Hess-2018-42

Dear Dr. O. Cirpka,

Thank you for submitting the responses to the three comments regarding your manuscript "**Contributions of Catchment and In-Stream Processes to Suspended Sediment Transport in a Dominantly Groundwater-Fed Catchment**".

You have addressed most of the comments in a manner such that I suggest to revise the manuscript accordingly. However, there are two overarching aspects – the erosion from agricultural land and the source apportionment - that are not convincingly treated. There are several aspect of these themes that require a more thorough improvement. I list these aspects below:

- Rev. 1 and 2 expressed doubts that the small contribution by erosion from arable fields was actually true. In your respective response to Rev. 2 you suggest to search for further studies supporting your findings. I highly recommend to do so and would like to point out that there is a high-resolution erosion risk map for Germany available (Auerswald, Fiener et al. 2009). Additionally, it might be worth contacting local practitioners to obtain region specific knowledge that is not available in the scientific literature.

- Rev. 1 expressed concerns (point 3) about the low value of the $C_h$ parameter and asked for a sensitivity analysis that would show how robust your findings about the relevance of urban versus rural sediment sources would be. You explain that due to the computational burden (please provide quantitative information), the model uncertainty was only calculated for the hydrological part but you ignore the comment on the sensitivity analysis. I think this request by Rev. 1 is solid and you have to provide some calculations that demonstrate the robustness of your findings.
  This directly links to one major concern raised by Rev. 2, which is the non-identifiability of model parameters and model structure based on the available data (Point 2). This severely limits the possibility to actually infer the sediment sources from your model results. It is possible that the results in section 4.2 simply reflect your prior knowledge because you could tune the model such as to produce what you expected to find. You argue that you will provide further evidence that the model assumptions were plausible. While this is very welcome, it should be complemented by a (local) sensitivity analysis demonstrating how the modeled sediment sources vary (or don't vary) with changing model parameters. With such a sensitivity analysis you respond properly to the comments/request by Rev.1 (see Point 3 there) and Rev. 2 (Points 2).

- Along a very similar line, Rev. 2 commented (point 15) that the infiltration rates were high in his opinion and asked how the results would change upon lowering these values. Although you provide a reference for these infiltration rates you fail to provide the more important answer which concerns how your findings would change upon less effective infiltration. Please provide simulation-based evidence for the robustness of your findings (or its absence!).

- Several times, you defend the model structure for erosion on arable land by your prior knowledge saying that runoff hardly occurs (because of low rainfall intensity compared to the infiltration rates and flat topography) and that therefore urban sources dominate sediment input into the stream network (e.g., responses 4 and 13 to S. Mylevaganam, response 4 to Rev. 1, responses 1 and 2 to Rev. 2). This creates a (potentially) vicious circle because you set up the model structure based on your prior knowledge in such a way that the model prevents proving your wrong.

One example illustrating this issue relates to the comment by S. Mylevaganam about the temporal invariance of the critical shear stress (point 13). You mention in your response that you implemented your simplified approach because of the limited non-urban contributions. However, by doing so, you may actually miss important non-urban fluxes, e.g., during winter when for example cereal fields may be very prone to erosion even under low intensity rain (e.g., Prasuhn 2011) due to high water saturation. Also the German Environmental Protection Agency points out that low intensity rain may be relevant for triggering erosion (see https://www.umweltbundesamt.de/themen/boden-landwirtschaft/bodenbelastungen/erosion#textpart-3).
Inspection of Fig. 5 in the manuscripts reveals an event where the model severely underestimates the observed sediment load (winter 2016). This might be potentially such a case where due to the seasonal conditions erosion on arable fields may have been relevant. Hence, a critical shear stress that varied with time might have led to a different result regarding the relevance of different sources for sediment delivery.

- Several times, you defend the model structure with the low contribution from arable fields due to sufficient infiltration capacities of the soil such that no critical runoff would occur. This argument is based on the assumption that overland flow is the only relevant trigger for erosion on arable fields. However, splash erosion (e.g., Fernández-Raga, Palencia et al. 2017) may initiate erosion (and with it overland flow) if the soil structure is not sufficiently stable and rain drops cause surface sealing. Upon surface sealing, infiltration rates may drop substantially causing erosion even if on intact soils the infiltration capacity would be sufficient. Because such aspects are neglected, the chosen model structure cannot prove your prior knowledge/assumptions wrong.

- In this context, it is also peculiar that for the priors of the critical shear stress on fields you refer to a non-published Master thesis (Bones 2014) developed in the context understanding scouring around foundations of bridges causing failures. There is no argument not to use such information, however given the large numbers of papers specifically dealing with critical shear stress on crop soils (e.g., as a random selection Léonard and Richard 2004).

- As a minor comment I'd like to add about your argument that crop rotations – for which you don't have specific information in time - would make it difficult to incorporate more complex agronomic aspects into the model (e.g., Rev. 1, point 2). Given the fact that farm size (0.2 – 0.4 $km^2$, I guess for this region) is much lower that the size of your sub-catchments (1.6 – 10.7 $km^2$ agricultural land) and on single farms the crop mix is rather stable across years (just single fields are cropped differently) cropping patterns for the scale of interest for your model approach would be rather stable in time.

In summary, I suggest that you revise the manuscript according to your reply and by seriously taking into consideration the comments described above.

Sincerely

Dr. Christian Stamm
Editor HESS

**References:**

Auerswald, K., P. Fiener and R. Dikau (2009). "Rates of sheet and rill erosion in Germany — A meta-analysis." Geomorphology **111**: 182–193.

Bones, E. J. (2014). Predicting critical shear stress and soil erodibility classes using soil properties Master thesis, Georgia Institute of Technology.

Fernández-Raga, M., C. Palencia, S. Keesstra, A. Jordán, R. Fraile, M. Angulo-Martínez and A. Cerdà (2017). "Splash erosion: A review with unanswered questions." Earth-Science Reviews **171**: 463-477.

Léonard, J. and G. Richard (2004). "Estimation of runoff critical shear stress for soil erosion from soil shear strength." CATENA **57**(3): 233-249.

Prasuhn, V. (2011). "Soil erosion in the Swiss midlands: Results of a 10-year field survey." Geomorphology **126**(1-2): 32-41.