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Reply on RC2

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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-AC2, 2022

RC2: 'Comment on hess-2022-208', Anonymous Referee #2, REPLY

General comments

The authors have conducted and described field research and analysis of the process of solute transport in the open channel under various conditions of the occurrence of aquatic vegetation. The work is particularly interesting from the point of view of river engineering and eco-hydrology. I recommend this paper for publication after discussion and revision.

> Thank you very much for your comments. We will revise the text to address all the weaknesses that you pointed out. Below we discuss your specific comments.

Specific comments

My main doubt is the definition of the mean water flow velocity U (UNV, UVEG), the changes of which the authors analyze in section 3.2 Influence of vegetation maintenance of flow hydraulics. If it is the value calculated according to formula (3) (line 159) and presented in Table 1, it is the speed of pollutant transport in the channel rather than the average water flow velocity (understood as U=Q/A, where A is the cross-sectional area). Here, the speed of movement of the substance is the result of both advection (water movement) and dispersion (the mechanical one, as well as the one resulting from the water flow velocity distribution). This should be explained and discussed by the authors. It would be important to show in the article what were the average flow velocities resulting from hydrometric field measurements (also examples of velocity distributions in channel sections) and to compare them with the solute transport speed. So, while I am convinced by the presented relationships between vegetation cover V and solute transport parameters (point 3.3, Figs. 10, 11), the conclusions about the influence of vegetation on the flow hydraulics are debatable. In my opinion, according to formula (3), the velocity U is also a parameter characterizing the transport of solutes, not the flow hydraulics. In my opinion, the confirmation of

this is the linear dependence of U(V) and DL(V).

> Yes, the value of U is calculated according to equation 3, using the method of moments based on the concentration data. It is the sub-reach mean velocity, which may not be exactly the same as the mean velocity computed based on the hydraulic measurements (due to different possible sources of error in both methods). However, given that the computations based on eq. 3 consider the time of passage of the centroid of the dye plume (not a peak), the obtained values should be a good representation of the mean sub-reach velocities. Indeed, they should be better considering also the uncertainty of hydraulic measurements due to low water level conditions and the presence of vegetation. Actually, for the channels for which a lot of cross-sections are necessary to determine mean velocity accurately or when hydraulic measurements are not possible due to other reasons, the methods based on the tracer studies are widely used (Rutheford, 1994). Unfortunately, we cannot compare both values obtained for our studies due to the low number of cross-sections used for averaging, and the uncertainty associated with our hydraulics measurements.

In fact, due to operational limits. (i.e., manpower and time, we had to finish all of them before the dye release to not disturb the velocity field and mixing during the tracer study), the hydraulics measurements focused only on deriving the flow rate. Therefore, all our calculations presented in the paper are based on the data derived from measured concentration profiles and vegetation coverage obtained using remote sensing techniques.

In the revised version we will more precisely describe (including Table 1) the mean sub-reach velocity to be clear that the value is obtained based on the measured concentrations. To avoid misunderstandings, we will improve the text according to these observations pointing out possible sources of uncertainty. We will also re-arrange the section related to the influence of vegetation on the hydraulics, to stress the limits of applicability of the observed findings (see also reply to the reviewer RC1).

My second general remark is that in my opinion a limited number of field measurements (covering the canal with vegetation 70-100%) is definitely not enough to present a linear relationship as in Fig. 9. But the authors are aware of this, as they write about in the article , however, the question is always whether this is the right time to publish these results. Perhaps it would be better to present the current results in the form of a scientific communication and publish the final results after completing the measurements and analyzes.

> Vegetation plays a major role in rivers and channels, and therefore it is becoming the main topic in many research projects. However, due to the costs, complexity and uncertainty of field measurements in natural channels, most of the ongoing research are concentrated on laboratory or semi-laboratory investigations for a selected vegetation type (e.g., submerged or emerged conditions), selected species or natural-like vegetation elements. Such studies are precious. However, the need for real cases data requires moving toward the analysis of the influence of vegetation on flow and mixing at the reach scale, as pointed out by many researchers (e.g., Nepf 2012b; Sonnenwald et al. 2017; Rowiński et al. 2018, Rowiński et al. 2022). Such research, looking at straightforward parametrisation and easy-to-apply metrics with sufficient explanatory power, is also unavoidable to be able to exploit the results in practical applications. These were the motivations of our study. We are aware of the limitations of our results, also in terms of vegetation coverage, and we acknowledge that, to extend our conclusions to other coverage values, more experiments are definitely needed.

However, the obtained results are valid in the analysed conditions, and since not many such experiments exist, we believe that our results are very valuable and worth to be shared with other scholars. Moreover, they may also suggest the research direction to other researchers. The additional conditions that we wish to analyse to have a complete picture are difficult to capture within the reasonable time of the study (the conditions are highly natural and weather dependent).

The obtained values of longitudinal dispersion coefficients are in themselves valuable, since they are not many of them available for small channels in low flow conditions, especially for naturally highly vegetated canals. We believe that they are a valuable contribution to longitudinal dispersion studies in natural streams.

To more clearly describe the limits of applicability of formulas in Fig. 9, as replied to Reviewer 1, we propose to revise and amend lines 214-215 roughly 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 forces, and the formula should be evaluated against a substantially larger dataset. The dependency may deviate from linear at coverages lower than presently investigated."*

We will also change Fig. 9, limiting the trendline to the vegetation coverage area that we analysed.

As pointed out by the reviewer, the obtained results are limited to the high vegetation coverage conditions and should be complemented by observations performed with different hydrological and vegetational conditions. Therefore, we will update the conclusions, stressing this need more and adding that we are going to perform such investigations in the future, aiming to provide more general results and conclusions applicable also beyond the present study conditions.

Technical Comments

Lines 197-199. Show examples of measured flow velocity distributions in channel sections.

> As we mentioned above, we performed the hydrological measurements for estimating the water discharge, selecting the most appropriate and accessible cross-sections for such a scope. Due to not having a sufficient number of measured cross-sections and the better accuracy of the concentration data (which actually were our main investigation), calculations presented in the paper are based on the data derived from measured concentration profiles and vegetation coverage obtained using remote sensing techniques.

Line 115. 'Using map algebra…' What does it mean?

> 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.

Lines 131-137. What was the frequency of the measurements?

> 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 we will provide in the IG Repository (https://dataportal.igf.edu.pl/).

Figure 7. There is no green triangle for the P5 - distance 506 m

> Thank you very much for having spotted it. We will update the figure.

Lines 213-214 '… and Figure 7c in Biggs …' I am not sure if this way of referring to the results of other researchers allows the reader to easily follow the discussion of the results

Thank you for pointing out this. To provide readers with more information on the results of other researchers, we propose the revision: *"However, qualitatively similar results can be inferred from Biggs et al. (2021), who reported an approximately doubled mean velocity when vegetation coverage was reduced from ~80% to 0%, and from Verschoren et al. (2017), who found that vegetation removal from the coverage of 90% to 0% decreased flow resistance to one fourth, indicating a substantially enhanced mean velocity.* "

Line 232 There is Fig.10, it should be Fig. 11

> Thank you very much for noticing this. We will update the reference to Fig. 11.