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Earth Surface Dynamics Discussions

Interactive comment on "Modelling braided river morphodynamics using a particle travel length framework" *by* Alan Kasprak et al.

Anonymous Referee #3

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Overview:

This paper presents a new morphodynamic modelling approach, whereby sediment transport is simulated through particle travel lengths algorithm rather than the more traditional flow-field and gravity-driven sediment algorithms. The model is applied to two case-study river reaches over different time scales.

Evaluation:

The paper is well-written and logically structured. The approach is novel and appears quite promising, both in terms of flexibility and initial results. I very much like the concept of the paper, and I would like to see it published eventually. However, the study could be significantly strengthened by a more comprehensive comparison with existing

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Discussion paper

modelling approaches, as outlined below. This additional element of analysis would probably constitute a major revision.

Comments:

1) The paper presents a new approach to simulating sediment transport in braided rivers. The authors claim advantages over existing simulation approaches such as reduced complexity modelling (RCM) which is lacking physical explanation and fidelity (p3 ln5) and computational fluid dynamics (CFD) which has too high computational overheads (p3 ln12). However, it is not clear from the paper that the new approach proposed by the authors produces comparable or better results than these existing approaches in terms of simulated morphologies. A direct comparing and contrasting with simulation results from these existing RCM and CFD approaches, highlighting both strengths and weaknesses of the particle travel lengths approach, would thus significantly strengthen the paper. The authors clearly are familiar with Delft-2D which should provide suitable CFD comparison simulations. On the RCM side, a model like CAESAR (with which at least one of the authors is also familiar) could provide suitable comparison simulations. The authors are of course welcome to choose other CFD and RCM models to compare to. But both these suggested models are able to simulate event-based scenarios and both are capable of simulating transport of multiple grainsizes, their simulations should be directly comparable to the simulations with the authors' particle travel lengths approach.

2) The authors note that the CFD models rely on the Exner equation to calculate bed elevation change (p3 ln10; p8 ln7). First it is worth noting that the RCM essentially do the same, in one way or another. More importantly, the authors suggest that they use an alternative approach to sediment continuity (p8 ln18). However, it is not clear that this indeed is the case. The authors present an approach to calculate the total erosion as a scour depth (p10 eq7). But surely, when it comes to adjusting the bed elevations the authors will still apply an equivalent of the Exner equation to ensure that the amount of material that is eroded matches this depth of erosion (or bed elevation

change).

3a) Lateral erosion is calculated in a simplified manner, i.e. scaled to near-bank shear stress (p9 ln18). In their conclusion the authors note that is unknown if this simplified lateral erosion model will provide stability over longer-term simulations. But it seems that their approach is similar, at least in its core concept, to the approach by Ikeda et al. (1981) that scales lateral erosion to near-bank excess flow velocity and that was later successfully applied in several studies over longer-term simulations (e.g. Howard, 1992, 1996; Sun et al., 1996, 2001; Stolum, 1998; and many others).

3b) It is not entirely clear how the lateral erosion module is implemented in practice. First all cells with steep slopes are identified, as possible targets for lateral erosion. For this steep slopes apparently are those with a gradient >7%. This seems excessively low, as most banks with gradients < 30% will be stable. The 7% threshold was identified through calibration (p9 ln25), but it is not clear how this calibration was done, to what accuracy, or on what data. Near-bank bed shear stresses are calculated using a 3x5 neighbourghood window, although it is not entirely clear how the orientation of the neighbourhood is determined. It seems to be based on the dominant cardinal aspect (fig 2.3A), but this is not explicitly identified as such. Finally, the total extent of the bank failure is calculated (p10 eq11). I presume this relates to the red delineated area in Fig 2.4, but it is not clear how that shape of that area is obtained – despite the authors attempt to describe this (p10 ln13). Further is not clear why there only are erosion values in the brown cells (Fig 2.4A) whilst these only are a sub-set of the red delineated bank erosion extent (Fig 2.4).

4) The authors lament the lack of physical explanation and morphological fidelity in RCM (p3 ln5), although they do not provide a proper argument or reference to support this claim. It is undoubtedly true that RCM, by their very nature, make some very simplifying, rule-based assumptions – but that does not necessarily mean that they therefore also lack physical explanation or morphological fidelity. Moreover, the authors make several very simplifying rule-based assumptions in their own approach – most

notably in the particle travel length approach itself, but also in the approach to sediment continuity, sediment deposition, and bank erosion. Thus, it could well be argued that the authors' model is itself a RCM (except for its CFD derivation of the flow field).

5) The model did not produce avulsions seen in the field (p22 ln13). Is the model inherently incapable of producing avulsions? Or is it capable in principle, but just did not do it in these simulations. If the former, this seems a major drawback (a fatal drawback??) for an algorithm that is designed specifically for braided rivers. If the latter, what would be the reason for not simulating the avulsions. This also relates to the broader discussion, where the authors claim that the model did reproduce all field-based braiding mechanisms (p28 ln29). This somewhat contradictory conclusion arises because the authors base this on a set of 10 braiding mechanisms (identified section 2.7.3). But, rather curiously, avulsion is not one of these braiding mechanisms. Subsequently, the authors claim that their model can simulate eight of these braiding mechanisms from sediment transport alone (p23 ln 7) and two more with additional algorithms. In other words, all 10 braiding mechanisms were reproduced in the simulations (p28 ln29). However, the key process of avulsion, although observed in the field (p22 ln13), is not considered in this – which seems rather flawed.

Minor edits:

p9 ln7: Eq.(10) –> Eq.(11)

p12 ln2: many –> may

fig 2: It is somewhat confusing that subfigure 3A is placed next to subfigure 2. Intuitively one would expect each of the detail views to be associated with the workflow view to the left of it. It is for 1, 3B and 4, but not for 3A.

fig 2: What is the colour scale for the figures in the third column?

fig 2: Caption needs adjusting to account for third column.

References:

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