Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-42-RC2, 2018
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Hydrology and Earth System Sciences
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 #2

Received and published: 28 March 2018

The manuscript presents an integrated sediment transport model including hydrological, hydraulic, sediment-generating and an in-stream transport model for the Ammer catchment in Germany. The attempt to assemble a fully integrated model and explain sediment dynamics fits nicely in the recent efforts to support integrated water resources management, the topic is absolutely timely.

I see two significant points that absolutely require improvement.

First is the conceptual explanation and model representation of overland sediment transport, where there is a gap between soil mobilisation (erosion) and reaching the streams. Retention during overland transport and its dependence on vegetation cover

is generally neglected and not included in any form, except if we consider it to be deeply hidden in the parameters of equations 4-6. This formulation makes sediment loads independent from landcover, which partially violates the Critical Source Area principle (not wholly, because slope and flow depth are still there), and makes the model unable to identify the impact of different cultivation patterns. Although the optimal solution would be to change the non-urban sediment-generating submodel to something more appropriate for such a large and diverse catchment, the absolute minimum is to mention this deficiency in the discussion.

The second major issue is the problem of identification and the credibility of model results. TSS concentrations were measured at a single site, the Pfäffingen gauge. All subcatchments and their various landcover classes contributed to this single data series through various transport processes including in-stream retention or resuspension. The identification of the contribution of each class requires specifying contrasting behaviour for all source types a priori, otherwise one cannot decide about the importance of each process from a single aggregate TSS series. Here it was done through the model specification, which one can partially debate, but that's not a principal issue. The problem is that the results, e.g. the importance of processes is finally conditional on the model specification, which is not mentioned here at all. Thus, the identified (and thoroughly analysed) contribution of each source is only true when the model assumptions are correct. This must be explicitly stated in the manuscript, considering that the applied model equations are not obviously the right ones (which is not a real problem, it just reflects the subjective decisions of the authors), and the outcome seems a bit strange (negligible agricultural contribution with 60% arable land). An additional point to this concern is the imperfect fit of the model to the observed data. While the fit is not worse than what one can usually achieve with TSS modelling, the uncertainty is large enough to make identification of different sources practically impossible.

A more objective decomposition of sediment dynamics could have been the analysis of time-dynamics, that is the identification of slow-medium-fast responding components and their precipitation or discharge trigger thresholds, and binding known mechanisms to them afterwards. In the manuscript the same happened in mathematical sense, but it is now stated with high confidence that a certain response pattern is obviously the effect of a certain process, which is simply not proven by fitting a model to the TSS data. Emphasising the subjectiveness of results is therefore advisable.

A logical follow-up study could aim at repeating the same exercise using multiple TSS time-series from different locations possessing different shares of sediment sources. This would strengthen the basis for attributing certain sediment dynamics to specific sources.

SPECIFIC COMMENTS

P2 L12 and L15: USLE is an empirical model of soil loss on the plot scale, it is not applicable to entire subcatchments, but not because of the lack of in-stream mechanisms. USLE cannot deal with heterogeneities along the transport pathways of soil particles, so it cannot model how much retention will occur during overland transport. So if we don't speak about a homogeneous plot stretching right down to the stream, with the same soil quality, cultivation method, slope, rainfall exposure, USLE will be a bad approximation.

P3 L7: "Towards this ends" sounds strange.

P3 L11-12: Dry weather sediments from a WWTP are much different from "normal" particles due to their different particle size distribution and much higher organic carbon content. Would this spoil the estimation of TSS from turbidity?

Figure 1: Would "Rural" be a better alternative to "Non-urban"?

P4 L1: It would be reasonable to introduce the Ammer first, as the following sections contain a lot of specific information, which cannot be judged without knowing the basic characteristics of the catchment and river.

P4 L7: When the aquifer is a karst, is "groundwater" the right term? Wouldn't "fracture-

water" be a better description? Or is this a mix of karst and non-karst?

Equation 1: This must assume that t is always restarted at the beginning of the accumulation phases. If this is the case, please mention.

Equations 4-6: This formulation ignores that (i) overland flow path lengths have a serious influence on sediment delivery [>90% of the mobilised sediment accumulates during overland transport], and (ii) surface roughness, permeability, flow concentration affect yield. How does landcover quality affect sediment transport here? How would buffer zones work in such a model? Given these shortcomings, please comment whether the landcover conditions in the Ammer catchment make these equations usable.

Figure 2: Given that cross-sections were 100 m away from each other, travel time between cross-sections must have been between 50 to 200 seconds. Then the hourly time step means that this model was solved for a series of quasi steady states. How did the dynamics of sediment sources relate to these times? Wouldn't this mean that some dynamics of the rapidly responding urban sources was lost due to improper numerical resolution?

P7 L9: A significant part of TSS and turbidity comes from the wash load, which practically never deposits. So it is a rather significant simplification that all fractions deposit at the same rate. Did this cause problems in the model fits?

P9 L 11-12: It would be logical to mention the peak NSE value besides the threshold.

Table 2: The applied value of Ch (0.001) is out of the specified range (0.003-0.05). Why?

Figure 4: Baseflow is perfect (which is a big achievement in a karstic catchment), but discharge peaks are seldom met. What does this mean for the TSS calculations? Most of TSS likes to travel with discharge peaks.

Figure 5: Please use log scale for TSS, this linear scale isn't very informative, the

reader can't figure out if the model was right or wrong.

P14 L4-5: Urban and karstic dominance in TSS loads would be exceptional with 60% arable land (which - with its barren soil surface in certain months - is generally considered as the most erosion-prone land use class, besides construction sites). Can you find specific reasons for this?

P14 L9: These infiltration rates seem to be a bit high. Design values (for example: https://stormwater.pca.state.mn.us/index.php?title=Design_infiltration_rates) for loam are around 8 mm/hr, for clay loam around 1-5 mm/hr, which would change the runoff picture significantly. Can you bring up a reference in support of these high rates?

Figure 10: Would be better to show the NET rate and slope along the river, because the present legend is confusing. Where can one see "NET EROSION"?

P22 L5-9: This paragraph is speculative. First it should be shown with further measurements that the model was right.

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