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Interactive comment on "Large-scale coastal and fluvial models constrain the late Holocene evolution of the Ebro delta, Spain" *by* Jaap H. Nienhuis et al.

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This manuscript describes an interesting application of two reduced complexity models to quantitatively characterize the long term impact of changes in flow rate and sediment loads on the progradation of the Ebro delta over the last ~2000 years. The application of the two models, a coastline evolution model and a river morphodynamic model, is novel in the sense that the output parameters of the river model are used to update the input conditions of the coastline evolution model. Although the models were not fully coupled because the input parameters of the coastal evolution model do not seem to change in time during a simulation, the results of this exercise are useful to determine what could have caused an the increased delta progradation rates that occurred about

∼2000 years ago. I consider the level of model simplification appropriate for the spatial and temporal scales of interest. I like the choice of not modeling autogenic river avulsions and backwater effects and to impose the orientation of the channels based on field observation. The model is well written and I have some general comments on the manuscript and I list them below.

Comment 1

The detailed description of recent changes in flow regime and sediment supply to the delta (section 2.4) is relevant to characterize the present Ebro delta, however this information does not seem to be used in the model application and in the discussion sections of the manuscript. Is the Ebro delta suffering of land losses or shoreline retreat? How are these changes (if they have been documented) related to the dam construction based on the four model scenarios considered in the manuscript?

Comment 2

It is not very clear how the effects of changes in flow regime and sediment supply to the Ebro delta were studied. One of the output parameters of the fluvial model can be the mean annual sediment load (I do not remember if the original model has it as output parameter or if the code needs to be slightly modified to print it). Are the authors imposing a variable sediment supply or its equilibrium value, i.e. the value at the end of the numerical simulation when the system reaches a new equilibrium state? I understand that equilibrium values of sediment supply were used in the simulations. I am not asking to do more simulations, but it can be nice to fully couple the two models in the near future and see how the coastline evolution changes in case of sediment supply that changes in time.

Comment 3

The description of the fluvial model can be improved and refined. I would clarify that since the authors are using a channel model, they consider the bed material only and do not model washload. In line 37-38 the description of equilibrium is not very clear and should probably be improved by saying that in the absence of subsidence/ uplift and sea level rise, if the flow regime and the sediment supply are constant in time rivers tend to reach a mobile bed equilibrium in which the channel bed elevation does not change in time. If streamwise changes in flow discharge and sediment load are not modeled, at equilibrium the bed slope does not change in space and time and the bed material transport capacity is equal to the mean annual supply of bed material everywhere in the modeled reach (Parker, 2004 and 2008).

On page 7, lines 4-15, the normal flow assumption appears and it is not linked to the rest of the text and this part needs some re-writing. I would reference to De Vries (1965) and/or Parker (2004 – chapter 13) to say that when the time scales of changes in channel bed elevation are long compared to the time scales of the changes in flow characteristics, the flow can be approximated as steady, i.e. the time derivatives of the Saint Venant equations are dropped. This is the quasi-steady approximation, which is at the base of the vast majority of the morphodynamic models. When it is further assumed that the flow is locally uniform, the quasi-steady approximation becomes a quasi-normal approximation and the flow characteristics are computed with the formulation that is implemented in the fluvial model used in this study. Thus, on line 9 the normal flow assumption breaks down when the flow is sufficiently non-uniform, i.e. the spatial changes of the flow have to be considered (not non-steady because steady refers to time and when this is the case you cannot drop the time dependence in the flow equations, as happens for e.g. tidal morphodynamics). There is a huge number of river and delta morphodynamic models that use the quasi-normal approximation for the flow (see e.g. Parker et al., 2008 and Paola et al. 2011 for references) and they have been used to approach the study of a large variety of problems. The choice of the quasi-steady or of the quasi-normal approximation depends on the problem of interest, on the available field data and on how the downstream boundary has to be modeled. I honestly do not think that the use of a quasi-normal approximation is a problem for this particular study.

Page 10 line 25, the authors are using a bedload transport relation for 0.2 mm sand. This requires some justification. Why not to use an Engelund and Hansen formulation (Parker, 2004 bulk load relation chapter) for total (bedload plus suspended) bed material load? The model should allow for it. Further, the change in reference Shields number in equation (3) from 0.047 to 0.0495 suggests that the authors are using the Mayer Peter and Muller bedload relation corrected by Wong (Parker, 2004), but they are not changing the coefficient of the load relation. This is perfectly fine with me, since the authors are obtaining reasonable results, but they should mention it in the text.

Comment 4

It is hard to understand how the intermittency factor was estimated.

Comment 5

Figure 6, does the figure become clearer if the temporal changes in bed elevation (eta – eta_initial) are plotted? Do the authors have one or two field data to add to the figure to show that the model is able to reasonably reproduce the field case?

Comment 6

This is a very personal request, can the authors express the sediment fluxes in million tons per year? It is very hard for me to understand how much sediment is delivered to the cost when the fluxes are given in kilograms per second.

Comment 7

Is there any evidence for a change in flow regime and sediment supply to the fluvial reach and to the delta between 6000 and 3000 years ago? It would be nice to have this information to justify the results of the modeling exercise.

Comment 8

A table with the values and the justification of the model parameters will greatly help.

References

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