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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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Authors' response to Interactive comment on "Sediment transport modelling in a distributed physically based hydrological catchment model" by F. Gallart (Referee)

We highly acknowledge the comments and suggestions of Dr. Gallart. Especially, his detailed comments are very valuable and will help to improve the manuscript. In this response letter we comment on the main points raised by the referee, whereas the detailed comments will be considered in the revised version of the manuscript. The referees comment is written in bold, our reply follows directly after the respective comments

The authors present the improvement of the distributed hydrological model TOPKAPI for the simulation of stream bedload transport and analyze the results obtained when the model was applied to a major event, in comparison with the results obtained with a more specialised sediment transport model (SETRAC). In my opinion the subject and approach are relevant although there are some methodological doubts that deserve consideration.

The hydrological part of the model was calibrated using not measured but reconstructed' discharges of the event. This part of the model is not new in the paper and therefore its evaluation is not essential; nevertheless the fact that the hydrological simulation could not be checked means that the possible (and unevaluated) errors in the hydrological simulation may result in large uncertainties associated with the sediment transport simulations.

We agree that uncertainties in the discharge simulations are translated into uncertainties in sediment simulations, but the main aim of the modeling study is not to reproduce the observed hydrograph of the event but to use the reconstructed event data to test the newly developed sediment module against the SETRAC model. We will reduce the description of the event data (reconstruction of discharge and sediment transport volumes) in the revised version and focus more on the model intercomparison using SETRAC as benchmark model. This has also been suggested by the anonymous referee #1.

One of the advantages of the comparison of the results with another model is that both models had very similar discharge forcing, so the role of possible errors in the hydrological part of the model was avoided. Indeed, the sediment loads simulated by both models were similar, so the authors may certainly conclude the success of the new developments of TOPKAPI in comparison with SETRAC.

We agree, TOPKAPI delivers almost identical results as SETRAC and is highly CPU efficient.

HESSD

7, C4266–C4269, 2010

Nevertheless, the comparison with the volumetric estimates was really not so straightforward; even when the macro roughness was taken into account, the overall volumes resulted in similar values, but the correlation between spatial estimates and simulations in Fig. 6 seems really poor. It is unclear therefore whether the approximate agreement between overall simulated volumes was physically due to the role of hydraulic macro roughness or the changes in the use of equation (15) just absorbed diverse structural and parameter model errors.

The equations used in the TOPKAPI sediment transport module are based on more than 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. The geometrical input data like river width sediment depth have been derived from field observations. We are therefore persuaded no significant parameter errors were introduced in equation (11), which has been applied within the hypotheses for which it was developed and using parameters that are considered in the literature to be plausible. We conclude accordingly that the considerable simulation bias can be due to macro roughness energy losses, which are not considered in eq. (11). Conversely, equations (13), (14) and (15) were successfully used to simulate flood events in several mountain streams with substantial bedload transport. Most of the examined case studies confirmed the importance of considering the effect of hydraulic macro-roughness to obtain a better agreement with observed bedload transport (Rickenmann et al., 2006; Badoux and Rickenmann, 2008; Chiari, 2008; Chiari et al., 2010; Chiari and Rickenmann, ESPL, in review). The application of equation 15 is therefore thought to be an adequate approach to take the underlying physical process into account.

Finally, the importance of the 'artificial redistributions' of sediment is unclear. From figure 11 this seems that this problem was avoided using a short time step, but this is unclear in the text and particularly in the 'conclusion and outlook' section.

HESSD

7, C4266–C4269, 2010

The following text is written in the conclusion section (p. 7615):

“An artificial redistribution of sediment by transferring additional sediment volumes to the next downstream cell is necessary to avoid blocking of the channel and to ensure positive downstream bed slopes. However, the routing of the additional sediment volume to the downstream cell cannot be explained physically because the transport capacity is already exceeded in the time step in which the redistribution takes place. The mechanism can be partly avoided by a small sub-time step”.

This statement indicates that aritifial redistribution is necessary to avoid the blocking of the channel, which occurs because of the exceeded volumetric transport capacity in a given time step. Using a smaller time steps reduces the risk of this circumstance and thus the occurrence of the artificial redistribution. On page 7614 lines 2ff the effect of the sub-time step length on the artificial redistributions is discussed. We will reformulate the conclusion section in order to make it clearer.

On the other hand, although the paper is generally well written, there are some inadequacies that may lead to wrong interpretations. Particularly, the overall methodological approach is not properly described in a ‘methods’ section, but scattered in the paper or in the ‘discussion’ section.

We agree and will revise the methodology section. The comment is well in line with comment one of referee#1.

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