Revision Note to Anonymous Referee #1

The authors thank the reviewer for the suggestions and comments on how to strengthen the paper. Our specific responses to the comments are as follows:

1) Comments to author:

The paper in itself is OK, but lacks clearness and remains too speculative. Vague words like "believe", "supposed" and "seem" indicate this.

Response

We removed unclearness as much as possible by providing clearer approach and rationale of doing so.

2) Comments to author:

The governing equation should be added to facilitate interpretation.

Response

Added.

3) Comments to author:

Measurement accuracy should be provided together with its consequences for the final results. The same holds for the reference of the USGS and Fread's methods. As the true discharge is not known, comparisons can only be valid if the measurement errors are taken into account.

Response

Thank you. Accuracy/resolution information for this specific pressure transducer is provided. In addition, an accuracy of discharges from USGS records is known to be within 5-10% (Hirsch and Costa, 2004) and an average RMS error for Fread's method is known to be approximately 4% (Fread, 1975). This information is also included in the revised manuscript.

Reference

Hirsch, R. M., and Costa, J. E.: US stream flow measurement and data dissemination improve, Eos, 85(20), 197-203, 2004.

Fread, D.: Computation of stage-discharge relationships affected by unsteady flow, Journal of the American Water Resources Association, 11, 213-228, 1975.

4) Comments to author:

Given the aspect ratio of the channel, not only bed roughness but also bank roughness/irregularities should be accounted for and thus addressed. Given the accessibility of the river reach characterisations of bed and bank roughness should not be a problem. Why is this information not used here? Are inferred roughness values realistic? With some information on the sediment composition, estimates regarding dynamic bed roughness can easily be made e.g. vanRijn, JHE 1984.

Response

Thank you very much for this great comment. We totally agree with the concerns and issues raised by the reviewer. Conventional practices of estimating channel roughness coefficients are via a) a direct estimation from known discharges and hydraulic properties; b) an indirect estimation from experimental equations (e.g., Bray, 1979; Jarrett, 1984; Sauer, 1990); c) an indirect estimation from published n-value tables (e.g., Dalrymple and Benson, 1967; Chow, 1959; Henderson, 1966; Jarrett, 1985) or photographs of similar channels (e.g., Barnes, 1967; Aldridge and Garrett, 1973). Approach c) is generally the outcomes from either approach a) or b), and an accuracy of the method largely depends on a hydrologist's experience. Approach a) is considered the most accurate among others as measured (steady or unsteady) discharges (i.e., calibration data) can directly be used to establish a stage-n rating.

As exemplified by the reviewer, we also included experimental equations as alternatives of estimating channel roughness coefficients (approach b)) that can account for bank roughness/ irregularities due to vegetated bank conditions as well as other flow retarding factors including particle diameters, cross-sectional irregularities, and variations in channel size. The review of those equations includes the methodologies proposed by Bray (1979), Jarrett (1984), and Sauer (1990). The performance of those equations that can be applicable to Clear Creek conditions is demonstrated.

Reference

Aldridge, B. N., and Garrett, J. M.: Roughness coefficients for streams in Arizona, U.S. Geological Survey Open-File Report, 87, 1973.

Barnes, H. H.: Roughness characteristics of natural channels, U.S. Geological Survey Water-Supply Paper 1849, 213, 1967.

Bray, D. I.: Estimating average velocity in gravel-bed rivers, American Society of Civil Engineers, Journal of the Hydraulics Division, 105, 1103-1122, 1979.

Chow, V. T.: Open channel hydraulics, McGraw-Hill, New York, 1959.

Dalrymple, T., and Benson, M. A.: Measurement of peak discharge by the slope-area method, U.S. Geological Survey Techniques of Water-Resources Investigations, book 3, chap. A2, 1967.

Henderson, F. M.: Open channel flow, Macmillan, New York, 493, 1966.

Jarrett, R. D.: Hydraulics of high-gradient streams, American Society of Civil Engineers, Journal of Hydraulic Engineering, 110(11), 1519-1539, 1984.

Jarrett, R. D.: Determination of roughness coefficients for streams in Colorado, U.S. Geological Survey Water-Resources Investigations Report 85-4004, 54, 1985.

Sauer, U.S. Geological Survey, written communication, 1990 as reported in Coon, W.F., 1998. Estimation of roughness coefficients for natural stream channels with vegetated banks (Vol. 2441). US Geological Survey.

5) Comments to author:

Was there any vegetation in the domain under study? How much effect would it have?

Response

There was some vegetation as it was growing season. Since we are using existing USGS stage-discharge rating curve (i.e., discharge data) to estimate the channel roughness using Manning's equation and the curve is being calibrated regularly, the effects of seasonal vegetation should already be taken into account. It is also identified that the rating curve is shifting around with a clear pattern depending on seasons.

6) Comments to author:

What is the rationale behind averaging the measured "unsteady slopes" knowing that the flow is subject to non-linear friction?

We eliminated this approach in the revised manuscript and proposed more scientifically based conventional approach in estimating the channel roughness coefficients.

Revision Note to Dr. M. Perks

The authors thank the reviewer (Dr. Matthew Perks) for the suggestions and comments on how to strengthen the paper. Our specific responses to the comments are as follows:

Referee #2 (Dr. M. Perks)

1) Comments to author:

The paper "Technical Note: Monitoring of unsteady open channel flows using continuous slope-area method" by Lee et al. seeks to adopt the use of low-cost pressure transducers to better understand the role of hysteresis in open channel flows. In its current form, the article is difficult to follow. Therefore considerable changes are required before publication can be recommended. The concept of applying the continuous slope-area method is poorly defined and described in the introduction, as is the utility of this concept. Under what conditions would applying this method be beneficial? This is the fundamental part of the manuscript so a clear explanation is required.

Response

Thank you. We modified introductions by adding better descriptions associated with the concept of applying the CSA method, utilities, and specific objectives of this research. Moreover, specific conditions that this method would be more applicable/beneficial are also added.

2) Comments to author:

For a Technical Note, there is a lack of detail in the Methods section. A clearly presented Data Treatment section is required wherein the equations/calculations are presented. A conceptual diagram would also be beneficial to illustrate how the method is constructed and applied. A more thorough presentation of results is required, rather than simply directing the reader to the Figures.

Response

Thank you. We added the methods section with relevant equations and added implementation procedure section 2.7 with a diagram (Figure 5).

3) Comments to author:

The data used to drive the CSA method appears to be based on flow measurement, I assume collected following the development of a stage-discharge relation(?) at the USGS Clear Creek monitoring station (no information or data presented). Does this rating adequately capture both rising and falling limbs of the hydrograph? Some sensitivity analysis and discussion of this approach is required.

Response

Thank you. The reviewer is correct. Flow data are available only from USGS field measurements when they constructed a steady-state stage-discharge rating curve. The rating curve does not capture unsteadiness. Further information is provided in Section 2.7 implementation procedures.

Sensitivity of computed discharges due to the uncertainties in the measurement of channel bed slopes are also conducted and discussed as it would affect significantly the estimation of channel roughness coefficients (see Figure 10).

Specific Comments:

4) Comments to author:

Page 2 Line 12 – 13: Reference required.

Response

Lines 7-13 are removed.

5) Comments to author:

Page 2 Line 16: The acronym 'CSA' (first used on page 2 Line 16) is not defined in in the main body of text. This could relate to the conventional, or continuous slope area method.

Response

Corrected.

6) Comments to author:

Page 2 Lines 16 – 20: Strange presentation of other research. Simply stating Steward et al (2012) following their findings would suffice. No need for information about USGS/Arizona.

Response

Corrected.

7) Comments to author:

Page 2 Line 23: "Steep" – be specific.

Response

Information on Page 3 Lines 21-24 (shown below) is moved to better define steep (>0.001) channels.

"For comparison, the average channel bed slopes in the studies by Smith et al. (2010) and Stewart et al. (2012) were approximately 0.009 and 0.012, respectively, and the effects of unsteady flows were negligible in those streams. Sudheer and Jain (2003) indicated that flood waves show a marked kinematic behavior when a channel bed slope is greater than 0.001."

8) Comments to author:

Page 2 Line 24: Replace "a.k.a" with i.e.

Response

Corrected.

9) Comments to author:

Page 2 Line 27: "They" – who is they? If it is the series of works referenced above then their findings should be placed prior to the reference.

Response

Thank you. The sentence in Page 2 Line 27 is placed before the reference and reworded.

10) Comments to author:

Page 3 Line 1: What is a "proper" reach?

Response

The sentence is reworded as follows:

"To achieve successful implementation of the CSA method, careful selections of channel reaches and measurements are important…"

11) Comments to author:

Page 3 Lines 8 – 16: Useful justification for site selection. However you do not state how your chosen site meets these criteria. This information could be presented in a table.

Response

Table 1 is presented and the sentences are reworded.

12) Comments to author:

Page 3 Lines 21 – 23: This information relating to bed slopes of sites used in other works is better suited to the introduction rather than a methods section.

Response

Thank you for pointing this out. We agreed on the reviewer's opinion, and moved this to Introduction section (See also the response to comments #7).

13) Comments to author:

Page 3 Lines 28: Assume that the Q data utilized in this research is in the form of a rating curve? This should be presented and actual method described.

Response

Yes, Q data is obtained based on steady-based stage-discharge rating curve. A section demonstrating implementation procedures is added as Section 2.7 in the revised manuscript.

14) Comments to author:

Page 3 Line 29: "Cross-sectional information" is vague. Be specific.

Response

The sentence is removed.

15) Comments to author:

Page 4 Lines 27 – 28: Any discussion provided by Smith et al (2010), or Stewart et al (2012) whereby the redundancy of their systems is discussed in order to back-up your use of only two sensors?

Response

Thank you. More discussions are provided.

16) Comments to author:

Page 4 Line 29: What pressure transducers were used? What is the associated precision and accuracy?

Response

In-Situ Level Troll 500 is used and accuracy/resolution information is provided based on brochure.

17) Comments to author:

Page 5 Line 16: Be specific – How exactly does it compare?

Response

The slope surveyed by USACE was also 0.00039. So, we replaced "closely agrees" with "coincides".

18) Comments to author:

Page 5 Line 19 – 20: Strangely formed sentence.

Response

The sentence is reworded.

19) Comments to author:

Page 5 Line 19 – 20: This is the first mention of the Fread method. How does this fit in with the experimental aims? A lack of detail is provided. If the modified Fread method is to be used then details need to be provided as the cited publication is not currently published.

Response

The Fread method is introduced herein to compare the CSA results with the numerical method as supplemental information because direct field discharge measurements are not available. While the paper is accepted as of Sep 19, 2016, the publication process has kept being delayed. However, it will be published online in Jan-Feb, 2017 timeframe based on their promise. The citation will be updated before this article is published.

20) Comments to author:

Page 5 Lines 22 – 23: Small to mid-size is subjective. Catchment sizes should be given. The contributing area of Clear Creek should also be presented.

Response

Rather than defining small to mid-size, the wording is changed to a low-aspect ratio channel (approximately less than 30:1 (width: depth) ratio).

21) Comments to author:

Page 5 Lines 25 – 26: Would be good to see these events placed within the context of the hydrological regime e.g. recurrence intervals.

Response

Relevant information is added.

22) Comments to author:

Page 6 Lines 2 – 3: Axis information should be placed within the Figure caption.

Response

Corrected.

23) Comments to author:

Page 6 Lines 7 – 14: This detail, although interesting, is not related to the results. Indeed, you do not observe clockwise hysteresis so why comment on the processes driving its occurrence?

Response

We agree with the reviewer's comment, so this redundancy is removed.

24) Comments to author:

Page 6 Line 16: Use of "strong" is a subjective term – be specific.

Response

We reworded the sentence to "As the event scale increases, dynamic forces would also increase.".

25) Comments to author:

Page 6 Line 22: Use of "very high" is a subjective term – be specific.

Response

Removed.

26) Comments to author:

Page 6 Line 22: Changes in the cross-section should be presented.

Response

Removed.

27) Comments to author:

Page 6 Line 25: "Sometimes not impossible" - double negative.

Response

Removed.

28) Comments to author:

Page 6 Lines 26 – 27: Evidence of no major floods is provided. A Figure showing a hydrograph spanning the entire monitoring period would help place the three analysed events within the hydrological context.

Response

Figure 3 is added.

29) Comments to author:

Page 6 Line 30 – "Large differences" – be specific.

Response

Removed.

30) Comments to author:

Page 7 Lines 30 – 32: Weak end to the conclusion. The final sentence should be more profound than being about time synchronization issues.

Response

Reworded.

31) Comments to author:

Figures:

General point: Appearance of all the figures and detail in the captions should be improved prior to publication.

Fig 1: A regional map as an inset would be useful to provide context. Credit to background image should be provided if appropriate.

Fig 2: Difficult to see details but at the peak stage, it looks like the steady non-uniform slope values are less that the rising and falling stage slope.

Fig 4: No useful information provided in the caption. Needs a better description.

Response

Modified.