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Interactive comment

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

Anonymous Referee #2

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Review of paper "Modelling braided river morphodynamics using a particle travel length framework", submitted to ESurf by Kasprak et al.

Overview

This is a well written exposition of a novel hybrid morphodynamic modelling approach that seeks to overcome the CPU-time limitations of conventional physics-based numerical models, thereby enabling the simulation of braided rivers over extended time and spatial scales. The CPU-time savings accrue from setting the morphological time step at the event scale. The event hydrograph is represented by a single discharge for which a robust steady-state hydrodynamic solution is derived. This is used to estimate entrainment from bank erosion and bed scour, with the entrained sediment then advected downstream using an assumed distribution for particle travel length and also

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Discussion paper

dispersed laterally. The approach is demonstrated using the case examples of the braided Rees and Feshie Rivers, with comparisons made between surveyed and modelled DEMs-of-difference in regard to volume-weighted elevation change distributions and classifications of braiding process. Nested "experiments" are also run that compare the model results when an event is represented by several quasi-steady flows rather than one, and when different types of path-length distributions are assumed. Extended discussion examines the limitations observed with the model results and around model assumptions, with good pointers on where to focus improvements.

Specific comments

1. On page 7 lines 1-6 nothing is said about the duration of the steady upstream discharge (which is set at the peak discharge). Surely, this is critical for scaling the volume and at least the erosion depth axes on the ECDs, and for matching these to the surveyed ECDs. How was this set?

2. Pursuing this further, applying a scaling-factor to the bedload transport function would induce similar effects to varying the event "duration", so what about using this concept as a formal calibration approach – adjusting the duration or BT function to align model ECDs to the few observed field results, then running the so-calibrated model for longer times scales? It seems that so far, the paper only considers using the Rees and Feshie field data for validation purposes, not calibration.

3. On page 7, around lines 20-25, it appears that only the median grain size is used for calculating the threshold stress, which is then used to calculate the bedload transport with equation 8 (for use in equation 7). While the method is claimed to be multi-fractional, there is nothing said about hiding factors, partial transport, different path-length distributions by grainsize, etc.). Equal-mobility is implicitly assumed, so effectively, entrainment and deposition are undertaken only considering the median size, so (apart from remixing of the active and sub-surface layers at the end of the event, as explained on page 30) the method doesn't actually do multi-fraction processes. This

means that it will not produce armouring, which can be an important control on scour depth. So, claiming the approach is multi-fraction is not true – it only pays lip-service to being multi-fraction.

4. On page 8, line 2 -3, it's not clear if 10 cells are being taken upstream and another 10 cells downstream or if 10 cells are being taken centred on the current cell. If the former, then in the case of the Rees mode,l 40 m averaging appears too excessive. Please clarify and justify if need be.

5. On page 8, lines 15-18, the logic around the alternative to the Exner equation is never completed. What appears to be done (but is not properly explained) is that "clear-water" scour is first calculated everywhere it can potentially occur, with a Ds assigned to each cell, then a deposited sediment thickness is assigned to cells downstream of erosion sites according to the path-length distribution and smearing algorithms. Thus, some cells will only experience scour, some only deposition, and some that scour will also experience deposition from sediment sources from scour sites upstream. Net elevation change at-a-cell is therefore calculated as the difference between scour and deposition. This needs to be explained, and if I have the story wrong, the right story needs explaining please.

6. On pages 10-11, a diagram (similar in concept to Fig 2) is needed to help explain the distribution of sediment deposition.

7. Page 14, lines 21-41, the methodology used for classifying braiding mechanisms is not stated. Was this purely subjective and done manually, or were algorithms employed that obeyed a set of rules? What were the guiding rules? Was the same approach applied to the field data and the model output?

8. Page 16, lines 6-9. Isn't it more a case that the Rees, by virtue of its frequent runoff events and significantly finer bed material (from its schist catchment), is a lot more labile which inhibits woody vegetation (and associated fine sediment trapping) from establishing?

9. Section 4.1.2 addresses only validation results, since no calibration was actually done. So, in line 11 on page 17 replace "calibration" with "validation". Also, on page 17 around lines 19-22 it would be useful to mention the Ddiff and Fc numbers given in the Figure. Also, I recommend also providing the RMSE of depth differences. The mean Ddiff statistic reported only informs on any overall bias in depth and tells nothing about how badly the depth may have been predicted locally.

10. Section 4.2, page 17-18, several things. First, some more information is needed here to clarify what was done in the modelling – particularly confirm what discharge was used and how long it was run for. Second, no mention is made at all in the text about thresholding but it is the thresholded results of volume change that are being discussed, not the gross ones. I would expect to see some comparison and discussion, particularly since the modelling doesn't have thresholding issues. Third, in lines 22-23 the statement that "average magnitudes of erosion and deposition agree well between field and model results" is a bit rose-tinted, because the differences appear to be in the range of 33-50%. Fourth, it would be very informative to compare the MoPHED-modelled results for the Rees event with the results of the full Delft3D model run of the same event (already done by Williams et al). Fifth (and again), what were the durations of the steady-flow model runs, how were they decided, and how do they impact on the results (I am assuming that when "model run time" is mentioned that this is processing time, not event duration). It would be useful to show the discretised quasi-steady hydrograph time-spans for the single event discretisation and for the 3-event discretisation on top of the hydrograph in Fig 7A.

11. Page 19, lines 20-29. Isn't all this just saying that you picked events when the flows were competent as indicated by Ashworth and Ferguson's observed threshold? The "low bankfull" flow story seems like an unnecessary complication.

12. Page 22, line 10. I disagree with this stated general agreement in form of the ECD. The model-predicted ECD is clearly more erosion skewed than what was observed. Indeed, what the model appears to have done with these prolonged runs is to cut

straight, deepening “ditches”. I’ve experienced this before with long Delft3D runs (e.g. Singh et al), and I suspect it stems from inadequate handling of bank erosion and probably also lack of armouring. The model has also produced net degradation that is 2+ times the magnitude of the net aggradation that was observed in the field. So, there is an un-natural emergent behaviour occurring here in the model, which needs to be discussed – and hopefully dealt to with some clever workaround. It would be useful to provide the changes in the sinuosity over the decade of model runs to quantify this trait.

13. Page 23, line 25-26. Yes, this is an important point – the latency between flow, sediment transport and morphologic response is not captured by the quasi-steady approximation, so transient features during floods cannot be captured. Indeed, at least in the case of single event model runs, the surveyed starting morphology is one modified by recession processes, and so may not be representative of the morphology during the event peak.

14. Section 5.2.1. Following on from above, the discretisation of events into a single “time step” does not allow for the possibility that multiple scour and deposition cycles may occur within the actual event, with topography being “recycled” and the ECD capturing less the signature of discrete processes but a blurred composite. This issue is an old one of course (and befuddles application of the “morphologic method” for measuring bedload).

15. Section 5.2.3. I suggest a Discussion paragraph here around the straight ditch-cutting behaviour shown by the decadal simulation of the Feshie. It’s an important concern for running the model for long periods, particularly since it turns an aggrading reach into a degrading one. This also compromises the statement on page 17, line 30-31 regarding unknown long-term computational stability – I’d say there is already a known issue appearing as it produces un-natural emergent behaviour.

Technical corrections/suggestions

P1, L20: What does multi-scalar mean here; in fact, why do you need to say this at all?

P7, L14: Supply exhaustion might affect the actual sediment transport but it doesn't change the transport capacity.

P8, L22: Qb is the unit bedload transport.

P15, L9: There's not much glacial melt in the Rees – it's mainly rainfall-driven with a seasonal snow and snowmelt signal.

P17, L11: These are validation results, not calibration results.

P 17, Equation 17: The union sign should appear in the denominator. As it is, the intersection sign appears in both numerator and denominator.

P19, L5: Section reference wrong – this is Sect 4.2.1.

P21, L6: Suggest replacing "model" with "models with the different PLDs".

P28, L10: Insert "a steady flow set to the event peak discharge and" between "using" and "path".

Table 1: It should be D84 in the 6th column, not D50 (as evident from lines 19-27 on page 6). Also, it should be C-W Roughness (after Colebrook-White) not W-C roughness.

Figure 10: The text is too small to read.

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