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Interactive comment on "Morphodynamic model of Lower Yellow River: flux or entrainment form for sediment mass conservation?" *by* Chenge An et al.

Anonymous Referee #2

Received and published: 17 July 2018

I have read the comments of Anonymous Referee #1, and I am in agreement with the general comments and most of the specific comments in that review. I have not repeated detailed comments here, except where to add further points to that referee's analysis.

The paper presents a very detailed and clear explanation of two related, but different, approaches to modelling suspended sediment transport in a large lowland river. The description and explanation of the model and the key equations is very helpful, and could itself become an excellent reference source in future. The substantive results of the paper are interesting, although somewhat unsurprising. Referee #1 notes that

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Discussion paper

the main question in the paper is actually "When does a modeller need to properly account for the lag of suspended load transport?". I concur with this point, and also that the flux form of the conservation equation is not necessarily synonymous with a capacity-based model.

Main comments:

1. My major concern is the generality of the results given the effect of the upstream boundary on model results in all cases where the upstream sediment input is less than capacity. In these cases, there is degradation at the upstream boundary and a gradual, exponential, rise towards capacity transport at some distance downstream. Such a situation is appropriate immediately below a dam, but without such a barrier to sediment transport the transport rate (sediment input) at the boundary would be in equilibrium with local flow conditions and availability of sediment on the river bed.

2. Much of the paper addresses the conditions under which the model predicts diffusion and/or advection. The analysis of this aspect is very useful and provides a way of evaluating how perturbations should be translated downstream. There is no consideration of the extent to which the model behaviour may reflect numerical behaviour. Morphological models of this type tend to be diffusive, for reasons that are considered in the paper and which relate to the damping effect as the bed surface (gradient and grain size) co-evolve in response to divergence in the sediment transport rate. Advection is not surprising where significant disturbances are introduced, especially where transport rates are relatively high. However, I find it difficult to understand how the model can generate successive advective waves (lines 627-35 are convincing, but for one wave), and wonder if there are aspects to the behaviour of the sediment transport function and/or sediment routing that lead to this. For example, it is unclear if the active layer thickness is maintained (ie is the surface layer mixed with the sub-surface at every timestep, or is the active layer exhausted and then replenished from the sub-surface only after this exhaustion is complete? This latter approach could readily lead to generation of additional waves of adjustment as the sub-surface will be finer than

the surface in the surface layer).

3. Lines 248-9: following the previous comment, it would be useful to know a little more about the way that equations (24)-(27) work. Did Ma et al (2017) use a size-specific formulation too?

4. Lines 88-90: I am unclear that the entrainment-based approach is more physically based than the flux-based approach. A properly-calibrated transport model should work just as well for the flux-based approach – if this predicts transport rates that exceed observations, this suggests to me a problem with the transport equation rather than needing a ‘supply-limitation’ correction factor applying. I also do not see the relevance of saying that Chinese researchers have a particular approach.

5. Line 92: is the additional computational requirement significant – I suspect not.

6. Lines 117-8 (and others): using a simplified geometry is entirely justified and makes a lot of sense. However, some consideration of the potential significance of these assumptions would be useful. For example, downstream of the dam is degradation uniform across the channel or is it concentrated in a thalweg leading to asymmetric cross-section geometry? On lines 253-4, can some indication be given about the observed variation in width and slope? I assume that there are no significant tributary inflows of water and/or sediment, and this could be stated here too.

7. Following the previous comment, it would be good to have some more assessment of model performance. Some annual flux estimate comparisons are made, which are encouraging. Are there other pieces of evidence (eg order of magnitude of degradation below the dam, and distance of propagation of the degradational wave after some years) that can be used to provide a general validation of the model?

8. Line 156 (and others): given the seasonal flow regime of this river, there will be variation in dune height during the sediment transporting period. I appreciate the use of constant flow and La for these simulations, but could the effect of La changing in

time be considered?

9. Model comparison (Line 364) – using absolute values is actually less informative than retaining the signs (or using yE/yF ratios). Table 2 (and later tables) – I did not find these tables very helpful, and wonder if it would be better to put this information onto the relevant figures as an additional plot (Lines 414-422 are very wordy as a result of describing Table 3 – would be easier to describe a graph of the same results). The figures on the table definitely do not need to be 2 decimal places.

10. Line 344: 'intentionally unrealistic' is ok, but readers might assume linear behaviour between your realistic and unrealistic boundary conditions. Do you know if this is a reasonable assumption to make?

11. Lines 451-2: I think (from memory – don't have the paper to hand) that this is similar to Philipps and Sutherland's formulation. If so, maybe reference this here.

12. Lines 467-8: another way to interpret this is in terms of settling velocity (ie there is a settling velocity, or Stokes number, above which adaptation length can be ignored).

13. Lines 555-7: I am not sure that the results in this paper can be used to make recommendations about event-scale modelling. It would be good to see some event-scale simulations to assess the significance of any differences between the two formulations.

Minor comments:

1. Line 28: I would say 'adapted for' rather than 'designed for'

2. Line 36: 'some aspect of sediment transport' is rather imprecise – can this be made more specific.

3. Line 119: presumably the grain-size distribution of the flux is also fixed?

4. Lines 271-2: can the sorting of the gsd also be given (maybe use the blank spaces on Figure 2 to put these numbers onto)?

5. Line 281: I think it is 400 cells, but 401 computational nodes.

6. Line 282: can you give Courant numbers for these timesteps?

7. Table 1: Add dx and dt to this – the table is a very useful quick reference point, so having these values here would be informative.

8. Figure 3: Could the water surface be added to this plot (maybe initial and final values only)?

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