Re-review of paper "Modelling braided river morphodynamics using a particle length framework" submitted by Kasprak et al.

This is a re-review of this paper so I will cut to the chase. The authors have done a generally good job of attending to previous review comments but there remain several issues with the present version that should be addressed. I will discuss those first, then list smaller/minor page by page things warranting corrections.

1. In the abstract (and later in the paper) the notion of representing hydrographs as a series of steady states is misleading – it continues to imply a duration associated with each steady state (note that I fully understand how time is removed from the calculations), and it implies that this expediency is at the root of the computational time saving over CFD type approaches. The implication is that by chopping event hydrographs down into smaller and smaller blocks the approach might converge on a CFD result. But that is never done in the paper – the example provided from the Rees is actually three events back to back – it is not a single event hydrograph split in 3 time blocks.
   Indeed, a key (unstated) assumption of the approach is that each event modelled has adequate duration to allow the bedload to get to its destination – so you can see that breaking events up into smaller and smaller intervals will ultimately badly overcook the results, not give them greater precision. While I appreciate that this is never actually done, the paper hints that it could be.
2. On page 9 there are two things that niggle me. The first is that Montgomery's event averaged relation (eqn 7) is applied to the event peak discharge then assumed to be representative of the whole event – so all of sudden event averaged Qb becomes Qb at peak discharge, and ditto for bedload advection speed. What Eqn 7 is really predicting at the event peak is the active layer thickness. The second is that Eqn 8 is not the MPM formula – the scaling coefficient (8) is missing as are the density terms. The net result is that a factor of ~ 0.4 is missing from the equation. Hopefully this was an omission in the text and not the model.
3. Section 2.5. The input boundary condition option used needs to specified for each model run. What was done in the cases of the first event in a series or for single events, like the first Rees example – since there is not a preceding event calculation?
   Also,what was done at the lateral boundaries? It reads like bedload was allowed to diffuse across them but was not returned (e.g. on the opposite side) so the modelling should progressively lose sediment mass.
4. Two things on page 19. First, on line 18, the notion of altering the model timestep is misleading (as discussed in 1. above). The example used is actually a multipeak event, but is dominated by the big middle one. Note on line 20, 254 should be 259 (as elsewhere).
5. At the top of page 21, again, the assumption needs to be stated that the undisclosed event duration provides adequate time for the bedload to get to its destination. It would be a good idea to calculate this as an assumption check.
6. On page 22, there are potential implications of the upstream BC that require discussion. Around about line 12 it is important to say which upstream boundary condition is imposed. Also, I would like to see some discussion around how the sequencing of events impacts the results – for example, a large event before a small one will deliver a large wad of bedload into the top of the reach for the small event - promoting deposition there. It is mentioned that the top 25 m of the reach is not analysed but that is because of the hydraulic boundary condition issue. Surely the sequencing of events will affect the net apparent change

between the first and last event in the Feshie sequence? I suggest a plot of mean bed level vs time to explore this?

7. Section 5.1, which discusses emergent behaviour, really needs to discuss the emergent scour behaviour that develops. Given only a relatively small number of iterations (i.e. 185 events in the period 2003-2013), the tendency to scour, degrade, and simplify the channel network is quite notable (e.g. -8.07 elevation fall is very severe and quite unrealistic). While this is noted on page 22 (around line 25) with reference to the Singh et al Delft3D run, it doesn't mention that the Singh et al run also produced unrealistic results. So this issue appears to be being swept under the carpet whereas it really shows that the model in its present form is not to be trusted for predicting decadal or longer scale morphological evolution. While this is discussed some more on page 30, and explanations are offered, it seems pretty clear that the main channel has gotten into a (scour, increased flow capture, increased shear stress, increased) feedback loop.

Page/by page comments:

P5, L 26: Need to say that Delft3D was run in 2D depth-averaged mode. Also, I suggest shifting the sentence on lines 9-12 of P6 to here.

P7, L1-4: Needs some edits. Also, give units of m to the Ks values.

P7, L11-12: Need to say at event timescales rather than coarser time intervals. The model is never being run at "coarser time intervals". The events are assumed to have undetermined duration. See also comment 1 above.

P7, L26-27: Sorry, but I've never heard of a braided river being exhausted of bedload through a hydrograph. This might be reasonable for suspended load. I suspect latency issues created by the upstream bedload feed boundary condition is something better to evaluate (see 6 above).

P8, L20: Needs to read Grad Qs. But also, why talk about Qs here then jump across to using Qb in Eqns 7 and 8?

P9, L 7: Need to say that Qb is the average unit mass-based bedload during the event.

P 9, eqn 8: give the dimensionally correct version of the MP-M formula.

P13, L17: Mode of what?

P13, L20: Suggestion only, but I struggle to see that S_T is particularly informative here, as it really weights the average sinuosity of individual channels (which is a useful measure of the morphological evolution) by the braiding index. In other words, if you use S_T you don't know if it's due to a sinuosity change or a BI change.

P13, L28: It's not large differences in the metric, it's large non-zero values (and say what -values indicate and what +values indicate).

P15, L24: Say 0.5 m grid models – otherwise it suggests models accurate to 0.5 m in elevation.

P16, L31: The model roughness parameter is the Nikuradse roughness length, ks.

P17, all of section 4.1.1: This is confusing and needs to be written. On P 7 you say you calculate ks off grainsize, then here you first suggest you derived this by trial error (i.e. by calibration), then finally you say you just used Williams et al's calibration result. Is it that you used an initial grainsize-based value of ks, saw no reason to change this during calibration runs, and this aligned with what Williams et al got?

P 20, L4: I'm curious – how much of the bed was mobile at 20 m3/s? Was that a good estimate based on the more detailed results coming from the model (which evaluates Tc everywhere)?

P25, L6-8: It seems hardly surprising that the big 259 m3/s event dominated compared to the "pups" that came before and after it – particularly considering the threshold of motion influence. So I see no reason for the speculation that follows. I suggest deleting.

P27, L22: I disagree that it replicates the magnitude, rather, it produces similar.

P27, L 24: It would be helpful to say if Williams et al thresholded their model results as done in this paper – so that that apples are being compared with apples.

P32: I suggest acknowledging the referees!