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Earth Surface Dynamics Discussions

Interactive comment on "sedFlow – an efficient tool for simulating bedload transport, bed roughness, and longitudinal profile evolution in mountain streams" *by* F. U. M. Heimann et al.

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GENERAL COMMENT

This is an interesting paper that provides a useful package of modelling tools for simulating fractional bedload transport dynamics in mountain streams. The authors are to be commended in trying to provide an accessible tool for computing bedload transport and channel adjustments in steep mountain streams. They do a good job summarising some of the recent approaches to the problem of calculating reach-scale bedload fluxes in steep mountain streams and report their methods in a clear and largely transparent way. This paper is the first of a pair of papers describing the sedFlow model and cov-

ers the main assumptions and key concepts that underpin the technical development of the model. In a second paper the model is applied to two case study catchments and the sensitivity of the model and its limitations are evaluated and illustrated. Whilst the overall standard of the paper is generally good and errors are very few there are a number of points that need further explanation and clarity.

SPECIFIC COMMENTS AND QUESTIONS

1. Adverse slopes and pondage - The whole idea of adverse slopes and pondages is not very clearly explained. These terms are somewhat ambiguous. Pondages is a term usually refering to water storage behind a weir – ponding or negative bed slope? In this case it appears to be water ponding behind sediment obstructions caused by the morphodynamics of the system which influences the water surface slope? Although this causes problems in the model (and all models of this type) these pondages are dealt with pragmatically but the advice fall short of giving clear quantitative rules for the application of the different modelling options e.g. what is the critical value at which a slope becomes adverse? (See discussion on p739/P740). It would be useful is clearer guidelines are established and an additional Figure be use to explain the problem and solutions.

2. Computational stages – In places (e.g. P744/P745) the discussion lacks clarity in what particular combination of equations is used in the model. Is Eq.10 or Eq.11 used for the hiding function or is Eq.13 a or Eq.13b used for the bedload correction factor. It is not clear which is recommended and which is implemented in the model. The final statement on P745 emphasises this 'it is hard to decode which approach is most plausible'. Elsewhere (P747) it is stated that three different approaches are available in sedFlow to characterise the active layer. It would be good to have a diagram that shows the overall structure of sedFlow with the various options indicated and key governing equations signposted. Having read to the end of the paper and the companion paper it becomes clear that the model has 'options' that are selected to fit the application. This is a real strength of the model and should be highlighted up front as this would alleviate

some of the confusion about the model design.

3. Channel geometry – How the sedFlow model deals with channel geometry is important for the efficiency of the model but may also be a limitation of the proposed scheme. Because the flow routing scheme of Liu and Todini (2012) requires a simple channel form (rectangular of V- shaped) then extending this to more complex channels involving morphodynamic adjustments is more complex and may reduce the computational efficiency of the approach. This also questions whether the assumption that bedload transport occurs across he full channel width is also valid? Although the work if Stephan (2012) is used to suggest that this is not a major constraint a quantitative assessment of the potential errors involved is not clearly given and the results are published in a Technical Report and not immediately accessible. Although Fig. 3. Is useful in illustrating the degree of under / over prediction varies and there appear to be significant (but unquantified) differences between the various methods. A simple table summarising % over / under prediction in relationship to the observed would be useful here. Also the authors do not clearly state that the dimensionality of the model is? It appears 1D as the evolution of the channel slope is presented in the vertical dimension(given the assumptions discussed in Section 2.2.2).

4. Active layer dynamics - The three methods used to characterise the active layer dynamics (Section 2.3) are generally clearly described but no real guidance is given to indicate the 'best' selection for a particular setting (although this is explored in the companion paper). Also whilst Figs. 4,5,6 are useful this would be a much better Figure if the three approaches were summarised in a single diagram and could therefore be more easily compared directly. Also in revising it should be possible to simplify somewhat. In particular, Fig. 4. Is probably overcomplicated (particularly in shading styles as no key is given) and could be simplified somewhat when producing a revised summary diagram.

5. Model comparison – The background is well discussed in relationship to available models but these are not tested directly against the simulations of the new model. In

the justification for this paper three models (Topkapi ETH, TomSed and SEDROUT) are compared with sedFlow (Table 1). This is done in a qualitative way through description and discussion. Whilst this reveals important differences a more rigorous evaluation would be to compare the results of the four models directly so that differences can be properly quantified and the limitations more directly assessed [this would also provide useful data to populate Table 1 in a more meaningful manner and give more gravitas to the discussion]. I recognise this may be difficult given accessibility to model code, etc. but I think this is something that could be usefully attempted. It would be useful to know if any such comparisons have been undertaken by the authors or are planned.

6. Discussion - The discussion (P750/751) tends to recap / review the various models but is less effective at advocating the main advantages of the sedFlow approach. For example, in the discussion of Adverse slopes ((Section3.2) the advantages of sedFlow (or limitations) are not really clearly stressed. I also expected to see greater discussion of river bed / sediment transport interactions as it is not made particularly clear how feedback from grain-size changes and/ bedload transport updates in the model and equates with changing macroroughness (bed roughness) of the channel. Generally at the end of each of these various discussion sub-sections I would expect to see a recommendation of how sedFlow can be most effectively implemented to deal with such applications. As it stands many of the sections feel rather fragmentary. The main message that seems to come out of the discussion is that a quick / flexible model has been developed. This is an important thing to emphasize but equally important it is necessary to define the limits to the proposed modelling approach. In particular a series of recommendations indicating the preferred model options/ setup would be useful. Perhaps the key phrase in the conclusion is that 'The new model sedFlow complements the range of existing tools for the simulation of bedload transport in steep mountain streams' rather than replaces them.

7. Frequently the concept of an 'efficient tool' is used in the paper and this appears in the title. I assume this refers to the low computation power used to run the model and

corresponding fast calculation speeds? However, this should be clearly defined with reference to a benchmark.

8. The paper does not really define what is meant by mountain streams / rivers and the threshold /condition that distinguish these from lowland river types. This needs to be defined and clearly explained as this is considered a fundamental distinction that underpins the basis of this approach.

TECHNICAL CORRECTIONS / MINOR ISSUES

P734, L6 – Abstract - what is meant by adverse slopes – need to be specific

P734, L6 – 'state of the art approaches' this is very vague – say what these are.

P734, Abstract could be a lot more informative about the nature and structure of model at the moment reads very generally and does not adequately characterise content of paper.

P734, L20 – add references citing examples.

P736, L24 – Do you really mean alpine here as all the talk is about 'mountain' streams.

736/P737 - The elements of the model listed here are important but not all key terms are defined or justified: P737, L1 – state of the art approaches – this is very unclear P737, L6 – You need to explain and justify why macro-roughness effects are important. This is not done explicitly. P737, L3 – Why is a knowledge of fractional transport rates important? P737, L4 – Adverse slopes – you mean abrupt changes in slope along the long profile. So in this sense the adverse relates to the computational problems of dealing with this? Use macroroughness term consistently, in places it is hyphenated.

Table 1 – This is not particularly useful as much of this is included in the text and the entries in the table are rather vague (e.g. intended applications) or lack clear detail / rigorous testing (e.g. speed). I recommend dropping these but moving references to text.

P737, L20 – processes involved rather than involved processes.

P738, L27 – to ensure this assumption.

P739, L6 – delete 'as well'.

P742, L8 – spelling 'macroroughness'

P742, Eq.6 – Make it clear this is the flow resistance equation which is used in the model?

P743, L2 – The statement 'As the application of Eq.(8) may result in too small thresholds for gentle slopes, ' – This warrants further explanation and the introduction of a minimum Shields value needs to be better justified.

P747, L15 – platform is not a good term – better to say The topmost layer of the bed interacts with the flow and is typically called the active layer.

P748, L5 – Not sure why this is 'trivial'?

P749, L19 – It is not clear what is meant by the statement 'Differences are explained in the context of the differing scopes of the respective models' – could this be expressed more clearly?

Interactive comment on Earth Surf. Dynam. Discuss., 2, 733, 2014.