Manuscript Esurf-2014-26 – Liang et al.
A reduced-complexity model for river delta formation: Part I – Modeling deltas with channel dynamics

Response to the associate editor and reviewers

We thank the associate editor (Dr. Castelltort) and both reviewers (Dr. Slingerland and Dr. Ashton) for pointing out the value of this reduced-complexity modeling efforts, and for their constructive and insightful comments. In the following document, we respond to the comments in the sequence of their posting dates. The original text is copied and divided into a series of questions numbered at the beginning of each paragraph (e.g. B3 means comment #2, question 3). The answers following the questions are led by a “>>>” symbol.

Response to Associate Editor, Dr. Sebastien Castelltort's comments:

Liang et al. present in this manuscript a model of "reduced-complexity" for the simulation and study of delta evolution. The results are in very good match with those obtained by more complex models including fluid dynamics. Regardless of philosophical discussions about the approach, my view is to salute the effort: if reduced-complexity models can indeed bring understanding of an inherently complex system, at less expensive computational costs, and with less parameterization than CFD models such as Delft3D, I think it is a useful and complementary approach to solving scientific problems such as those posed by rivers and deltas.
>>> We thank Dr. Castelltort for the detailed comments. We address your comments below.

I wanted to share some comments and questions I have on the paper:

A1: *At the end of Phase 2 (equation 12, page 835), it is not clear to me how the conservation of water mass is insured by the algorithm used to compute water elevation.*
>>> The conservation of water mass is achieved by the balance of water discharge at each cell in Phase 1 (the total number of water parcels entering a cell must equal the total number of water parcels exiting that cell), which is not affected by the update of water surface elevation during Phase 2. Once calculated, the unit discharge vector $\vec{q}_w$ remains the same until the next time step. Therefore, as water surface elevation being updated, only water flow depth h and velocity $\vec{u} = \frac{\vec{q}_w}{h}$ are adjusted accordingly.

A2: *Line 17, page 839: "for the erosion of both types of sediment parcel…": I find this a little confusing as in the paragraphs above you mention "deposition from a parcel", not "of" a parcel. Thus, I understood the "parcels" to be like "packets" of sediment or water that, as they pass on a cell, can leave or take sediment. Could it be better to write "for the erosion by both types of sediment parcel…". Now if this is true, could you give a word on why you choose to use this as an erosion law (the Garcia and Parker U/U rule) rather than some classical stream power dependency of bed erosion on water velocity? (I found some of the response in the main paragraph of page 848, but it is still unclear to me).*
>>> Yes, the parcels are exactly like traveling "packets" that can leave or take sediment. We follow the suggestion and change "the erosion of" to "the erosion by".
Here we explain why the classical stream power relation cannot be applied in DeltaRCM. The stream power relation requires the estimate of channel slope. (Expanding from the brief explanation in Page 848) Deltas are low gradient environment and typically have subcritical flow. In this case the energy slope of the system can only be calculated from the water surface, and the slope of the bed is to some extent decoupled from the transport of water and sediment. For the sake of model simplicity, we choose the velocity relationship.

A3: *Equation 21b: typo? the >= sign should in fact be <= right?*
>>> Yes it should be <=. We also noticed in 21a the condition should be "$U_{loc} > U_{ero}$". Both Eq.21a and 21b are corrected in the revised manuscript.

A4: *When you erode the bed, how do you know what kind of material is taken in transport? Do you keep a record of the thickness of sand and mud on each cell? How are masses of sand and mud conserved during erosion and deposition? (again, after reading further, I found some response in page 847 and in the conclusion, but it would perhaps be better to make it all clear upfront in the method section).*
>>> In the revised manuscript we provide more details in Section 5 on the method we use for recording stratigraphy. Here we answer the above questions briefly by explaining first how bed layers are recorded and then how the conservation of mass works.
To record stratigraphy each cell in the domain is viewed as a storage column (shown in Figure 2), and the volume below the bed surface is further divided to thin layers of an equal thickness (these layers are visible especially in Figures 11 and 12). The thickness is chosen to be about a thousandth of the reference depth, h_0, although it can be set to different values by users for different vertical resolutions. Each layer is recorded with a value associated to it – currently it's either the percentage of sand (a value between 0 and 1) or the age of the deposit (represented by the number of time step). For example, if a cell has net deposition, the volume it received from passing parcels will fill up as many layers as needed above the previous bed surface, and all values associated with these layers are set to the ratio between the volume of sand deposited and the total volume of sediment deposited during this time step. If a cell has net erosion, the bed surface will be lowered and all values associated with the layers above the new bed surface will be erased (by resetting these values to -1 in the code).
In terms of sediment mass conservation, it's straightforward during deposition because the volume dropped from either a sand or mud parcel is directly added to the bed and recorded in the code using the method we described above. For erosion, as pointed out in the questions, ideally the sediment mass entrained from the bed should match the layers being removed, however in the current model setup we assume that a parcel can only take sediment of its own category (e.g. sand or mud), and the volume is equal to the total volume entrained. Given that deltas are predominantly depositional environments this method overall gives a good sediment conservation. In future work we will improve this by letting each parcel carry multiple sediment categories.

A5: *In the Murray and Paola model of braided river, a crucial component to the dynamics of braiding was the inclusion of a rule for lateral erosion (as a proportion of vertical erosion). Here you also have a dynamic shifting of channels, why is lateral erosion not important?*
>>> There's lateral erosion in this model achieved by doing the topographic diffusion (Eq. 24). This diffusion is applied to the topographic surface and is scaled to local sediment flux to approximate the effects of topographic slopes. This process allows sediment to move laterally and therefore producing channel migrations. In the revised manuscript we will provide a supplement material describing the effects of key model parameters and processes, including this topographic diffusion.

A6: *Page 847, lines 18-22: how do we know that "a reasonably accurate representation of the water surface and the inclusion of suspended sediment deposition and entrainment" are needed "to achieve even qualitatively correct model results"? It would be good to see model results without the algorithm for water surface, and model results without suspended sediments, but perhaps varying another parameter. The morphological differences observed through variation* *of the sand/mud ratio could perhaps be equally achieved by another forcing, e.g. tidal/wave action?*

>>> In the revised manuscript we will provide more tests to support our findings. Especially we show that by switching off the water surface algorithm channels do not bifurcate or avulse anymore; and by switching off suspended load a stable channel network cannot be achieved in the model.

We are aware that the effects of varying of sand/mud ratio can possibly be achieved by varying other parameters or adding more processes – we will consider this in our future work. Currently the model does not have wave or tidal effects yet.

Thanks for submitting this paper to ESurf, All the best, SC

Response to reviewer, Dr. Rudy Slingerland's comments:

General Comments

This paper presents a well-thought out and novel reduced-complexity-model (RCM) predicting delta morphodynamic evolution and stratigraphy. The authors take great effort to identify the minimum physics necessary for capturing delta dynamics, and this elevates the research from simple model description to insightful science. As such the paper addresses relevant scientific questions within the scope of ESurf.
Besides offering a useful tool to the community, the paper offers two important ideas. Firstly, it convincingly argues that delta systems are fundamentally different from other morphodynamic systems such as erosional landscapes, braided fluvial, and eolian dunes because of 3 factors: 1) deltas are a low-gradient gravity-driven system in which water surface gradient and fluid inertia play a much larger role than say, erosional landscapes; 2) the low Froude Number of deltaic flows enables information from both upstream and downstream to propagate into the system; and 3) the macroscopic emergent behaviors of deltas can NOT be decoupled from the microscopic physics. Secondly, the authors point us towards the key processes and state variables that any model must accurately predict, such as bar growth at channel mouths, evolving backwater profiles in response to bar growth, both suspended and bedload fluxes, and cross-channel water surface slopes. These are substantial conclusions that follow from sound analysis.
>>> We thank Dr. Slingerland for the detailed comments. We address your comments below.

Specific Comments

B1: *The model is tested by comparing its output against experimental and other numerical deltas, but the authors use words like "the resultant deltas…… are consistent with", and "our results give similar behaviors…", rather than a more rigorous comparison using metrics like number of distributaries, avulsion frequency, etc. It would be comforting to the reader to see some quantitative comparisons.*
>>> While the original scope of this work was simply to build a parcel-based reduced-complexity model that is able to resolve channels, we strongly agree that quantitative comparisons will better describe and capture the similarities and differences between the results from different deltas. In the revised manuscript we selecte a few metrics to apply to our model results. These metrics are:

- Shoreline rugosity is applied to model results on the effects of input coarse/fine sediment ratio (Section 4.1);
- Avulsion time scale is applied to model results on experimental fan deltas (Section 4.2);
- Number of distributaries is applied to model results on the effects of basin depth (Section 4.3).

It's also worth noting that the development of metrics for delta morphodynamics is still not mature. Some of them are not discriminating enough to extract key features, such as fractal dimensions. Existing work of Wolinsky et al. (2010), Edmonds et al. (2011) and Passalacqua et al. (2013) provide a rich platform for future work. In fact one of our future work along the line of delta RCMs is to use the model as a tool to develop more robust metrics for deltas.

Last but not least, unlike morphodyanmic systems such as erosional networks, in modeling deltas it is surprisingly difficult to get a model even to the point of producing behaviors that are not “obviously wrong”, i.e. is qualitatively similar to real deltas. The use of metrics is often the second level in evaluating the performance of a delta building model.

B2: *There are a great number of user-defined (and sometimes seemingly ad hoc) constants/parameters in this model, e.g.,* h_{dry}, θ, γ, ε, U_{ero}, *etc. It would be useful to comment on how many there are, and the logic you used to set the magnitude and ranges of each.*
>>> We provide a table listing these constants/parameters and the rational for the value range chosen in the revised manuscript.

B3: *The amount of erosion and deposition of the bed by a sediment parcel is limited by certain criteria. Please explain the logic of the criteria and the specific magnitudes.*
>>> There are two main factors taken into account when choosing the rules for sediment deposition and erosion: 1) We wanted to keep the parcel-based routing scheme (the same of water flow routing), that sediment transport is modeled in a Lagrangian approach by treating traveling sediment parcels as packets which can leave onto or take sediment from the bed; 2) we use the simplest non-linear relation between flow properties and sediment transport, assuming the capacity of a sediment parcel to carry sediment flux is scaled to the flow velocity to the power of 3. A more detailed rationale and results showing the model’s responses to different choices of sediment transport formulas will be provided in the revised manuscript.

B4: *The model attempts to incorporate the effect of fluid momentum (inertia) in determining flow direction, but not in flow velocities. Also turbulent energy is not carried around so it can impede grain settling in cells where it would otherwise deposit. Can the authors comment on the magnitude of errors expected from these simplifications?*
>>> Yes inertia only appears in determining flow routing direction, where the magnitude of flow velocity does not affect the flow routing. Also turbulent energy in not taken into account as sediment entrainment and deposition are based on a velocity threshold formulation for simplicity. As in more traditional models that do explicitly account for advection of turbulent kinetic energy, we are assuming that the local flow velocity adequately represents the energy content of the flow as it pertains to sediment transport. This error would likely be largest in places where strong local accelerations and/or localized turbulence production (e.g. in localized shear layers) lead to decoupling TKE from the local mean flow velocity. Handling situations like this is beyond the scope of a reduced-complexity model such as this one, and would be expected to lead to poor morphodynamic predictions in areas like channel bends where the flow is changing rapidly. In the companion paper (Part II) we start with assessing the performance of the flow routing scheme by comparing its output to a well-established hydrodynamic model (Delft3D) for a quantitative measurement of the error.

B5: *It would be helpful to include a table defining symbols, abbreviations, and units*
>>> We provide a table of notation in the revised manuscript.

Technical Corrections
1. p. 831/line 3: insert “an” after “carries.”
2. p. 831/line 6: Replace “And” with “Likewise.”

3. p. 831/line 7: insert “an” after “carries.”
4. p. 831/line 21: delete “s” on “interests.”
>>> All changes are made.

Response to reviewer, Dr. Andrew Ashton's comments:

The authors here present the background and some general results from a new model of river-dominated delta evolution. The description of the model components is well presented and there is sufficient detail that the research can be understood and the model reasonable reproduced from the description. The authors then present a few test cases where the model results are, for the most part, compared to other model and lab results. These results show that the DeltaRCM model can reproduce many general features seen in these other test cases. There is also a brief comparison some features of the Wax Lake delta system. Overall, this paper serves a useful role as a description of the model components and a broad demonstration of its capabilities.
>>> We thank Dr. Ashton for the detailed comments. We address your comments below.

C1: *I have similar thoughts as other reviewers that the model appears to have a number of specifically unconstrained and heuristic parameters that are essential in the model behavior. This is fine, but it would be nice to have a table to summarize these unconstrained parameters and the values that are used in simulations that are shown (not the just the input parameters as in Table 1). While the authors have made a nice presentation here of model capabilities, it would be interesting in another format or future work to see how these parameters (particularly the smoothing terms) affect model behavior. As such, here I feel that the authors leave us with an impression that the only variability in model results is from a change in the input parameters (i.e. inputs and boundary conditions). It is likely that both inputs and model parameters have strong effects on the output.*
>>> We provide a table in the revised manuscript listing these constants/parameters and the rational for the value range chosen. We also add sensitivity analysis on several parameters in the revised manuscript. These analysis will be a supplement file supporting the revised manuscript.

C2: *In "3. Model Construction" it would be informative if the authors could more specifically state their objectives for the construction of this model. In lines 7-8 on p. 828 there is a general scope made, but it would be nice to motivate up front the specific decisions that were made in terms of what type of reduction of nature the authors have selected and what aspects of delta evolution they specifically hope to achieve. The choice of complexity reduction is an important part of any RCM.*
>>> We strongly agree with Dr. Ashton on the importance of the choice of complexity reduction in RCM development. Before the general scope on p. 828 is introduced, the previous paragraph (p. 827, Line 19-29) we listed 4 difficulties in developing RCMs for delta evolution, which we intend to tackle during our model development. We do realize, however, an extended explanation can be provided regarding the specific choices made in developing the parcel-based routing framework, such as rules for calculating routing directions and for constructing water surface profile. We provide more details in the revised manuscript.
In addition, we would like to point out that DeltaRCM is the outcome of a series of reduced-complexity modeling efforts that aim at producing channel dynamics in deltas. Liang's dissertation gives a complete description of this series of models.

C3: *I am not in total agreement that the authors have fully established that the four items listed as needs in an RCM on P851 L6-10. To make these assertions, I would think the authors would present model tests to show how each of these are needed. In particular, the authors have not*

motivated that both bedload and suspended load transport are necessary (at least in the model results that are presented). To my knowledge, such a distinction is not apparently needed in the Seybold et al. model to capture certain essential aspects of delta evolution. Expanding upon that thought, it would be nice if the authors could more specifically address some of the process distinctions between their RCM and the one by Seybold. I'm sure there are many, but some discussion would be useful. As an extension, there are features that this model does not seem to be able to recreate, such as leveeing and birdfoot formation that have been a large discussion item in the field as of late. Perhaps the authors could address why this may be the case. i.e. how do the simplifications only allow fan deltas?
>>> In the revised manuscript we will provide more tests to support our findings. Especially we show that by switching off the water surface algorithm channels do not bifurcate or avulse anymore; and by switching off suspended load a stable channel network cannot be achieved in the model.
In comparison with the work of Seybold et al.:

- Although the flux between cells are defined with local properties such as flow depth and water surface elevation, Seybold et al. does solve iteratively for water surface elevation to satisfy the conservation of mass and momentum, which in spirit is closer to CFD method rather than to classical cellular flow routing method that does not require solving a set of equations over the whole domain;
- In Seybold et al. the deposition and erosion of sediment are not purely based on bedload formulations. In fact the two terms in their sediment transport equations gives the effects of cohesive and non-cohesive sediment behavior respectively.

As for the formation of levees and birdfoot deltas, we would like to point out that there are distinguishable levees formed in our model results, e.g., the 90% mud run, which features elongated channels into the ocean at the delta front. From the time evolution these channel banks are stable over a considerable period of time. The levees are not as pronounced in the case with higher percentage of sand in the upstream input.
In fact the model is able to produce a birdfoot delta (i.e. a single elongated lobe) if the parameter gamma is set to a very small value (~0), which eliminate the spreading of flow caused by lateral water surface slope. We will add our findings with physical explanation in the revised manuscript. This is likely to appear as part of the sensitive analysis of the value of the parameter gamma. Also, it's worth noting that DeltaRCM is not built to resolve the 3D turbulent jet details at the channel mouth, which has been shown to be responsible for the elongation of single channelfor birdfoot delta (Falcini and Jerolmack, 2013).

Overall, this is interesting work and a useful addition to the literate and I am glad to see it in published form.

Regards,
Andrew Ashton - - -

Other notes:
The abstract could use some attention to wording and grammar; it is not up to the standards of the rest of this manuscript. The first line of the abstract is odd as the authors claim to essentially make a model type of model.

>>> In the revision we will update the abstract.
P846 L2. "John Shaw" is awkward. (To clarify, I am not suggesting that John Shaw is awkward, but rather that this turn of phrase is awkward.)
>>> Changed to "By Shaw (2013) and Shaw et al. (2013).
P846 L21. As with all good autobiographies, deltas also get to remove and rewrite parts of their history. . .
>>> Good point…We changed it to "A delta writes (and rewrites) its own autobiography by preserving sedimentary record from deposition and erosion."

>>> References (addition to the manuscript and the comments above):

Edmonds, D. A., C. Paola, D. C. J. D. Hoyal, and B. A. Sheets (2011), Quantitative metrics that describe river deltas and their channel networks, J. Geophys. Res., 116, F04022, doi:10.1029/2010JF001955.

Falcini, F., and D. J. Jerolmack (2010), A potential vorticity theory for the formation of elongate channels in river deltas and lakes, J. Geophys. Res., 115, F04038, doi:10.1029/2010JF001802.

Passalacqua, P., S. Lanzoni, C. Paola, and A. Rinaldo (2013), Geomorphic signatures of deltaic processes and vegetation: The Ganges-Brahmaputra-Jamuna case study, J. Geophys. Res. Earth Surf., 118, 1838–1849, doi:10.1002/jgrf.20128.

Wolinsky, M. A., D. A. Edmonds, J. Martin, and C. Paola (2010), Delta allometry: Growth laws for river deltas, Geophys. Res. Lett., 37, L21403, doi:10.1029/2010GL044592.