Author's response to Editor's decision and comments from Reviewers

Influence of vegetation maintenance on flow and mixing:
case study comparing full cut with high-coverage conditions

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We would like to thank the Editor and Reviewers for all their comments and suggestions. In the revised version, we improved the text and figures, addressing all the issues raised by the reviewers. We hope this revision is satisfactory for the further processing of this paper.

Dear Editor,

we would like to thank you very much for your feedback. We revised the manuscript in detail, aiming to answer all reviewers' points and provide more insights into the study rationale and the obtained results.

We provide answers to comments from Reviewers below. For clarity, each answer is structured as follows: **(1) RC# comments from Referees (blue)**, (2) > author's response (black).

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 manuscript, which allows 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 2019.

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 usual in the field circumstances. 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.

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 it was noted in the table footnotes and the 173 lines, 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 stressed this better in the revised paper, and taking this into account, we changed the text 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 revised the text, pointing out possible sources of uncertainty and including the slope effect in Fig. 9. We added an additional paragraph in section 2.1, and we rewrote section 3.1.

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. 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. 11) in the revised manuscript, complementing Figure 10. 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 discussed the problems associated with DL scaling more extensively. We added and commented on (the second to last paragraph in section 3.2) the figure showing the DL/U (Fig. 11).

The authors have not included a data availability statement.

> Together with publishing the paper, we will make available the data used in the manuscript published in the Zenodo Repository.

Kalinowska Moninka B. (2022). Dataset of tracer study experiments on influence of vegetation on flow and mixing in small channels [Data set]. Zenodo. https://doi.org/10.5281/zenodo.7385385

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 expanded the information about the longitudinal dispersion coefficients in the Introduction section

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 include 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 Fig. 5 in the revised manuscript. Practical applications with “disorderly” natural vegetation motivated our work, which aimed at investigating physically sound but straightforward to measure parameters like vegetation coverage.

We extended section 2.2 by adding mentioned information about the encountered vegetation in the channel (together with Fig. 5) and better describing our motivations and ideas concerning the usage of the vegetation coverage.

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 added 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 not to 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 concentration profiles and the vegetation coverage obtained using remote sensing techniques. To improve the clarity, we decided to split Tabel 1 into two parts (Tablas 1 and 3 in the revised version), moving the first part, describing the properties of the channel to the study side description (Section 2.1) and placing the second part counting the data obtained during the tracer studies, used in the analysis, to the results section (Section 3).

The acquired data on concentration and vegetation coverage, used in the computations presented in the paper, as mentioned earlier, will be provided in the Zenodo Repository and linked 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 literature (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 method of the moments (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 commented on the possibility of using other methods (the second paragraph in Section 2.4). We stressed more precisely that we collected the water samples in unequal time intervals (Section 2.3).

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 reported the data for each sub-reach, while Table 2 reported information for each cross-section. Accordingly to the Journal's guidelines, tables were placed where they were cited for the first time. However, considering your comment and to increase the paper's clarity, we decided to split Table 1 into two Tables (1 and 3 in the revised version). Now, the dispersion coefficients are presented after the concentration profiles.

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

> Thank you for the comment. We removed *"On the other hand"*.

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

> Thank you for the comment. We changed 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 changed 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 revised 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 changed the text accordingly.

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.

In the revised paper, we expanded the text by adding the citation to the paper describing the using of Map algebra in GIS studies (Cmara et al., 2005)

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

During Exp. 1, we started sampling with 10 minutes intervals (except for the cross-section 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 quick, we sampled faster. Sampling frequency varied from 1 to 10 minutes. We sampled more frequently, close to the expected peak of concentration (from 30 seconds in P1 to 1-3 minutes in other cross-sections), and less frequently for the tailing edge from 5 to 10 minutes. The sampling period was adjusted to the actual cross-section concentration changing (using a handheld fluorometer on site).

We revised the text to clarify this point and added the sampling frequency information to the second to last paragraph in Section 2.3. The frequency may also be derived from the data that will be provided in the Zenodo 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 decided to not include the values and figures with mentioned leading- and trailing- edges in the text. Therefore we removed 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 revised 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.

> We added the figure in the revised manuscript (Figure 9).

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., but yes, the peak concentrations may be read from the other figure, so we removed the mentioned figure in the revised version.

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 expand our discussion accordingly and added the references to Rutherford (paragraph just before Section 3.1).

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 further stressed the limits of applicability of the proposed relationships.

To highlight the case-specificity of the formula, we revised 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 revised and amended lines 214-215 (see Section 3.1).

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

> We checked all figures and the references in the last version of the manuscript.

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

> Thank you, we revised 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 changed the label of the y-axis from *"Concentration [-]"* to *"Normalised concentration [-]"*.

We also stressed this in the text, 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.

> The present x-axes were used to compare the two experiments in terms of their duration. However, we expanded the x-axis in Fig. A1, also presenting the results from Exp. 2 together with the Exp. 2'.

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 use the green colour for Exp. 1 (with vegetation) and blue for Exp. 2 (without vegetation) in all figures.

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 prepared Figure A1 according to your suggestion.

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 revised the text to address all the weaknesses that you pointed out. Below we are providing our detailed feedback on 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, also considering 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 measured 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 to disturb the velocity field 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 more precisely described (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 improved the text according to these observations, pointing out possible sources of uncertainty. We will also re-arranged 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). We added an additional paragraph in section 2.1, and we rewrote section 3.1.

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 revisde and amend text in Section 3.1, we added an additional paragraph in section 2. We also slightly changed Fig. 9.

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, updated the conclusions (last sentence in the second paragraph in Section 4), stressing this need of 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 flow discharge, selecting the most appropriate and accessible cross-sections for such a scope. However, due to not having a sufficient number of measured cross-sections but much more accurate concentration data (which was 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. To better clarified the issue we extended Section 2.1, adding the two last paragrphs.

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.

In the revised paper, we expanded the text by adding the citation to the paper describing the using of Map algebra in GIS studies (Cmara et al., 2005)

1. 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 started sampling with 10 minutes intervals (except for the cross-section 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 quick, we sampled faster. Sampling frequency varied from 1 to 10 minutes. We sampled more frequently, close to the expected peak of concentration (from 30 seconds in P1 to 1-3 minutes in other cross-sections), and less frequently for the tailing edge from 5 to 10 minutes. The sampling period was adjusted to the actual cross-section concentration changing (using a handheld fluorometer on site).

We revised the text to clarify this point and added the sampling frequency information to the second to last paragraph in Section 2.3. The frequency may also be derived from the data that will be provided in the Zenodo repository mentioned above.

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

> Thank you very much for having spotted it. We updated 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 rewrited the text in the last paragraph of Section : *"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 updated the reference to figure.