Final Author Comments – Notes on estimation of resistance to flow during flood wave propagation

M.M. Mrokowska, P.M. Rowiński, M.B. Kalinowska

We would like to thank both Reviewers. First of all, we would like to thank Reviewer#1 for encouraging comments and suggestions on how to improve the manuscript. We are grateful to Reviewer#2 - Prof. Tommaso Moramarco for his thorough review of the paper, which stimulated us to clarify a number of issues.

General comment:
The target group of this paper are those who study processes in unsteady flow which are tightly connected with resistance to flow. Good representation of resistance in unsteady flow is necessary, for example, in research on sediment transport. We believe this paper will be interesting also for a wider group of hydrologists, as it touches fundamental problems of resistance to flow which are significant in every aspect of hydrology and hydraulic research.

The motivation of this manuscript stems from the rising interest in application of friction velocity formulae derived from flow equations in unsteady flow studies. We recommend to use friction velocity as a variable expressing resistance to flow, and for this reason application of formulae on friction velocity is widely described. As Manning n is the most popular resistance parameter, we feel that it is beneficial to discuss its applicability in unsteady flow. As some pieces of research (cited in the manuscript) showed that trends of Manning n are highly dependent on channel geometry conditions, we would like to stress that Manning n is not a universal parameter, and it may be difficult to interpret. Moreover, it has not been created originally for unsteady flow conditions, so it should be applied and interpreted with caution. In the paper, we compare trends of friction velocity and trends of Manning n to show problems with interpretation of results that one may face.

We would like to stress that the paper is not on flood routing, and all methods applied are analytical and based on measurement data of artificially created flood waves in a small river.

When it comes to novelty, this paper discusses the problem of evaluation of resistance to flow taking into account aspects not discussed in literature so far. In this context, we present two crucial issues that have not been raised in the literature, despite the fact they are of primary importance in efficient assessment of resistance: (1) high uncertainty of resistance to flow evaluated by formulae derived from flow equations and methods of minimizing this uncertainty, and (2) interpretation of variable Manning n in unsteady flow in the light of variable resistance to flow. As pointed by Reviewer#1, "one of the important findings of this manuscript is the description of how to determine the friction velocity for unsteady discharge" - we provide comprehensive review of existing methods with extensive comments and our own methods.

Current knowledge and methodology of evaluation resistance in unsteady flow is inconsistent. Hopefully, this paper will contribute to systematic research on resistance to flow in the future.

We provide answers to comments from Referees below. For clarity each answer is structured as follows: (1) RC# comments from Referees, (2) AR# author's response, *(3) AC# author's changes in the manuscript.*

Answers to Comments of Anonymous Reviewer #1

RC#1: The introduction and title suggest that the manuscript is about the difference between friction velocity and roughness coefficients. This is only a very small part of the manuscript, the majority deals with determining the friction velocity itself. This shows exactly the issue in dealing with friction velocity which is not stated in the conclusions: friction velocity is highly uncertain and difficult to determine. However, the authors still suggest to use friction velocity. This requires at least a thorough discussion after section 3 or in the concluding remarks.

AR#1: We have expanded discussion on differences between friction velocity and Manning n to balance the proportions (please refer to the revised manuscript). Manning n is equally difficult to evaluate in unsteady flow as friction velocity. In fact, the same methods are applied in this study to evaluate both of them – the relationships derived from flow equations. We stress it in the revised version. We suggest using friction velocity to describe resistance to flow (excluding numerical modelling where Manning n is used as a parameter) throughout the paper. The main argument is lack of sound physical interpretation of Manning n. Our recommendation is illustrated and strengthened by the comparison between the trends of friction velocity and Manning n in Section 4 - Results. In fact, friction velocity is uncertain, and this paper aims at, among others, giving suggestions on how to minimize this uncertainty.

AC#1: We have added in Conclusions:

As friction velocity is recommended to express flow resistance, the method of friction velocity derived from flow equations is scrutinised. It also applies to Manning n. Analysis of resistance evaluation in the Olszanka River shows that friction velocity is a demanding task, highly uncertain, and difficult to determine.

RC#2: Secondly, the structure of the manuscript can be improved. Now it is difficult to distinguish new findings by the author from existing knowledge from literature. These should be in different sections (like separate data, method and results sections).

AR#2: We have restructured the paper to make it more clear. Now it is divided into 1 Introduction, 2 Experimental data, 3 Methods, 4 Results, 5 Conclusions.

RC#3: In my view one of the important findings of this manuscript is the description of how to determine the friction velocity for unsteady discharge. This part of the research is not introduced properly in the introduction and should be one of the objectives, so explicitly state that in the introduction.

AR#3: We have reformulated the objectives of the paper, and clarified the motivation for this study.

AC#3: We have added:
The objectives of this paper are (1) to describe how to determine friction velocity for unsteady flow with critical review of existing methods based on flow equations, (2) to provide

methodology to minimise uncertainty of resistance to flow evaluated by relationships derived from flow equations, (3) to illustrate inconsistency between the results of friction velocity and Manning n in the context of resistance evaluation in unsteady flow. The paper is structured as follows. Section 2 presents settings of dam-break field experiment and measurement data. Methodology of evaluation of friction velocity and Manning n in unsteady flow with focus on detailed aspects of application of formulae derived from flow equations is outlined in Sect 3. In Sect 4 results of computations of friction velocity and Manning n are presented for field experiment. In Sect 5 conclusions are provided.

RC#4: P13315 L6: "Then, the relations ..." state explicitly that you use flow equations to determine friction velocity and explain why you choose this method.

AR#4: We agree that this paragraph may have been be a bit vague. We have improved it.

AC#4: We have added:

In this paper we have applied formulae derived from flow equations to evaluate both friction velocity and Manning n, because these formulae require input variables which are feasible to be monitored during passage of a flood wave – flow rate or velocity as well as water stage.

RC#5: P13315 L16-19: "paper discusses following aspects" can you structure this list that now seems a random list of aspects. Clarify the methodology (maybe in a separate methods section)

AR#5: We have clarified it in Section 3 - Methods.

AC#5: We have added:

Scarce and uncertain measurement data very often restrict the relationships on resistance to simplified forms. Among simplifications applied in literature there are simplifications of momentum balance equation terms (i.e. type of wave) and simplifications of channel geometry that affect the number of terms in the relationships. Another simplification which is very often applied due to limited spatial data refers to the evaluation of $\partial h/\partial x$*. Simplified methods are welcome, especially for practitioners. However, they must be justified properly, and there seems to be a gap here. It is crucial to choose the best method for a case under study. In proceeding sections a thorough review of each aspect of simplifications, description of uncertainty evaluation and finally a methodology for evaluation of resistance to flow is given. One may choose the best method to considered case from the presented herein. To the best of our knowledge such analysis is presented for the first time.*

RC#6: P13322 L19: You suddenly present a new method. Why did you do this? It provides the same results as the Jones equation. (it might be easier to use, so show that in the results and state that in the conclusion that you recommend to use this approach).

AR#6: In fact, it is easier to use.

AC#6: We have added:

We propose another approach for evaluation of υ, which is compatible with the kinematic wave concept, but does not require the evaluation of temporal derivatives, and for this reason may appear to be easier to be used in some cases.

We have added in Conclusions:
We have demonstrated that some simplifications such as linear approximation for $\partial h/\partial x$ evaluation may result in high incorrectness of results. On the other hand, as simplified methods are very often a must when data are scarce, when data from only one cross-section are available we recommend the translation method, introduced in this paper, to evaluate $\partial h/\partial x$ instead of Jones formula.

RC#7: P13325 L4: Please clarify what you mean by "maximum uncertainty".

AR#7: Friction velocity is a variable evaluated indirectly from input variables (Eq. 15). Hence, the uncertainty of friction velocity depends on uncertainties of input variables, which is reflected by Eq. (22). The question is how to assess uncertainty of input data, if there is only one series of measurements. It is the case in data presented in the paper – there was only one run for each wave conditions. For this reason, deterministic approach is applied to evaluate uncertainty of input variables (which is parallel to statistical approach with assumption of uniform distribution of the uncertainty of a variable, and they may be easily recalculated). Maximum uncertainty of input variables is evaluated based on knowledge of measurement techniques and experimental settings. Consequently, uncertainty of friction velocity is a maximum possible. This approach was presented in detail and with extensive references in (Mrokowska et al. 2013) cited in the manuscript.

Mrokowska, M. M., Rowiński, P. M., and Kalinowska, M.: The uncertainty of measurements in river hydraulics 5 – evaluation of friction velocity based on an unrepeatable experiment, in: Experimental and Computational Solutions of Hydraulic Problems, 32nd International School of Hydraulics, GeoPlanet: Earth and Planetary Sciences, edited by: Rowiński, P. M., Springer- Verlag, Berlin, Heidelberg, Germany, 195–206, doi:10.1007/978-3-642-30209-1_13, 2013.

AC#7: We have added:

In this method the highest possible values of uncertainty are assessed based on the knowledge of measurement techniques and experimental settings. Hence, this method provides maximum uncertainty of a result.

RC#8: P13326 section 3.1: Present the roughness characteristics of this section. Is there vegetation in this river stretch? are there bends? what is the bed composition? do you expect bed forms?

AR#8: Measurements were performed at the beginning of vegetation season (end of April and beginning of May), the vegetation was not developed then - only banks were slightly vegetated. There are no bends in this river stretch. The bed was composed of fine sand and silt. No bed forms were reported in original papers on the experiment, and we do not expect significant bed forms in this river stretch.

AC#8: We have added:

Measurement data used in this paper were collected at the beginning of vegetation season when banks were slightly vegetated. The bed was composed of sand and silt with no significant bed forms.

RC#9: P13327 L15-16: repetition, same statement and list of references as on P13323 L5-8.

AR#9: The sentence has been removed from P13327.

RC#10: P13330 L5-8: How did you determine these uncertainties? please explain in couple of sentences.

AR#10: Uncertainties of input variables are evaluated based on knowledge of measurement techniques and experimental settings.

- U (mean velocity) is evaluated by velocity-area method from measurements taken by propeller current meter in three verticals of a cross-section at two water depths. Executors of the measurements assessed that this method gave about 10% error. They based their assessment on the manufacturer information and their experience.
- Data on water stage (H) were collected manually by staff gage readings. The resolution of a device was 0.001 m, but we assess that it was possible to read the water stage with the accuracy of 0.01 m.
- Uncertainty of derivatives as well as uncertainty of bed slope are assumed to be on a level of the first significant digit.

AC#10: We have added:

based on knowledge of measurement techniques and experimental settings

Two variables were monitored: the velocity and the water stage. Velocities were measured by propeller current meter in three verticals of a cross-section at two water depths. Water level was measured manually by staff gage readings. Geodetic measurements of cross-sections were performed prior to the experiment.

Mean velocity has been evaluated by the velocity-area method from propeller current meter readings and flow depth has been calculated from geodetic data and measurements of water stage.

RC#11: P13332 L14-17: "reverse trend observed by Julien" explain why they observed a reverse trend. Might by due to bed forms? If your data did not contain bed forms then it is logical that there is another trend. The difference in trends that you see in your data is the main result of your study. This warrants some more discussion. can you explain the reason for the difference in trends.

AR#11: Thank you for suggestion; however, we think that a key reason for the difference in trends is difference between the wetted cross-sectional area. In the case of the Olszanka River water flowed in a compact channel of small width to depth ratio, while the River Rhine has different cross-sectional geometry with large width to depth ratio. Reversing trends of Manning n for such cases were observed in literature which is cited in the introduction.

AC#11: We have added:

The authors discussed extensively impact of bed forms on Manning n. However, we would like to emphasise another aspect – the shape of inundation area which determines the reverse trend. In (Julien et al. 2002) interpretation of rising n as rising resistance is qualitatively correct, while in the case of the Olszanka River false conclusions may be drawn from the analysis of Manning n, that the bulk resistance decreases with discharge.

We have added in Conclusions:

Discrepancy between trends of Manning n versus flow rate for the Olszanka River studied herein and the River Rhine reported in (Julien et al., 2002) is most likely due to different geometry of inundation area between these two cases. Resistance cannot be compared between these two cases based on Manning n.

AR#11: Our comparison is a good illustration of this difference. This is our argument for using friction velocity for interpretation of variability of resistance instead of parameters such as Manning n; friction velocity always rises with rising resistance. We have no evidence that bed forms may have impact on these trends.

RC#12: P13333 L16-19. "These aspects ... under consideration". The sentence should be in the introduction to introduce your approach.

AR#12: We have described it in Methodology section as we mentioned in AR#5.

RC#13: Furthermore, also a matter of scale plays a role. The roughness coefficient deals with larger scale resistance and shear velocity deals with local resistance (bed and walls only). Therefore, it might not always better to use friction velocity. Also depends on scale. These kind of aspects require a more thorough discussion.

AR#13: In this study, we focus on the influence of flow unsteadiness on the representation of resistance. It seems that the important problem of its scale goes beyond the scope of this study. It is a complex issue itself without introducing the unsteadiness, and was partially studied by one of the authors in (Rowiński et al, 2005) and by Pokrajac et al (2006).
The scale of resistance expressed by friction velocity depends on the area chosen, and it may be local as well as reach-based, depending on definition. We base this approach and classification e.g. on (Yen, 2002).

Pokrajac, D., Finnigan, J., Manes, C., McEwan, I., and Nikora, V.: On the definition of the shear velocity in rough bed open channel flows, in: *Proc. of the International Conference on Fluvial*

Hydraulics, Lisbon, Portugal, 6-8 September 2006, River Flow 2006, edited by Ferreira, R., Alves, E., Leal, J., and Cardoso, A., vol. 1, pp. 89–98, 2006.

Rowiński, P.M., Aberle, J., Mazurczyk, A. Shear velocity estimation in hydraulic research. *Acta Geophys Polonica*, 53(4):567-583, 2005.

Yen, B.: Open channel flow resistance, J. Hydraul. Eng.-ASCE, 128, 20–39, 5, doi:10.1061/(ASCE)0733-9429(2002)128:1(20), 2002.

RC#14: Fig 2: used dh/dx in figure and theta in caption. Please, be consistent Fig 4: used symbols to indicate h_max and U_max. In other figures you used vertical lines.

AR#14: We have corrected caption in Fig 2, and added vertical lines in Fig 4 to be consistent. Please note that in the revised paper the order of figures has changed. Fig 2 is now Fig 5 , and Fig 4 is now Fig 3.

RC#15: Throughout the manuscript you use lin, kin, T&G, wt. This is a bit confusing. Suggestion to compare once and then only compare lin and wt (or only wt). This will increase the readability of the figures.

AR#15: We hope that having all the results in figures will give readers the opportunity to analyse to what extent incorrect methods may affect the result. We agree that it is at the cost of readability, but we hope the reader will benefit from studying all the results.

Answers to Comments of Prof. T. Moramarco (Reviewer #2)

RC#1: A nice piece of work in my opinion should be based on a clear description of i) objective(s) to pursue and why, ii) dataset used to achieve the objective, iii) the methodology with details on strengths and weaknesses, iv) results and sound findings. I read the work and I didn't find any of the above points and I'll try to explain for each one my concerns.

AR#1: Thank you for this critique which has motivated us to restructure the manuscript to make it more clear for a reader, and to highlight objectives of this work. The structure of a revised paper is as follows: Introduction (with motivation, objectives), Experimental data, Methods, Results, Conclusions.

RC#2: i) Objective. The first problem of this work is just the objective which is not clear in terms of relapsed on flood routing matters.

AR#2: We have listed objectives in the introduction as is presented in AC#3 for Reviewer#1. When it comes to flood routing, the paper has not been intended to propose new methods for modelling the flow. The presented method is applied only when measurement data are available, and the paper concentrates on the evaluation of the friction velocities from the experimental data in unsteady flows. When there are data on flow rate or mean velocity vs. time and water stage vs. time, then temporal and spatial derivatives may be evaluated, and the method may be applied. Hopefully, new findings will be beneficial to flood routing in the future. To achieve this goal, systematic studies on processes in unsteady flow have to be performed first. As the methodology on friction velocity evaluation is spread out in the literature and rather superficial, in this paper we intend to give thorough critical review along with our recommendations to stimulate systematic research.

RC#3: The authors emphasize the benefit to use the friction velocity versus the roughness coefficient such as expressed by Manning (n) or Chezy formula and for that they investigate simplified formulae for friction velocity considering wave types. Under this umbrella they propose a formula of friction velocity for dynamic wave along with a way to estimate the flow depth gradient (pressure force term). However, friction velocity and roughness are dealt with as separated variables but they are tightly connected. Indeed, considering u*=(gRS)ˆ0.5 (by Eq.2) and the Manning formula both written for unsteady flow, i.e. S is expressed considering all terms in SV eqs (as emphasized by authors at section 3.3), one obtains u*=(u n)gˆ0.5/Rˆ1/6, where u is the velocity. Replacing u* by Eq(15) in the previous formula, n can be similarly formulated as a function of inertial, gravity and pressure forces.
Therefore the same graph of u* in Fig.8 can be depicted for n as well, using the dynamic equation and this is not a new ground definitely. That's why I didn't understand the novelty of work as well as the ultimate objective in the context of hydraulic literature and in particular on practical applications.

AR#3: We agree that friction velocity and Manning n are easily recalculated from one another. In fact, in the manuscript Manning n is calculated this way. The problem lies in the interpretation of these values.

AC#3: We have added:

In this paper we apply formulae derived from flow equations to evaluate both friction velocity and Manning n, because these formulae require input variables which are feasible to be monitored during passage of a flood wave – flow rate or velocity and water stage.

Please note that the same approach may be applied to evaluate Manning n from flow equations.

To evaluate Manning n or friction velocity, friction slope S is extracted from the above set of equations and incorporated into Eq. (1) or Eq. (2), respectively. We would like to stress again that it is questionable if this way of S assessment in evaluation of Manning n is meaningful. However, relations derived this way are applied in this paper for comparative purposes.

In fact, Manning n may be also recalculated from friction velocity results. All analyses of simplifications and evaluation of υ presented above apply to evaluation of Manning n, as well.

Both, friction velocity and Manning coefficient, are derived from flow equations.

AR#3: This paper is primarily addressed to researchers who analyse hydraulic properties of a wave from measurement data, both in field and in laboratory. This kind of research is cited amply in the manuscript.

The novelty of this work lies in the fresh view on resistance to flow in this kind of studies. As we have stressed in general comment at the beginning of this document, the novelty of this work manifests in two areas. (1) The paper draws attention to high uncertainty of resistance to flow evaluated by formulae derived from flow equations, which is omitted in literature analyses - we present methods of minimizing this uncertainty. (2) We give arguments for not using Manning n to interpret magnitude of resistance in unsteady flow, as it is physically incorrect. On the other hand, Manning n is good as a modelling parameter. In the revised version it is clearly stated at the beginning of this paper in the introduction.

AC#3: We have added:

It is not clear how to interpret this variability in the light of resistance definition.

However, in such cases resistance coefficients are mainly model parameters, since physical interpretation of variable Manning n is not obvious. Further aspect of possible misinterpretation of variable Manning n is the fact that trend of n versus flow rate Q may be falling or rising depending on the geometry of wetted area. Fread (1985) reported, based on computations of n from extensive data of flood waves in American rivers, that the trend is falling when inundation area is relatively small compared to inbank flow area; in reverse case the trend is rising.

As shear stress and friction velocity describe directly physical processes, there are no background theoretical doubts about their soundness in unsteady flow unlike in the case of Manning n. Moreover, interpretation of their variability is straightforward – they rise with rising resistance to flow.

RC#4: About Eq.(15) I have also to say that this is not a new finding because it is a trivial consequence by reversing the momentum equation (14) and considering dU/dx by Eq.(13) (see e.g. Rowinski et al. 2000). So, what is the added value of this analysis is not clear to me at all.

AR#4: Derivation of Eq. (15) is not difficult indeed, but its further application to real (measured) data is complicated and constitutes a subject of an ongoing debate in literature. Note that although it is trivial, the relationship for a trapezoidal channel is provided and applied for the first time. Very often wrong relationships were applied to trapezoidal channels in previous studies.

RC#5: ii) Dataset. The data used for the analysis refers to an experiment along the Olszanka River where a temporary dam was constructed and through its removal three flood waves were generated. Two downstream cross sections, CS1 and CS2, were considered and measurements were carried out there. The Authors refer to some papers for details of experiments (two in Polish). Considering that the work purpose is based on these data, one would expect that at least few information were given about, e.g., monitored variables, temporal resolution, equipment and so on. To be more precise, how the velocity and stage plotted in Fig.4 are obtained it is not given to know. For the former, I guess that a continuous flowmeter sensor is setup. Isn't' it? Considering Fig.4 I don't think that velocity is estimated by rating curve. Another fundamental missing data is the time of removal of dam. All these information are crucial to understand the context of the wave dynamic.

AR#5: Below please find details you queried for:

- Two variables were monitored: velocity and water stage. Water stage data were collected manually by staff gage readings. Readings were taken every 30 s during a rising limb and every 60 s during a falling limb of a wave.
  Velocity measurements were taken by propeller current meter in three verticals of a cross-section at two water depths. Measurements were taken every 5 minutes.
- Mean cross-sectional velocity plotted in Fig. 3 was evaluated by velocity-area method directly form data obtained by propeller current meter. Data that we obtained for this study have been previously pre-processed (removal of outlier data, data smoothing, etc.).
- We do not know the exact time of dam removal, however, it was aimed to remove a dam as fast as possible in field conditions. Care was taken not to create a shock wave.

AC#5: We have added:

Two variables were monitored: the velocity and the water stage. Velocities were measured by propeller current meter in three verticals of a cross-section at two water depths. Water stage was measured manually by staff gage readings. Geodetic measurements of cross-sections were performed prior to the experiment.

Mean velocity has been evaluated by the velocity-area method from propeller current meter readings and flow depth has been calculated from geodetic data and measurements of water stage.

RC#6. Indeed, for what I was able to figure out, the wave generated through the removal of the wooden dam may be a shock wave and, in my opinion, immediately downstream, and hence at section CS1, Eq.13 and 14 are unsuitable, while a weak form of the conservation law of mass and momentum should be used (Abbot, 1979. Computational Hydraulics). If so, even Eq.(15) is not valid at least in CS1 river site, and for that section all findings drawn from the work are wrong. About CS2 river section I can say nothing, except the fact that the downstream boundary condition given by Wilga River can affect, considering the proximity, the hydraulic conditions there; unfortunately, authors don’t make any mention about.

AR#6: We are sure that no shock waves appeared in this case. When it comes to downstream boundary condition, there was no measurements in the Wilga River. The Olszanka River is its tributary; however, knowing the site, we doubt if the conditions in the Wilga River affected waves during the experiments. To clarify methods that we have used, we have not done any modelling. All data come from measurements of real waves. Froude number is below 0.33 in each case, as is given in original manuscript (P13326 line 26).

RC#7: ii) Methodology. Based on Eq.(15) the friction velocity is formulated and, at the end, the problem is connected how to estimate the pressure force, dh/dx, at a single site. For that, the work proposes a central difference scheme by exploiting the wave travel time. However, information on the celerity assessment are missing. The authors should have had to specify how Eq.(21) has been applied and, hence, how dx has been selected in Eq.(22).

AR#7: In Eq. (21) Δs was set to 10m, after testing performance of Δs from the range (0,20). 10m appeared to be the smallest distance that gave smooth results. Celerity C was assessed by Chezy formula. Eq. (22) was not applied to Olszanka data sets, as there was no two neighbouring cross-sections to CS1 and CS2 to apply central quotient. Please note that numbering of equations beginning from Eq. (19) has changed in the revised manuscript.

AC#7: We have added:

Wave translation (Eq. 20) denoted as υ_{wt} proposed in this paper with Δs= 10 m, and C evaluated from Eq. (18).

Due to not enough measurement cross-sections, this approach could not be tested for data sets from the Olszanka River.

RC#8: Moreover, the analysis is based on data recorded at CS1 and CS2 which are affected by the downstream boundary connected to Wilga River. Therefore, details on the Wilga River's hydraulic conditions during the experiments are necessary, otherwise it's really hard to get any conclusion on the wave types.

AR#8: No measurements on the Wilga River were performed during these experiments. And as pointed before no modelling exercise has been carried out in this study.

RC#9: iv) Results and Conclusions. In Fig.7 the magnitude of different terms in balance equations (3,4) is shown. Unfortunately, it's not given to know how these terms have been computed. I don't think that they are inferred from the recorded data. I guess that they are assessed by using Eq.(3) and (4). If so, what is the hydraulic model applied and which boundary conditions are applied?

AR#9: Terms are calculated from recorded data. No hydraulic model has been applied.

AC#9: We have added:

All terms are evaluated analytically from measurement data.

RC#10: Nowhere one can get all these details and the confusion in which the reader can fall is huge. In addition, I have to stress again the need to verify if shock waves occur and if so the equations to be used are different. The fact, then, that in CS2 the terms of balance equations are different from CS1 (see Fig.7) might be explained, as already pointed out above, by the downstream boundary that affects the flood routing.

AR#10: Thank you a lot for all the questions and remarks that have shown us that the manuscript may be confusing. We believe that the main misunderstanding comes from the fact that this paper is not based on results upon solution of any differential equations, as was expected by the Reviewer. We have done our best to clarify all issues in the final version of the paper.

RC#11: Based on that, I'm not so convinced that in CS1 the wave has a dynamic behavior; as the same authors have specified and looking at Fig.7, the inertial and convective forces (acceleration terms) have opposite sign (as expected) and are not so significant (p.13328). As a result, the pressure force should be prevalent also in CS1. To remove any doubt, the better thing would have been to compute the difference between convective (f(du/dx)) and local inertial force (f(du/dt)at that section and maybe plotting it in Fig.7 as well.

AR#11: Your doubts are right. In fact, it has not been stated that the wave in CS1 is dynamic for sure. In P13329 lines 5-10 of the original manuscript it is said that for the wave in CS1 more terms of momentum balance equation are significant than for waves in CS2, and for this reason application of dynamic formula should be considered and verified. We treat presented case as a working example of application of friction velocity formula, and want to go through all problematic aspects of this case with the reader to show him/her all possible problems. Please note, that after deeper analysis we have concluded that the wave may be described as diffusive (P13330 lines 12-14 of the original manuscript).

AC#11: We have added:

In the case of data set Ol-1, along the rising limb local acceleration term is slightly bigger than the advective one, which may indicate dynamic character of a wave.

RC#12: As far as the wave types are concerned, there are a plethora of works addressing which kind of wave can occur according to the flood and channel hydraulic characteristics. Authors could attempt a comparison with them.

AR#12: We have cited a number of papers which give information on the type of a wave expected for specific conditions (regarding bed slope) in Methods Section of the original manuscript (P13317 lines 9-19). The conclusion is that no specific recommendation may be given to assess the type of a wave based on channel characteristics. For this reason we recommend checking it when data are accessible.

RC#13: Finally, it is quite arduous to state in the conclusions "the methods could be applied to any watercourse" when the analysis is based on a single river reach and few inbank floods generated, inter-alia, by a removal of a dam, which from a hydraulic point of view is not so simple to manage. I think that a statement thus strong should be supported by analysis far more extensive than here presented.

AR#13: Thank you, we have corrected this statement. Note, however, that the idea behind the article was due to the fact that very often friction velocity is evaluated by the relationships derived from flow equations by applying the simplest formula – the formula for kinematic wave approximation, without giving comment if other terms of momentum balance equation are significant. This is not only the case in studies in natural settings, but also in laboratory conditions. We would like to encourage other researchers who apply this method to consider carefully the significance of all terms and avoid oversimplifications. We do not force specific formula to be universal for any watercourse, rather we promote the presented approach of investigating a wave.

RC#14: I guess Eq.(4) is wrong. The second to last term should be u*ˆ2/gR

AR#14: Thank you for this remark. It is a typeset error. The fourth term should be “S” instead of “Su*/Rg”.

RC#15: Is there a particular reason for which authors use Chezy in Jones formula to estimate the kinematic wave approximation? Why not Manning?

AR#15: Chezy and Manning formulae provides very close results, which has no impact on the result. It could be Manning formula , as well. To avoid confusion we have deleted Manning formula.

RC#16: P.13332. line 15-20. It’s not clear the meaning of this part, maybe due to a confusion with terms: Manning n, friction velocity, resistance

AR#16: Thank you for this comment. We have clarified this paragraph.

RC#17:P.13331. Line 8-10. Do you mean nwt?

AR#17: We have found a typeset mistake – “theit” instead of “their” which could have caused confusion. It has been corrected.

RC#18: The uncertainty analysis is based on selected ranges of values for each variable without providing, however, where they come from.

AR#18: It has been described in answers to questions of Reviewer#1: AC#9.

RC#19: P.13332. line 24-25. The ground for which the authors made this statement is not clear to me.

AR#19: We hope that answers given to previous comments have clarified the aim of this study and the approach presented in the manuscript.