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Hydrology and Earth System Sciences Discussions

Interactive comment on "Soil erosion and sediment delivery in a mountain catchment under land use change: using point fallout ^{137}Cs for calibrating a spatially distributed numerical model" *by* L. C. Alatorre et al.

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Interactive comment on "Soil erosion and sediment delivery in a mountain catchment under land use change: using point fallout ^{137}Cs for calibrating a spatially distributed numerical model" *by* L. C. Alatorre et al.

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Answers to reviewer 1

Reviewer: I reviewed the paper "Soil erosion and sediment delivery in a mountain catchment under land use change: using point fallout137Cs for calibrating a spatially distributed numerical model" by Alatorre et al. in HESS. Overall, the paper is well written and understandable. The applied methods and results are easily understandable and clearly explained in the text. I have however a number of concerns. Although re-

sults appear to be very good, I have questions on the validity of the used calibration method. The paper is well written and it has a good scientific quality. In the following paragraphs I will discuss my concerns on this paper. I believe that this paper is suited for publication after some additional analysis.

Authors: We acknowledge the work done by the reviewer. We have gone through all his specific comments and have ammended the original manuscript when necessary. In the following lines we provide answers to the specific comments.

R: My main concern for this paper is on the use of the calibration dataset. The authors are using 19 cesium-derived erosion/deposition values for calibration, and a 7-year sediment yield of the catchment for validation. This kind of models is very sensitive for errors at the pixel scale, while aggregation to larger scales reduces errors largely (e.g. Van Rompaey et al., 1999). As a consequence, calibration will be largely hampered and the high model efficiency (0.8) may provide a false idea of accuracy of the model. In order to overcome the problems with cesium-data (point data) for the calibration of the model, a large number of cesium data can be used, in order to get a catchment integrated result. E.g. Van Oost et al. (2005) use 36 samples for a much smaller catchment/study area.

A: We agree that a higher number of Cs derived soil redistribution data would be greatly beneficial for model calibration. However, acquiring new Cs data is very costly in time, personal and budgetary resources. At this respect, we believe that a modelling exercise based on 'only' 19 Cs data is a valuable one, because it demonstrates that a well-designed sampling in a relatively complex catchment is able to provide useful information yielding to very acceptable results. We understand the reviewer's concern about the low representativity of a 5x5 m grid cell. Another reviewer has raised similar concerns, and also referred to spatial aggregation as a possible solution. As the reviewer mentions, spatial aggregation will most likely reduce errors. We find this suggestion an interesting one that merits further research, but we do not believe that

our approach (comparing point Cs with one cell model estimates) is invalid. In fact, we believe that spatial aggregation would most likely produce even higher model efficiency, since errors would be reduced.

R: In addition, the difference between the calibration set (watem/sedem calibration based on the Cs derived erosion-deposition) and the "application of the model" (p 11143 r 6 – r17) is not entirely clear to me. Don't you compare here twice the same things (Cs-derived values with the modeled values, and this for the optimal Ktc-calibrated values); and if you compare the same things, why is there such a large difference between an R^2 of 0.5 and the model efficiency of 0.8?

A: We agree that this paragraph might be confusing. We are not performing a true independent validation there, since the 137Cs redistribution rates were used for calibrating the model. We have pointed that out in the revised manuscript. The purpose of the paragraph was mainly to stress on the influence of single points on the calibration process. About the differences between R2 and model efficiency, we do not find it strange since these are different measurements of goodness-of-fit and are influenced differently by individual errors.

R: A possible solution for these calibration problems could be to test whether the number of use calibration points is large enough. In order to achieve this, a monte-carlo type approach can be used. A large number of iteration (n >100) can be used for which each time the Ktc factors are calibrated, each time using 80% of the input points (randomly selected). In this way the error on the Ktc calibration values can be assessed.

A: We considered in fact using a Montecarlo approach in an initial stage of the research, but limitations of the interface of WATEM/SEDEM made it impossible (at least in the version available to us). The model has a clic-based user interface, so no scripting is possible; that means that we had to manually perform the one hundred simulations in which the calibration is based, with a significant effort in terms of human dedication. Implementing a Montecarlo approach would require a much higher number of simulations (one or two orders of magnitude higher), which would be impossible to perform in a manual way. Providing a command-line interface would be a very easy-to-make improvement to the model and would make it suited for more complex calibration procedures.

R: A second point of concern is the application on the past land use map. For this map the study area is fully occupied by annual crops (p 11145 r 19). However, the contemporary land use contains almost no cropland (fig 2B), which may result in a bad (or no) calibration of the Ktc values for this type of landuse. This problem may by partially tackled by the abundance of other land use types with a comparable C-factor, but it will be appreciated if the authors at least mention this potential problem.

A: We agree that this is a potential problem of our study, and we have included a reference to it in section 3.3 of the revised manuscript. We believe that the presence of other land use types with comparable C-factor (and hence similar expected values of Ktc) reduces this uncertainty, and we have stated that in the manuscript.

R: Smaller comments: The Ktcmax and Ktcmin factor are used for crop-types with c-values which are larger or smaller than the "Ktc limit value". The authors do not mention which Ktc limit they used, so I assume they used the standard value of 0.1. Please mention this.

A: We have clarified that in the revised manuscript.

R: On the discussion of the SDR (p 11148-11149): take care to take into account the different difinitions and calculation methods for erosion and/or export! See also the paper of Parsons (2011; Progress in physical geography).

A: We agree that it is very difficult to compare between different studies due to differences in methods for calculating or measuring erosion and sediment yield, but also due to differences in catchment size or characteristics. We have included a note of caution in the discussion about sediment delivery ratios in the revised manuscript.

R: When looking at fig 7 & 8 (especially 8A), a pattern appears with banded structures of erosion and deposition zones. Are they realistic and e.g. caused by terrace like structures in the landscape. Such banded patterns may also result from unrealistic banded patterns in DTMs. This can be avoided by applying different kernel-filters to your DTM. Such filter techniques alter the altitudes of the DTM minimally (only a few cm) but may have a large influence on the modeled erosion/deposition pattern.

A: In our study area there are remainings of old terraces that alter the original topography, and at least some of the banded patterns seen in the figure are legitimate. It is difficult to say so for all banded patterns. We preferred to not alter the original DTM.

R: Technical corrections. Abstract: please mention in your abstract the size of the study catchment.

A: We have added the catchment size to the abstract.

R: P 11137 r 5: "soil erosion and sediment transport" should be "soil erosion and sediment redistribution" as it not only models transport but also deposition

A: This has been corrected in the revised manuscript.

R: P 11138 r 16: please make clear that Ktcmin and Ktcmax are not only the two extreme values Ktc will take, they are just the two only possible values it will take!

A: We have removed the word 'extreme' to correct this.

R: R 20: please mention a reference for the proposition to take the ratio between Ktcmin and Ktcmax fixed.

A: We have added a reference to Verstraeten (2006).

R: P 11140 r 3: is the mentioned slope gradient of 0.28 m/m or 28% correct? If it is correct, this is not a low slope gradient.

A: That was a mistake. The NW slope has an average slope of 28%, while the valley bottom has an average slope of less than 10%. We have corrected these values in the revised manuscript.

R: P 11143 r12: "5 and 2" should be "points 5 and 2" to avoid confusion.

A: We have corrected this.

R: Add a clear conclusions paragraph!

A: We have added a conclusions paragraph stressing the most relevant messages of our study.

R: Please add some information on your different land use scenarios (contemporary, past and future): Âů A table with an overview of the area of each land use type (in km^2 or %); Âů Clear maps of your future and past land use scenarios.

A: We have included a new figure (Figure 8) with the land use scenarios.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 11131, 2011.