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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A 2-D hydro-morphodynamic modelling approach for predicting suspended sediment propagation and related heavy metal contamination in floodplain: a sensitivity analysis" *by* R. Hostache et al.

Anonymous Referee #2

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Review of A 2D hydro-morphodynamic modelling approach for predicting suspended sediment propagation and related heavy metal contamination in ïňĆoodplain: A sensitivity analysis by Hostache et al

The authors present a straightforward application of a model for hydrodynamics and sediment transport to rivers in Luxemburg, and an analysis of the results of the model. Section 5 "Conclusions" highlights the salient innovative features of their work: (1) the unique measurement database of water surface elevation and discharge combined with

dissolved trace metal and suspended sediment concentrations; (2) the aim of the study to see if trace metal concentrations can be used to calibrate the model in a way similar to the use of tracers for calibrating hydrologic models, resulting in the conclusion that this is not the case. However, none of these salient features appears in the abstract. It is recommended to re-write the abstract in this respect. It will make the paper more appealing to potential readers.

The authors rightly note in lines 35-38 that they do not study sediment deposition in view of topographical evolution, but in view of their role in contamination. This means, however, that the modelling presented is not morphodynamic but related to water quality, for which it is common to consider the transport of dissolved matter and sediments in suspension. The word "morphodynamic" refers to topographical evolution and is hence inappropriate in the title as well as at other locations, e.g. line 282, line 398, line 592, Section 4.2 and the caption of Figure 5. It remains possible to recall that Sysiphe can be used for morphodynamic computations too, but the term needs to be corrected at other locations.

It is somewhat confusing that the same symbol is used for the momentum diffusion coefficient in Equations 2 to 3 and the tracer diffusivity coefficient in Equation 4. The authors might consider a clearer distinction.

Line 345 defines "bathymetry" erroneously as "shape and elevation of the river bed". "Bathymetry" means "spatial distribution of water depth", which is an output of simulations rather than an input because water depth depends on water level. The correct term in line 345 would be "river bed topography".

The units of the Strickler coefficient values are missing in lines 452-453. Incorrectly, the Strickler coefficients are even stated to be dimensionless ("unitless") in line 603. The Strickler coefficient is the reciprocal of the Manning coefficient and has the unit mˆ(1/3)/s.

The simulation results in Figure 6 deviate considerably from the observations. Expla-

nations are given in the text, but at the same time line 672 asserts that the simulated concentration “captures correctly the temporal evolution”, and lines 679-680 assert that “the overall ïňĄt between observed and simulated sediment concentration is rather good”. These strong statements are not supported by the results and need to be tuned down into a more fair assessment.

The manuscript is well-written, but still contains a number of typos: Line 59: “explains” must be “explain”. Lines 226 and 235: “Partheniade” must be “Partheniades”. Lines 228 and 243: “sheer” must be “shear”. Line 229: “inbetween” must be “between”. Lines 222, 237, 242, 319, 614 and 646, along with Fig. 5 (Figure + caption): “Shield” and “shield” must be “Shields”. Line 269: “Particle of such” must be “Particles of such”. Lines 460 and 631: “set” must be “sets”. Line 466: “A” must be “An”. Line 479: “This” must be “These”. Line 498: “condition” must be “conditions”. Line 527: “entertainment” must be “entrainment”. Line 658: “exhibit” must be “exhibits”.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 1741, 2014.