Interactive comment on "Laboratory modelling of urban flooding: strengths and challenges of distorted scale models" *by* X. Li et al.

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We would like to thank Referee 1 for his/her positive feedback and useful comments on how to improve the manuscript. We provide hereafter a point-by-point response to the Referee's comments.

1. As to the first comment about the comparison of uncertainties from distortion effects to those from hydrological data (L17 in the Abstract), we agree with the Referee that this should not be included there since it is not supported by factual material in the manuscript. Hence, L17 of the Abstract will be deleted in the revised manuscript as suggested by the Referee.
2. A second point raised by the Referee relates to the difficulty of conducting point velocity measurements on experimental models of urban flooding at the district level, with which we concur. Indeed, most existing lab studies representing urban flooding at the district level include only depth and discharge measurements (Finaud-Guyot et al., 2018) or additionally *surface* flow measurements (LaRocque et al., 2013). Only few studies report point velocity measurements for urban flooding at the district level (Güney et al., 2014; Park et al., 2013; Smith et al., 2016), while Zhou et al. (2016) used PIV to provide insights into the flow field in the wake of an array of buildings. As pointed out by the Referee, more studies involving detailed velocity measurements are coming up (e.g. Martins et al. (2018)), particularly for more local analyses (e.g. at the level of a single manhole, not at the level of an entire district). This issue will be discussed explicitly in the revised manuscript. We suggest to emphasize the need for pointwise velocity measurements as a perspective in the Conclusion of the revised manuscript.
3. As suggested by the Referee in his/her Comment 3, we will be more specific in the revised manuscript with respect to what we call "artefacts" in scale models. They correspond to deviations between up-scaled model measurements and real-world prototype observations due to governing non-dimensional parameters (i.e. force ratios) which are not identical between the model and its prototype (Heller, 2011). This may include alteration of the flow regime (transition vs. complete turbulent), or of the relative importance of frictional resistance …

4. Although the recommendations by Chanson (2004) look very reasonable, little background information is provided by Chanson (2004) to support the stated recommendations (Reynolds number above 5,000 in the model, distortion ratio below 5 to 10). We will nonetheless investigate this point and report our findings in the revised version of the manuscript.
5. In his/her Comment 5, the Referee highlights that scale effects in “dual drainage” models become even more intricate due to the combination of pressurized pipe flow and surface flow. Since the body of the present manuscript focuses on surface flow in a network of streets (not dual drainage), we propose to include a brief discussion on dual drainage as a perspective in the Conclusion of the revised manuscript. We already have a sentence stating that “More controlling parameters should also be considered, such as the bottom slope …”. We will add a short paragraph on the importance of dual drainage for urban flooding and the associated experimental modelling challenges.
6. The Referee is right that information on the inflow discharge uncertainties were missing in the manuscript. For the dataset of Araud (2012), the uncertainty on inflow discharge is estimated at about 1 % (Finaud-Guyot et al., 2018), while in the study of Velickovic et al. (2017), the accuracy of the flowmeter measuring inflow discharge is of 1 ls^{-1}.
7. As stressed in the Referee’s Comment 7, the representation of frictional resistance in scale models of urban flooding is particularly challenging. In this respect, we will clarify the following aspects.
    1. When we write that “friction is expected to be underestimated in more distorted models”, we mean “underestimated” compared to the case of an *undistorted model* with similar bed material (not necessarily compared to the prototype!). This is true because the more distorted the model, the higher the Reynolds number and the lower the relative roughness ($k_{s,m} / R_{H,m}$). Both of these effects contribute to lower the friction factor, as confirmed by the experimental results in Figure S23.
    2. In the original manuscript, we hardly explain that, even without distortion, frictional resistance may not be properly represented in the scale model, particularly when smooth material is used to construct the bottom and the walls of the model. There are two competing effects (lower Reynolds number in the model compared to the prototype; but also lower relative roughness) which, in general, hampers a definite prediction on whether frictional resistance is over- or under-estimated compared to the prototype. We will discuss this explicitly in the revised manuscript.

    We will then highlight in the revised manuscript that, in distorted models, frictional resistance becomes indeed relatively smaller compared to the case of an undistorted model; but not necessarily compared to the prototype scale.
8. In his/her Comment 8, the Referee is right that “the third type of effects” being dominant is not well supported at this stage. This would require additional detailed measurements and/or interpretation based on computational modelling. Therefore, this paragraph will be rephrased in the revised manuscript.

All Technical comments by the Referee will be accounted for in the revised version of the manuscript.

On behalf of all authors,

Xuefang LI

References

In the reference list below, we include only the references which were not cited in the original manuscript.

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