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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.

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Anonymous Referee #3

General Comments

Referee: 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.

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

Response: We thank the reviewer for these generally positive comments, and have addressed their suggestions in the pages that follow.

Specific Comments

Referee: 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.

Response: We appreciate this comment and agree that such a benchmarking study would be both interesting and insightful with regard to the path-length model's utility and validity going forward. At the same time, we feel that such an endeavor is well beyond the scope of the current manuscript, which simply seeks to present and provide case studies for the use of a novel modeling approach, one which we believe makes significant strides in computational efficiency by moving beyond the traditional particle-tracking and/or Exner-based approaches for computing morphodynamics. Such a benchmarking approach would be a logical next step in our development of this model and would make for an interesting follow-on manuscript; while not part of the current work, we have added several pages to the Discussion within a new section (5.2.3), which take advantage of past RC/CA and CFD modeling done by Williams et al. (2016a,b) on the Rees River to directly compare the results of all three modeling approaches and suggest improvements common among the techniques going forward.

Referee: 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).

Response: This is correct, and was a question that was also raised by Reviewer 2 above, and we have elaborated on the approach used on page 8, along with adding a new Figure (Fig. 4) to illustrate the erosion and deposition algorithms used in the model.

Referee: 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).

Response: We appreciate the Reviewer noting the similarity between our use of near-bank shear stress to predict bank retreat with those approaches developed by previous researchers. We have included the references listed above on page 11. When we raised the question of whether such an approach would yield stability over longer-term simulations, we were largely referring to the use of a once-per-flood (i.e., event-scale) technique for eroding bank material, as opposed to computing lateral retreat many times over the course of a flood. We have clarified this on page 29.

Referee: 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).

Response: The text within Section 2.3 has been significantly revised in accordance with Reviewer 2's suggestions above. We have noted in the revised text that while the 7% threshold is certainly below the angle of repose, it provides an inclusive first-approximation of cells that may be candidates for lateral retreat, and these cells are further refined through the use of a near-bank shear cutoff. In addition, because lateral retreat was only computed once per flood, we took an inclusive approach (i.e., low slope threshold) here to account for those cells that may undergo slope failure from progressive bank steepening over the course of a flood event. The 7% threshold was initially chosen by examining the average slope of cells that subsequently underwent lateral retreat in field data, which has been noted on page 10. The 3x5 window was indeed oriented in the cardinal aspect of the candidate cell, and we have now noted this in the text on page 10. The values within the brown cells (Panel 4A, Figure 4) correspond to the computed extents around those cells where bank erosion occurs – essentially, the brown cells are "padded" by this value, and the cells falling within that padding window undergo bank erosion, which produces the red polygon in Panel 4. We have clarified this approach on page 11.

Referee: 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).

Response: This is a fair point, and we have restricted our statement on RCMs to simply note that one shortcoming of these models is that they are often unsuitable for prediction of any specific geomorphic system (in the sense that the Murray and Paola model produced a characteristic planform of 'a' braided river, but not a specific braided river), and are thus useful for investigating the effects of shifting boundary conditions in a generalized sense, but not for explicit predictions of channel dynamics for any one system.

Referee: 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.

Response: The model did produce avulsions, the most notable of which is the development of an incised anabranch on the left-hand side of the braidplain at the upstream end of the reach (Figure 11), where no channel existed prior to the simulation. On page 22, we were referring to the fact that the model did not produce avulsions in the same locations as observed in the field, and overall, the model produced a more stabilized (i.e., incised) channel planform than that seen in the field, which implies that the model did not produce avulsions at the rate seen during the period 2003-2013 on the Feshie; this is further evidenced by the reduction in channel nodes in the model as compared to field data. Algorithmically, the model is indeed capable of producing avulsions through the lateral retreat of banks (and direct downcutting of braidplain/bed areas when flow is sufficiently high). We have augmented the text on page 22 for clarity.

We used the braiding mechanisms developed by Ashmore (1992) and expanded by Wheaton et al. (2013), avulsion was not explicitly included in the list of ten mechanisms assessed here. We would argue, however, that avulsions are the product of one or more of the braiding mechanisms we did assess. In particular, bank erosion, chute cutoff, and central bar development, among others, can lead to avulsion (e.g., Ferguson, 1993). In our simulations, it is possible that these processes did not occur with sufficient magnitude relative to channel incision such that the frequency of avulsions seen in the field was matched by that in the model. We have added a paragraph to the Discussion (page 24) to emphasize this.

Technical Corrections

Referee: p9 ln7: Eq.(10) –> Eq.(11)

Response: We're unsure what correction the reviewer is suggesting here; we've checked the numbering and references to each equation and can't find any mistakes.

Referee: p12 ln2: many –> may

Response: This section (originally 2.6) has been omitted from the text in the revised manuscript.

Referee: 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.

Response: We agree, and have added arrows linking the sub-figures with their appropriate panels in Figure 3 to avoid confusion.

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

Response: We believe that the reviewer is referring to Figures 7, 9, and 11 here. The colors correspond to each of the braiding mechanisms, which are described in subpart (G) of each figure. We've updated the caption to reflect this.

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

Response: See above

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