Review of “Numerical modelling of the evolution of a river reach with a complex morphology to help define future sustainable restoration decisions” by Yassine et al. (2022)

Reviewer: Clément Misset, Grenoble, France - March 30, 2022.

General comments

This manuscript presents the 2D numerical modelling of morphodynamics in a braided river reach during a large flood event. To develop and test the 2D model, it uses observed morphological changes (DoD obtained from Lidar data) as well as hydrological data and field measurements. This is thus a nice dataset. The manuscript is generally well written and addresses important questions for both researchers and practitioners, regarding the modeling of morphodynamics in braided systems. Several friction laws and bedload transport formulae are tested in the numerical model in order to evaluate the most relevant modeling approach. Criteria to evaluate the model performance in such context are discussed as well as future improvements. I think this work is of interest for the reader of Earth Surface Dynamics.

I have however several concerns, that I suggest to consider in a revised version of the manuscript:

(1) I think some relevant literature is missing, in particular regarding braided morphodynamics modeling and criteria that have already been proposed to evaluate 2D morphodynamics models (see detail comments).

(2) More details could be added when presenting the studied site as well as some of the model parameters, for the reader to better understand the setting and modeling choices (see detail comments).

(3) Concerning the sediment transport deposition partitioning, I have a major concern: can the historical partitioning observed with a lake configuration be used for the braiding setting? I guess this is not the case. Using the same values (8-16% of bedload) should be discussed regarding the hydrodynamics observed/modeled, typical values observed in previous works, etc. This assumption seems to be critical, in particular for the model evaluation.

(4) The choice (critical) of the upstream boundary condition for bedload flux would beneficiate to be better argued.

(5) Concerning the performance criteria, I think the paper would greatly beneficiate to include other criteria adapted to braided rivers. The statistical distribution of erosion and disposition have for instance been used in a similar study by Williams et al. (2016a). There are also several braiding index that have been used by Rifai et al. (2014). These aspects are already discussed in the current manuscript, but I think using more appropriate index could significantly improve the analysis.

(6) It could be considered to add a discussion section, to better distinguish the results description and their interpretation.

To sum up, I think this work is original and will be of great interest for geomorphologists. I however think that substantial revision could be made to improve the manuscript regarding the previously mentioned aspects.

Detail Comments

Introduction: I suggest to better define "complex" morphology. I also suggest stating sooner and more clearly the main objectives of the paper, for the reader to understand why a numerical model is developed.

L19: Sentence is not clear, are you citing Rickenmann et al. (2016) or Reisenbüchler et al., 2019? Consider rewrite the sentence

L27: the term "condition" is vague, be more specific. Natural hazards have been already discussed in previous lines; I suggest removing it from this sentence

L36: I suggest adding that hydrodynamics is also driven by morphological changes

L41: using valid sediment transport formulae (friction law, etc.) is not only needed to introduce reliable boundary conditions but to develop realistic modeling "inside" the model domain (at the grid scale).

L65: I think there is some relevant literature about braided morphodynamics modeling that is missing. I suggest considering Williams et al. (2013), Williams et al. (2016a), Williams et al. (2016b), Rifai et al. (2014), Gonzales de Linares et al. (2021). Comparison with these works would also be relevant in the discussion section.

Study area description: I think it would be of interest for the reader to add a more detailed description of the site, in particular, including a river profile showing slopes, the tributary, solid material input, etc. If available, I suggest to use a hillshade map of the catchment in Fig.1 (instead of the grey areas) to better show the relief and the mountainous environment. Maybe also add pictures illustrating the upstream main tributary morphology?

Fig2: I suggest writing directly on the picture the dates and mention "before the 2013 flood", "after the 2013 flood".

L104: "compared to 90 m3/s annually in the same period" -> do you mean: compared to the monthly averaged discharge of 90m3/s?

2.4 Restoration implications: I have a concern regarding the model aim. I am not convinced that once calibrated on a rare event such a the 2013 event, the model will automatically be applicable for long term modeling, in particular for low magnitude events that would likely contribute to transport the stored sediments from the LDG to the downstream reaches in case the weirs are removed.

L128: I would add something like "Thus, to help decision makers, a hydro-morphological 2D model…"

3.2.1 Friction laws: specify that the friction law is not coupled with the bed surface grain size (which is constant because fractional transport is not considered in the morphodynamical modeling).

L170: I wouldn't say "The friction coefficient for the Ferguson (2007) law is the D84", I would say something like "the Ferguson law uses the D84 as a proxy of the bed roughness" or remove this sentence

L185: I found the sentence not clear, K/K' is used for shear stress partitioning if I'm right. Do you use this shear stress partitioning in your 2D modeling? According to Gonzales de Linares et al. (2020, 2021), such correction might not be relevant in massive bedload transport.

The recking formula: I have a major concern here. You use a 1D "morphological" formula that takes implicitly into account the cross-section averaging (variability of shear stress, grain size distribution, etc.) to deal with non-linearity effect of bedload transport. In your 2D model, the shear stress is spatially discretized so that the Shield number used to calculate the bedload flux is a "local" Shield number and the bedload transport formula does not need to take into account cross section averaging. I suggest considering the following papers: Recking (2013) and Recking et al. (2016).

3.2 Sediment transport and bed evolution module: Could you specify if an avalanche mode (Slope sliding) is used and what are the parameter considered (angle of repose)? Same question regarding the deviation of sediment transport on transverse slopes? Both could have significant effect on the morphological modelling, it is thus of interest for the reader to know which parameters were used.

L223-224: could you provide the point density of the Lidar and the raster resolution of the used DEMs? Could you specify here if bathymetric data for the low flow channels were available?

L235: It is not clear for me how you estimate the bedload fraction? Do you have GSD measurement of the dredging? This should be clarified. The range 8-16% seams possible but much wider range could be considered, see for instance Turowski et al. (2010). I also wonder if you can consider the same partitioning between the pre and post flood situation: Fines will likely deposit more easily in a lake (even if they will deposit in the braiding situation). Note that the sediment transport partitioning might also evolved with the event return period and the flow/sediment supply conditions so that dredging might not be fully representative of a specific event. The following papers could be of interest to consider a gravel matrix fine fraction commonly observed in gravel bedded stream: Mueller and Pitlick (2013), Navratil et al. (2010), Misset et al, (2020)

4.1.2 Input hydrograph: do you have points with liquid discharge measurement to calibrate/validate the model? This should be specified.

Fig5. It is not clear for me if the large model uses a fixed bed? I suggest to add a title above each mesh (large, finer mesh).

L277: the equilibrium load upstream condition: Is this relevant? Were the upstream profile and cross section stable? Did you compare these upstream fluxes with an averaged 1D formula? This hypothesis is probably critical for the modeling of the event.

L285: I suggest specifying that under the hydrodynamics calibration conditions, morphological changes and bedload transport were limited. If bathymetric data were not available, this can probably lead to a non-negligeable uncertainty on the water surface elevation, which can be considered acceptable compared to other uncertainties, this need to be specified.

Also, did you vary the D84 considered in the Ferguson friction law? Or did you use the measured value?

L305-306: maybe use the term DoD (DEM of differences)?

4.4 Performance evaluation: Braiding and morphological changes in braided rivers are somewhat stochastic processes. Is it thus relevant to use single long profile/cross sections and the Brier Skill Score objective function? I mean, your model can be considered relevant in reproducing the morphodynamics of the studied reach as it reproduces well some general morphological evolution (for instance the braiding index, the number of active channels, the statistical distribution of erosion and deposition, the average long profile, etc.) but the objective function can lead to a bad score (you do not have exactly the same DEM evolution).

4.4.3 Comparison of the deposited volumes

Comparison of the global volumetric budget of the reach is of great interest. I have however the filling that DoD obtained for the 2018 flood includes much more information. You could for instance compare that statistical distribution of erosion/deposition in the reach, is is done in a similar study by Williams et al. (2016a).

Fig7: I suggest using red for erosion and blue for deposition (commonly used colors for such map), to include a scale bar and to remove the pixel within the uncertainty range of the DoD (by using transparent color so that only significant morphological differences can be seen).

L349: Friction laws link velocity to depth and roughness

5.1 General visual comparison of eroded and deposited areas: according to a previous comment, you are comparing visually the erosion and deposition distribution depending on the friction law and transport formula used: comparing these distributions could strengthen this analysis.

Another aspect that could be discussed regarding the effect of the friction law is the use of spatially and temporally constant roughness parameters (n or D84). It is likely that these parameters are not constant in both space and time the braiding reach, which can explain some of the differences between modeled and measured evolutions.

L372: If I understand, the model seems to be not fully relevant in the downstream part of the reach, as almost no evolution is modeled. I agree that such model, already complex, doesn't take into account all the complexity of such braided system. I however wonder if this disagreement is only due to the fact that suspended load is not modeled. Do you have observations showing that the downstream part is mainly driven by suspended load processes? Based on aerial photograph, the downstream part of the reach seems to be at least partially composed of gravel bars? I suggest moving this discussion of the results in the discussion section.

Fig.8 and 9: I suggest adding on the map "observed changes" and the names of the transport formula and friction law.

5.2 Longitudinal profiles and cross-section comparison: I wonder if comparing long profile in braided morphology is relevant? Maybe, using an "averaged" long profile to capture the general trend and being less sensitive the the active channel position would be a better option? I also suggest to move from this section all sentences discussing this specific point and the relevance of the BSS index.

L415: Can we consider the deposited bedload fraction is similar between the 2018 braiding situation and the pre 2013 flood situation with a lake? I have the feeling that the deposited suspended load fraction would be higher in a lake configuration. Were the hydrodynamic conditions low enough (backwater effect, low velocities, low shear stress, etc.) during the 2018 flood so that more than 80% of the deposited volume corresponds to suspended particles? If it is not the case, this fraction seems to be really high. I suggest comparing it with previous works on fine particle stocks/content in gravel bedded streams (see Mueller and Pitlick (2013), Navratil et al. (2010), Misset et al, (2020)).

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