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Interactive comment on "Modelling of sediment transport and morphological evolution under the combined action of waves and currents" *by* Guilherme Franz et al.

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General comments

This manuscript presents a new modeling system for coastal morphodynamics, coupling existing hydrodynamic (MOHID) and wave (SWAN) models, with a sediment transport and bottom evolution model. It is therefore in the scope of Ocean Science. The application of the modeling system is illustrated through several synthetic cases and a real beach/inlet case. Particular attention is given to the bed load transport. The results presented are realistic.

The manuscript is well organized and clear. The title reflects the content of the

manuscript. The detail of the model description is appropriate and the figures are clear. The test cases represent a wide range of conditions in which the model can be applied.

Overall, the manuscript is a good contribution to the scientific literature. However, some aspects lack a discussion. The paper – and the reader – would benefit from the insights that the authors gained during the development and application of the model. Some suggestions on how to enrich the paper are provided in the specific comments.

Specific comments

Introduction. The manuscript lacks a literature review on coastal area morphodynamic models, even though several models are mentioned. Such a review would allow the clarification of how the new model differs from existing ones and extends the state-of-the-art.

Page 2, line 32. The authors stress the use of a slope limiter as a solution to the deterioration of the results in long-term simulations. Yet, as is mentioned later (page 9, line 5), a similar filter was used before (Roelvink et al., 2009). Other authors use the same approach to improve numerical stability and to improve the simulation of tidal inlet migration (Nahon et al., 2012; Fortunato et al., 2014). This should be mentioned. The differences (if any) between the approach implemented by the authors and by Roelvink et al. (2009) should be mentioned and discussed. Also, since the authors consider this approach to be a significant contribution of the paper, they could show an example of a simulation without the slope limiter, in order to discuss its importance.

Page 2, line 34. "This approach may be more appropriate. . ." Explain why.

Section 3. More details should be provided about the coupling between the models. In particular, the way the information is exchanged between modules is critical for numerical efficiency and should be mentioned.

Section 3. An interesting aspect in the model is the solution of the transport equation

even in 2DH mode instead of the use of empirical formulae. This approach is not the most common, and has given poor results in the past (e.g., Galappatti and Vreugdenhil, 1985). A discussion on this issue would be very useful. For instance, it is unclear how the model deals with the vertical integration of the sediment fluxes. Are the velocity and the sediment concentration profiles assumed constant in the vertical? Are analytical profiles assumed? The approximations associated with this approach and the associated errors should be discussed. Similarly, the benefits should also be stressed, in particular the ability to represent the wash load, which cannot be represented by the equilibrium empirical formulae. Such discussions could be theoretical or based on a comparison between two simulations for the same test case, one in 2D and the other in 3D.

Page 4, line 20. In the description of the equations assumptions, incompressibility is missing. Also, it would seem more correct to call the governing equations "shallow water equations" than "Navier-Stokes equations".

Page 5, 1st paragraph. How is the turbulence associated to wave breaking taken into account?

Page 6, line 14. It is stated that lateral friction can be important, and it is mentioned later that it is taken into account in some tests. Please explain how is it computed in the model, or provide a reference.

Page 7, line 16. vH is defined as the current velocity. Yet here it is used to determine the wave bed shear stress. Please explain.

Page 7, equation 8. Considering that a spectral model is being used, what is the wave height Hw? Is it the significant wave height?

Page 11, line 11. “The maximum slope in 3D simulations is useful to represent the sand motion induced by excessively steep slopes.” Are the slope effects included in the bedload formulation (e.g., as in Lesser et al., 2004)? If so, is the slope limiter used

to represent a physical process (explain which one) or to improve numerical stability?

Figure 10. This figure could be enriched by showing the forcing model’s results, i.e, the time series that are actually being used in the simulation.

Figure 12. Two different time steps are shown. If the two are required, then a discussion of the differences should be useful. Otherwise, I suggest eliminating one of them.

Page 14, line 8. “A more efficient coupling method is currently being developed inside the MOHID code to further reduce the computational time.” It would be useful to provide some indication on the computational performance of the model in its present stage.

Technical corrections

Page 2, line 7: “bi-“ should be “two-“

Page 2, line 11: “for a hydrodynamic” should be “to a hydrodynamic”. Similarly in line 12, “for a wave model” should be corrected.

Page 3, line 27: “generates” should be “generate”

Page 10, line 9: Dean’s equation requires units.

Page 10, line 30: “enhances” should be “increases” or “grows”

Figs. 1-6. Better resolution is required. It is difficult to see the arrows’ heads.

Page 11, line 6. “longer groyne length”: remove “length”

Page 11, line 22. “which remains equal to the barotropic flux given by SWAN”. By “barotropic” do the authors mean “depth-averaged”? I do not understand the use of the word “barotropic” in this context.

Page 12, line 20. Capes Raso and Espichel are mentioned in the text. They should be indicated in figure 8. Same for Cova do Vapor and other place names that I may have missed.

Page 13, line 7. “Caparica was” should be “Caparica were”.

Page 13, line 18. “as the waves propagate with higher velocities”. Replace “velocities” by “celerity”.

Page 13, line 27. The reference Freire (2006) should probably be Freire et al. (2006). Also, this reference seems incomplete.

Page 17, line 21. “Saville, T.: Experimental determination of wave set-up, 1961.” This reference is incomplete.

References

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