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Interactive comment on "The sensitivity of landscape evolution models to spatial and temporal rainfall resolution" *by* T. J. Coulthard and C. J. Skinner

Anonymous Referee #1

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General comments:

This paper deals with a very interesting and relevant question for the scientific community working on sediment transfers in mesoscale river basins: how do the spatial and temporal resolution of the meteorological forcing impact modelled sediment yields? While this issue has already been addressed from a purely hydrological standpoint, it remains understudied in modelling approach dealing with landscape evolution and soil erosion. However it seems to me that the conclusions raised by the authors are not supported by enough simulations. My main concern is about the potential effect of changes in soil hydrological properties (spatially and temporally) as the spatiotemporal resolution of rainfall is changed. This is not at all considered by the authors in

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their simulations while they recognise at the end of the discussion that it may change considerably the sensitivity of landscape evolution models to rainfall resolution. As hydrological properties might be scale-dependant, changing only the spatiotemporal resolution of rainfall between runs without considering potential scale interactions between rainfall and soil behaviour may lead to erroneous conclusions on the sensitivity of landscape models. I know that adding runs in which the soil properties are randomly changed (m and K parameters) will need considerable additional computation time but the conclusions of the paper would be more supported and strengthened. Concerning the structure of the manuscript, the result section is very short and could be expanded, particularly if additional simulations are presented. The discussion section is rather heterogeneous in answering the 3 research questions written in the introduction. Section 4.2, addressing question 2, is very short and does not fully address it, as the authors recognise that more simulations would be required. Also Section 4.3, addressing question 3, is not supported by the data (no reference to them). I would suggest focusing the results and the discussion on question 1. If this question is fully addressed according to the above mentioned issues dealing with the hydrological basin properties, this would represent a substantial contribution to earth surface mass transfers. For those reasons, I do not recommend acceptance of the manuscript in its present form.

Specific remarks:

P2 L31-33: “Improved model performance” is not only “tempered by increased uncertainty surrounding precipitation data”, but by the uncertainty in the budget of precipitation (P) versus infiltration (I) or storage in the soil.

P3 L8-9: I fully agree with the authors here. This also refers to the ability to simulate correctly P-I budgets, spatially and temporally. As argued in the general comments, the authors should try to address that issue in their numerical sensitivity analysis.

P4 L30: I found the description of the model spatial discretisation quite confusing (not sure to have completely understood yet), mentioning here “area lumped parameters” and later (P5 L18) “grid cell size Dx” without giving any typical size for Dx. Is it the DEM resolution (50m) mentioned P7 L31? Could the authors try to be more specific on spatial discretisation and if possible limit the reference to previous papers to very specific model details that are not essential for that study?

P6 L1-3: As far as I understand, the hydrological model is adapted to the rainfall grid. Thus I agree with the authors that it enables having different levels of storage and runoff in each cell, but only due to rainfall variations. Varying also m and K would also create variations (i.e. P-I budget), but the authors kept constant those parameters (P8 L3).

P7 L29: How can the authors be confident in their conclusions with only 2 additional long term random simulations?

P8 L1 : Which initial grainsize distribution was used to run the 30 year model used as an initial condition? Which grain sizes are given in Table 3?

P8 L16: “considerable differences”: this is not new and references should be given to situate these data.

P8 L21-23: I agree that these changes are minor. Could they be significantly different if m and K parameters were also randomly changed from one run to another?

P8 L28: “also drains an additional tributary”. This may be critical. What is the drainage area of this tributary? How are the authors confident with that comparison between model and measurements?

P9 L11 : Very little difference is observed between random 1 and 2. This relates to a previous comment. Are 2 random simulations enough? Why not having done more, as presented later with the jumbled runs (P10 L9) for answering another question. Could these differences be more important if additional random simulations were performed? Otherwise this result (little difference) contradicts somehow with the results in Figure 7 showing a great dependence of the sediment yield to the rainfall allocation. Overall, it seems to me that the authors tried to address too many questions in the paper without running enough simulations to address each of them.

P10 L8-9: if this issue is so important, it should be introduced as an objective of the paper. As written it appears like an additional side issue. The description of these jumbled runs should be added in the method section and removed from the discussion.

P10 L5-10: I fully agree with the authors that it is a major limitation to this study. Thus I recommend the authors to try to assess how m and Ks variations could impact the sensitivity analysis as it will help to generalize their findings.

P10 L10: Why 20 different records? Does this number has an impact on the range covered in Figure 7 (i.e. from -7 to 2,5% for X-axis and from -15 to 60% for Y-axis)?

P18 Table 3: Evaporation was set to 0. How does it impact the conclusions? K is missing in this table.

P27 Figure 6: Total rainfall : The authors should specify over how many years.

P28 Figure 7: Why was this analysis done on the upper Swale only? The complete basin is characterised by more rainfall cells and would have probably exhibited more variations in the random redistribution of rainfall (see author's comment in section 4.2, line 5). I find this figure very interesting in addition to the results from Table 8 for example, as it clearly shows the impact of the jumbled runs. This sensitivity of the sediment yield to different spatial and temporal distribution of the rainfall raises again the question : would this sensitivity be the same if also m and K parameters were included in a wider jumbled run numerical analysis.

Technical correction:

P2 L15 : delete reference at the end of the sentence P3 L19-20 : can not find those three references in the reference list P3 L28 : Coulthard et al. (2013a) P5 L4 : Add units for Qtot P6 L11-13 : n (Manning) is missing in the list P7 L29: were then compared P28 Figure 7: homogenize the colors for 6 hour (yellow and purple)

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