REVIEWING: 1 – Laurent Lacaze

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 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:

1.1) 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 main objective of our manuscript is to show how a numerical model, in our case Nays2D, could enrich experimental data in experiments with 1s-10s mm flow depth, i.e. close to the viscous sublayer. This approach results in spatio-temporally continuous data of water depth, flow velocity and bed shear stress, of which the latter two are difficult to be obtained from experiments by in-situ measurements. Additionally, the use of a numerical model aids in future experiment setup design as the model allows for testing different boundary conditions, thus reducing the number of tests in the lab.

This method does not change the main findings in the published experiment case studies (i.e., Van Dijk et al, 2013; Leuven et al, 2018), but makes new findings possible in the near future. Here we have shown the model to be applicable in very different experimental systems that allows answering very different scientific questions, such as the initiation of chute cutoffs (does that start on the upstream or downstream end of the pointbar?) or the reduction of tidal prism by sedimentation (does that start on the upstream end of the tidal system or everywhere at the same time?). Similar questions can now be raised in many experiments recently published. A study of the physics of those full systems is therefore well beyond the scope of this paper.

We will clarify our aim in the introduction with the proposed text under comment 2.1 of Reviewer 2.

Additionally, we will more clearly present to the reader in the discussion section how the experimental data-model integration could be used to derive new data that are difficult to acquire via in-situ measurements, as were already touched upon in the first version of our manuscript. We show that (1) maps of nondimensional bed shear stress can be linked to measured morphological change (Fig. 12 in first manuscript), and; (2) that maps of water depth and flow velocity can be used to extract tidal prism (Fig. 10 in first manuscript) and create inundation maps (Figs. 12g in first manuscript and Supplement Figure 2).

1.2) The coupling approach has already been proposed for other applications, or experimental devices. 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.
Indeed, this is a 'one-way' approach. The reviewer suggested a more advanced coupling approach would be more appropriate. Below we explain why we think this is as of yet infeasible to attain our goal of complementing experimental bathymetry data with modelled hydrodynamic data.
Firstly, we interpreted 'a more advanced coupling approach' as including the modelling of sediment transport and hence morphological change, and comparing the newly modelled bathymetry with the morphological change of the scale experiment. There are a few reasons why we have chosen not to do this.

1) Including sediment transport and morphological change goes beyond the goal of complementing experimental data and comes closer to (indirectly) substituting experiments.
2) Morphological change heavily depends on which sediment transport predictor (e.g. Meyer-Peter & Müller, 1948; Engelund and Hansen, 1967) is used and may require unrealistic parameterisations to get the morphology 'right' (Baar et al. 2019). Nonetheless, to get an indication of the amount of sediment transport and morphological activity, Nays2D produces maps of sediment mobility (i.e. nondimensional bed shear stress) that can be used for this purpose.

Baar, A. W., Albernaz, M. B., van Dijk, W. M., & Kleinhans, M. G. (2019). Critical dependence of morphodynamic models of fluvial and tidal systems on empirical downslope sediment transport. Nature communications, 10(1), 1-12.
Engelund, F., & Hansen, E. (1967). A monograph on sediment transport in alluvial streams. Technical University of Denmark 0stervoldgade 10, Copenhagen K.

Alternatively, we interpreted 'a more advanced coupling approach' as data model assimilation, which is used to improve model forecasting (e.g. bathymetric surveys, wave buoy data). However, we intend to complement bathymetric data and not forecast flow conditions at later stages in the experiment.

Regarding the comment on the use a static/final DEM, please see under comment 2.2 of Reviewer 2 for our justification and proposed text changes.

We believe the strength of the experimental data-model integration lays in applying the numerical model not just to the final DEM, but to the series of DEMs throughout an experiment. This enables the user to track the development of e.g. tidal prism, area of flood inundation and flow patterns, which are easier to acquire via the model than from measurements. In this manuscript, we present the use, accuracy and new data of the data-model integration. At a later stage, we intend to apply the integration to a series of DEMs. We will clarify this in the introduction (See under comment 2.1 of Reviewer 2).

Specific comments:

1.3) 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.

We use part of the discussion section to present how the reader may use the experimental data-model integration (1) to outperform measurements, and; (2) to derive new data that are normally impractical to measure in landscape scale experiments. For example, we show quantitatively how tidal prism based on measurements goes very wrong, as the law of mass conservation does not apply on the erroneous data, as opposed to modelled tidal prism. Next, a few examples are given on how the modelled data may be used to derive new information that is interesting for our reader, such as maps of inundation and maps of nondimensional bed shear stress (Fig 12 and Supplement Fig. 2).

Please see under comment 2.2 how we intend to change the discussion section.

1.4) 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.

The 10 % error for water depths and 15 % error for flow velocities as mentioned in the manuscript are based on the mean absolute errors given in Figures 5 and 7a. Indeed, there are locally larger errors between measured and modelled water depths. However, most larger errors correspond to incorrect measured data due to lighting overexposure, differently coloured substrate and obstacles between the experiment and measuring equipment. The point of the modelling was to avoid low quality results from measurements despite extensive filtering and extrapolation, so here we simply disregard erroneous measured data in the computation of the mean absolute errors. This is explained in the caption of Figure 5 and 9, and we will clarify this in the methods section as well (see proposed text changes below). We prefer to show the absolute values in the manuscript and added a figure to the supplement containing the relative errors.

1.5) According to the fact that topography is imposed from the experiments (from my understanding, clarify otherwise),

This is correct.

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.

See under comment 1.2 and 1.4: the main error is in the measurements. For reasons of parsimony, we used a constant friction factor for the entire DEM, which ignores spatially variable friction. It is of course possible to calibrate this spatially, but then all measurement errors (lighting, sediment colour, PIV) are also attributed to friction which would be a poor application of data assimilation.

-> We will clarify our choice for a constant friction factor in the methods section.

In the discussion section we already indicate that the error could perhaps be reduced by using a spatially variable friction factor in Nays2D. This would require grain size maps of the experiment, which can be calculated from overhead imagery.

1.6) 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.

Yes, it is a technical issue and we will clarify this in the manuscript. We did not intend to justify the approach, but instead, we would like to explain to the reader that not all numerical models are suitable to modelling flow in scale experiments (i.e. in the order of a few mm to cm), for example due to build-in thresholds for minimum water depths. To this end, we start the methods section with our motivation for using the relatively unknown Japanese model Nays2D instead of often used models such as Delft3D and FLOW-3D.

1.7) p20. l6. I understood that no sediment transport is used in the modelling used in 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.

The remark that Nays2D computes sediment mobility (i.e. nondimensional bed shear stress/Shields stress) is because of the following: (1) sediment mobility is an important parameter for comparison to natural systems, and; (2) it gives vital insight in sediment transport fields and morphological activity without computing actual sediment transport.

We agree with the reviewer that modelling sediment transport is an interesting step to take with our model, but we have chosen not to do this, as explained under comment 1.2. In the future, we intend to explore the sediment transport in the model versus experimental observations, but the advanced imaging techniques required for this make this well beyond the present scope.

Other comments:

1.8) p2. What does the author mean here by biomorphodynamics. Is there something I have missed here?

Biomorphodynamics involve the morphological development of e.g. a river, estuary or coast in conjunction with plants and/or animals. These living species affect the morphological development by e.g. increasing the hydraulic roughness (plants, oysters) and consolidating/stirring sediment (bioturbating animals). In turn, the flow conditions determine where these species may thrive, creating a feedback loop. In the context of scale experiments, the simulation of biomorphodynamics predominantly entails the addition of vegetation (e.g. Tal and Paola, 2007; 2010; Braudrick et al, 2009, Lokhorst et al. 2019). See for example Viles (2012) and Viles (2019) for further information on bio(geo)morphodynamics.

Viles, H. A. (2012). Microbial geomorphology: a neglected link between life and landscape. Geomorphology, 157, 6-16.

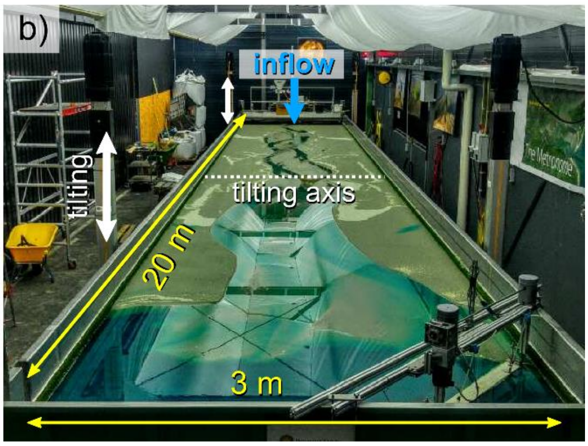
Viles, H. (2019). Biogeomorphology: Past, present and future. Geomorphology.

Upon review, we found that we only used this terminology once in the manuscript. Therefore, we will substitute 'biomorphodynamics' with 'morphodynamics' and clarify how and why vegetation may be used in landscape scale experiments.

1.9) 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.

We will improve the description of the tidal forcing in the manuscript and refer to the supplementary movie for visual aid. The new text will be*: … The tilting motion alternately generates a slope in the landward direction during flood and a slope in the seaward direction during ebb, which result in tidal currents strong enough to move sediment along the entire estuary (Kleinhans et al., 2017) (Supplementary movie). …*

Additionally, we will add information to Figure 2b to further clarify the setup and functioning of the flume the Metronome (additions in white: *short central axis over which the Metronome tilts*, and *up-down arrows next to the actuators to show the vertical movement of the Metronome*). Together with the text, the supplementary movie and the citation of the flume papers (i.e. Kleinhans et al., 2017; Leuven et al., 2018), we believe this to be sufficient to explain the tilting mechanism of the



Metronome.

1.10) p7. Typo: visocosity nu instead of v, according to equation (3)
Changed.

1.11) p8. And figure 3. What is meant here by boundary conditions? "boundary" instead of input?
We distinguish between boundary and initial conditions as inputs to our model. For example, river discharge and tides are boundary conditions, which are applied throughout the model runtime, and the water depths at the beginning of the model run is an initial condition. The parameters in the box 'Boundary conditions for numerical modelling' are all boundary conditions.

Upon review, we will change the parameter 'initial height' of the downstream weir in Figure 3 into 'mean water depth' to avoid confusion between initial and boundary conditions.

1.12) p10. l15. computes -> computed
Changed.

1.13) 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 require 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?
Thank you for asking this question. We meant to indicate that the model shows that flow velocity and flow volume concentrate into one sinuous branch, despite (1) the multi-channel character of the DEM with cross-cutting channels over the point bars and (2) despite the considerable overbank flow. This is important but not surprising as this tendency was found in many types of systems in the literature.

We will clarify this in the manuscript. The proposed change text (changes underlined): *…Similar to the meandering river experiment, the model produces <u>flow that is focused in a single sinuous channel,</u>*

especially for x > 4 m (Fig. S1), despite the multi-channel character of the DEM and the considerable overbank flow. This distinction of a main sinuous channel and swale channels …

1.14) 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.
Please see our answer to comment 1.4.