We would like first to thanks the two reviewers for their careful reading of the paper and their pertinent remarks and comments. As supplement files, we post the answers to reviewers and the revised version of our manuscript according to reviewers' recommendations. In the revised version of the manuscript, red characters indicates the changes that have been made.

Answer to anonymous Referee #1:

Review of the paper 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. MS No.: hess-2013-617 The paper studies the deposition of fine sediments in floodplains and performs a sensitivity analysis of different model parameters for the hydraulic and morphologic model part. A 2D-model was applied to two flood events in an about 2km2 floodplain. The results of the modelling exercise were compared to measurements of dissolved concentrations of three trace metals and to concentrations in the upper soil (0-5cm). 1.Does the paper address relevant scientific questions within the scope of HESS? - Yes. The distribution of fine sediments with associated pollutants in floodplains is from
great scientific interest and tools should be developed to forecast the erosion, transport and deposition of particle bound pollutants.
2.Does the paper present novel concepts, ideas, tools, or data? - There are not so many papers that describe in a proper way a combined sensitivity analysis (SA) of hydraulic and morphologic model parameters. So I expected from the heading and abstract that there would be such a combined SA but there wasn't for different reasons given later by the authors. The reason is mainly the computational effort. There were no information if a parallel code was used what is possible with Telemac.

According to reviewer1's comment we add a sentence in the paper mentioning that we used the parallel version of the Telemac2d-sisyphe code with 16 parallel nodes.

The concept (SA), idea (SA for hydraulic model and later for morphologic model) is not new. The tools for using TELEMAC/Sysiphe in Monte Carlo analysis were surely new developments but they were programmed for internal use. The Telemac/ Sysiphe system itself was not developed further. The data are very interesting, it is a good data set that deserved to be published with the proper model exercise. - Using spatial distributed friction coefficients is quite difficult – it was stated from references from 2005, 2010 – so it would be a progress to present some new things in this direction, but unfortunately it wasn't. –

We agree with reviewer 1 that evaluating the added value of calibrating distributed friction parameters could have been a primary focus of the study. However, as it was suggested by the reviewer, the computational effort would have been significantly increased. More importantly, we would like to argue that because of a lack of evaluation data it would have been very difficult to analyse the merit of such an exercise. We believe that for making such an evaluation it is necessary to install a high number of intermediate gauge stations along the river in order to enable a meaningful analysis of the benefit of finely tuning distributed friction values.

The new thing is the comparison of the spatial distributed soil samples to

the deposition rates. But the comparison exercise should be improved or at least more discussed/assessed. - There is the general question, if the spatial and temporal high heterogeneous behavior of fine sediment flakes can be really described with a simple sedimentation model like that one used in Sysiphe?

To answer to this comment we propose to detail the discussion on the comparison of deposition information from the model and contamination observation through section 4.3. The question raised by the reviewer is actually of general interest especially because Sysiphe do not consider suspended sediment flakes. Unfortunately, since we do not have observation of the depositions that occurred during the January 2011 flood event it is difficult to draw any conclusion about the capability of the model to accurately simulate local suspended sediment behaviour.

3.Are substantial conclusions reached? - The authors stated that the “innovative point in this study is the rather unique measurement database”. That’s true and I can imagine the effort behind it. - The conclusions drawn regarding the SA could be expected and are known - The qualitative comparison between calculated deposition and upper soil concentrations looks quite interesting

We thank reviewer1 for this comment.

4.Are the scientific methods and assumptions valid and clearly outlined? - In general yes - The hydraulic and sediment transport model is standard and the Telemac/Sysiphe documentation is available - The authors stated that the number of points in the parameter space for the SA “might be sufficient for capturing parameter sensitivity”. That should be explained a little bit more. Because from the literature (Beven, Saltelli) there is a clear advice to use large number of samples. The argument of high amount of computation time can’t be used in this point. Than the used method is not adequate.

We understand this concern. Indeed the number of generated parameter sets may appear as rather limited with respect to the numbers often recommended by K. Beven et al. for similar analyses. However, the required number of samples clearly depends on the objective of the study and the structure of the model. In the studies cited by the reviewer the objective was to estimate the uncertainty of hydrological models with a high number of free parameters. As argued in the paper, we believe that the chosen number of parameter sets is sufficient for capturing the sensitivity of this study’s model to the considered parameters. We did not plan to estimate the parameter calibration uncertainty. The figures 3 and 5 clearly show to which parameters the model is the most sensitive. Since this was the main intention of the analysis, we would argue that the number of parameter sets is sufficient.

5. Are the results sufficient to support the interpretations and conclusions? - Yes. 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
- In general, Yes. The procedure how the samples were extracted from parameter space, could be explained in a little more detail.

This has been added in the revised version of the article. We developed this part as follows:

The sensitivity analysis is based on a random sampling of model parameter sets from an a priori defined range of physically plausible parameter values. To do so, each parameter value is randomly sampled from a uniform distribution having a range corresponding to the minimum and the maximum plausible value of the considered parameter. Once the parameter sets have been generated, model simulations are

carried out for each generated parameter set. Finally, each model result is compared to a set of observations and subsequently, model skill scores are computed for each parameter set.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? - Yes. 8.Does the title clearly reflect the contents of the paper? - Yes. 9. Does the abstractprovide a concise and complete summary? â˘A´c Yes 10. Is the overall presentation well structured and clear? - In general, yes. I suggest some parts from results and discussions to move to the material and methods section. 11. Is the language fluent and precise? - Yes. 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.
13.Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? - Yes. I give my suggestions as comments in the PDF.
14. Are the number and quality of references appropriate? - There is one reference from 2013, all other are 2010 and older. I wonder if there are some more newer references regarding to the topic. Is there only one important reference in the last four years? Please check again.

15. Is the amount and quality of supplementary
material appropriate? - There was no supplementary material given Further remarks and suggestions are given as comments in the PDF.

We take the recommendation of reviewer1 into account throughout the paper.

Specific remarks from the PDF file from reviewer1:

- ***Following the reviewer1's suggestion we moved the beginning of part 2 (method) to the introduction.***
- ***We specified the simulation time step and the mesh node distances in the paper.***
- ***We have to admit that we did not fully understand the reviewer's remark about the grid size influence on model results. The mesh used for the modelling exercise is unstructured. It is refined in the riverbeds and close to the roads, dikes and riverbanks. As a matter of fact we believe that changing the distance between nodes is not of primary concern for the model as long as the mesh is sufficiently refined in the riverbeds and close to the "topographic structures" like riverbanks, dikes and roads.***
- ***With respect to the flood return period, we specified it in the paper: the December 2011 flood event is the 1-in-1 year flood event whereas the January 2011 one has an estimated 8 year return period.***
- ***In order to motivate the choice of Gadolinium we would first like to mention that the three selected trace metals were all chosen because they are considered as PHE and may therefore have a strong impact on water and soil quality. Concerning the choice of Gadolinium in particular, we argue that, contrary to Pb and Zn, which are not all the time conservative, Gd can be considered as highly conservative.***
- ***With respect to the remark related to the ISCO autosamplers, we do not know what kind of reference we could add to the manuscript. In our opinion these are commercial products that are commonly used in geochemistry.***
- ***For the request related to the floodplain soil sample, we can of course provide the data as supplementary material, although the added value is not clear for***

us. We detailed our explanation about data acquisition with the following sentences: 100 soil surface samples (0-5cm depth) were collected in spring 2006 on a regular grid of 100 m spacing over a large part of the floodplain in the study area. The Zn concentration was determined by ICP-MS after digestion of the soil samples using a HCl/HNO3 mixture.

- ***We modified the conclusion according to reviewer1's comments.***

Answers to anonymous Referee #2

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.

According to reviewer2's suggestion we added a paragraph in the abstract.

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.

We fully understand the concern expressed by the reviewer. However, we argue that Sisyphe is used for morphodynamic calculations in the sense that :

- ***the deposition of fine sediment in the riverbed and the floodplain is one output of the model,***
- ***these topographical evolutions are expected to be useful for characterising floodplain soil contamination.***

Indeed the topographical evolution is important in our study since it is closely linked to soil contamination via sediment deposition. By comparing in the end simulated sediment deposition and observed soil contamination we would argue that the term 'morphodynamic' is appropriate. Moreover, as Telemac-2D and Sisyphe are dynamically coupled, the variation in the riverbed/floodplain topography due to erosion/deposition is taken into account in the hydrodynamic simulation. Therefore, to our understanding, we believe that our modelling system is indeed hydro-morphodynamic.

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.

We changed the notations as suggested by reviewer2. The tracer diffusivity is now identified as D_T.

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”.

We thank reviewer 2 for highlighting this mistake. We changed the text accordingly.

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.

Yes, of course. We added the units.

The simulation results in Figure 6 deviate considerably from the observations. Explanations are given in the text, but at the same time line asserts that the simulated concentration “captures correctly the temporal evolution”, and lines 679-680 assert that “the overall ïnˇA¸t 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.

We apologise for these admittedly overoptimistic sentences. What we wanted to highlight is that while the absolute values of concentrations are underestimated, the concentration peaks are well synchronized. Therefore we modified the two mentioned sentences.

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”.

We thank reviewer 2 for his careful reading and corrected the mentioned errors.