Review of the manuscript

'Application of Wave-Current coupled Sediment Transport Models with Variable Grain properties for Coastal Morphodynamics: A Case study of the Changhua River, Hainan'

by Wu et al.2024

In this paper, the authors investigate erosion and deposition in the lower reaches of the Changhua River Estuary by implementing hydrodynamic and sediment transport modelling. They attempt to highlight the waves' influence on sediment dynamics in rivers with low sediment concentration. They make use of a wave model that incorporates the Van Rijn formula for the calculation of bed load and suspended sediment transport. They also consider various sediment properties both at the estuary and the main river channel. After model calibration, they examine erosion and sedimentation at several locations.

The manuscript is not very well written and critical information is missing especially regarding their model's set up. Because of this deficiency, it is very difficult to judge the validity and robustness of the modelling, and the results and conclusions cannot be really assessed unless the authors provide more information and improve the quality of their manuscript including their figures. There are also some fundamental issues that I am going to list here:

Main comments on the hydrodynamic model

1. The authors claim to use a three-dimensional model (lines 131-132) while the equations they present are depth-averaged two dimensional in x and y. It is also never mentioned which model the authors use. Is it a well established and validated one? Is it open-source like e.g., Delft3D and TELEMAC? is it an in-house one? Please provide this information and where and if it has been implemented for similar applications.
2. Not enough information is given regarding the wave model parameters. Please give details on how the wave radiation stresses are calculated in the model and how the waves are incorporated in the model equations i.e., via the mentioned JONSWAP spectrum, the frequency of occurrence of different wave heights and directions and number of days in different seasons and the wave fixed parameters at the open boundary (lines 295-300).
3. Although the simulation period is never explicitly mentioned in the manuscript, it seems that this coincides with the calibration period (23/04-30/04/2023). To be validated though, the model needs to run with the same setup for another period. In addition, there is no mention in the manuscript of the calibration parameters (e.g., roughness, diffusion, viscosity etc.) and how these were modified. For example, do the authors use uniform values or spatially varying ones?
4. The model grid is not presented, is it structured or unstructured? Is it regular or curvilinear? What is the model's resolution? The limits of the white rectangular in Figure 6 do not coincide with the grid coordinates as given in Line 280. The authors claim in lines 286-289 that the resolution of the ETOPO1 data is not sufficient for this research however it seems that these are the data used for developing the bathymetry of their model as it is written in the legend of Figure 6. The time step is also missing.
5. The hydrodynamic results (section 3.3) are ill-presented and not adequately analysed. At which moment the results given in Figure 9 correspond to? Are these depth and/or time

averaged? The depicted current of Figure 9 is in some instances referred to as a tidal (line 345) and in others as a coastal one (line 430). Are these averaged, combined tide and wave currents over this week of simulation or are taken at a certain time of calculation and if so, which one? Furthermore, it is written that the results are given at the times of high and low water but tidal currents at these times are minimal. What's the situation at slack times? Finally, there is no mention in the manuscript of whether we are looking at a period of neap or spring tide. Water levels in Figure 7 remain almost constant during the simulation period and in any case a spring-neap cycle takes place within 15 days and not one week (lines 473-478).

6. The authors underline the fact that the tide is the most prominent effect at the point of comparison against wind and waves (lines 335-336) but under these circumstances, their validation process cannot be considered as a validation of the combined effect of current-wave but only of tide. Therefore, a case where all factors are important is required to demonstrate the relative capability of the model.

Main comments on the sediment transport model

1. The manuscript misses information on the initial and boundary conditions and rates for both bed load and suspended sediments transport. The sediment motion equation implemented in the model (line 363) needs to be presented. How are the two modules coupled? Is it an online or offline coupling?
2. As mentioned earlier, the model's given equations (1)-(4) in the paper are two-dimensional depth averaged. How is then the velocity vertical profile computed in equation (7)?
3. There is a serious confusion in section 3.4.1. The authors calculate a uniform over space sediment thickness from their model equal to 4.1 cm. If what they write in lines 365-366 is right, then they get results after the one-week simulation that they did for 23/04/2023-30/04/2023. But then they use the estuary's sediment discharge of 2022 (line 389-390) and they compare their model result with field data covering one month (July 2022)!! Not surprisingly then, there is a serious discrepancy of 1.1 cm between the model and the observations which is too big to be ignored. Besides, the assumption that the uniform over space result from a one-week simulation for a certain year can be representative of any year for that specific area is too crude.
4. From what is written in section 3.4.2, it seems that the authors not only have sediment data for an entire month (July 2022) but that they run their model for this period as well. Why don't they do then the comparison based on results from this simulation? In any case, the fact that they present results analysis for different time periods is already problematic.

General comments

1. In their figures, the authors fail to provide all the necessary information regarding their study case. They refer to stations that can be nowhere found in their figures and so the reader cannot understand from which location the results and data are taken? Specifically, Dongfang Ocean station, Danchangcun, Xiantiancun and Jiuxiancun stations cannot be found in the figures. The same for the ADCP.
2. Chapter 4 does not discuss the scientific output and fails to explore and highlight the importance of the outcomes. It only provides a list of potential measures to be taken against siltation which have only a local interest. These can be hardly supported

considering that they are based on conclusions from not adequately justified modelling. I suggest that the authors rewrite this section after they have addressed all the raised issues in their modelling approach but from a non-local perspective.

3. The conclusions read more like a summary. What are the key findings of this research, which messages can we take from this and why these would contribute further to the research on this topic?