The paper presents a hydrosedimentary model that couples the TOPKAPI-ETH hydrological model to a physically based and spatially distributed erosion and sediment transport model. Erosion on hillslopes is represented by the process of detachment/deposition by runoff using an empirical formulation. Sediment transport is provided by a mass conservation equation. The study examines the effect of spatial variability of precipitation and surface erodibility on sediment dynamics by performing continuous simulations over a period from 2003 to 2016, the first year being considered as a warm-up period for the model. In the simulation of reference, precipitation and surface erodibility are spatially distributed. In the other three scenarios, either precipitation, surface erodibility or both variables are spatially averaged over the catchment area. The results are analyzed using the Q-SSC relations at the catchment outlet, the erosion-deposition maps at the end of the simulated period and the terms of gross erosion, sediment yield and sediment delivery ratio. The topic covered by the article is original and of interest to the journal. The scale of the study (mesoscale catchment) is little discussed in the literature and the fact that there are two control points within the catchment in addition to the outlet is an added value to be highlighted. The article is well written. The authors provide the data used in the article in the « data availability » section at the end of the paper.

However, the questions asked are not precise enough. It is a bit ambitious to want to answer such generic questions with only 4 scenarios. The model is very little evaluated in terms of erosion before analysing the results of the different scenarios. It is therefore difficult to give credit to the results obtained. My advice would be to reformulate questions thar are compatible with the framework offered by the tested scenarios and to rework the results and discussion sections according to these new questions.

I therefore recommend major revisions for this paper.

General remarks :

- The authors do not mention the DHSVM model although it would be a very relevant tool for this type of catchment. It is necessary to justify the development of a new model compared to existing models such as DHSVM.

- The description of the erosion model did not seem clear enough to me, especially the distinction between the representation of hillslopes and river processes.

- The authors use data from Swiss operational services. However the temporal frequency of SSC data is too low for a catchment of this size located in a mountainous area. Flood events are most likely under-sampled. High SSC values are probably missing from the data set for this reason.

- One could be interested in the impact of the scenarios on the hydrological response and the indirect impact this may have on sediment dynamics.

- The concepts of structural and functional connectivity, widely present in the literature, are not discussed although they are at the heart of the subject developed in the paper.

- Connectivity indices are not used.

- The process of detachment by rain is not taken into account in the model. Only the process of detachment by runoff is taken into account. This is questionable when the objective is to estimate the effect of spatial variability of precipitation.

- Connectivity index maps could be used to study the spatial organization of erosion (Section 4.2). It is questionable whether there is any real added value in using the model presented in this study to address this issue.

- In section 3.4, the authors examine the results at the temporal scale of the flood event. It is difficult to examine the effect of soil moisture on erosion and sediment transport without giving guarantees on the performance of the model in reproducing flows under dry and wet conditions.

- The summary at the beginning of Section 4.3 is interesting.

Specific remarks :

-p2 l33: I would suggest adding "especially in small to medium catchments (up to 1000 km2) after "the strong non-uniqueness of suspended sediment concentrations (SSCs)".

-p2 l37: I would suggest adding "and transfers " after "in sediment mobilization".

-p2 l39 to 52: rewrite this part which is not clear and take into account the concepts of structural and functional connectivity.

-p2 l48: add reference Misset et al (2018)

Misset C., Recking A., Legout C., Poirel A., Cazilhac M., Esteves Michel, Bertrand M. (2019). An attempt to link suspended load hysteresis patterns and sediment sources configuration in alpine catchments. Journal of Hydrology, 576, 72-84. ISSN 0022-1694

- p2 l56: replace "transport" by "transfer" in several places in the text

- p3 l65: replace "cesar-lisflood" with "caesar-lisflood".

-p3 l62 to 75: add the reference to the DHSVM model and explain the added value of the model presented in relation to this model

-p3 l77: "a physically explicit spatially distributed deterministic model": simplify the formula. What does "explicit" mean here?

- p3 l83: "mean annual discharge" instead of "average discharge".

- p3 l87 : " mostly driven by overland flow ". What about rainfall processes ?

- p4 l97: what is the scale for the soil map?

- p4 l103: Is it really 2D whereas the equations presented p5 are 1D?

- p5 l105 to 114: I do not understand how the hydrographic network is represented and discretized. The same question applies to the hillslopes. A specific part is missing for describing the discretization used in the model.

- p5 l107: " catchment scale ": it is not precise enough. What scale?

- p5: put the dimensions of the variables presented in the equations. I do not understand the distinction between hillslopes and rivers in terms of erosion and transport processes. What is the link between the terms D and E?

- p5 Eq.4: I do not understand the definition of X. It should be a width rather than a length for the calculation of the flux.

- p7 l166-167: is this a wash load hypothesis?

- p8 Fig3 : SSC values seem low for a mountainous catchment area. This is certainly related to the lack of observed values during floods.

- p8 l186-188: it is questionable to use the slope of the Q-SSC relation given the dispersion that exists between these two variables (even in log scale)

- p10 l226-228: what forms of erosion are observed within the basin?

- p10 l234: "Hinderer et al. (2013)" is not present in the reference list.

- p10 l237: "The underestimation of sediment load (...) we do not like to reproduce the largest measured sediment concentrations". This is a working hypothesis that should be placed in « Material and Method ».

- p10 Fig5: indicate the observed data as red dots on the SSC time series.

- p11 Fig6(a): over which periods are the intensities calculated: over the rain periods only or over the whole period of simulation?

- p11 l242: I suggest modifying "where SIM 2 and 3 are compared respectively with SM1".

- p13 Fig8(b): There is a black dot without a text caption

- p17 l369-372: I am not convinced by this hypothesis, which depends heavily on the nature of the soils and the infiltration model used.