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

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We are grateful to both referees for their detailed and constructive comments. Below we respond to each of the referees' main points using the same order as in the individual reviews.

1 Answer to comments of Jeff Warburton

1.1 Introduction and context

For the introduction, Jeff Warburton suggests "*to comment on the importance of bedload fluxes for causing damage to engineering structures, bedrock erosion etc.*". We agree and will include bedrock erosion and damage to engineering structures together with example references of illustrative case studies.

1.2 Case study catchments

Jeff Warburton states that "*the initial description of the catchments does not emphasise that both sites are impacted and show a good range of examples of engineering intervention typical of many mountain catchments. It is important that this is mentioned earlier in the paper and highlighted in the description and discussion. There is also a need to emphasise the contrast in the two study catchments more.*". We agree and will put more emphasis on the engineering and management interventions as well as contrasts between the catchments.

1.3 Model structure and typology

We agree with Jeff Warburton in the points that he raises in this section. That is: We will further clarify the determination of time step lengths. We will note earlier in the paper that sedFlow is really a package of modelling tools that can be adapted for use to suit particular catchments. We will include a summary diagram of the model and a diagram of the order of computations within one time step in the companion paper.

1.4 Channel morphology and roughness

Jeff Warburton comments on the assumption of the substitute rectangular channel in a natural channel setting. We do agree that this assumption is an important characteristic of the presented model sedFlow and we have discussed it in detail in section 2.2.2 of the companion manuscript. There we have demonstrated, for the case study streams of Stephan (2012) affected by a large flood event, that the influence of this assumption is negligible compared to the overall uncertainties of bedload transport estimation. We will more clearly elaborate there that this is likely to be mainly true for the simulation of floods. For low-flow conditions the channel geometry may play a more important role. Further on, the assumption of a rectangular channel is close to the situation in the natural channel, especially in erosional reaches. As described in the manuscript on page 790 in lines 10 to 12, in depositional reaches, the choice of an effective channel width is associated with a considerable uncertainty and this width is therefore used as a kind of "integrative" calibration parameter.

1.5 Reference bedload volume

Jeff Warburton raises concerns that using simulation results for the sediment outflow at the mouth of the Brenno in the reference data may compromise the independence of the model validation. We agree and will clarify in the text that the sediment outflow just defines the general level of accumulated bedload transport, which still allows for examination of the spatial variation of ABT.

1.6 Description of the sedFlow model

Jeff Warburton recommends that "*a diagram (typology chart) showing the structure and interlinkages of the model would be a really useful addition and a very effective means*

of communicating the nature of the model very efficiently". We will include a summary diagram of the model in the companion manuscript.

1.7 Sensitivity analysis

Concerning the sensitivity analyis, Jeff Warburton asks "*why are the results only reported qualitatively?*" and recommends that we include a simple figure. However, the quantitative results of the sensitivity analysis are already displayed in Figure 8.

Further on, Jeff Warburton states that it "*would be useful if there was some justification for varying the parameters by 30 %*". The mentioned value of 30 % fits the order of magnitude of the different uncertainties typically involved in bedload transport simulations. For example discharge values are associated with the uncertainties of rainfall-runoff simulations. The grain size distribution of river reaches is measured at individual points and therefore cannot capture the spatial variability of this parameter. The value of the minimal threshold for the initiation of bedload motion θ_{cmin} varies along the river length and, as described in the manuscript on page 790 in lines 10 to 12, the effective channel width is associated with a considerable uncertainty in depositional reaches.

1.8 The conclusion at the end of Section 3.1

Jeff Warburton discusses the "*conclusion at the end of Section 3.1 (P793) that 'the complete variation of input values caused considerable variation in the simulated ABT, but caused very little variability in the simulated erosion and deposition'*". In this context, we would like to highlight the following note, which is given in the manuscript on page 798 in the lines 5 to 13: "*In the complete range sensitivity study (Figs. 12 and 13) all input variations have been applied to the complete length of the river. This may explain why the simulated erosion and deposition show only limited variation compared to the simulated ABT. Erosion and deposition are a function of gradual changes* *of channel properties (gradient, width, GSD, inputs) along the river. Applying the input variation to the complete length of the river keeps the relative changes of channel properties the same. Even though bedload transport is not a linear system, the input variation on the complete length of the river did not cause considerable variation of simulated erosion and deposition.*"

1.9 Minor issues

"*P777, L17-18 – It would be useful to have a simple definition diagram showing this, as this is a fundamental definition for the paper*" The authors agree that a clear definition of the terms "net and gross channel gradients" is useful. Therefore, we have given this definition on page 777 in the lines 17 to 19. However, we regard the distinction between net and gross gradient as a side issue and not as important enough to justify the inclusion of another figure. Furthermore, the inclusion of an additional definition diagram contrasts the comments of anonymous referee #2, who suggested to delete the only other definition diagram Figure 3. To keep the extent of the manuscript manageable, we believe we should not include further definition diagrams.

"*Figs 1 and 2 could be combined into a single Figure and a North arrow should be added.*" As suggested, we will include North arrows in Figs. 1 and 2. However, we disagree to combine the two figures into one, as the two separate figures can be aligned better with the column boundaries in the final layout.

Except these points, the authors agree with all minor comments and will implement them in the final manuscript.

2 Answer to comments of Anonymous Referee #2

2.1 Structure in form of two companion manuscripts

The referee thinks "*that the two manuscripts would make more sense if combined into one paper, albeit a quite large one*".

The authors have chosen the structure in form of two companion manuscripts as an effective way of adressing the two different target audiences. The first manuscript documents the methods implemented in sedFlow and describes the main assumptions and key concepts that underpin the technical development of the model. The second manuscript shows how the model can be applied, outlines the advantages and limitations of the model and demonstrates in a proof-of-concept study that sedFlow can be used to recalculate bedload transport observations. The first manuscript is targeted to an audience that is interested in the technical details of sedFlow, such as, for example, model developers considering to modify the model code or to use some of the concepts for their own model. In contrast, the second manuscript is targeted to an audience that is mainly interested in the applicability of sedFlow, such as, for example, practitioners planning to use the model in some of their projects. Before submission, this approach of audience targetting in two companion manuscripts has been discussed with and approved by Tom Coulthard, the managing editor. The authors would like to keep the structure of two companion manuscripts, as a combined text would be extremely long at the expense of readability. This is even more true as the referees of the two manuscripts are interested in further details and request different additions to the texts.

2.2 Reference bedload volume

The referee states that 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.*". We agree and will clarify in the final manuscript that the sediment outflow just defines the general level of accumulated bedload transport, which still allows for examination of the spatial variation of ABT.

2.3 Calibration metrics

The referee recommends to "*explain this quantitative part of the calibration process more carefully in the main text*". We will clarify in the text that we visually examined the calibration criteria.

2.4 Exchange mechanism between active layer and subsurface alluvium

The referee states that "*the authors do not mention which sediment exchange mechanism between flow and channel bed was used*". We will include in the text that we used threshold-based interaction with 70 cm active layer thickness.

Further on, the referee recommends to "*include the impact the sediment exchange mechanism in the sensitivity analysis as well, or provide a reasonable argument for excluding it*". To the authors' knowledge there are unfortunately no in-depth studies, which assess the influence of different representations of active layer dynamics on the simulation results. Therefore the role of the exchange mechanism between active layer and subsurface alluvium is an open question in science and may be the starting point for a study of its own. As sedFlow contains three different formulations of active layer dynamics in the same modelling tool, it provides the base for a future study on the effects of different active layer algorithms. However, such a study would be quite extensive and is therefore beyond the scope of the current manuscript.

2.5 Temporal dynamics

The referee states that there "*is little mention of temporal detail and temporal variability*". In the introduction of the manuscript on page 775 in lines 23 to 25 it is outlined that for sedFlow, "*the focus is not on the details of the temporal evolution of sediment transport, but rather on a realistic reproduction of the overall morphodynamic results of sediment transport events such as major floods*". Therefore we have concentrated the manuscript on the simulation of overall morphodynamic results.

2.6 Model validation

The referee "*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*". Unfortunately, fluvial bedload transport is characterised by a complicated network of feedbacks and interactions and the transport systems differ substantially. Therefore it is not possible to validate a bedload transport model in a catchment different from the one, in which the model has been calibrated. Splitting a study catchment for independent calibration and validation, does not work as parts of the calibration (e.g. local grain size distributions) are spatially distributed. To the authors' knowledge, there is no study presenting an independent calibration and validation of a bedload transport model based on field data. In contrast to the present manuscript, most studies do not even discuss the limitations of a calibration-only approach.

2.7 Inter-model comparison

In the companion manuscript(Heimann et al., 2014), sedFlow is compared with three models (Topkapi ETH, Tom^{Sed} and SEDROUT). This is done qualitatively through description and discussion. The referee suggests "*to compare the results of sedFlow simulations to results of other models as mentioned in the companion paper*". We fully agree that such a quantitative comparison would be interesting. However, such a comparison is a study of its own and therefore beyond the scope of the present manuscript. Already in its current state, the manuscript covers the usual extent of the presentation of a numerical model in the geomorphology community. For example, the presentation of CAESAR (Coulthard et al., 2002) also just includes qualitative comparisons and the presentation of Tom^{Sed} (Chiari et al., 2010) does not even contain a qualitative comparison with other models. An in-depth, quantitative comparison and review of existing bedload transport models as suggested by by the referee represents an interesting option that the authors prefer to save for future research papers.

2.8 Sensitivity analysis

2.8.1 Selection of analysed parameters

The referee suggests to extend the presented sensitivity analysis and to include further variables. The authors agree with the referee on the importance of sensitivity analyses for a good understanding of the model's dynamics. Therefore we have included the presented sensitivity analyses in the manuscript. For the sake of readability, we have restricted the analyses to the parameters that have the largest influence on the simulation results. As mentioned in the manuscript on page 797, lines 16 to 19, the

selected simple structure of a one-at-a-time sensitivity study has limitations for the analysis of non-linear processes (Saltelli et al., 2006). However, an adequate global sensitivity analysis, in which the complete parameter space is covered, would go beyond the scope of the manuscripts. Furthermore, the presented sensitivity analyses are well beyond the usual extent of the presentation of a new numerical model. For example, the presentations of Topkapi ETH (Konz et al., 2011), Tom^{Sed} (Chiari et al., 2010), SEDROUT (Hoey and Ferguson, 1994), or the model of Mouri et al. (2011) do not contain any sensitivity analysis at all. Therefore, the authors prefer to save a more extensive in-depth sensitivity analysis for future research articles.

The referee lists several variables which have not been included in the sensitivity analysis. In the following we will comment on each of these variables:

- For e, previous studies have shown that in various cases and conditions the value of 1.5 performed well in reproducing available observations. Therefore we have not included e in our sensitivity study and instead recommended the use of a default value of 1.5 for this parameter.
- λ is commonly only used in simulations of test reaches longer than 30 km, as this is the minimum distance for λ to have considerable influence.
- The hiding exponents m_{wc} and m do not fit in the concept of the presented sensitivity analysis, which is the variation of a best fit value by a certain percentage. m_{wc}, which we used at the Kleine Emme, is the result of a defined function and thus cannot be reasonably varied by a fixed value; and the best fit value of m in the Brenno is 0 (i.e. no consideration of hiding effects), which precludes the variation by some percentage.
- η_{pore} will rescale the ordinates for both the simulation and the reference data in the same way.

2.8.2 Graphical presentation of results

For Figure 8 the referee recommend "*adding a zero mark, indicating the reference result, and a plus and minus symbol to the ends of each of the lines*" as well as "*including parameter changes of +10% and -10% in the sensitivity analyses*". In Figure 8 the ordinate displays the "*Median ABT per unit median reference ABT [-]*". Therefore the value of 1, around which the figure is centered, represents the requested zero mark. In a new version this fact will be highlighted by a horizontal line at this value. In addition, we will include plus and minus signs as suggested as well as marks indicating deviations of +/- 10 % and +/- 20 %.

2.9 Minor comments

Even though the referee suggests to drop Fig. 3, the authors would like to keep it in the manuscript, as it supports the comprehensibility of the text and especially of Eqs. (2-4). Furthermore, the removal of the definition diagram Fig. 3 contrasts the comments of Jeff Warburton, who suggested to add even another additional definition diagram.

Except this point, the authors agree with all minor comments and will implement them in the final manuscript.

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