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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A 2-D process-based model for suspended sediment dynamics: a first step towards ecological modeling" *by* F. M. Achete et al.

Anonymous Referee #2

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In the following are my comments on "A 2-D process-based model for suspended sediment dynamics: a first step towards ecological modeling" by Achete et al.

This work is a timely demonstration of the application of the previously developed numerical model, D-Flow FM. The delta of San Francisco Estuary (the Delta) indeed has a complex geometry which presents a significant challenge to computational models. The results show a nice match between the model and the observations. Overall the writing is solid with enough material to support the authors' arguments for the site chosen. I feel that the paper is suitable for publication in HESS if stronger discussions and conclusions could be provided.

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In my opinion there are three key points that are being addressed in this paper:

(1) Calibration/validation of the numerical model. I understand that the technical details on the numerical method and validation/comparison with other schemes have been given in Kernkamp et al. (2010), but it would be helpful to comment on alternative methods for this specific site. Are there other numerical models done for the Delta, e.g. just the hydrodynamic part? For example, Delft3D in the depth-averaged mode seems to be a close competitor which handles both hydrodynamic and morphodynamic transport. A few comments would be nice, such as: a) how many grid cells are involved in the calculation of the model setup presented in this work, versus how many grid cells would have to be used for the total triangular mesh? b) How much time would it take for other methods to reach the same level of accuracy with the same computation power (only one desktop)?

(2) Insights of the specific field site. As the authors stated, the Delta provides critical habitats for ecological purposes. I can see how this model and its predictions become handy for a chain of models. Could the author use the model results to inform more into the monitoring and data collecting practice? For example, what data set should be collected to help the model make better predictions? And where could be a good location for building a new monitoring station? Addressing issues like these, or even in a discussion of future work, will expand the scope of this work and gives a higher scientific impact.

(3) Information on suitable systems for model application. The Delta is a relatively well-surveyed region. What should be taken into account for potential users of this model, when applying to a different region with less available data? Also, it seems that the configuration of the channel network does play a role determining the SSC level (the effects of closing/opening DCC). Would the authors give more insights into how changing the configuration of the network affects the distribution of fluxes?

Below are some minor comments:

HESSD

12, C581–C583, 2015

Page 1514, Eq. (2): “M” is also used later as the subscript for “Model”.

Page 1515, Line 3: Winterwerp (2006) is missing in the reference list.

Page 1515, Line 5: sentence is not complete, “the second term in equation (Eq. 1)... is close to zero?”

Page 1515, Line 20-25: will changing the 5m threshold for mud/sand affect the results?

Page 1519, Line 15-20: uRMSe given by Eq.(3) cannot give negative values. Seems that it needs to be multiplied by the sign of the difference between modeled and observed SD (see Eq. (7) in Bever and MacWilliams, 2013).

Page 1519, Line 23: “Willmott, 1981” is missing in the reference list.

Page 1524, Eq.(7): Why is the term “[A’C’][U]” eliminated?

Page 1542, Fig. 5: The values of the parameters are vague by presenting them as multiples of the standard run (e.g., w*0.5, E*100). I personally prefer the form in Fig. 6 with absolute values.

Page 1544, Fig. 7: the black rectangle is missing in the left panel.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 1507, 2015.