

Interactive comment on “Implementing a hydrodynamic model to complement water depth and flow velocity data for physical scale experiments of rivers and estuaries” by Steven A. H. Weisscher et al.

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General comments:

This paper deals with the evolution of river models, including meandering river and braided river, as well as their connection to the dynamics of estuaries. The study is based on laboratory experiments performed by some of the authors and available in the literature. They are composed of two sets of experiments in two different apparatus: one focusing on the influence of fine particles on the generation of either braided river

C1

or meandering river, and the other one modelling the dynamics of the estuary. The aim of the present paper is to provide a numerical modelling of the fluid flow for these different experimental observations, in place of experimental measurements, which are hardly accessible for such complex systems. In particular, a shallow model solver, Nays2D, is used for that purpose. From a general point of view, the paper is well written, clear, and well documented to understand the necessary background from the previous experimental paper. Moreover, the general idea of providing coupled numerical and experimental devices to describe the entire dynamics of the strongly coupled fluid dynamics and morphodynamics system makes sense and would probably be a very interesting approach to be implemented in river modelling. The obtained results are clearly presented and are shown to be of relevance with experimental observations. In particular, to prove the relevance of the modelled fluid dynamics, maps of water level difference (between numerical results and experimental measurements) are provided. Pictures and movies show that the global qualitative trend of the fluid evolution is recovered with the numerical model. The error between numerical model and available experimental data are shown to be within 10%. Even if I feel that the general purpose of the paper would be of interest for the scientific community, I have some doubt about the scientific content required for publication in an international journal in the state. In particular, the novelty of the paper is to be found in the strategy of coupling experimental measurements with numerical models to provide a full characterisation of the system. Altogether, we imagine that the aim is to provide a global analysis of the river system then leading to a more general understanding of its genesis and its morphodynamics. According to that, I have two major issues: - No new input is given in this paper on the understanding or description of meandering or braided rivers, according to the objective of linking a numerical model to experiments. Only a validation of the numerical model is proposed here showing somehow its relevance for this more general objective. In my opinion, it weakens the paper, and a deeper investigation of the physics of the full system using this combined approach should be proposed. - The coupling approach has already been proposed for other applications, or experimental devices.

C2

This is somehow a branch of data assimilation. Here, the method used for the numerical approach is one-way (and slightly weak), in the sense that there is no loop of control between the two approaches. The only “link” between the experimental measurements and the numerical model is to provide the final bathymetry of the river system obtained from the experiment as the initial condition for the numerical model. Then the shallow water model is run on this steady topography. I guess that more advanced coupling approach would be more appropriate. The proposed approach does not seem to be in the state of the knowledge on numerical algorithm dealing with coupling experiments and numerical modelling.

Specific comments: - The paper is, in my opinion, too wordy regarding the available results. Most of the discussions sound a bit as assumptions on possible scenario than actual results supported quantitatively. - The authors should provide relative error maps instead of absolute. In the state, I am wondering if 10% error is not an underestimation of the actual error. - According to the fact that topography is imposed from the experiments (from my understanding, clarify otherwise), and only the fluid flow is modelled using shallow water equations, 10% error on water height (and maybe more in some places), sounds quite significant to me. As somehow mentioned by the authors, one of the weaknesses of shallow type approach is the modelling of the friction factor. This is where data assimilation would make sense for such a complex system. Could the authors justify that their one-way coupling approach makes sense, and is sufficient, in their system to provide relevance. - I do not understand the comment on the use of Nays2D instead of Delft3D. I am not familiar with these specific codes, but from my knowledge of shallow model, the main adjustment is on the friction model, which can indeed have an impact on the typical scales that can be solved with relevance. However, such issue regarding the maximum height that can be solved with one of the code does not make any sense to me. This is maybe a technical issue, but it cannot be a fundamental issue with this kind of approach. Could the author develop, or remove that comment, as in my opinion, they can use the shallow code they want without justification. - p20. l6. I understood that no sediment transport is used in the modelling used in

C3

this paper. So what does this remark mean here? Actually, if this is available, I would be curious to see how the system evolves from the one obtained using the experimental topography, as the fluid flow characteristics obtained numerically are probably not the ones of an equilibrium state according to the stability of the sediment bed and its evolution due to sediment transport.

Other comments: - p2. What does the author mean here by biomorphodynamics. Is there something I have missed here? - p4. The description of the forcing flow into the Metronome flume should be improved. I had trouble to understand the tidal forcing model in the state. - p7. Typo: viscosity ν instead of v , according to equation (3) - p8. And figure 3. What is meant here by boundary conditions? “boundary” instead of input? - p10. l15. computes -> computed - p10. l22. “the model produces a single sinuous channel. . .” Two options, (i) I have missed something in the approach proposed in the paper (linked to my major comments) which therefore would required a serious revision of the paper to provide more clearly what is actually performed or (ii) the remark here is irrelevant. In case (ii), as the topography is imposed from the experiments how could it be otherwise? - Figure 4. In my opinion, a relative error would show more than 10% discrepancy between experiments and numeric. Deeper discussions and arguments on the relevance of the numerical model should therefore be proposed.

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C4