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Discussions

Interactive comment on "Hydrodynamics of long-duration urban floods: experiments and numerical modelling" *by* A. Arrault et al.

Anonymous Referee #1

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Comments on the paper titled "Hydrodynamics of long-duration urban floods: experiments and numerical modelling" by Anaïs Arrault, Pascal Finaud-Guyot, Pierre Archambeau, Martin Bruwier, Sébastien Erpicum, Michel Pirotton & Benjamin Dewals.

The paper deals with the computation of large urban floods; it is thus perfectly in the scope of the journal. The paper provides firstly an interesting database gathered on a huge laboratory set-up reproducing several streets crossing to form a block. This database is then used to evaluate a 2Dh computational code based on shallow water equations. This code is used to perform a sensitivity analysis to roughness, grid refinement and turbulence models. A discussion addresses the problem of upscaling and the improvements with to the use of a porosity model. The paper is easy to read and well written. It would benefit from addressing some remaining issues. I would recommend the paper for publication after minor revisions.

General comments:

In the present form, the paper is classically written with (i) experiments, (ii) code evaluation and (iii) additional simulations. Yet, this code was already used and evaluated in several studies cited in the reference list. The novelty of the paper, apart from the experiments in a very original facility, would take benefits from addressing more deeply some physical aspects, with the help of the additional flow features provided by the simulations. First, the results are very little influenced by the roughness. But, what was the expected effect of the roughness and why it has no influence? This could be attributed to a predominant role of control sections, but the modification of the recirculation zone when changing the turbulence modelling seems to hardly affect the discharge distribution. This should be commented. The two preceding questions are connected to a last one: is there an explanation to have a 60%-40% downstream distribution instead of 50%-50%? Is it possible to compare this distribution to the one of a single crossroad with the same boundary conditions, using one of the references cited?

Questions and specific comments :

Q1/ Up to 7 authors co-signed the paper. This paper has a very significant experimental part. From references throughout the manuscript and from the acknowledgements, it can be understood that a significant part of these experiments were performed by Araud. Yet, (s)he is not one of the co-authors. This is maybe justified but is mandatory to be checked.

Q2/ Page 3, it is stated that "flash floods" are out of scope of the study. Nevertheless, the time scales associated with present study and flash floods should be detailed (can't it be considered as a succession of steady states), so as the time scale which is considered to discriminate the two kinds of floods. This time scale could be addressed in the section about upscaling, and discussed along the time scales characterizing the laboratory small-scale experiments.

Q3/ About boundary conditions: I understand from the text that the experiments were conducted with a horizontal bottom but this should be stated more precisely in the manuscript. Could the authors confirm, as it seems to be stated L26-27 page 4, that the linear (q=Q/b) inlet flow rate was constant within all the inlet streets? Finally, are the free-flow conditions performed thanks to chutes?

Q4/ Can some details be provided about the "optical gauge" P5L7? The uncertainty seems quite high (+-1mm) compared to classical devices.

Q5/ I completely agree with authors about the use of a Darcy-Weisbach coefficient instead of a Manning coefficient (p6L11). Nevertheless, the reason could be stated more clearly. Both of these coefficients are "process oriented": one for the channel, one for the pipes. As far as I am concerned, the use of a Darcy-Weisbach coefficient is required here due to the limited values of the Reynolds numbers in the laboratory experiments. The "fully rough regime" is not guaranteed, which prevent from using safely a Manning coefficient depending only on the wall roughness.

Q6/ P7 L8-15 : the downstream discharge distribution is about 60% in the streamwise (inlet) direction and 40% in the crosswise direction. This is not 50%-50% and can some reasons be proposed for this ratio: a slope (but I understand the slope was nil, see Q3), a reference with a single crossroad?

Q7/ P9 L4-8. The location of the water depth profiles drawn should be specified: middle (centreline) of the street, average on a section, . . . Notably, "significant variations" are commented but the comments should account for a possible crossing of the recirculation zones or, instead, of the vena contracta. I expect slightly different comments regarding one case or the other.

Q8/ P10L30 : multiplying the cells by a factor 4 increases the computational cost by 8. Can this be commented?

Q9/ Typical values of the Froude number should be added in table 5

Q10/ The Reynolds number is defined using the water depth, i.e. assuming that the

hydraulic diameter can be assimilated to 4h. This assumption should be valid for prototypes 2 and 3 but is more questionable for the laboratory model and the prototype 1. Was it taken into account to compute the values of the Darcy-Weisbach coefficient reported in section 5.1?

Typing errors:

- Page 1: the third address in the authors' affiliations is not complete - P6 L25: "a first test series of tests" - P8 L27-30: What is the "supplement" cited twice? In case it is on the website of the journal, please do not account for this comment.

Does the paper address relevant scientific and/or technical questions within the scope of NHESS? (Y) Does the paper present new data and/or novel concepts, ideas, tools, methods or results? (Y) Are these up to international standards? (Y) Are the scientific methods and assumptions valid and outlined clearly? (Not always) Are the results sufficient to support the interpretations and the conclusions? (Y) Does the author reach substantial conclusions? (Not always) Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)? (Y) Does the title clearly and unambiguously reflect the contents of the paper? (Y) Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained? (Y) Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience? (Y) Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them? (Y) Is the size, quality and readability of each figure adequate to the type and quantity of data presented? (Y) Does the author give proper credit to previous and/or related work, and does he/she indicate clearly his/her own contribution? (See my remark Q1 concerning authors) Are the number and quality of the references appropriate? (Y) Are the references accessible by fellow scientists? (Y) Is the overall presentation well structured, clear and easy to understand by a wide and general audience? (Y) Is the length of the paper adequate, too long or too short? (Y) Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated? (N) Is the technical language precise and understandable by fellow scientists? (Y) Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience? (Y) Is the amount and quality of supplementary material (if any) appropriate? (Y)

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