Hydrol. Earth Syst. Sci. Discuss., author comment AC1
https://doi.org/10.5194/hess-2022-208-AC1, 2022

Reply on RC1

Monika Barbara Kalinowska et al.

Author comment on "Influence of vegetation maintenance on flow and mixing: case study comparing fully cut with high-coverage conditions" by Monika Barbara Kalinowska et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-208-AC1, 2022

RC1: 'Comment on hess-2022-208', Anonymous Referee #1, REPLY

General Comments

The authors have highlighted a need for measurements of hydraulics and mixing at the reach-scale in vegetated channels, and identify vegetation coverage as a key factor. To investigate, dye injections are carried out before and after vegetation cutting and these results are analysed, finding decreasing longitudinal dispersion coefficient with increased coverage. This work is of sound quality, of use to engineers interested in water quality in channels, and is suitable for publication in HESS. I recommend the article for publication with minor revisions.

> Thank you for all your comments, including the language one. We are grateful for the thorough review of the paper, which will allow us to clarify several issues to make the article clear and more readable.

Specific Comments

On line 198, the authors observe a doubling of discharge and a 3-5 times increase in velocity after the vegetation is cut, suggesting that the flow area has at least halved after cutting. How do water depths compare? Was discharge measured immediately before and after cutting? Is the difference in discharge from reduced cross-sectional blockage area? Or is there possibly some other variation also affecting flow rates, e.g., did it rain recently? Have these possibilities been taken into consideration for the author's proposed UNV/UVEG relationship? I recognize this is difficult to pick apart, but if U has increased by a factor of 2 due to other factors than vegetation cutting, this would impact the slope of the line in Figure 9. While it would not impact the conclusions from the figure, the specificity of the relationship should be further discussed in the text.

> The conducted experiments, including all measurements, refer to vegetated (Exp. 1, September 2019) and non-vegetated (Exp. 2, mid-October 2019) conditions, while the cutting was performed at the beginning of October (lines 88-95).

As you noticed, flow-discharge changes can generally be associated with a different cross-sectional blockage area, but also other factors like weather conditions. In our experiments, most of the changes should be related to the decreased vegetation coverage as no extreme events (e.g., heavy rainfalls, droughts) were recorded in the study period, which also precludes major changes in cross-sectional geometry due to erosion/deposition between Exp. 1 and Exp. 2. However, since we were not in the laboratory conditions, and it was not feasible for us to control all environmental factors influencing the hydraulic conditions, the possible influence of such, should be borne in mind, as usually in the field circumstances. In the revised version, we will discuss this point in more detail.

As visible in Table 1, both experiments were performed with a comparable water depth in the reach-averaged sense. However, the water depth was lower, particularly in the two most downstream sub-reaches in Exp. 2, leading to a lower cross-sectional area compared to Exp. 1, which likely partly explains why the flow velocity increased more than the discharge. As the flow resistance and thus water surface slope in the study reach notably changed between Exp. 1 and Exp. 2, it was impossible to obtain exactly the same water depths in each sub-reach between Exp. 1 and Exp. 2.

Please note that the reported flow velocities are based on the tracer data (see also Responses to Reviewer 2) and may thus slightly differ from the mean velocity classically determined as Q/A. They should actually better take into account the change in the sub-reach mean U due to the presence of vegetation. However, as noted in the table footnotes and the 173 line, the first sub-reach in Exp. 2 is uncertain since the tracer in the first cross-section (P1) was not well horizontally mixed during Exp. 2. We will stress this better in the revised paper, and taking this into account, we will change in line 197 "*... and mean velocities 3-5 times higher …*" to "*... and sub-reach mean velocities 3-4 times higher …*".

To avoid misunderstandings and following the reviewer's hints, we will revise the text according to these observations, pointing out possible sources of uncertainty, and including the slope effect in Fig. 9.

On lines 239/244, the authors comment on the unsuitability of scaling longitudinal dispersion with bed shear stress and water depth. Do the authors have any comments on/suggestions for what alternatives might be used instead? The results in Table 1 seem to suggest scaling by mean longitudinal velocity would be suitable, and this is commonly used in literature for uniform vegetation, e.g., Nepf (1999). Considering that velocity may have been increased by a factor of two independent of vegetation cutting, how would scaling dispersion by velocity change Figure 11?

> The scaling of the longitudinal dispersion coefficient for multiple flow rates in naturally vegetated channels, generally characterised by different species of plants having a variable density, is rather an open question. We, of course, tried to find a good scaling option, also considering the mean velocity (see Fig. R_1). Still, to receive the proper units, it will be necessary to find parameters like the water depth (like in the general formula for DL = ahu*, whre a-dimensionless coefficient, u*-shear velocity) or, e.g., stem diameter (used for uniform vegetation, for example, together with the mean velocity by Nepf, 1999).

Bearing in mind that we only have data for the two flow rates values, and vegetation consists of varied plants, we decided not to provide suggestions for scaling, but stress that this a crucial issue to be addressed in future research since for the best scaling approach we need a lot of observations, potentially not only from one channel. However, in reply to your comments, yes, it is possible to scale DL versus U (we also tried the DL/Uh, to have non-dimensional units). Finally, considering your suggestions, we decided to include the figure (Fig. R1_1) in the revised manuscript, complementing Figure 11. It may be beneficial since it shows that, although we may expect a linear relationship between vegetation coverage and DL for dense vegetation, the relation may differ for channels with low vegetation coverage.

We hope that the results presented in this article and their analysis will also interest other researchers, allowing for increasing the number of available sets of data from field experiments performed in different flow conditions and accounting for changing vegetation coverage and characteristics. The great interest in our research, which we noticed since the preprint was published, looks promising.

In the revised version, we will discuss the problems associated with DL scaling more extensively. Moreover, we will add and comment on the figure showing the DL/U (Fig R1_1).

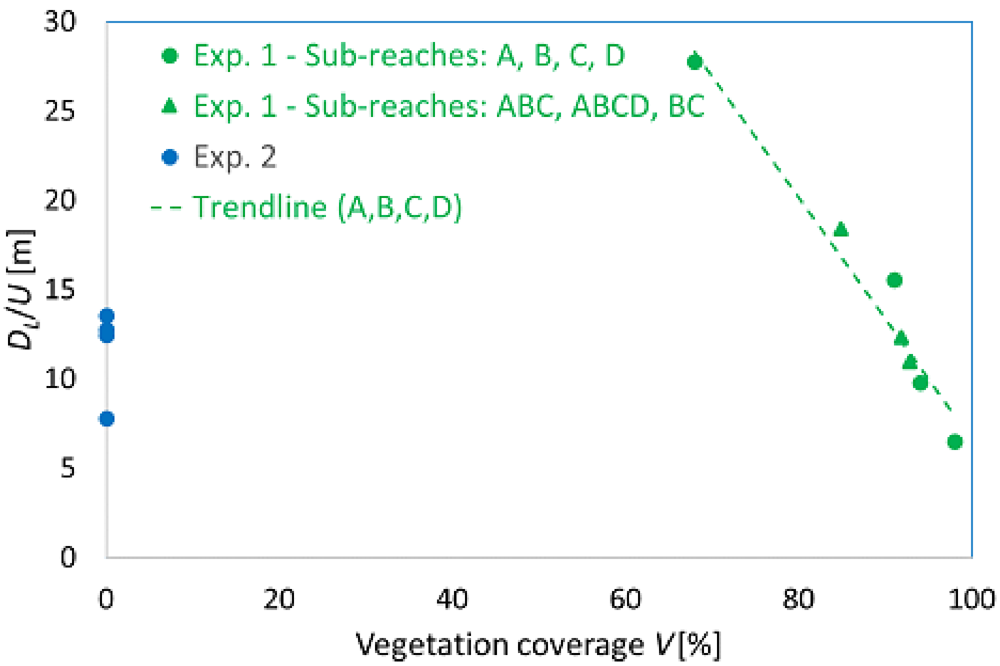


Fig. R1_1. Longitudinal dispersion coefficient DL scale against the mean sub-reach velocity U.

The authors have not included a data availability statement.

> Together with publishing the paper, we will publish data used in the manuscript in the IG Repository (https://dataportal.igf.edu.pl/).

Technical Comments

Section 1: The introduction implicitly links mixing and longitudinal dispersion coefficient, but this link could be made more clear, e.g., by the inclusion of the advection-diffusion equation or by explaining that the longitudinal dispersion coefficient describes a rate of spread in the streamwise direction.

> Thank you for the remark. We will expand the information about the longitudinal dispersion coefficients in the Introduction section.

We will update lines 66-69 as follow:

"Our primary focus is the determination of the longitudinal dispersion coefficients (DL) and their dependence on the vegetation coverage. These coefficients describe the rate of spreading (dispersion) of the solute in the streamwise direction. The DL is present in the 1D advection-diffusion/dispersion equation (ADE), commonly used to describe the mixing and transport of admixture in open channels. They appear in the 1D equation due to the averaging of the 3D ADE over the depth and width of the channel. Their values are required to run relevant numerical models to simulate the spread of pollutants in time and space. In fact, they are the most important and, at the same time, the most difficult to determine factors characterising the mixing processes (Czernuszenko, 1990; Kalinowska and Rowiński, 2012). It is still challenging to determine values of dispersion coefficients for a particular channel (Kalinowska and Rowiński 2012), especially for natural channels with vegetation. Tracer experiments remain the best source of information for estimating their values under complex, natural conditions."

Section 2: Is there any additional description of the vegetation? Species? Stem diameters, density, etc.? The methodology mentions that surface slopes and cross-sectional velocity profiles were recorded, could these be presented or otherwise included with an accompanying data set? (And also include concentration profiles, dispersion coefficients, planform vegetation coverage, channel geometries, etc., in that data set?)

> Unlike most available studies, the research proposed herein is not focused on individual plants or patches, but on vegetation coverage at the reach scale, in complex natural conditions.

The plants that could be encountered in the analysed channel includes: Phalaris arundinacea L., Phragmites australis, Glyceria maxima and Sparganium Emerson. However, during our experiments, it was not our intention to collect detailed physical information on the vegetation present in the channel. Instead, as described in the manuscript, we aimed to investigate the influence of vegetation at the reach scale, and it is known that at the reach scale, the coverage is the factor mainly influencing the flow hydraulics, and thus we hypothesised that also the solute transport would depend on coverage. Contrary to laboratory investigations, where researchers can deal with controlled and well-described hydraulic and vegetational properties, in the field, we are dealing with mixed vegetation (e.g., submerged and emerged, different species and densities, etc.), see, e.g., Fig. R1_2. Practical applications with "disorderly" natural vegetation motivated our work, which aimed at investigating physically sound but straightforward to measure parameters like vegetation coverage.

Fig. R1_2. Channel Warszawicki during the Exp. 1.

The channel slope was measured by multiple geodetic levelling of the water surface over a length of 60-100 m. Slopes were similar for vegetated (Exp. 1) and unvegetated (Exp. 2) cases and were around 0.1 ‰. We will add the information about the slope in the revised version of the manuscript.

Due to operational constraints (i.e., manpower and time, as we had to finish all of them before the dye release to not disturb the velocity field and mixing during the tracer study), the hydraulic measurements have been done in a speditive way to measure and eventually compare the flow rate. Due to the uncertainty of such measurements in natural channels under low water depth conditions, our calculations presented in the paper are based on the data derived from the measured concentrations profiles and the vegetation coverage obtained using remote sensing techniques.

The acquired data on concentration and vegetation coverage, used in the computations presented in the paper, as mentioned earlier, will be provided in the IG Repository (https://dataportal.igf.edu.pl/) together with a link to the published version of the article.

Section 2.4: Did the authors consider optimisation of the routing solution to the advection-diffusion equation as a means of obtaining longitudinal dispersion coefficient? If the sampling frequencies were very low when calculating moments, was the trapezium rule used to increase the accuracy of the integral?

> Yes, we considered using the routing procedure. Different methods may be used to compute the DL and U based on the concentration data, like the method of moments or routing procedure. We did not intend to compare those methods here, as this could be found in the lietarure (e.g., Heron, 2015). We have chosen the method of moments as suitable enough for our data. Due to the operational limits, we measured the

concentration based on the samples taken in not fixed time intervals, while, in the routing procedure, a constant time step is required. Therefore, it would be additionally required to interpolate the data. Taking into account that the methods of the moment (with truncated data) gives similar results to the routing procedure, as well as other sources of uncertainty during such field measurements, we decided to use the simplest methods of moment where the U and DL could be easily and directly computed based on our measured data.

In the revised manuscript, we will comment further on the possibility of using other methods. We will stress more precisely that we collected the water samples in unequal time intervals. In this respect, please see also the response related to the sampling frequency information below.

Section 3: Why are the dispersion coefficients presented before the concentration profiles, given that the concentration profiles are used to derive the dispersion coefficients? Consider moving them to Table 2, and placing Table 2 in Section 3.3.

> Table 1 reports the data for each sub-reach, while Table 2 reports information for each cross-section. As the dispersion coefficients refer to the sub-reach, we provided them in Table 1.

Accordingly to the Journal's guidelines, tables are placed where they are cited for the first time.

Line 24: Is "On the other hand" needed?

> Thank you for the comment. We will remove *"On the other hand"* in 24 line.

Line 31: "by the very local" is odd wording, suggest just "by local" or "by very local".

> Thank you for the comment. We will change the text as you suggested using *"by very local"* instead of *"by the very local"*.

Line 32: "demonstrated" should be "have demonstrated".

> Thank you, we will change the text accordingly.

Line 32: Please clarify "vegetation-induced flow alterations are significantly influenced by plant arrangements" - "vegetation-induced" and "influenced by plant" can be read with the same meaning, e.g., "thing is caused by thing".

> We aim to revise the text as: *"In this respect, a growing number of studies demonstrated that the influence of vegetation on the flow hydraulics significantly depends on the plant arrangement, such as patch shape, density and coverage."*

Line 45: "unfeasible" should be "infeasible".

> Thank you for having noticed it; we will change the text.

Line 115: Please add an explaination of "map algebra".

Map algebra is a general formulation widely used in GIS studies. This term refers to mathematical operations used in algebra, but the key difference is that it only applies to raster data. Map algebra uses math-like operations like addition or multiplication to update raster cell values. The most common type of map algebra is a cell-by-cell function. This type has rasters directly stacked on top of one another. Then, the function applies to cells aligned with each other.

We will expand the explanation and add the citation (Cmara et al., 2005) in the revised paper.

- Cmara, D. Palomo, R. Cartaxo, M. Souza and R. F. D. Oliveira, "Towards a generalized map algebra: principles and data types.", In Proceedings of the Workshop Brasileiro de Geoinformtica, 2005.

Line 136: What were the sampling frequencies?

> As written in line 136, we changed the sampling frequency based on expected/checked concentration values.

During Exp. 1, we generally started sampling with 10 minutes intervals (except the P1, when we started immediately with 5 minutes intervals), then the sampling frequency was increased to 5 minutes close to the expected peak (2-3 minutes for P1), and returned to 10 minutes (after the peak was captured). Finally, we measured from 10 to 60 minutes for the tailing edge, as the concentration changed more and more slowly.

In the case of non-vegetated conditions (Exp. 2), since the passage of the dye plume was rapid, we sampled faster, and the sampling frequency was from 1 to 10 minutes. More frequently close to the expected peak of concentration (from 30 seconds in P1 to 1-3 minutes in other cross-sections), and less for the tailing edge from 5 to 10 minutes, adjusted to the actual cross-section concentration changing (using a hand fluorometer on site).

We will revise the text to clarify this point and add the sampling frequency information to the paragraph in lines 131-137. The frequency may also be derived from the data that will be provided in the IG repository mentioned above.

Line 146: Were the leading- and trailing-edge times presented? Were these calculated the same way as for calculating moments (line 165)? Suggest clarifying/removing.

> We did not finally include the values and figures with mentioned leading- and trailing-edges in the text, so we will remove the information about them from the revised text.

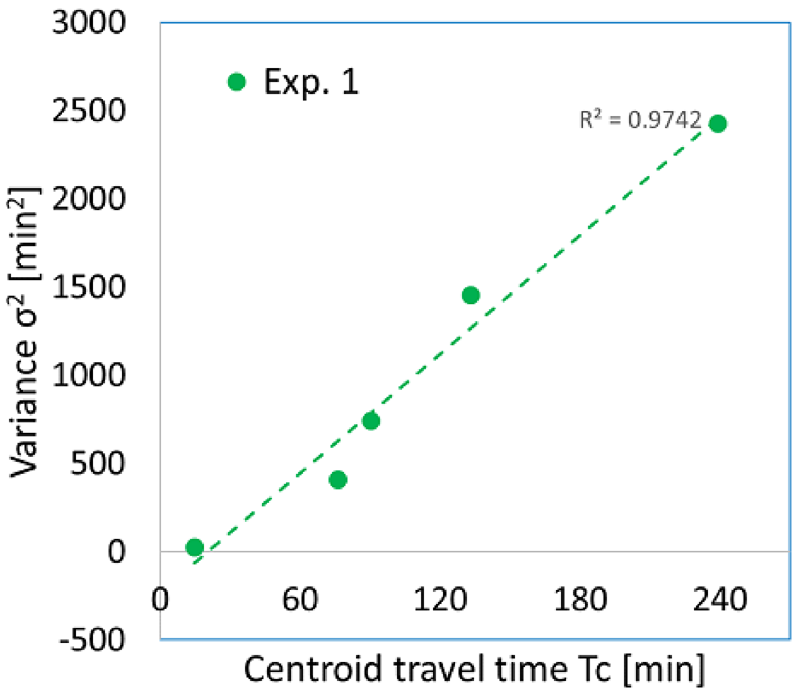
Thank you for noticing.

Line 154: Upstream should be "1" and downstream should be "2" to be consistent with Equation 2 and cross-section numbering increasing further downstream (Figure 4).

> We are sorry for the overlooking. We will revise the text (1=upstream, 2=downstream).

Line 172-173: Considering plotting variance against travel time. If mixing was complete, this should be a linear relationship, e.g., Rutherford (1994) Figure 4.4. This could be a good complement to Figure 7.

> Yes, we have plotted variance (actually, variance against the distance), but we did not include them in the final manuscript. See the figure with plotted variance against the centroid travel time in the attachment (Fig. R1_3). We will add the figure in the revised manuscript after Figure 8.



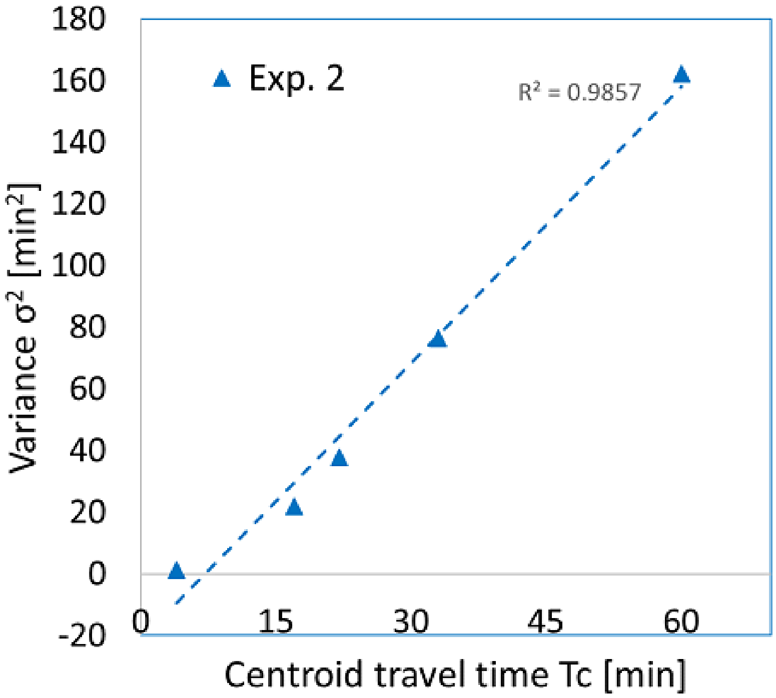


Fig. R1_3. The variance of the concentration distributions against the travel time for Exp. 1 (left) and Exp. 2 (right).

Line 187/Figure 8: Is this figure required? The peak concentrations can be read relatively easily from Figure 6.

> We created this figure to better show where the concentration peak is, depending on the distance from the release point.

Line 194-196: It might be worth commenting that dispersion in natural channels can vary significantly, increasing the challenge of creating these data sets (e.g., Rutherford, 1994, Table 4.2).

> Thank you for your comment. We will expand our discussion accordingly and add the references to the Rutherford.

We will change lines 194-196 accordingly: *"Longitudinal dispersion coefficients in natural channels can vary significantly (see, e.g. Rutherford, 1994; Heron, 2015). Due to the large variety of conditions in rivers and canals, the reported values may differ by several orders of magnitude. Although there are not many data sets available for the longitudinal dispersion coefficients in small natural streams (Heron, 2015), particularly for low flows, the values of the coefficients obtained during both experiments under not vegetated conditions (from 1.27 to 1.77 m2s−1) are in good agreement with those previously published and collected by Heron (2015)."*

Line 206/215/227/Figure 11: Please comment on the limits of applicability of these relationships.

> Thank you for pointing out that the applicability needs to be more clearly stated. We are aware that these conclusions refer only to our results (lines 236-238). In the revised version, we will further stress the limits of applicability of the proposed relationships.

To highlight the case-specificity of the formula, we will revise line 206 to: *"If we assume that UNV/UVEG = 1 when V = 0, linear regression analysis indicates that under the presently studied conditions, the influence of the vegetation cut on the flow velocity can be approximated as UVEG = UNV/(0.03V + 0.9) ".*

We will revise and amend lines 214-215 as: *"We assume that the linear dependency between velocity change and vegetation coverage can be extended as a first order approximation to other trapezoidal channels with such high vegetation coverages >~68%. However, the slope coefficient of the formula likely depends on channel geometry and flow forcing, and the formula should be evaluated against a substantially larger dataset. The dependency may deviate from linear at coverages lower than presently investigated"* .

Line 232: Should this be a reference to Figure 11?

> We are sorry for the mistake, and we will update the reference to Fig. 11.

Line 273: "pollutants concentration" should be "pollutant concentrations".

> Thank you, we will revise the text following your comment.

Figure 6: Is the y-axis C/Cmax? A relative concentration of some kind? Please revise the y-axis label.

> Yes, you are right; this is a relative concentration. As written in the figure caption on the y-axis, the normalised concentration distribution is presented. We normalised the concentration with the maximum concentration in the first cross-section P1.

We will change the label of the y-axis on Figures 6 and A1 from *"Concentration [-]"* to *"Normalised concentration [-]"*.

We will also stress this in the text at line 175, adding the sentence: *"The concentrations have been normalised by the maximum concentration value recorded in the first cross-section P1."*

Figure 6b: Please expand the x-axis to make the recorded profiles clearer.

> We may revise the figures accordingly. However, the present x-axes were used to compare the two experiments in terms of their duration.

Figures 9 and 11: Consider making the circles for the main vegetation reaches green to be consistent with Figure 10, and similarly adjusting the symbols/colours in Figure 8, etc.

>Thank you for the hint.

In the revised version, we will use the green colour for Exp. 1 (with vegetation) and blue for Exp. 2 (without vegetation) in all figures (7-11).

Figure A1: Consider plotting against time since injection on the x-axis and plotting the two concentration data on the same axes for a more direct comparison.

> Thank you for the comment; we will consider it in revising Fig. A1.