



EGUsphere, referee comment RC1
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Comment on egusphere-2022-181

Enrico Balugani (Referee)

Referee comment on "Does soil thinning change soil erodibility? An exploration of long-term erosion feedback systems" by Pedro V. G. Batista et al., EGU sphere,
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The Authors present a numerical thought experiment, based on the soil erosion model MMMF, to show the importance of including soil truncation processes in soil erosion models. The main thesis of the Authors is that, in the long term, soil truncation will result in a change in the soil properties, and thus in its vulnerability to erosion, leading to a change in the erosion rate. I want to commend the Authors for the style of writing (very easy to read) and for the clarity of images and equations shown. The results are well documented and discussed. The study is interesting, sort of an "opinion paper" backed by modelling experiments, helping raising an issue on something which is very interesting: which erosion processes that we disregard at short time scales may be relevant at long time scales?

However, I have some issues with what comes before the results: in my opinion, the article would benefit from a rewriting or restructuring of the abstract, introduction, and materials and methods sections, and from a more-focused title. While reading the manuscript, I had to "read between the lines" for assumptions and work-flow, piecing the real objective of the study together just as in a detective story, just to find the culprit confessing in the results and discussion (I do love detective stories, and I enjoyed doing this reading quite a bit, but I still think it is not the best way to write about science).

I want to stress that I believe this was an interesting study, and that my (extensive) comments are only aimed at improving the manuscript.

* The title of the article, even if alluring, is not very informative: the reader is left in darkness about what the article is about, and what those feedback system is. I'd suggest to try to use the title to inform the reader about, at the very least, the keywords of the manuscript, namely: long-term erosion, UK (more on this later), numerical thought experiment.

* If the title of the manuscript may be more "catchy" than informative (depending on the style of the Author), the abstract really needs to be informative, especially filling in what is not in the title. The abstract should be clear about (a) the model used, (b) the type of approach to modelling (if "numerical thought experiment" phrase is used in the abstract, that would be enough to wet the appetite of a theorist), (c) the scope of the research - the fact that the result of the analysis may be limited to conditions similar to those studied (i.e. UK), with possible usefulness to other areas where "saturation excess" is the dominant overland flow mechanism.

* The Introduction section should include a paragraph about modelling, since this article is based on it: which models are the most used, how and why do they keep soil erodibility fixed throughout the soil profile, what is MMMF model and why it was selected above the others. The Authors should also make it clear to the readers what has been observed in the field and what is not known yet (observations part), what was done already in research for long-term soil erosion predictions, etc... The introduction should state clearly the scope of the study: in this case, the results may be applicable only to the UK, even though the problem raised (soil truncation) is certainly important in all areas of the world. Finally, the Introduction should state clearly the objective of the study; even better would be to state a research question that will be directly answered in the conclusions; even-even better would be to state a null hypothesis to be tested statistically through the numerical experiment (but this may be very limiting). As a general comment, the Authors should try to create a clear connection between the introduction and the conclusions they reached.

* Finally the Materials and Methods: in my opinion, the work-flow followed is not very clear, I suggest to include a figure in the article detailing what was done step by step, e.g.: 1) MMMF model (already described) was modified to include topsoil truncation (state assumptions made); 2) soil data from UK was used to derive pedotransfer functions; 3) soil data and pedotransfer functions were used to create N instances of the model with soil parameters as in the 265 soil profiles; 4) the climatic and LUC information from the 265 experiments were used to set mean and variance for the respective parameters in MMMF, and this normal distribution was used to run a Monte Carlo analysis for each of the N instances of the model; 5) the results were analysed by (describe). Note: I may have missed or misinterpreted something in the example given, and this should show that the work-flow was a bit confusing. If not with a figure, the workflow should nevertheless be made more clear by restructuring the section 2.4.

* Bonus point: Discussion. Even though the discussion section is very nice, I noticed that it does not discuss the limitation of considering 500 yr soil truncation when assuming no sedimentation (no input of soil material to the modelled profile). Moreover, I think that it would be nice to organize the discussion by dividing clearly the limitations of the study depending on: inherent limitations/assumptions of the MMMF model;

limitations/assumptions in the modifications introduced on the model;
limitations/assumptions in the dataset and simulations performed. However, the assumptions could be also introduced in the Materials and Methods section (in the respective sections 2.2, 2.3 and 2.4).

A major question: why did the Authors did not perform a Global Sensitivity Analysis of the modified MMMF model, using as parameter space the edaphoclimatic conditions of the UK sites, after deriving their probability density functions?

Minor correction: line 283, "with the variation in soi losses over" is missing an "l" in "soil".