Manuscript Esurf-2014-27 – Liang et al.
A reduced-complexity model for river delta formation: Part II – Validation of the flow routing scheme

Response to the reviewers

We thank both reviewers for their detailed and constructive comments. Their questions helped identify ambiguous points in the manuscript, and their suggestions pointed out good directions for future work. In this document, we respond to the comments received. The original text is in Italic style, copied and divided into a series of questions numbered at the beginning of each paragraph (e.g. B3 means review #2, question 3). The answers following the questions are led by a ">>>" symbol.

Response to Anonymous Referee #1:

All landscape evolution models require some quantification of the flow depth/velocity/shear stress over complex terrain. As such, flow routing models with a range of complexity are needed (from the simplest type of RCM to large-eddy simulation models that capture the full details of turbulence). This paper uses a weighted-random-walk solution method to estimate the depth-averaged flow velocities and flow depths in complex topographic environments. It describes a potentially useful approach. However, I had difficulty understanding some details of the method and the paper could be strengthened by including comparisons to existing RCM flow routing algorithms.

>>> Thank you for the thorough and insightful comments. We address your comments below.

Comments on the general presentation:

A1: *The introduction discusses/advertises RCMs in a manner that is too general. A general discussion of RCMs is appropriate for part 1 of this two-part paper but since this part 2 paper focuses on flow routing, the introduction should focus on RCMs of flow routing specifically rather than more general RCM references that are barely relevant (e.g. Werner's dune evolution model). Currently, the paper includes almost no mention of alternative RCMs of flow routing despite the fact that the literature has many such models that are similar to the algorithm proposed in this paper. For example, a standard RCM method for computing flow over complex terrain is the "storage cell" approach developed in the early 1960s. Such algorithms iteratively solve the continuity equation together with a drag relation (e.g. Manning's equation) over complex topography, typically using Newton's method (e.g. Bates and de Roo, 2000). With recent improvements, the LISFLOOD-LP scheme originally developed by Bates and de Roo has been shown to almost exactly reproduce the analytical solution of water surface profiles in benchmark cases, with computational speeds orders of magnitude greater than explicit solutions of the St. Venant equations (e.g. Bates et al., 2010). Another example is the "successive flow routing" method introduced in section 3.4 of Pelletier (2008) in which flow is introduced to a prescribed upstream cross-section into a complex topographic environment. The flow depth is then predicted using Manning's equation together with a multiple-flow-direction routing algorithm that honors conservation of discharge. A fraction of the predicted flow depth is then added to the bed topography to create a water-surface model that becomes the basis for flow partitioning in the subsequent iteration and the procedure is repeated until convergence. These methods and similar ones should be referenced and, at least qualitatively, compared with the proposed model.*

>>> We agree with the reviewer that a more detailed introduction on alternative reduce-complexity flow routing methods should be included. In the revised manuscript, we will provide an extended literature review and an extended discussion section to include the comparison between our routing scheme and other existing methods. These comparisons, however, will focus mainly on the choice of physical processes represented in the models, as currently they apply to different hydrodynamic environments such as coastal distributary network and fluvial floodplain inundation. We are not aware of any application of LISFLOOD-FP or of the successive flow routing method by Pelletier (2008) to deltaic environment. In fact, some of the existing flow routing methods will likely need adaption to be applicable to a deltaic environment due to its extremely low surface gradients, backwater effects, and the decoupling between fluid transport and bed topography, as mentioned in our companion paper (Part I, Liang et al., 2014). The RC flow routing method introduced by Nicholas et al. (2007, 2012) has shown promising performance for low gradient, low Froude number environments such as large sand-bed rivers, but it features a "row-by-row" routing approach (common among existing cellular routing models) that is not suitable for deltaic environments where features such as multi-thread and multi-direction flow paths are present.. We also would like to point out here that FlowRCM is part of our delta building model DeltaRCM (refer to Part I). The primary goal of this paper is to assess the flow routing scheme alone, which serves as a step stone towards a more complete understanding of the controls on delta morphology using DeltaRCM as an

exploratory tool. FlowRCM is designed to tackle the difficulties typical of deltaic environment modeling, such as low-Froude number flow, backwater effects, and complex terrain with distributary channel networks and islands. We hope to inspire the RC modeling community by presenting both the model's strength and weakness and we look forward to further improving our approach.

The inter-comparison between flow routing schemes in deltaic environments is certainly worthwhile and will be subject of future research.

More detailed comments on the method:

A2: *FlowRCM predicts upstream surface elevations that are generally >50% larger than Delft3D (Table 1) for the single bifurcation tests. This is a very large difference/error. No details are presented as to why such a large error exists. Given that RCM flow routing models exist that match analytic solutions for water surface profiles to a few percent or less in a broad range of cases (e.g. LISFLOOD-FP), the fact that the proposed method produces errors of 50% or more in some cases is troubling. In other words, this method could represent a step backwards in RCMs of flow routing.*

>>> It is true that the error in water surface calculation is large compared to other methods. We have identified the cause for this error and we will include our findings in our revised manuscript. FlowRCM calculates the discharge vector at each cell by 1) summing up the volume of all passing water parcels at each time step to obtain the magnitude of the discharge vector, and 2) doing a vector summation of the moving directions of all passing water parcels (each will be a unit direction pointing to one of the eight neighboring cells) to obtain the direction of the discharge vector. By doing so, in the area wherethere is flux convergence, some cross-stream flux caused by the parcels travelling almost sideways to the main flow direction will be counted as contributing to the total downstream flux as well, because they all exit from downstream. An alternative method would be directly doing a vector summation of the discharge vector represented by each passing parcel, but this way an under-estimation of discharge and water surface elevation would happen, but with a much smaller error.

As we continue to improve the prediction of water surface, we would like to mention that as part of DeltaRCM, the goal of FlowRCM does not include an accurate solution of the water surface. In fact, the error in the surface calculation does not seem to affect the ability of DeltaRCM to reproduce the morphodynamic features that so far have been obtained only from higher-fidelity models. As discussed in the companion paper (Part I), the inclusion of water surface is required but the accuracy can be relaxed when modeling deltas. We also would like to point out the value of the Lagrangian approach used by FlowRCM which potentially offers more flexibility in flow routing in complex terrains with low wet-to-dry ratio and frequent changes of wetted area.

A3: *It is unclear how the random walkers relate to unit discharge and/or flow depth. The paper states that "water flux is represented by a large number of small water parcels…" However, equations (3) and (4) show that the probability for routing the walkers is proportional to flow depth, not unit discharge. It seems more reasonable to me that the probability for routing the walkers should be based on unit discharge, not flow depth, (assuming that the number of walkers is used, at each iteration, as an estimate for unit discharge) since discharge is the quantity being conserved. The authors really should clarify how the "cumulative movements" (p. 7, line 21) of walkers relates to flow depth and/or unit discharge. My understanding of the method is that equations (1)-(4) exist simply to identify the streamlines input into equations (5) and (6). How those streamlines, defined as the distance coordinate, l, relate to the number of walkers or their cumulative movement or something else is not clear. More generally, it is necessary for the authors to show that the solutions satisfy flow continuity (to machine-level or at least very high accuracy) and that the results are independent of the grid resolution.*

>>> Here we clarify that at each iteration, both flow depth and unit discharge calculated from the previous iteration are used to calculate the new routing probabilities. More details can be found in the companion paper (Part I). To obtain a new unit discharge at each cell, the "cumulative movement" of all

passing parcels is calculated as a vector with direction equal to the average direction of these passing parcels and magnitude given by the summation of the flux carried by these parcels together.

In this parcel-based flow routing method, flow continuity is always satisfied because 1) there is no gain or loss of the water flux represented by each water parcel; 2) at each cell in the domain the number of parcels coming in is always equal to the number of parcels leaving that cell.

Eq. (5) and (6) not only use the streamlines identified from the new unit discharge field, but also use the value of discharge itself to update the water surface profile. During this process, the model assumes that the discharge at each cell remains the same, while the depth and velocity are updated according to the new water surface elevation. Therefore flow continuity remains satisfied.

As for dependency on grid resolution, FlowRCM does show a grid dependency, as each cell only sees its immediate neighbors and the flux is routed using cells as units rather than physical distances. Similarly, grid-dependency has been shown in a number of cellular routing models (e.g., Döschl and Ashmore, 2005) and was considered a fundamental property of cellular approaches (Nicholas, 2005), unless a relationship is introduced between RC model process parameterization and the spatial resolution (Nicholas et al., 2012). We will expand this discussion in the revised manuscript.

A4: *It would be helpful to see more details (e.g. tables with numerical values) summarizing the results of the different tests. For example, Figure 7 shows color maps of normalized flow velocity for Delft3D and FlowRCM. Were these normalized to the same maximum velocity? If not, it may be that FlowRCM is predicting velocities that are very different (in absolute value) from Delft3D even if the spatial variations in velocity that are similar to those predicted by Delft3D.*

>>> Yes, in Figure 7 results from both models are normalized to the same reference velocity, which is the upstream inlet channel velocity. And the colorbar on the left applies to both plots. To provide more details on the results, in the revised manuscript we will add more quantitative measures and discussion. We would also like to point out that in the original manuscript Fig. (8) and (9) provide two simple quantitative analyses of our test results. Figure 8 shows the velocity magnitude on top of the bump structure for both FlowRCM and Delft3D for the whole range of bump heights, scaled to the same reference velocity. Figure 9 shows a comparison of discharge asymmetry in a single bifurcation against the cross-sectional area asymmetry for both models.

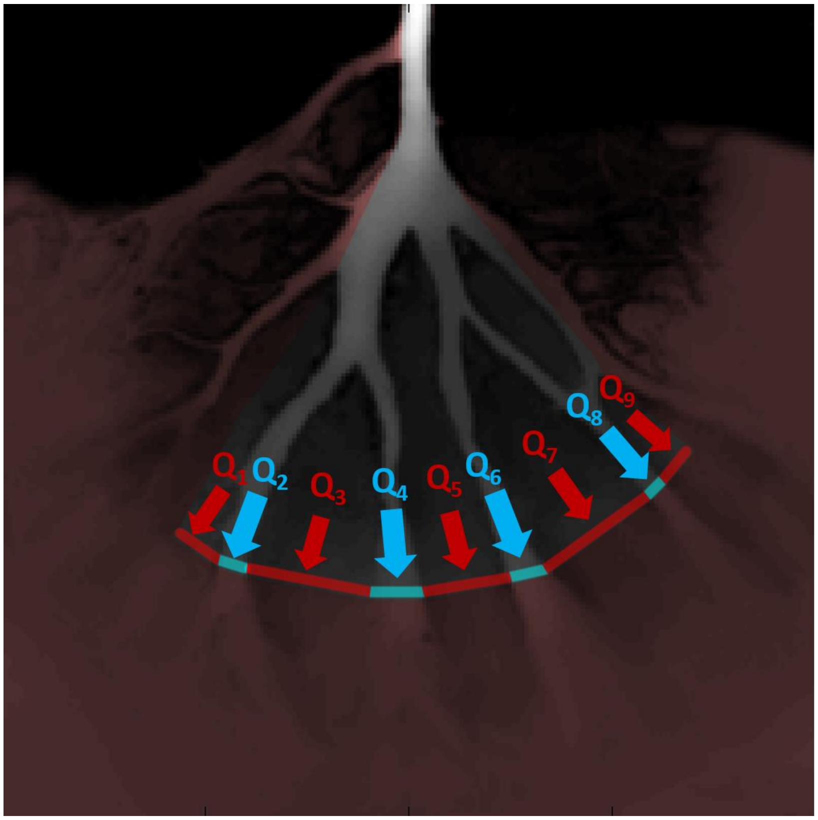
A5: *The paper would also be strengthened by including more detail on the computational speed of the method and the tradeoff with accuracy. No detailed information is given on the number of walkers that must be introduced in order to predict the streamlines with some prescribed level of accuracy nor is any specific information provided on the number of iteration steps required to achieve a specified level of convergence. The fact that the results do not converge is also troubling. It is stated that this problem is mitigated by computing the mean prediction over multiple iterations. An average over how many iterations? I appreciate that Monte-Carlo methods always have some variations due to finite sample sizes, but FlowRCM results in "oscillations" between multiple solutions that are troubling and not (as stated) a general byproduct of all probabilistic methods.*

>>> FlowRCM is written in Matlab and typically runs in 1-2 hours on a personal laptop for our tests. The running speed can be reduced by an order of magnitude if the code were written in more efficient languages (such as C) and optimized. That being said, we did not intend to design FlowRCM to replace other hydrodynamic solvers but rather to propose a flow routing approach that addresses the characteristics of deltaic environments, as mentioned in our answer to question A1. As for model convergence, we agree with the reviewer that more detailed information should be provided, and we have added that to the revised manuscript. Here we briefly address the "oscillation" behavior that is of main concern to the reviewer. The oscillations described in our manuscript are not entirely caused by the probabilistic approach. In fact they are also related to our reduced-complexity water surface calculation method which requires a large under-relaxation to achieve smooth transition in the surface profile. This

under-relaxation causes a delay between the response of the water discharge distribution to changes in water surface slope, therefore a "swing" in the cross-stream direction discharge distribution over 10-20 iterations. By averaging over several iterations, e.g. in the tests we use the last 100 iterations of the total 500 iterations, the results are very stable. More details are included in the revised manuscript.

A6: *It is stated that typically hundreds to thousands of walkers are introduced. Why is this the optimal range? Without any information provided on the speed advantage of this algorithm (with respect to Delft3D or any other alternative), it is difficult to evaluate the efficacy of the method. In general, implicit methods are much faster than explicit (e.g. finite-difference schemes), so I would be surprised if the proposed method is found to be faster than existing schemes (e.g. Bates et al., 2010).*
>>> With the "random walk" algorithm, the model works better with a large number of parcels. At the same time calculation time increases with increasing number of parcels. We found that the magnitude of one thousand works best in terms of efficiency. For model running time and efficiency please refer to our answer to question A5 above. As for the comparison between explicit and implicit methods, we agree that FlowRCM is not exhibiting superior running efficiency. We will explore in the future possible modifications to use an "implicit" routing scheme for better efficiency.

A7: *The choice of the 0.05 value for the gamma parameter appears to be entirely ad hoc. The relative strength of the inertial term to the bed slope term must come from (and vary with) the physical parameters of the system. Put another way, what would an appropriate value of gamma be for a steep alluvial fan (e.g. slope of 5%)? My guess is that it would be much greater than 0.05. How is gamma supposed to be chosen for a particular application?*
>>> The value of gamma affects the results of FlowRCM by controlling how sensitive is the flow response to the water surface slope. As described in the original manuscript, gamma is the partitioning coefficient between the routing direction by flow inertia (based on flow field from previous iteration) and the routing direction by water surface gradient. Ideally if the water surface elevation is resolved rigorously, such as using 2D shallow water equations, then water surface gradient alone could provide the routing direction. However, our 1D water surface calculation algorithm is not designed to resolve to that detail and is simplified to give just enough indication for adjustment in flow direction between iterations. For example, in the case of a distributary network, the higher concentration of water flux in channels will result in a slightly higher water surface profile than that of surrounding floodplain, which causes a lateral surface gradient pointing away from the channels to the floodplains. This mechanism in effect controls how much flow "escape" from channels and spread onto surrounding floodplain. We provide a figure below to show the effects of gamma quantitatively, based on the test 4a in our original manuscript. We change the value of gamma from 0.025 to 0.1 while keeping all other parameters exactly the same as test 4a, and we measure the ratio of the discharge coming out from four main channels and the discharge coming out from the downstream side of the islands between these main channels. It clearly shows that with a higher value of gamma, more flux come out from the islands. In future work we will compare this to field data from the Wax Lake Delta, when more detailed bathymetry map is available to replace our synthetic bathymetry.
The reviewer is correct that for steeper terrain such as alluvial fans it should be more than 0.05. The choice of the 0.05 value in our tests was partially based on trial-and-error, and partially based on a scale-dependent parameterization process that we have developed. This is a similar process as suggested by Nicholas et al. (2012), mentioned above in our answer to question A3 on dependence on grid resolution. The main idea is that by introducing scale-dependent parameters cellular routing schemes may become grid-independent. Currently we are testing the formulation of gamma, expressed as a function of a characteristic slope of the system, grid size and characteristic velocity. In this way, the steepness of the environment is directly taken into account. Our revised manuscript will provide more details on this parameterization process with explanations and test results.



$$Q_{channel} = Q_2 + Q_4 + Q_6 + Q_8$$

$$Q_{island} = Q_1 + Q_3 + Q_5 + Q_7 + Q_9$$

Gamma	$Q_{channel}/Q_{island}$
0.025	1.2554
0.05	1.1417
0.075	1.0161
0.1	0.9713

Figure (for A7): Here we show a quantitative analysis of the effect of parameter gamma based on test 4a in the original manuscript. The value of gamma changes from 0.025 to 0.1, while all other parameters are kept exactly the same as in test 4a. We measure the ratio of the discharge coming out from four main channels and the discharge coming out from the downstream side of the islands between these main channels. The results show that with a higher value of gamma causes more flux spread out from the channels onto the islands. This provides the base for calibrating gamma in future work.

A8: *It should be clearly noted in the manuscript that the method is really 1D. The portion of the method used to compute flow streamlines in 2D but the equations used to solve for the water surface gradients (equation (5) and (6)) are 1D. I suspect that the model would perform poorly in strongly 2D flows (i.e. flows of rapid changes in effective flow width) because of this. If the authors disagree, they should provide a test of FlowRCM versus Delft3D for a case with a transition from confined to totally unconfined flow.*

>>> The flow routing method we described in the manuscript is a 2-D flow distribution with a 1D component to construct water surface elevation. The water surface calculation is 1D, or it assumes that along the streamline the flow is 1D with no strong variations so that Eq. (5-6) apply. However, with the combination of a large number of flow streamlines that cover many flow paths, the constructed surface is a 2-D profile - this is equivalent to weaving a surface with multiple threads that go in different directions. This surface calculation does not work for strongly varying 2D flows. In terms of the performance of FlowRCM in a transition from confined to unconfined flow, both tests 4a and 4b use a distributary network that features flow spreading from a single feeding channel which is strictly confined by non-penetration high walls to an approximately 90 degree or 180 degree open space. Water not only flows through the channel network but also goes onto the flood plain. Therefore the whole delta surface, except for the upstream tips of a few higher islands, is inundated. The test results of 4a and 4b actually show that at the distributary network scale, FlowRCM achieves good performance compared to Delft3D, despite the simple 1D surface calculation involved.

A9: *It is worth noting in the revised manuscript that a key advantage of CFD models is that they are not depth-averaged. CFD models predict the bed shear stress directly (from the velocity gradient normal to the bed), not just the depth-averaged velocity. Any method that reduces the output of a CFD model to depth-averaged quantities is, in effect, dismissing one of the key advantages of such models. So, while it is good (and necessary) that the proposed model reproduces the depth-averaged velocity predicted by Delft3D in some cases, it should not be implied that such a model is equivalent to Delft3D even in cases in which the depth-averaged flow is exactly the same as that predicted by Delft3D. Any model that accurately predicts the 3D velocity/shear stress distribution is capable of modeling sediment transport with much greater fidelity compared with a model that predicts only depth-averaged flow properties.*
>>> CFD has the advantage of being able to resolve three-dimensional details such as turbulence structures. It is true that, ideally, everything should be simulated in unsteady 3D with advanced turbulence closure schemes, temperature and so forth, but at some point other effective descriptions take over. For example, depth-averaged models with appropriate parameterisations for 3D aspects (e.g., spiral flow in bends) can certainly be used for realistic sediment transport modelling. Nonetheless, FlowRCM cannot, and is not designed to, compete with CFD approaches. That being said, simple rule-based RCMs actually exist which resolve hydrodynamic details previously believed to be produced by CFD exclusively. Such models share many common characteristics as the Lattice-Boltzmann method for fluid flow (which is a sub-class of CFD), such as simple cellular automaton that can emulate the microscopic behavior of molecules in a fluid and reproduces the structure of vortices behind blocks (Wolfram, 2002).
As for sediment transport, the reviewer has a very good point; the detailed 3D structure flow may be key to many morphodynamic features, which cannot be captured by simple depth-averaged flow routing methods. In our revised manuscript we extend the discussion on this topic, including what we learned from our modeling experience on the meaning of “accuracy” in hydrodynamic simulations with sediment transport modeling as main goal.

References:

Bates, P.D., and A.P.J. De Roo (2000), A simple raster-based model for flood inundation simulation, J. Hydrol., 236 (1–2), 54–77.
Bates, P.D., M.S. Horritt, and T.J. Fewtrell (2010), A simple inertial formulation of the shallow water equations for efficient two-dimensional flood inundation modelling, J. Hydrol., 387, 33-45.
Pelletier, J.D. (2008) Quantitative Modeling of Earth Surface Processes, Cambridge U. Press.

Response to Dr. Andrew Ashton:

In this companion paper, Liang et al. conduct a series of tests of the hydrodynamic component of the model presented in Part 1. In essence this is more of a nitty-gritty paper that has the objective of assuaging those that would take objection to the simplification of hydrodynamics employed in the full morphologic model. I think that it was a good decision to split this test of hydrodynamics into a separate submission as the tests here are meant to develop trust in the model approach, but the results here do not necessarily affect any of the findings in the first part.

>>> We thank Dr. Ashton for his constructive comments and for pointing out the connection of this work to our delta modeling efforts and yet its independent value. Please see our responses below.

B1: *Perhaps I am being overly pedantic, but my largest concern with the MS is the use throughout of "validation" as an objective and an outcome of the research that is presented. Of course, much technical and philosophical debate can be made about "validation," and the authors touch upon some of this discussion in the background. To me, validation typically would require a "thumbs up" or "thumbs down" final assessment, or at least a quantification between these binary limits. However, I'm not sure that the types of model tests here are best termed as validation. First, they are varied and the comparisons are, for the most part, generally in the category of "sniff tests" of qualitative behavior. Second, and more importantly, the tests here are mostly comparisons to the results of the RCM with theory or depth-averaged runs of the Delft3D model, neither of which is nature. These are mostly model-model tests comparison of the model results to nature is of course is what is needed for a true validity test. Perhaps I am only arguing semantics, but I think the authors could use more useful and specific terminology to describe the types of model tests and comparisons that they are making.*
>>> We agree with Dr. Ashton on the meaning of "validation", which has a quantitative nature and usually requires a "pass" or "fail" conclusion. Accordingly, we have changed the word "validation" to "assessment". However, in the context of DeltaRCM (refer to Part I), the model was not developed for hydrodynamic simulations. We would rather see it as a simple large-scale flow approximation (similar to Murray and Paola, 1994) that can be "stretched" to simulate hydrodynamics at in-channel scale.
The goal of these tests is to identify the level of complexity that FlowRCM can resolve, with a well-established and well-tested CFD model (Delft3D) as reference.

- We consider that the shallow water solution of Delft3D is already trustworthy as a close approximation of nature.
- The goal is not to match Delft3D, but to identify similarities and differences in modeling results. We are interested in finding what is needed to model a delta and what hydrodynamic feature is responsible of which morphodynamic outcome, although we are aware that even if a relation is identified is not necessarily the real or the only cause-and-effect in nature.
- We hope to inform other researchers in this field and provide intuitive insights that are different from reductionist point of view.

B2: *This follows from comments I had about the unconstrained parameters from Part 1, as the objective here is a test of the model hydrodynamic routine, I find it surprising that the authors present no systematic tests of the vital unconstrained hydraulic parameters. Otherwise it seems like the model recreates hydrodynamics once those values (may) have been sufficiently tweaked, with the exception of the backwater test (which may or may not be calibrated).*
>>> We agree that parameters especially related to our flow routing scheme should be explained and tested in more detail in this work. In the revision we provide such explanation and tests (e.g. the choice and the effect of the parameter gamma). Please refer to our answer to question A7 from review #1, which has a brief explanation of the effects of parameter gamma. We also provide guidelines for potential users

on how to set the parameters. For a complete list of the key parameters, we have added a table listing the physical meaning of the model parameters and their effect on the modeling results.

B3: *As this work has a strict focus on hydrodynamics, I agree with the anonymous reviewer that perhaps the authors could do a more thorough job reviewing previous published applications of RCM's for fluid modeling (rather than the morphodynamic ones listed here). One that might be of interest is the Murray and Reydellet, 2001, model that also uses simplifications and smoothing for flows where there are small slopes. Other models exist for estuaries and lagoons. Regardless, the background should address hydrodynamic models.*
>>> This is the same issue addressed by reviewer #1 and we strongly agree with both reviewers on the importance of a more comprehensive review on hydrodynamic models, especially existing RC flow routing schems. Murray and Reydellet (2001) is indeed a good example of explaining the formulation process of an RCM. Please refer to our answer to A1. The revised manuscript will include more details.

B4: *I find the format of presenting all of the experiment set-ups and then all of the model results to be rather distracting. This seems to be one of those times when the methods results approach becomes too limiting. Why not present the model set-up and then the results for each test case immediately after? It certainly would help the reader from flipping back and forth through the MS.*
>>> We rearranged the tests set-up and results as suggested above in the revised manuscript.

Otherwise, I appreciate the effort that the authors have taken to perform these numerous tests of the model algorithm. Some tests seem to be more successful than others, but there is a benefit that these differences are highlighted. I don't know if these tests necessarily add more significance to the general results of Part 1, but it does help establish that the hydrodynamic model is doing most of what it should be doing, at least in terms of other hydrodynamic models.

Regards,
Andrew Ashton

References
Murray, A.B., Reydellet, G., 2001. A rip current model based on a hypothesized wave/current interaction. J. Coast. Res. 17,3,517-530.

- - - *Other notes:*
P879 L13. "both the x"
L881 L13. "domain sizes"- check for consistency for how groups are addressed.
P882 L6. Change of tense. Check for the same tense (past or present) throughout.
Figure 6. This figure could use some touch up: the model result boxes are so large and numerous that the empirical backwater curve cannot be seen. This leaves the impression Also the caption text could use cleaning up.
Figure 7. Legend needs labels.
Figure 10. Caption needs cleaning up.
>>> All the above changes have been made

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

Doeschl-Wilson, A. B. and Ashmore, P. E.: Assessing a numerical cellular braided-stream model with a physical model. Earth Surf. Process. Landforms, 30: 519–540. doi: 10.1002/esp.1146, 2005.
Wolfram, S.: A new kind of science, Wolfram Media, Champaign, IL, 2002.