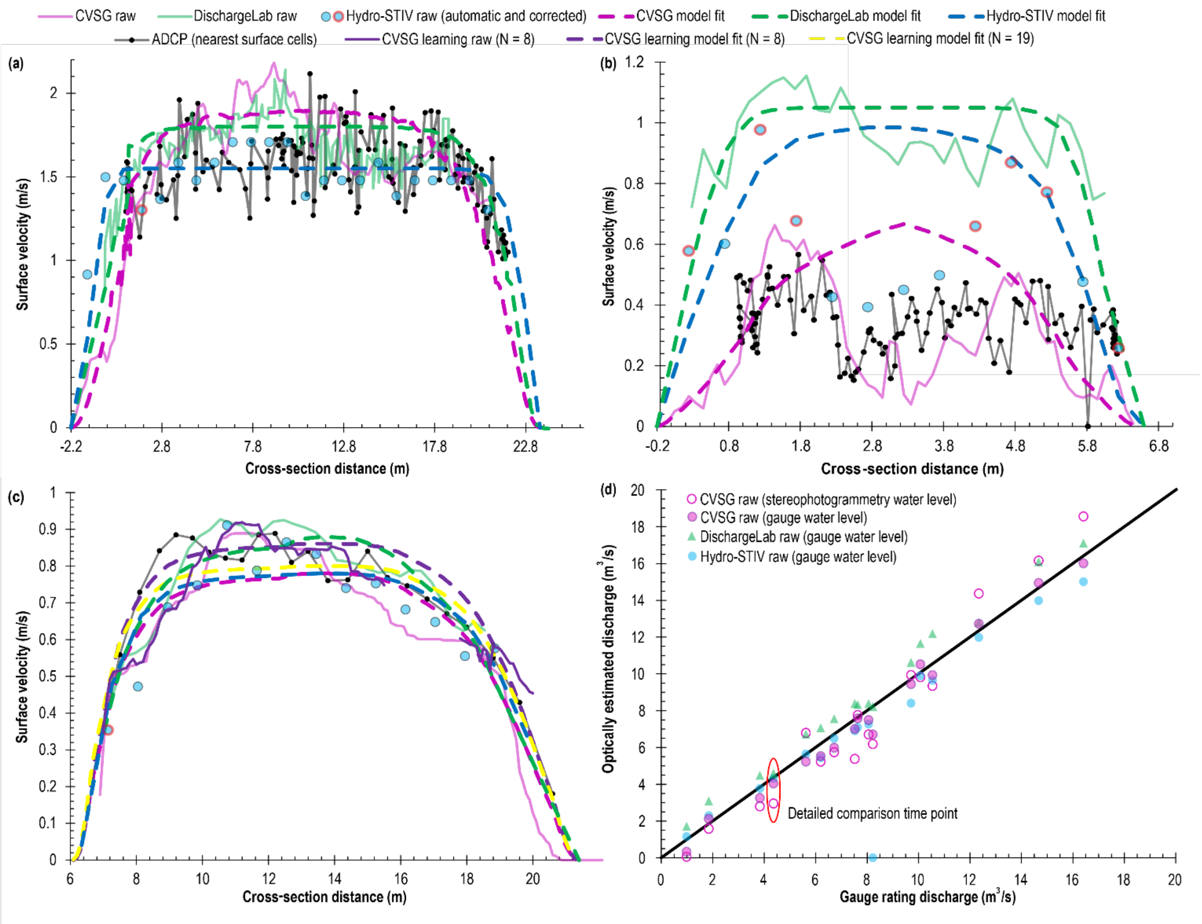

Figure 1: Detailed time point comparison raw and model fitted velocity measurements plotted with nearest surface ADCP measurement cells over the cross-sections at (a) Castor River, Ontario, Canada, (b) an irrigation channel in NSW, Australia, and (c) Tyenna River, Tasmania, Australia. (d) Correlation plot between the gauge rating and optically estimated discharges at comparison time points at Tyenna River, Tasmania, Australia, with the detailed comparison time point indicated. CVSG 5-second duration surface velocities shown for (a) Castor River, Ontario, and (b) the irrigation channel in NSW, Australia. CVSG 10-second duration surface velocities shown for (c, d) Tyenna River, Tasmania, Australia. Hydro-STIV velocity estimates outlined in black were automatically produced, whereas the estimates outlined in red were corrected to the Fourier result or manually corrected to reduce automatically overestimated velocities resulting from the higher frequency surface wave patterns or underestimated tracer-poor search lines.

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.

Reply: *The authors thank the reviewer for this comment, however the intention of the manuscript was to apply the methods presented equally to each site and condition as exampled. We believe that it is important to clearly show the circumstances in which aspects of the methodology presented are not applicable, and as such we seek to highlight these negative results.*

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).

Reply: *We agree with the reviewer's assessment of the data, noting that the measurements indicate overall shifts in the discharge rating over the longer time scales presented.* *We have edited L461 to make this point clear, as well as adding emphasis to the main point of the figure with regards to showing the results of CVSG in context with the manual gaugings over a more significant timespan.*

Modification from L461: "However, it is important to note that the variability in CVSG discharge estimates is minimal compared to the variation in manual gauging estimates from similar water levels since 1989. This variation in discharge estimates over time is often a function of cross section changes and subsequent ratings shifts. Relative differences are expected to be within the realm of uncertainty of the true discharge, particularly as the discharge has only been measured at this water level once in 1966, with measurements within 0.005 m occurring five times (most recently in 1989), and 37 measurements within 0.05 m (the two most recent occurring 2 years and 8 years prior to the time of this case study recording) (Figure 4)."

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.

Reply: *Thank you for your comment. We agree that the stereo camera-based stage measurement is the greatest source of error in the discharge measurements presented in this manuscript. We have been very clear about that (see line 537). However, the comparison of stereo image-based stage measurements against other approaches such as radar is outside the scope of this manuscript.* *We have added a line to the introduction about the application of radar gauges to L61 to improve the background information contextualisation provided to the reader.* *We have strong beliefs that the best tool to apply is dependent on the specific site to be monitored and the objectives/requirements of the monitoring to be undertaken.*

Modification from L61: "Therefore, non-contact and affordable solutions such as radar (Rahman Khan et al., 2021) or optical, offer the potential to overcome these challenges by measuring velocity and stage without in-situ sensors. Similar to one of the oldest manual methods to measure velocities in a waterway by measuring the displacements of surface floats over time, the passive optical measurement of surface velocities using relatively inexpensive camera systems has been an attractive approach to stream gauging (Dobriyal et al., 2017)."

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…

Reply: *We thank the reviewer for raising this point, however we are not sure how the reviewer suggests that errors are being compensated for while all evaluations of the discharge rating curve occur on the same gauged reference datum. As a result of this comment, we have identified an improvement in the clarity of these lines describing the results. It should be highlighted that the cause of the similar raw discharge estimation errors between the analysis using the stereophotogrammetry estimated water levels and the analysis using the gauge water levels owes to the timing of these errors occurring during flow events with poor surface velocity visibility for the raw measurements acting independently of any learning surface velocity distributions.* We have updated these lines from L550 to more clearly explain the presented results.

Modification from L550: "Interestingly, the magnitude of raw CVSG discharge estimation errors was remarkably similar between the remotely sensed and gauge water level cases due to the most significant errors in the raw measurements occurring during flow events with poor surface velocity visibility. In these cases, the learning surface velocity distribution fitted model demonstrated significant improvements to the raw optical measurements. Further to this, the reduced water level estimation noise when using the gauge water level (Figure S8b) displayed significantly reduced error in the CVSG learning discharge estimations converging much faster between the real-time and 4-month hindsight rating estimates."

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.

Reply: *The authors thank the reviewer for their comment, and we agree with every aspect of the statement by the reviewer. While we discuss this more broadly in the discussion, the authors do not see how this discussion point would be appropriate at these lines in the results.*

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.

Reply: *We thank the reviewer for their feedback, and note that this confusion in the assumption of the rating curve has been addressed from a prior comment from the reviewer.*

More generally, it is a pity that no uncertainty intervals around the rating curve estimates are presented, whereas methods are ow 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.

Reply: *We thank the reviewer for this feedback, and note that similar comments were submitted by a previous reviewer. The authors have decided to leave out the "envelope" results representing a form of uncertainty bounds (which are generally enveloping of all available discharge estimation technologies) to avoid cluttering the results, and focus on the differences between the best estimate provided (particularly given that a single number is ordinarily taken as the best estimate from the gauging stations compared). The authors agree with the reviewer around the need for proper uncertainty estimates, however we consider what constitutes a 'proper' uncertainty estimate to be reasonably debatable, and the authors do not yet seek to present techniques or support of any particular set of uncertainty estimation approach in connection with this work beyond the methodology detailed. The key points in the source mentioned by the reviewer reinforce the wide* *variety of uncertainty estimates possible using different methods, requiring careful understanding of the assumptions behind the uncertainty methods used for interpreting the results of any uncertainty estimations provided.* *We have added a line in the discussion at L636 on this point with reference to the reviewer's suggested citation.*

Modification from L636: "Alternative methods for estimating the uncertainty of stream discharge rating curves have been compared in Kiang et al. (2018), finding a wide variation in uncertainty estimates resulting from different methods which demonstrated the necessary careful selection and communication of the assumptions of the uncertainty estimates provided."

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).

Reply: *We thank the reviewer for their suggestion for more formally organising the discussion section around precise questions.* *We have added a line to the discussion referencing the valuable additional citation suggested by the reviewer (Kiang et al., 2018) for strengthening the manuscript at L636 (a modification from the previous comment). We have also added a reference in the discussion to Stumpf et al. (2016) which applied and evaluated a photogrammetry technique for measuring water level and discharge using cameras with different perspectives at L667 (now L730).* *However, we do not feel it is appropriate to discuss surface velocity radar stations which did not form any part or comparison in the study, just as we do not discuss every measurement technique beyond the scope of this work. Further to this, we do not believe restructuring the discussion will improve the readability, as the ideas in the discussion have been structured already with the intention of providing the reader with a logical order and natural flow.*

Added to L667: "Significant work has been undertaken towards developing and applying photogrammetry techniques operating using different camera perspectives from more than one camera for long-term automated water level and discharge measurements (Stumpf et al., 2016)."

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.

Reply: *The authors thank the reviewer for their feedback on the conclusions section.* *We have removed the word successful from the conclusions and added a sentence highlighting the specific challenges that remain to be addressed through future work.*

Modification from L693: "This study has demonstrated the development of an automated operational optical stream gauging system employing methods providing improved reliability for remotely gauging streams using state-of-the-art surface velocimetry technologies across varying flow and lighting conditions. Evaluation of the existing best practice in available stream measurement technologies and published discharge ratings across the array of site conditions evident in this work demonstrated that the methods in this study were similarly effective for gauging stream discharge to existing accuracy benchmarks. This work did not address errors associated with cross-sectional area changes and the capability of the CVSG system to extract stereophotogrammetry estimated elevations of the dry channel areas to inform changes to discharge ratings, which is recommended for future research using stereo imagery-based optical stream gauging approaches. In addition, the challenges associated with analysing surface velocity at night and quantifying water level through stereophotogrammetry under a range of lighting conditions and greater distances provide opportunities for future work. Despite these challenges, non-contact and automated solutions offer a significantly greater density of velocity-stage observations resulting in up-to-date adaptively learning discharge ratings through time. As climate-driven extreme weather events increase in frequency, it is increasingly important to develop and apply flexible monitoring tools, such as CVSG, that can reduce the human and environmental risks associated with traditional approaches and deliver real-time data to water resource managers."

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?

Reply: *The authors thank the reviewer for raising this question relating to this line in the abstract. We had intended the reference to the 'within the best available measurement error margins of 5-15%' to refer to the general range of results between the best available measurement approaches which were evaluated in this study.* *We have updated this line at L21 to better clarify the meaning of this general result summarised in the abstract.*

Modifications from L18: "Evaluations between reference state-of-the-art discharge measurement technologies using DischargeLab (using surface structure image velocimetry), Hydro-STIV (using space-time image velocimetry), ADCPs (acoustic doppler current profilers), and gauging station discharge ratings demonstrated that the optical surface velocimetry methods were capable of estimating discharge within a 5-15% range between these best available measurement approaches."

L46: Doppler

Reply: *We thank the reviewer for highlighting this oversight.* *We have corrected the proper capitalisation of Doppler.*

Modification from L41: "Intrusive methods range from the resource intensive installation of hydraulic control structures to measure discharge rates analytically using simpler water level measurements within a designed range by obstructing and controlling the flow through a standardised geometry (Boiten, 2002) (often to the detriment of aquatic species (Mueller et al., 2011), as well as sedimentation and erosion (Pagliara and Palermo, 2015; Ogden et al., 2011)), through to the risking of people and equipment entering the stream to measure velocities using passive mechanical current meters or active acoustic Doppler velocimetry profiles (Gordon, 1989)."

L526: true dischargeS

Reply: *We thank the reviewer for highlighting this grammatical point.* *We have corrected the pluralisation of 'discharge' in this line.*

Modification from L526: "Even though the true discharges at the measurement times are not known, the CVSG learning discharge estimations using the gauge water levels at the time overestimated the discharges of events occurring in April 2021 relative to the latest gauging station discharge rating by up to 20%."

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

Reply: *The authors thank the reviewer for raising this minor point, and agree that a more specific description of the amount of overestimation would improve the manuscript.* *As such, we have removed the vague descriptor, 'somewhat', and added to the end of the line 'by up to 20%'. The modification of this line is included in the modification for the previous comment.*

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

Reply: *We thank the reviewer for highlighting this repeated 'and' in the figure caption. We have removed this redundant 'and'.*

Modification from Figure 6 caption:

Figure 2: Correlation plots for the latest gauging station rating discharge timeseries against the CVSG estimated discharge timeseries at Tyenna River, Tasmania, Australia using (a) stereophotogrammetry estimated water levels, and (b) gauge water levels, as well as at Paterson River, NSW, Australia using (c) stereophotogrammetry estimated water levels, and (d) gauge water levels.