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Earth Surface Dynamics Discussions

Interactive comment on "A reduced-complexity model for river delta formation – Part 2: Validation of the flow routing scheme" *by* M. Liang et al.

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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.

Comments on the general presentation:

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.

More detailed comments on the method:

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 metod could represent a step backwards in RCMs of flow routing.

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.

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.

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.

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).

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?

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.

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.

References:

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Interactive comment on Earth Surf. Dynam. Discuss., 2, 871, 2014.