

Interactive comment on “Rapid and objective characterization of channel morphology in a small, forested stream” by Carina Helm et al.

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Dear Associate Editor, and Reviewer 1:

Authors have responded to all comments below in full, with Reviewer 1 comments shown in bold, and responses as regular text. Notable new additions to the paper include, a simplified title, and sharpening of sections at the request of Reviewer 1. See below for detailed replies to all queries from Reviewer 1.

REVIEWER 1 COMMENTS

C1

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General comments
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The topic of the paper is interesting and tackles an important question related to the efficient field measurements of the river systems, which are having forest canopy. The paper is overall good, and especially the researchers from the fields of remote sensing and fluvial geomorphology will be interested in reading it. The authors have done huge work in field and with data processing. The methods are up-to-date and the paper is unique. However, before being possible to publish it, the manuscript would need clarifications in many sections, and rearrangement of the sentences / paragraphs. The terminology related to the spatial scales would be needed to define more precisely, so that readers would understand more easily what is meant with large, small etc. Overall, precision in the statements would make the paper more easily readable. The justification of the paper would be needed to write more clearly in the introduction section. The texts and figures presented in the results and discussion sections would need also re-arrangement. Also attention should be paid to the sub-titles. Overall, clarification of the text and justification of the importance of the selected topic, methods and gained results would be needed throughout the paper. Therefore, major modifications are suggested.

The authors are grateful for the constructive comments and thoughtful suggestions from Reviewer 1. Improvements have been made to the paper to add precision to our statements. In addition, we have incorporated more text into the introduction clarifying why we believe RPAs should be considered for surveying small forested channels and added justification for the variables included in our study. We have also sharpened several of the subtitles in the paper and reorganized some portions of the text at the

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request of Reviewer 1. These changes are further described in the queries below.

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Specific comments
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Title of the paper: Consider deleting words rapid and objective from the title. Introduction or aims do not include these words, and justification and need of the rapidness of the techniques does not come clearly evident from the introducing sections. Or, if wanting to keep those words, add description about the rapidness and objectivity of the approach in the introduction section. I also suggest that the close-range remote sensing approach could be good to appear in the title some way or another.

The comment has been accepted. At the suggestion of Reviewer 1, the title has been changed to "Characterization of morphological units in a small, forested stream using close range RPA imagery".

Abstract: The following sentence is slightly contradictory, as you talk about both large areas and small streams. "This paper seeks to demonstrate an objective method for characterizing channel attributes over large areas, using easily extractable data from RPA imagery collected under the forest canopy in a small (width = 10 to 15 m) stream.." What do you mean with large areas? Could you clarify and modify the sentence so that it does not cause the reader to be confused between the different spatial scales under question.

The intention was to highlight that the survey was conducted in a small forested

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stream, over a large section of the channel's longitudinal profile (3 km). At the suggestion of Reviewer 1, we have modified this sentence in the abstract of the revised text with the following text:

"This paper seeks to demonstrate an objective method for classifying channel morphological units in small, forested streams and to provide information on the spatial scale necessary to capture the dominant spatial morphological variability of these channels. This was achieved using easily extractable data from close-range RPA imagery collected under the forest canopy (flying height = 5 - 15 m above ground level) in a small (width = 10 - 15 m) stream along its 3 km of anadromous salmon-bearing channel."

Abstract: "The results demonstrate that sub-canopy RPA surveys provide a viable alternative to traditional survey approaches for characterizing these systems, with 87% coverage of the main channel stream bed." Cold you specify already here, what are the traditional survey approaches? Does this relate to the flight altitude?

We have adjusted this line to show that the intent was to refer to ground-based approaches (e.g. total station, automatic level) with the following text:

"The results demonstrate that sub-canopy RPA surveys provide a viable alternative to traditional ground-based survey approaches for mapping morphological units. . ."

These classification approaches are further described in the introduction of the revised text, which have historically been better suited and more widely applied to streams like Carnation Creek, with the following text:

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“Traditionally, characterization and classification of channels through field surveys has required the use of a variety of GPS-based tools and linear-survey methods involving automatic levels, theodolites, and total-stations”

In addition, it would be actually important to also mention the flight altitudes (etc. details, which show how your method differed from the traditional approaches) in the abstract, as I would imagine that in the sub-canopy flights the height of the platform was low.

This is correct, the flying height of the RPA was quite low. This information has been added to the abstract of the revised text with:

“This was achieved using easily extractable data from close-range RPA imagery collected under the forest canopy (flying height = 5 – 15 m above ground level) in a small (width = 10 – 15 m) stream along its 3 km of anadromous salmon-bearing channel.”

Lines 20-22: You mention that “These characteristics can lead to a high degree of spatial variability and...”. Could you clarify the sentence, especially “spatial variability” of what? Both the first and second sentence of the introduction are slightly vague, and would need clarification, so that the start of the introduction would be stronger. It feels like there is repetition also in those first two sentences. Thus, make the beginning of the introduction sharper.

We have merged these sentences together with the sentence below to clarify:

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“Their classification may be particularly important in forested, gravel bed streams, where episodic and transient geomorphological processes (Pryor et al., 2011; Wohl and Brian, 2015; Hassan et al., 2019), can lead to a high degree of channel complexity, even within a relatively homogeneous channel type (Madej, 1999; Nelson et al., 2010; Gartner et al., 2015).”

At the suggestion of Reviewer 2, the first two paragraphs of the introduction haven been reworked for clarity and terminology as well.

In addition, it would be good to mention already in the first paragraph in detail what are the channel characteristics, which are important for the “management”, and for the study, and why those are important? Is it only gradient, as that is the only one mentioned? The justification for the variables/metrics and their wider applicability does not come clear from the introduction. Therefore, the sharper beginning of the introduction and also more clearer justification for the study (parameters, and why their detection is important) would enable the reader to understand the uniqueness and importance of the paper more clearly.

The authors agree with this suggestion. This has been incorporated with the following lines:

“RPAs are likely advantageous in these systems, as they may easily permit the extraction of a greater set of variables to aid in channel classification. These variables include features such as channel slope, water depth, and grain size characteristics, all of which reflect larger basin-scale controls on channel morphology (Buffington and Woodsmith, 2003). Channel slope is a key variable to consider, as it has been

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shown that there is a general progression of channel morphologies from pool-riffle, plane-bed, and step-pool to cascade morphologies with increasing slope (Montgomery and Buffington, 1997). Water depth metrics are important for discriminating between pool environments and other shallow water environments. Finally, grain size is a key variable, as there tends to be a coarsening in bed material from glides and pools to riffles and runs. Acquiring this suite of variables is a difficult task, one that RPAs may be uniquely suited to.”

Lines 50-54: The authors refer to Kasvi et al. (2019). That study has been done in a river system, having small channel width especially during the low flow periods. Therefore, please clarify the sentences so that the readers do not get an idea that Kasvi et al. (2019) paper has been done in larger river system. Again, please, define also in those lines 50-54, what do you mean with larger system / how do you define larger system?

The authors appreciate this comment from Reviewer 1. Relative to Carnation Creek, the authors would consider the stream investigated by Kasvi et al., (2019) to be large. We have added the definition of channel size from Hassan et al., (2005), to the following lines of the revised text:

“However, much of this work has been limited to larger systems. Herein we consider the classification by Hassan et al., (2005) for small to intermediate streams in the Pacific Northwest as those where the ratio between bankfull channel width to wood length is close to or greater than one and the ratio between log diameter to bankfull depth is close to or greater than one (see Table 2 of the paper for more details). Streams on the intermediate side of this spectrum, where the ratio between bankfull channel width to wood length is close to one, differ from larger systems as they can be greatly influenced by wood delivered to the channel. They are often overlain by dense

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forest canopies that are poorly suited to observation from above the forest canopy. This limitation has historically excluded a large fraction of river network length from RPA-based surveys.”

Line 57: What is meant with “continuous RPA-derived data”? Is that spatially or temporally continuous?

The authors had intended to be referencing the fact that data acquired from an RPA is continuous across the channel (i.e. space, rather than time). By contrast, field methods for surveying channels often involve discrete cross sections or points that must be interpolated. This has been clarified by adding “spatially continuous” to the sentence.

Lines 100-101: What is the altitude of the low-level flights? Please, specify already here (i.e. where you first time mention these flying specifications), and not in he later sentences.

We have rearranged the paragraph so that the flying specifications appear when the RPA survey is first introduced in the revised text.

Lines 100-102: You write “The RPA survey involved low-level flights conducted in tandem with placement of Ground Control Points (GCPs) that were surveyed with a Leica TPS 1100 total station.” Did you take the reference points from the sub-water areas also? Or how do you calculate the accuracy of the bathymetry cells, which you talk about in results section 4.1? Please add in the methods section clearly, how the reference points for these RMSE and ME calculations were measured, and did you measure them also from the sub-water area and

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how (also with a total station similarly as the dry land areas and the GCPs)? Thus, some clarification and sharpness would be needed to the methods section also.

The authors have reworded these lines to make the survey method clearer. To reiterate, all GCPs and checkpoints were surveyed with a Leica TPS 1100 total station. The GCPs were only positioned on the dry exposed bars, whereas checkpoints include both the dry exposed bars and submerged points. This is described in the following lines of the revised text:

“A minimum of ten ground control points (GCPs) were placed along dry exposed bars in each of the 80 channel segments to provide precise image georeferencing, with additional points positioned on the dry exposed bars and below the water surface in order to serve as independent checkpoints, to assess the accuracy of the model outputs. All GCPs and checkpoints were surveyed with a Leica TPS 1100 total station. The majority of the GCPs were distributed in a zig-zag fashion along dry exposed bars in the periphery of the channel segments, with a smaller number situated towards the centre.”

Line 114: You mention riparian vegetation here for the first time. How high is the riparian vegetation and what are the species. Was there grass and shrubs, or do you mean the “dense forest canopy composed of both coniferous and deciduous tree species”, which you talk about in the study site section? In addition to mentioning the heights of the riparian vegetation (which were cleaned away from the data based of the filters), it would be good to also introduce the riparian vegetation in the study site section.

This comment has been accepted and is addressed with the following lines in the

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study area section of the revised text:

“The riparian vegetation includes a variety of tree species including western hemlock (*Tsuga heterophylla*), Amabilis fir (*Abies amabilis*), western redcedar (*Tsuga plicata*), Sitka spruce (*Picea sitchensis*) and red alder (*Alnus rubra*). The height of the riparian canopy is variable, between approximately 15 and 40 m. The riparian forest floor is composed of a variety of ferns and shrubs, e.g., salmonberry (*Rubus spectabilis*), sword fern (*Polystichum munitum*), trailing blackberry (*Rubus ursinus*) and thimbleberry (*Rubus parviflorus*), that may provide some cover to the channel.”

Relates to the methods and discussion section: Did the canopy effect on the pixel values of the water area? As you defined the bathymetry based on Dietrich et al. (2017) method, did the shadows and reflections of the canopy harm the water pixel colors and bathymetry calculations? What was the turbidity of the water? That information would be important to add, from the measurement times. The success of the Dietrich et al. (2017) method could depend on how turbid / clear the water was. Please, discuss about this in the discussion, and present how the turbidity was taken into account in the methods section.

At the suggestion of Reviewer 1, we added additional text pertaining to this in the revised text with:

“The method requires that the water be clear such that the channel bed can be captured. The low flow conditions present at the time of the survey resulted in clear water that permitted viewing of the channel bed. Removal of overhanging vegetation using the Cloth Simulation Filter in Cloud Compare, and subsampling the DEMs to a spacing of 0.02 m using the minimum elevations in the point cloud, helped to

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ensure that the refraction correction was based on channel bed points, and not on overhanging vegetation points that may have been incorporated in the point cloud.”

As now described in the paper, Carnation Creek was very clear and turbidity was not an issue at the time of the survey. The point cloud was cleaned to remove anomalous points from overhanging vegetation that may have been incorporated into the cloud. The individual effect of factors such as shadows and reflections on the bathymetry calculations was not investigated, but rather the total errors between RPA derived submerged elevations and total station measured elevations presented in Fig. 4.

Lines 139 -145: The authors introduce here the method for grain size estimation. However, this is the first time grain sizes are mentioned in the manuscript. Thus, there is no background literature in the introduction section, or justification why this calculation is important to conduct. You mention “a metric often of interest to river managers”, but it would be important to justify here, why these metrics are important for your study. Please, add in the introduction and/or in the methods section, why the grain size is needed to be defined. To some readers the necessity to define the grain sizes is not self evident.

See our reply to your previous comment on “In addition, it would be good to mention already in the first paragraph in detail what are the channel characteristics, which are important for the “management”, and for the study, and why those are important? “. We have reiterated this in the methods section where we note that grain size is a frequently described metric in classification schemes, such as Montgomery and Buffington’s (1997) classification scheme.

Line 173: I am not a native English speaker, but I think this following part of the

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sentence is missing one preposition “data along the. . . change it as “data along WITH the first”.

We have clarified this sentence with:

“Following the PCA, the k-means clustering algorithm was run to identify groupings that may have been present in the data along its first three components.”

Many of the figures appear only within the discussion section, and the results defined in some of the figures are not analyzed in detail in the text of the results section. For example, Fig. 8 appears on page 15, but it is talked with two sentences on the page 10. Thus, rearrange the appearance of the figures so that the text and figures appear “hand-in-hand”.

The authors accept this comment. The figures have been rearranged such that the text and figures appear “hand-in-hand”.

Despite the channel morphology was one of the main topics talked in the introduction section, the channel variables and the results of the morphological detection have not been given full attention in the results section. So, please, add text in the results section related to the morphological characteristics and their spatial variation.

The authors accept this comment. We have added text to section 4.2.1 further describing differences between the identified channel types and their relative positions:

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“However, plane-bed and coarse riffle morphological units are mostly located near the upstream limit of the survey extent in this region. This area represents the outlet and downstream entrance of the canyon reach, where steeper gradients and coarser sediment are found. This is highlighted in Table 2, which shows that on average these morphological units are located 3160 m upstream, with steep reach scale slopes of 0.042 m/m and coarse material with an average D50 of 8.21 cm. Similarly, the coarse riffle morphologies were located approximately 2980 m upstream on average, with relatively steep slopes and coarse material (reach scale slope = 0.0024 m/m and D50 = 6.74 cm). By contrast, the average positions of the riffle, glide, run and pool morphologies were approximately 1500 m, midway along the channel’s profile, which follows suit with the uniform distribution of these morphological units. Grain size was generally similar between these morphological units, except for the riffle unit, which was slightly coarser with a D50 of 4.10 cm. Pools were the deepest, with average water depths of 1.04 m and near zero water surface slopes, whereas riffles were the shallowest with average water depths of 0.13 m and relatively steep water surface and reach scale bed slopes. Glide and runs were intermediate between these morphologies, with glides often retaining negative local slopes, corresponding with the exit of pools, and runs with large positive local slopes, corresponding with the entry of pools.

Discussion: Many of the sentences (such as on lines 249- 255, and 278–285) should already be presented in the results section. Therefore, rearrangement of the discussion would be needed. I am not pointing out all of the sentences in question, as there are many of them. My advice is that when you present something for the first time based on your analysis or the data sets, move those sentences under results section. Discussion is then reflection of your results (presented already previous sections) against other studies.

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The authors appreciate this perspective from the reviewer. It was the intention of the authors to have the Discussion reiterate the key findings of the study, and then introduce relevant references that help situate these findings. We have reworded lines 249-255 of the original text to make relationships between reiterated findings and references included clearer.

Regarding lines 278-285 of the original text, the table those lines discuss was included to make it easier for the reader to compare the mean values of the morphologies in our study to others in the literature. The values presented in the table for our study were extracted from Table 2 in the results. As suggested by Reviewer 2, we have also added a new Table 1 to the introduction summarizing the criteria for the classification schemes by Church (1992), Anonymous (1996) and Buffington and Woodsmith (2003). As those lines situate the results of our classification scheme in the literature, the authors feel they are still suited to the discussion and are in line with the reviewers suggestion that the “Discussion be a reflection of your results (presented already previous sections) against other studies”.

Many of the sub-titles of the results and discussion section are methodological in their nature. Go through the titles of the manuscript and modify them so that they show that it is results and discussion in question, and not an introduction to the methods. Now the titles give slightly different idea of the content than what the content actually is: such as, “5.2 Classification approach” sounds like the section would include an explanation how the classification method was used, even though it is discussed about the “success of the classification approach”. Thus, the titles of the results and discussions sections are misleading.

The authors accept this comment. As suggested by Reviewer 1, we have changed the titles for the following sections: 4.1, 4.2. 4.2.1, 4.3, 5.1, 5.2.

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