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Discussions

Interactive comment on "Notes on the estimation of resistance to flow during flood wave propagation" *by* M. M. Mrokowska et al.

T. Moramarco (Referee)

t.moramarco@irpi.cnr.it

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I have gone over the manuscript and, honestly, I am not well figured out what is the soundness of this work. This might be due to a lack of clarity in description of the target, dataset and methodology. For that I have also read the comments of the anonymous reviewer and although I concur with him about the impossibility to infer the novelty of this work in comparison of findings in literature, I disagree on the fact that this is a "nice piece of work" for the reasons in the sequel.

General Comments

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 eachone my concerns.

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

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. 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.

iii) 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). 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.

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? 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. 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. 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. 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.

Specific Comments

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

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

- 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

- P.13331. Line 8-10. Do you mean nwt?

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

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 13311, 2014.