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Earth Surface Dynamics Discussions

Interactive comment on "Recalculation of bedload transport observations in Swiss mountain rivers using the model sedFlow" *by* F. U. M. Heimann et al.

Anonymous Referee #2

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Overview

This paper presents an evaluation of the sedFlow model as presented in the companion paper (Heimann et al., 2014, ESDD, 2, 733-772). The model is calibrated for two different river reaches, both located in Switzerland, and a simple sensitivity analysis is performed.

Evaluation

Results from the calibrated simulations seem to provide reasonable agreement with the observed data. Although the model is interesting, and the preliminary results presented here are encouraging, I think more can be done with this. The authors, quite aptly, refer

to this paper as a proof-of-concept study (p774, ln12). The paper is lacking some detail that would make it a more comprehensive analysis of the sedFlow model. Based on this proof-of-concept manuscript, I would agree that the model has the potential to adequately simulate bedload transport in alpine river systems. But I would prefer to see a more rigorous analysis of the model, as mentioned in the comments below.

Comments

1) I am not sure why this paper is separated from its companion paper (Heimann et al., 2014, ESDD, 2, 733-772). Because of the separation, this paper contains 5 pages (section 2.4; pages 784-789) simply summarizing what is detailed in the companion paper. I think that the two manuscripts would make more sense if combined into one paper, albeit a quite large one.

2) In setting the reference data (section 2.3.2) the authors describe how various components of real-world sediment budgets were obtained so that the model results can be compared to these data. For the Brenno River, they indicate that one of the components, the sediment outflow at the mouth of the Brenno River, was unknown. The authors mention that they used the results of the simulations to obtain a best guess for this parameter (p781, ln25). It therefore seems that the results of the simulations were used, partially, to obtain data to which the results of the simulations can be compared. I am not sure I understand how this process works, and how it can result in an independent evaluation of the model's performance. Please clarify.

3) A number of criteria are to be adhered to in calibrating the model (p789, ln16-22). Among others, the simulated erosion and deposition should be as close as possible to the observed pattern, and the simulated ABT should be as close as possible to the one reconstructed from field observations. It is not clear how this closeness is measured. What metrics are used? From Table 2, it can be learned that Nash-Sutcliffe and RMSE were measured for the ABT, but it is not clear which of these was optimized for during the calibration. No mention is made of metrics for the comparison of erosion

and deposition. Please explain this quantitative part of the calibration process more carefully in the main text, and include full quantitative data of the calibration metrics for the erosion/deposition comparison in table 2.

4a) In describing the set-up of the numerical experiments for both calibration and sensitivity analysis, the authors do not mention which sediment exchange mechanism between flow and channel bed was used. According to the companion paper, three options are available in the sedFlow model. Please clarify here which one was used. 4b) In the sensitivity analysis, the influence of the sediment exchange mechanism between flow and channel bed, is not evaluated. The authors do, however, evaluate the impact of other options in the sedFlow model, i.e. the choice of flow roughness equation (two options available) and the choice of hydraulic scheme (three options available). As the sediment exchange mechanism will affect the grain size distribution in the active layer, it may have an impact on subsequent bedload transport. It, therefore may have an impact on the simulation outcome. I would therefore ask that the authors include the impact the sediment exchange mechanism in the sensitivity analysis as well, or provide a reasonable argument for excluding it.

5) There is little mention of temporal detail and temporal variability. Most importantly, it is not clear what the time period of the calibration simulations is for each of the rivers. Related to this, figures 4 and 5, depict accumulated bedload transport along each of the two river reaches, but do not mention the timeframe of the accumulation. It is also not clear if the erosion/deposition at any point is stable through time, as we are only presented with the net result at the end of the simulation. Do locations of erosion and deposition move spatially during a simulation, either in response to upstream and downstream changes, or in response to hydrological changes ? If so, do they do so systematically (e.g. move upstream, move downstream) or is it more haphazard? This also ties in with the potential impact of the sediment exchange mechanism between flow and channel bed (see comment #4), as it implies that previously deposited sediments can be re-entrained, in which case the grainsize distribution in the active layer

and the subsurface layers become more important.

6) The authors perform a calibration of the model on two different rivers. No independent validation is performed. Effectively the authors have found that by choosing one of several different bedload transport equations, tweaking some parameters (notably a threshold for initiation of motion, and the exponent m in the hiding function), choosing one of two flow resistance relation, and by making some local reach-scale adjustments, they can make the model more-or-less fit an observed dataset. For a different dataset, a different combination of transport equations, parameter values, and flow relation was needed. In the absence of an independent validation, this basically means that the model has sufficient degrees of freedom to conform to any reasonably plausible dataset. This is useful if observed data is available, but this exercise does not provide any sense of the model's abilities when no observed data for calibration is available – and presumably this is one of the main intended applications of the model. I would very much like to see an independent validation test of the model, i.e. using a dataset which was not used in the calibration of the model. The authors do comment on this issue (p795, ln4-16), mainly citing a lack a of observed datasets. But at least some other datasets are available in principle, albeit for shorter river lengths (p794, ln28).Why can the model not be validated on these? Or on the three rivers depicted in Figure 3 of the companion paper? Or, alternatively, why can the model not be validated using some sort of data-splitting on one or both of the rivers used in this study, i.e. use half of a river's observed data series for calibrating the model, and the other half for validating it – as is common in the field of hydrology (which the authors refer to).

7) In the absence of a validation test, it would be useful to compare the results of sedFlow simulations to results of other models as mentioned in the companion paper (e.g. SEDROUT, TomSed). One of the advantages is that comparison to other models' results might alleviate the need for additional observed data (see previous comment). Although not as strong a test as an independent validation against observed data, this would still provide another evaluation of the reasonableness of the sedFlow's outputs.

It would also enable a direct comparison of its computational efficiencies.

8a) The sensitivity analysis evaluates the model's response to a change in a number of input variables, model parameters or model settings. But the sensitivity to many parameters is not assessed, e.g. e (eq 6), λ (eq9), m (eq 10), npore (eq15) [equation numbers refer to the companion manuscript]. For some of these parameters a recommended value is provided from the literature – which, in the absence of other data, is fine for calibration purposes, but it would be nice to know how sensitive the model is to changes in these parameters (and to the choice of sediment exchange mechanism; see comment #4). 8b) Figure 8 illustrates the result of the one-at-a-time sensitivity analysis. It does so by depicting the range of outputs resulting from the change in input values. What it does not show is the direction of change, or what the value of the reference scenario was. Maybe adding a zero mark, indicating the reference result, and a plus and minus symbol to the ends of each of the lines, indicating the result of a +30% and -30% change in input, would help in assessing this. Alternatively, and preferably, including parameter changes of +10% and -10% in the sensitivity analyses would give more data points for more detailed graphs, and would allow to assess if the simulated ABT responds linearly or non-linearly to changes in each of the inputs.

9) minor comments

p775, ln21: Replace "(this article)" with "(Heimann et al., 2014)"

p775, ln22: Define "intermediate spatial scales". Also mention the intended temporal scales.

p781, ln2: Replace "To validate the model" with "To test the model". By the authors' own admission they do not attempt to validate the model (p795, ln5)

p781, ln9: Typo: "minmum"

p799, ln19: Typo: "or" should be "of"

Figure 3: not needed

Evaluation Criteria

Does the paper address relevant scientific questions within the scope of Esurf? Yes

Does the paper present novel concepts, ideas, tools, or data? Yes

Are substantial conclusions reached? Somewhat - see comments above.

Are the scientific methods and assumptions valid and clearly outlined? Somewhat - see comments above.

Are the results sufficient to support the interpretations and conclusions? Somewhat - see comments above.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Somewhat - see comments above.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

Does the title clearly reflect the contents of the paper? Yes

Does the abstract provide a concise and complete summary? Yes

Is the overall presentation well structured and clear? Yes

Is the language fluent and precise? Yes

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Figure 3 can be eliminated

Are the number and quality of references appropriate? Yes

Is the amount and quality of supplementary material appropriate? n/a

Interactive comment on Earth Surf. Dynam. Discuss., 2, 773, 2014.