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Interactive Comment

Interactive comment on "Evaluating scale and roughness effects in urban flood modelling using terrestrial LIDAR data" *by* H. Ozdemir et al.

Anonymous Referee #1

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1. This paper is an interesting application of a high-resolution hydrodynamic model that is a contribution to the literature. The authors' principal thesis is that coarse resolution of $O(1m)$ combined with modifying the drag coefficient is insufficient to capture the correct physics. This may be true, but the present test case does not prove the point. The authors show that reducing the drag coefficient to $1/2$ and then to $1/100$ of its original value has extremely small effects on the solution. However, this does *not* indicate that modifying the friction will not work; the model might have numerical dissipation that is greater than the modeled contribution at the scales in question, which would cause the same result. Indeed, I suspect that Manning's n could be set to zero and the simulations would not change substantially because numerical dissipation has become the controlling factor. Because numerical dissipation is a function of the numerical method,

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at best the authors have shown that for this model, and only this model, the numerical dissipation at 1 m grid resolution is the controlling factor in the solution. This brings up an important question - are the results at 10 cm dominated by modeled or numerical dissipation? I think it would be useful to show some results for simplified test cases that evaluate the numerical dissipation at different scales for this model. Note that I'm actually very supportive of the authors' thesis: I suspect that there are indeed fine scale geometries that are not well-represented by modifying the drag model. However the authors work simply is not sufficient to demonstrate this as the model numerical error effects are not sufficiently documented. I have my doubts that it can be substantiated with this model, so I would suggest that the authors change the basic thesis of their study. I find it quite interesting that Figs 8 and 9 seem to show that the different grid resolutions are more similar for a given drag regime than the different drag regimes are at a given resolution; i.e. this seems to say that the resolution is less important than the drag regime, which is really the opposite of the claims made with Fig 11 and 12.

2. pg 5922 - I'm surprised that there is no discussion of Froude number limitations. It appears from de Almeida et al, 2012 that the model instabilities are likely driven by Froude number issues. For a simple steady, uniform flow it can be shown that the governing equations provide that $Fr > 1$ will occur for $S_0 h^{1/3} n^{-2} g^{-1} > 1$, which will occur for sufficiently small n on sufficiently steep slopes. It appears that the basic model becomes highly dissipative for $Fr > 1$ as means of stabilisation, which might be part of the issue as the n was reduced in tests.

3. pg 5905, line 14-15. A minor point - The statement that flood risk due to surface water runoff is less advanced than flood risk due to coastal and fluvial sources seems to be broad and over-sweeping, especially as it is backed up only by a political paper rather than a peer-reviewed article. I imagine that if you consulted insurance companies, they would tell you that they have equally advanced risk quantification analyses for all systems. It may be that we perhaps have better abilities to model the physics of the coastal and fluvial systems, but that does not necessarily mean that our understand-

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ings of risks is less advanced, particularly because understanding risk quantification is an interplay between both probability and magnitude.

4. pg 5913, line 1 Typographical - the $i-1/2$ and $i-3/2$ in the text have the $-1/2$ and $-3/2$ as subscripts, whereas they should be on the same text line as the i.

5. pg 5917 line 2-4. The subtle detail described cannot really be seen in Figure 6, even when blown up to 500%.

6. pg 5917 line 29. I don't think the statement that all these simulations are"grid independent" has been proven. Here I am taken the numerical modeling viewpoint of what it means to be grid-independent: that is the results do not change with further refinement of the grid. For this to be shown true for the 1 m grid would require simulations on finer grids that do not include finer topography. I don't think test described of a single simulation at one drag coefficient with unidirectional flow for one slope can prove grid independence for a system with bi-directional flow, multiple slopes and a range of depths.

7. pg 5118 lines 1-3. This statement is sort of true, but is confusing in its causes and effects. Two effects that are caused by increasing friction are slowing velocity and increasing depth. It is the increased depth which increases the wave speed, which is thus an indirect result of friction for the specific case. If friction is increased but some other factor maintains the depth constant then the wave speed would not increase.

8. pg 5919 - the "scalar velocity" should be written as"speed"

9. Figures 8 - 9 Why does Figure 8 use depth and 9 elevation?

10. Typographical: Figure 10 - No units are provided for velocity axes.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5903, 2013.

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