Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-17-AC2, 2018

Earth Surface Dynamics Discussions

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

Alan Kasprak et al.

akasprak@usgs.gov

Received and published: 22 June 2018

Anonymous Referee #2

General Comments

Referee: 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

Printer-friendly version

Discussion paper

sediment then advected downstream using an assumed distribution for particle travel length and also 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.

Response: Thank you for these positive comments on the paper and its relevance to the field.

Specific Comments

Referee: 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?

Response: This is because we did not attempt to scale sediment transport with event duration; the model presented here is purely event-based, operating at a timestep of a single flood, regardless of that flood's duration. Our aim was to assess the predictive capability of such an approach. We have added text to clarify this, along with specifying that floods of longer duration, specifically those capable of producing multiple entrainment events for particles, or which fundamentally alter bar spacing and path length distributions, may require multiple model timesteps. Further, Section 4.2.1. does provide an experiment where a single flood of long duration is discretized into multiple timesteps, thus demonstrating one approach for dealing with extended periods of competent flow. We have added text to Section 2 (specifically on pages 8 and

9) to emphasize this point.

Referee: 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.

Response: This is an interesting suggestion, and although a rigorous calibration using adjustment of the bedload transport function or event duration was not employed here, it may represent a promising way forward in improving model fidelity. Further, a simple scaling of the bedload transport function (Eq. 7) may elucidate the variability in scour depth on a system-to-system basis, providing insight into the propensity or resistance of various channel planforms to geomrophic change. While such a calibration exercise is beyond the scope of the present manuscript, we have alluded to the potential for future use of such an approach on Page 27 (Discussion).

Referee: 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 multifractional, there is nothing said about hiding factors, partial transport, different pathlength 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.

Response: Because this manuscript only presents the results of single-fraction modeling, as the reviewer rightly points out, we've removed this section, and the original Figure 4, for simplicity.

Referee: 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.

Response: It is the former; we have added text to clarify this. While smaller values for this averaging may be suitable on the Rees, we found that this approach avoided morphologic artifacts (e.g., scouring excessively deep pools) during model runs. We also believe this averaging distance is justified given that the confluence-diffluence spacing on the Rees, and hence the average distance between areas experiencing scour and deposition, is ~350 m (Figure 3), which is approximately 9 times the averaging window used here.

Referee: 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 "clearwater" 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.

Response: This interpretation is completely correct; we have further clarified these process representations on page 9.

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

Response: We've added a new figure (revised Figure 4) that explains the algorithm for sediment distribution.

Referee: 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?

Response: This was completed for both field and model data. The approach is subjective and interpretive, similar to geomorphic mapping of surficial deposits. However, the approach has been widely used, and efforts aimed at automating the process have been introduced in the literature (see, for example, Kasprak et al. 2017, ESPL). At the same time, divergent braiding mechanisms are revealed quite well in DoDs (for example, high-magnitude narrow swaths of erosion are indicative of bank erosion, while thin-mantled widespread deposition indicate that overbank sheets were responsible for elevation changes in a given area).

Referee: 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?

Response: This is likely the case, although it's unclear whether vegetation inhibits geomorphic change or vice versa; we've inserted text near here to note that the dynamism of the Rees likely precludes vegetation establishment.

Referee: 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.

Response: We've changed 'calibration' to 'validation' throughout this section, and have stated the Ddiff and Fc values in the text as well as in the associated Figure (6), along with listing the RMS of depth differences in Figure 6.

Referee: 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.

Response: In Section 4.2, we've now clarified that a single discharge of 75 cumecs was used in this modeling; there was no duration component to this value (see item 1 above); this also pertains to the 5th suggestion provided by the reviewer here. There is no 'duration' for an event modeling run (that is, modeling was in no way scaled by the duration of the event, but instead can be thought of as a single 'snapshot' in time. We have also indicated that the thresholded model results are being presented here; the rationale for using the thresholded results was simply to allow for consistent comparisons between field and model DoDs. See also our response to Reviewer 3, but in short we've added a new section in the Discussion (5.2.3) that directly compares our results with those of Williams et al. (2016, WRR). We've removed the sentence about agreement between field and model average depths of erosion and deposition, given that the reviewer rightfully points out the discrepancy between these values, and the fact that this sentence interpreted results and was out of place in this Section.

Referee: 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.

Response: We've eliminated the references to 'low bankfull' and have simply noted that these events were competent.

Referee: 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.

Response: We have modified the sentence regarding agreement between field and model ECDs to only refer to the depositional fraction, which was similar between both field and model, and have discussed this and referenced Singh et al. (2017)'s work using Delft 3D on page 22. The sinuosity (and change therein) is presented in Figure 11, but exhibited little variability following the model run, likely because many of the anabranches in 2003 did not completely fill in (as a result of the lack of avulsions), but rather remained hydrologically active in addition to the deepened central anabranch.

However, the number of channel nodes provide a good indication that after 10 years, the channel planform was simplified, and this reduction is presented in the text along with Figure 11.

Referee: 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.

Response: Thank you for pointing out the importance of this statement.

Referee: 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).

Response: This is a good point, and the text we've added to address comment (1) above mentions that this is a particular disadvantage for using an event timestep to model long-duration floods.

Referee: 15. Section 5.2.3. I suggest a Discussion paragraph here around the straight ditchcutting 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.

Response: We have added several sentences on this behaviour on page 28, incorporating the reviewer's suggestion that this over-scouring may be an artefact of a lack of bed armouring, improper bank erosion representation, or a combination thereof.

Technical Corrections

Referee: P1, L20: What does multi-scalar mean here; in fact, why do you need to say this at all? Response: We've removed 'multi-scalar' from the abstract text.

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

Response: Removed 'capacity' from this sentence.

Referee: P8, L22: Qb is the unit bedload transport.

Response: Augmented the text here to note that Qb is referring to the unit bedload transport rate.

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

Response: Corrected to note that floods are the byproduct of snowmelt and rainstorms.

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

Response: Changed 'calibration' to 'validation' here.

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

Response: Thanks for catching this! Replaced the intersection with union sign in the denominator of Equation 17.

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

Response: As in response to Reviewer 1's comment, we've removed this section reference.

Referee: P21, L6: Suggest replacing “model” with “models with the different PLDs”.

Response: We’ve changed this to “modelling different path length distributions”.

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

Response: This text has been inserted as suggested.

Referee: 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.

Response: We’ve changed both of these column headings as suggested.

Referee: Figure 10: The text is too small to read.

Response: Thanks for pointing this out – we’ve enlarged the text accordingly

---

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2018-17, 2018.