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Discussions

Interactive comment on "Contributions of Catchment and In-Stream Processes to Suspended Sediment Transport in a Dominantly Groundwater-Fed Catchment" *by* Yan Liu et al.

Anonymous Referee #1

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The manuscript illustrates a coupled catchment-stream model for water quantity and sediment productions and transport developed for the Ammer River Basin (Germany), which has some important karstic contribution to baseline flow and suspended sediments. The physics- based model that is proposed includes a complex one-dimensional hydraulic component for calculating shear stress. Erosion rates are then based on shear stress concepts applied to erosion of bed and bank material (either deposited sediments or consolidate beds), as well as in-stream deposition. The model was developed to tackle Ammer Basin hydrology in particular, however it is proposed as an integrated model of general applicability. The model is built on components of other hydrological and sediment models. It appears to be very focused on in-stream pro-

cesses. Conversely, sediment sources from land processes (soil erosion) seems too simplified. I have some concerns about the paper and its content. 1) first of all, it is not true that existing models do not account for in-stream processes (P2 L24). For example, I know that SWAT model offers several ways to tackle in-stream sediment transport and erosion, including some physics based approaches based on shear stress and the possibility to include cross section of reaches. Some literature has shown that these SWAT approaches work well. The authors should therefore revise their statements. 2) Sediments from urban land is modelled with a well-known wash off/build up approach. Instead erosion from non-urban areas is tackled with an (to my point of view overly) simplified approach (eqs. 4-6) whose main drivers are runoff and slope. Only one shear stress threshold is considered despite agricultural land diversity, which includes a variety of crops like cereals, vegetables and natural vegetation. This approach does not consider any variability in soil erodibility, or changes in crop cover during the year, which instead impact soil erosion from agricultural land especially among seasons. This flaw limits very much results drawn from the model especially in terms of seasonality and 'hot moments' of erosion. 3) I have some concerns about the calibration and validation of the model. The model has 14 calibration parameters and is calibrated vs 1 single station at the outlet of the Basin. I also note that calibrated parameter Ch (Table 2) which regulates the non-urban sediment loads, is lower than its initial range. The risk of over parameterization of this model is very high. Some sensitivity analysis should be shown and discussed as this represent a limit of potential conclusions of the paper. Calibration was driven by a LHS scheme but conducted manually. The authors state that calibration parameter sets were retained to derive 90% confidence intervals. However these confidence intervals are not shown nor further commented expect for a vague comment at P 11 L 14. The model runs at hourly time step. At what time step calibration and validation were conducted? Water discharge was calibrated for 2013-1014 and validated for 2015-2016. Sediments were calibrated for 2014 and validated for 2016. Why data for 2015 was not used in this exercise? Data was available as shown in fig 5 but model simulations are not shown. However, model simulations are used for sediment balance considerations e.g. figures 6 and 7. Please explain. The model missed simulation of 2 large rainfall events (one in 2014 and one in 2016), where the highest sediment concentrations occurred. The explanation offered (P11 L 14, p12 L1-2) is that rainfall precipitation measurements 'may be missing'. This should be verified in the input data. In any case, these 2 events were the most important for sediment load, so all sediment balance is flaw as it cannot consider these main events. It would also be good to see some events in more details given the high temporal discretization of the model. 4) the results of the model indicate that urban land is the major source of sediments in the catchment. this is possible, but I find hard to believe that 67% of the basin (agricultural land) contributes almost nothing to sediment loads. Even if runoff production is very low and land is gentle sloping (P 21 Lines 10-12), I would expect more contribution. The authors should check with other lines of evidence (e.g. literature of soil erosion from agricultural land in the region) if their results are realistic. 6) Fig 8 indicates an increase of sediment sources following a power-law with discharge, which may make sense. However, I wonder if an excess of sediment transport capacity of the stream was considered in the model. This may regulate deposition when sediment sources are very high. I do not see this being considered in the model (but I may be wrong). Please discuss. 7) what data was used to set karstic sediment loads? 8) section 3.1 should precede model description. The model was built for the Ammer and some important information driving model conceptualization is given in this section, so this should come first. Information about measurement data should be given in this section. Please move P 9 Lines 14-17 and P10 Liens 18-25 to after current P 8 line 14. 9) please change color of Load urb and load bed in figs 6 and 7 to better distinguish them. 10) schematic text at P 18-19 lines 12 onward should be given as a table. 11) reference in the conclusion to events with 2-year return period (P 22 L 2) is surprising. No reference to return period is done before in the manuscript. Given that model failed to simulate two large rainfall events of the region, I find it hard to believe this statement.

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