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Hydrology and Earth System Sciences Discussions

Interactive comment on "Sediment transport modelling in a distributed physically based hydrological catchment model" *by* M. Konz et al.

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Response to interactive comment of Anonymous Referee #1

We highly appreciate the detailed review of the anonymous referee #1. The comments help to improve the paper and to focus on the core message. In this short response letter we respond to each point raised by the referee and hope to foster an ongoing discussion because some of the points might need further comments from the referee. For better readability we subdivided the original text of the review into different subsections and numbered them. Our comments are written in italic.

The article by Konz et al. comprises very interesting work in the field of bedload modelling in mountains regions and presents a new modelling system (TOPKAPI), the derivation of validation data and the application of the model to a major runoff event for a study catchment in the Bernese Alps and a model comparison with another, more complex model (SETRAC). The described sediment-transport module of the model contains a novel and innovative sub-grid level for bedload processes which enables an improved spatial discretisation of cross-section variations important for accurate calculation of bedload storage and deposition.

1. Although the article is generally well composed, I have several reservations towards its current form. The manuscript reads to a certain extent more like a project report for a larger study containing a very broad perspective on model development, description, application, validation and scenario simulations. The objectives of the paper are not clearly defined, and within the first several chapters it is not clear if the paper is supposed to give a description of a new model system and/or the derivation of validation data using LIDAR based digital elevation models and only in chapter 5 the major goal of the study is stated: to compare two models (with each other, and not against real validation data). I would suggest that certain parts of the article should either be extensively reduced and thus the focus of the article more narrowed or, contrarily, certain chapters require a substantial amount of more information to enable a comprehensive view on the methods used.

We agree that the manuscript should be better focused and the objectives of the study should be better and earlier stated in the manuscript. The main goal of the study is indeed to present the newly developed sub-grid modeling scheme for sediment transport simulations within the framework of the distributed hydrological model TOPKAPI. For the evaluation of the model performance we used the well tested (in the laboratory and in the field) SETRAC model as a benchmark model. We perceive this model as more sophisticated due to its more detailed representation of the cross-section geometry. Furthermore the model has been developed exclusively to simulate sediment transport in alpine torrents. TOPKAPI is conversely a catchment distributed hydrologi-

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cal model, which has been improved by developing a novel sediment transport module, which required several assumption and simplifications, such as the downscaling of the discharge from grid cell level to sub-grid level, which are not made in SETRAC. Therefore the SETRAC simulations serve primarily as a benchmark for TOPKAPI. In relation to the use of the recalculated sediment volume associated with the considered storm event we mention that these data have already been published in several studies. However, in the manuscript we summarize the methodology of their processing and briefly discuss the accuracy of the data for sake of completeness. The interested reader is asked to refer to Chiari et al. (2010) for further details relevant to the sediment transport observations. However, we believe that the application to real data, though these are affected by errors difficult to quantify, can be a demonstration of the ability of the scheme adopted in TOPKAPI to mimic a state of the art model for channel erosion and sediment transport, rather than being a proof of the ability of the model to reproduce the overall sediment balance during a storm event. Moreover, the comparison to some observed data allows to show the relevance of the losses due to form roughness simulation. Accordingly, we are ready to reduce the discussion about the observed data by condensing the relevant discussion and focusing more on the model intercomparison.

2. At the moment no information on the quality and uncertainty of the validation data is given. No runoff data are available from the study catchment and I think it is questionable to reconstruct streamflow measurements from neighbouring gauges for the assessment of bedload rates of a single flood (high risk of error-propagation). The information on how the LIDAR dems are used to derive bedload rates is not explained well enough to give any estimate on accuracy and uncertainty (spatial and temporal) and the whole section should be excluded if the information have been published somewhere else (Chiari et al 2010?). Especially the information contained in Figure 2a (reconstructed bedload transport), which are then later used to attempt a model validation, are not derived properly.

We fully agree with the comment on the reconstructed flow data, but we would like to

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observe that these data are used to get a hydrograph that could be used as reference in relation to the estimated volume of sediment transport obtained from the LIDAR surveys. These discharge data have not been used for the calibration of TOPKAPI, which has been conversely calibrated to match the discharge data used by SETRAC, in turn reconstructed to match the order of magnitude of the hydrograph reconstructed from the neighbouring catchment. This is more relevant for the model intercomparison than the reproduction of the observed discharge, as stated on page 7606 line 17ff. With regard to the estimation of the sediment volume, we intended to provide an overview of how the LiDAR data are used on pages 7595 and 7596 including the relevant equations and discussing (pp. 7609 and 7610) the reliability of the method also providing an assessment of the volume error (line 18 on p. 7610). We are persuaded that this contributes to demonstrate the credibility of these data, at the same time honestly indicating that they are estimates, which can provide only magnitudes and should not be considered as true values. We acknowledge that shortening the description of the reconstruction methods and referring to the relevant literature can improve the paper. We believe, however, that the comparison between simulated and reconstructed data contributes to demonstrate the plausibility of the conceptualization of the sediment transport module of TOPKAPI. Figure 5 could be thus deleted and the consistency between the simulated and the reconstructed flows could be simply mentioned in the text.

3. Some of the TOPKAPI model equations are not adequately explained, eg. Equation 8: why is this equation adequate for alpine channels, how where they derived (lab, what psd, slope etc.) or Equation 11-14: which D50, D90 values were used in the original studies by Rickenmann and Chiari, - are the particle size distributions comparable to the ones in the study channel?

We acknowledge that the entire section requires some revision in order to explain properly the methodology concerning the sediment transport component of the model. The equations used in the TOPKAPI sediment transport module are based on more than

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300 field measurements in torrents and gravel-bed rivers. Equation (11) was proposed by Bathurst et al. (1987) based on field data, and was slightly modified by Rickenmann (1990). Therefore, it can be assumed that the particle size distribution is comparable to the conditions of our study torrent.

4. Can the empirical formulas be used for 1-sec time steps?

The empirical formulas are strictly valid for steady uniform flow conditions (similar to a Manning equation or a log-type flow resistance equation). Since changes in flow rate with time were relatively low for the 2005 flood event, assuming steady uniform flow conditions is considered to be a valid approximation.

5. Model application in Chapter 5 does not give enough information on the three model scenarios M1 to M3; it appears that exponent alpha has a huge impact on model result, reducing the bedload rates by nearly one magnitude between scenarios M1 and M2. The entire chapter requires more information on scenario setup or should be excluded from the study.

We agree that the scenarios have to be described better. Since the model performance with or without considering form roughness is different and the results are considerably influenced by form roughness we believe that the section should not be removed, rather improved. Accordingly we will provide more details in the revised version of the manuscript.

6. I disagree with the authors that the performance of the TOPKAPI model is satisfying (as concluded in Chapter 8). The validation study (and the non-existing information on uncertainty of validation data) did not show that.

We concluded on page 7614 lines 18 to 20 that the model performance of TOPKAPI is satisfying in comparison to SETRAC. We did not make a similar statement with regard to the comparison with the reconstructed field data. The two models compare well and TOPKAPI is significantly faster, therefore we conclude that the performance is

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satisfying.

7. In summary, the authors might want to consider changing the scope of the study by only concentrating on the presentation of the TOPKAPI model development and a model inter-comparison with SETRAC – it would still make a very interesting and innovate study and would focus on the novel aspects of their model in comparison to previous approaches.

We agree that the discussion of the comparison of TOPKAPI simulations with reconstructed data can be condensed and that more focus should be put on the comparison between the two models. However, as already mentioned above, we believe that the comparison with the reconstructed sediment transport volumes can add value to the plausibility of the results, though in qualitative terms, and helps demonstrating that losses due to form roughness have to be considered.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7591, 2010.