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Discussions

Interactive comment on "Citizen science flow – an assessment of citizen science streamflow measurement methods" *by* Jeffrey C. Davids et al.

Anonymous Referee #2

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The paper presents results on three simple and easy to use discharge estimation methods appropriate for citizen science (SC) that the authors applied in the Kathmandu Valley, Nepal. They assessed the agreement between the methods and compared estimated discharge to selected measurements using a doppler radar device. The text is short but mainly well written, the graphical presentation is clear and appealing. I recommend to state explicit research questions at the end of the introduction (currently missing). I have also some major concerns about parts of the analysis and the interpretation of the results: 1) While the authors do well in terms of reporting statistical significance of their results, the use of the Pearson Correlation Coefficients seems not appropriate for the properties of the dataset. I therefore recommend the non-parametric Spearman Rank Correlation and the associate non-parametric statis-

tical test. 2) I question whether it is meaningful or informative to correlate the slope of the salt dilution calibration k to latitude or longitude and elevation and would suggest to skip (or better explain this analysis). 3) Instead I recommend to also show the comparison of discharge estimated by salt dilution and by the Bernoulli method. 4) I would ask the authors to quantitatively prove that they can compare discharge estimates taken during the CS-campaign with doppler radar observed discharge taken +/- one month (!!) before/after the campaign or skip that part. In the discussion they state themselves that the flow might have decreased during that time. As the remaining analysis is probably too short for a full publication, I suggest the authors to check whether their dataset would allow additional analysis e.g., on the difference of the quality of the measurements taken by experts and citizens (see my suggestions in the pdf). The current paper is interesting, the dataset promising but the current state of analysis is not enough for a full publication. I therefore encourage the authors for major revisions and additional data analysis.

In the following I summarize my suggestions for the individual sections and ask the authors to also check my detailed comments and suggestions that I have included in the pdf (uploaded as supplement):

1) Introduction: The introduction is on the short side and starts a bit philosophical. I would focus more on streamflow and introduce to the problem that large parts of the words still have limited number of gauging stations (especially remote and developing countries) and that measuring devices - while still decreasing in costs - have their limitations. Some other citizen science studies are briefly mentioned but the findings of other studies could be described a bit more in detail. The same applies to the existing methods for low-cost streamflow assessments. Their pros and cons could be compared using a table. I also do not agree that the are no tools on the market, that allow direct measurements of discharge with smartphones and added one link to an example. The research questions should be clearly formulated at the end of the introduction. Please also see more specific comments directly marked in the pdf (uploaded as supplement).

2) Methods: The method section needs a better description of the experimental setup; study area, test with students, repetitions y/n etc. (parts are mentioned at the end of the method section but should be stated at the beginning)! The catchments and streams used for testing need a better description (see my suggestions in the pdf). The same applies to the training of the students. The explanation of the different methods is long but can be useful for some non-hydrological readers. I suggest to consider to present all this information in the introduction section. I would however include a list of objective criteria why these three and not other methods have been selected. Please also see more specific comments directly marked in the pdf (uploaded as supplement). The method section should be clearer about the two datasets collected a) dataset with n=20 samples (I assume collected by the authors themselves = exports) and the CS-campaign with n=145 samples collected by citizens. One issue seems critical to me: Authors compare observed discharge using the doppler radar with CS-discharge measurements done +/- 1 moth earlier/later. The authors should prove statistically that the mean daily flow in the month before and after the CS-discharge measurements is not significantly different. In fact authors state in the discussion section that flows decreased during that period.

3) Results: Graphical presentation of the results is good and I appreciate that the authors report about statistical significance of their results. For some of the dataset I suggest to used the Spearman Rank Correlation Coefficient as the assumptions for using the Pearson Correlation seem to be not fulfilled. I also suggest to mention that, while statistically significant, some of the relations show relations difficult to interpret (definitely not linear or exponential but complex or clustered). I ask the authors to explain why they think Figure 2 is informative to the reader expect for presenting the measurements. I question whether it is meaningful or informative to correlate the slope of the salt dilution calibration k to latitude or longitude and elevation and would suggest to skip this analysis. Instead I recommend to also show the comparison of discharge estimated by salt dilution and by the Bernoulli method. As mentioned in the method section I would ask the authors to quantitatively prove that they can compare discharge

estimates taken during the CS-campaign with doppler radar observed discharge taken +/- one month before/after the campaign or skip that part. Please also see more specific comments directly marked in the pdf (uploaded as supplement).

4) Discussion: The discussion is short and not very into depth. Parts of it would better fit into the result section. While the background concentration is certainly affected by the geology I have strong doubts that correlating k with latitude and longitude or elevation is meaningful. At least better explain why the authors think these predictors are meaningful and the correlations not spurious. I suggest the authors to check whether their dataset would allow additional analysis e.g., on the difference of the quality of the measurements taken by experts and citizens (see my suggestions in the pdf). 5) Summary and future work Is well written in general. However, addressing the outcome of this work in the light of research questions (that I suggest to include in the introduction) would improve this section.

I hope these suggestions are useful to work on a more advanced version of the manuscript!

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-425/hess-2018-425-RC2-supplement.pdf

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