Dear Enrico,

Many thanks for your thoughtful comments. We highly appreciated your remarks, and we look forward to addressing them in a revised version of our manuscript. Below we respond to your questions and suggestions and explain how we plan to incorporate them into our revisions. Please note that your comments are displayed as bullet points followed by an author response (AR).

- 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?

  AR: Thanks again for your comments, we are happy to know you appreciated the writing style. Indeed, the idea of an opinion paper backed up by modelling experiments is pretty much what we had in mind when we worked on this manuscript.

- 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.

  AR: We highly welcomed your comments for improving the manuscript and we apologise if the some of our assumptions and workflow were not more explicitly stated.

We also enjoy detective stories, and although as scientists we engage in similar semiosis as investigators (see Baker, 2017), we fully agree scientific manuscripts should be written in a precise and clear manner. Hence, we plan to improve the abstract, introduction, and methods following your detailed suggestions below.

- 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.

  AR: In all honesty, we already had some debate about the style of the title of the manuscript. As you have also pointed out that the title could be improved, we will work on a more informative one for the revision, including some of the key works you mentioned.

- 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.

  AR: We fully subscribe to your suggestions. We will expand the abstract to include the information about (a) the MMMF model, (b) the modelling approach (clearly mentioning “numerical thought experiment”), and (c) the geographic / edaphoclimatic limitations of the results.

- 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.

AR: Apologies for the lack of a paragraph about soil erosion models – we agree this should be included in the introduction. In the revised manuscript, we will introduce different erosion models and modelling approaches. Regarding to why models and model users do not typically include the effect of soil truncation on a profile's erodibility, we suppose this stems from: i) lack of knowledge of the potential interactions between soil erosion and erodibility, ii) lack of data (particularly spatially distributed) about subsoil properties and soil depths, and iii) the temporal scale of the model applications, which in many cases might not cover a sufficiently long period for these feedbacks to develop. We will add these considerations to the introduction.

Moreover, we will expand our reasoning for choosing the MMMF model. Besides the parsimonious process-oriented approach and the fact that the model was developed and tested for different soils and land covers in the UK (as mentioned in lines 87-91 of the manuscript), we chose MMMF because i) it incorporates particle size selectivity during erosion, transport, and deposition, ii) it incorporates the effects of stone cover on soil detachability, and iii) it does not require calibration (i.e., parameter optimisation).

- 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...

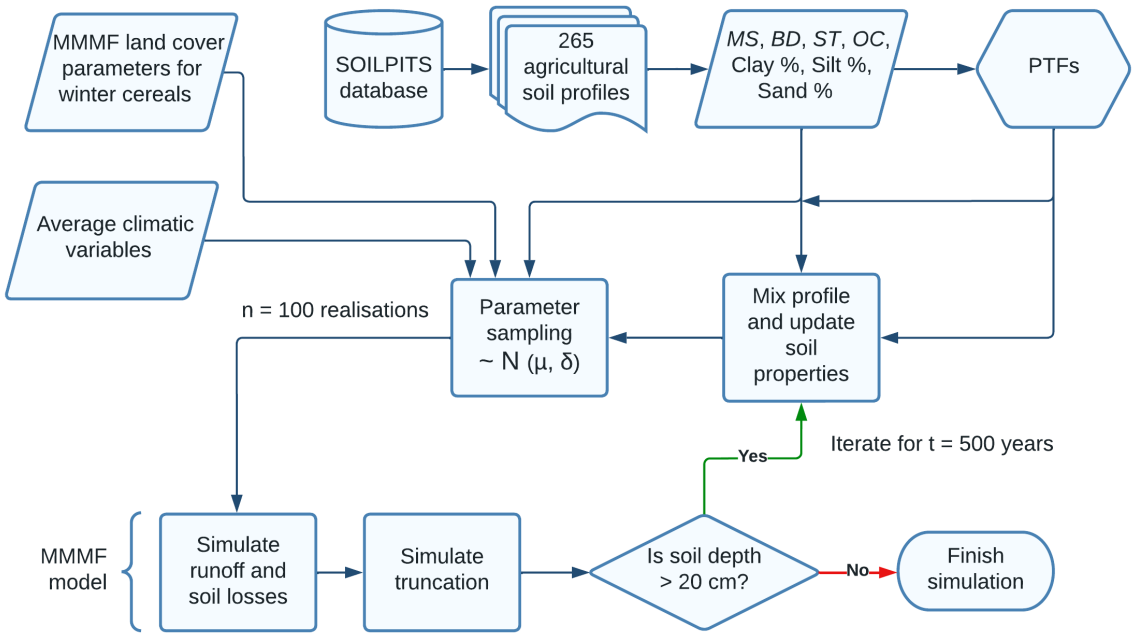
  AR: Thanks for pointing this out, we will incorporate more references from the literature about what we already know about potential feedbacks between soil erosion and soil thinning. Is this what you had in mind?

- 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.

AR: These are very good points. In the revised manuscript we will be more precise about the scope (mentioning the geographic limitation of the input data) and the objectives of our work. Although this research was somewhat exploratory, we did start with clear research question (do erosion-induced changes to soil properties affect soil losses and runoff formation?), which could be formulated as a hypothesis from the beginning (erosion-induced changes to soil properties affect soil losses and runoff formation). We believe this will create a better connection between the introduction and the results/conclusions – thanks again for this comment. However, we are not sure if stating the null would be the best way to go here. We performed a numerical thought experiment built on multiple assumptions and auxiliary hypotheses and, hence, we would be wary of adopting a stricter falsificationist stand regarding our simulations, in this particular case.

- 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.

  AR: Thanks for this suggestion – we completely agree that a flowchart would make the methodology easier to understand. Hence, we would like to add the flowchart below to the revised manuscript:



Legend *MS*: soil moisture at field capacity (% w w^{-1}), *BD*: bulk density (Mg m^{-3}), *ST*: stone cover (proportion 0-1), *OC*: soil organic carbon content (%), PFTs: pedotransfer functions.

Please note that, as displayed in the flowchart and explained in lines 195-200 of the manuscript, the (mean) land cover / land use parameters values were taken from the MMMF guidelines. The values represent an approximate average of the soil cover parameters for the crop growing season (for conventionally tilled winter cereals, in our case). Apologies if this was not more clearly stated.

- 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).

AR: Thanks for the bonus questions! The idea behind not simulating sediment input from upslope of the modelling spatial unit was based on the assumption that this unit would be subject to a negative erosion balance. That is, it is only possible to evaluate the sensitivity of soil losses and runoff formation to erosion-induced changes to soil properties in areas where i) soil erosion outpaces soil formation and ii) erosion rates are higher much than deposition rates. Of course, different positions in the landscape will be more deposition prone, which will lead to very different feedbacks (see Quinton et al., 2022). We do agree, however, that neglecting sediment input and the lack of a spatial/landscape component are limitations of our approach, which should be addressed in the discussion. We would also be glad to add sub-headers to the discussion, as you suggested.

- 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?

  AR: We believe Figures 5 and 6 ultimately display the results of an all-at-a-time sensitivity analysis (although not global if we take the definition from Pianosi et al., 2016, as we did not explore different model structures). That is, we analysed how erosion-induced changes in specific parameter values explained the changes in erosion rates, while sampling the parameter space for each time step through a Monte Carlo simulation. Since we were much more interested in the parameters which explained the changes in soil erosion rates over time than apportioning the output uncertainty (i.e., the annual soil losses simulated by the model) to different parameters, we believed such type of sensitivity analysis would not be necessary. More classical sensitivity analyses of different versions of the Morgan-Morgan-Finey model were performed by Batista et al. (2019) and Quinton (2004). If you think it would be important to include such a sensitivity analysis to the manuscript, we would be happy to accommodate that.

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

  AR: Thanks for noticing the typo, we will take care of that in the revision.

References

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