Dear referees and editors,

Many thanks for the constructive and detailed reviews. We very much appreciated the comments and suggestions, which have largely improved the quality of our work.

Following the initial discussion, we now provide final response to your comments, in which we explain how we have incorporated your comments and suggestions to our revised manuscript.

Please note that reviewer comments are displayed as bullet points followed by an author response (AR) and that line numbers refer to the marked (tracked changes) manuscript.

With thanks and best wishes,

Pedro Batista (on behalf of the co-authors)

Reply to reviewer #1 (Enrico Balugani)

Dear Enrico,

Thanks again for your thoughtful comments. Following the preprint discussion, we have made substantial modifications to the manuscript. On top of your suggestions, we have also corrected some model equations – as you might have seen in our replies to the referee comments Andres Penuela and Joris Eekhout. Below we explain how we incorporated your comments into the revised manuscript. Please note that your comments are displayed as bullet points followed by an author response (AR) and that line numbers refer to the marked (tracked changes) manuscript.

- 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 improved 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: We have changed the title to "Does soil thinning change soil erodibility? An exploration of long-term erosion feedback systems". We believe this includes more information regarding what kind of feedbacks we are looking at.

- 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: Thanks for highlighting this. We expanded the abstract to include the information about (a) the MMMF model, (b) the modelling approach (clearly mentioning "modelling experiment"), and (c) the geographic / edaphoclimatic limitations of the results (L1-30)

- 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: We have included a paragraph on modelling in the introduction (L60-70) and expanded our reasoning for choosing the MMMF model in the methods section (L125-135).

- 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 this comment, we found it particularly interesting to improve this part of the manuscript. We expanded the introduction with more references from the literature about what we already know about potential feedbacks between soil erosion and soil thinning (L45-50).

- 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: Following our previous discussion, we more clearly stated our hypothesis and the geographical scope of the research (L95-100).

- 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: We have included a flowchart (Figure 4; L243), thanks for this suggestion.

- 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: As we explained in the preprint discussion phase, 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. We do agree, however, that neglecting sediment input and the lack of a spatial/landscape component are limitations of our approach, which we now addressed in the discussion (L468-473; L530-535).

Regarding the rearrangement of the discussion: after trying to implement separate sub-sections, we had the impression this was not improving the readability of the manuscript. Hence, we would appreciate if we could keep the current structure/flow of the discussion.

- 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 6 and 7 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, have corrected it (L384).

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Reply to reviewer #2 (Andres Peñuela)

Dear Andres,

Thanks again for reviewing our manuscript and for picking up on that error in the leaf drainage equation. Below we explain the changes in the manuscript prompted by your comments. Please note that your comments are displayed as bullet points followed by an author response (AR) and that line numbers refer to the marked (tracked changes) manuscript.

- In this manuscript the authors study the effect of soil truncation/thinning in the evolution of the annual soil erosion rates over 500 years. For this purpose, they used a parsimonious soil erosion model and 265 profiles in different locations in UK. They considered in the simulations the change in soil properties as subsurface layers get exposed as a consequence of the removal of the soil surface by soil erosion. In order to isolate the effect of soil truncation, factors usually considered as variables, such as climate, land cover and topography, are considered as constant. First of all, I would like to congratulate the authors for a very interesting paper and for developing a simple but effective method to consider long term soil truncation in soil erosion modelling. In general, I think that the manuscript is well written and structured.

AR: Thank you very much, we were happy to receive your feedback.

- My one major concern is that one of the equations applied is wrong. The error is Eq. 6, this equation should be multiplied by LD, in other words, the KE of LD should be proportional to the annual LD, the same way as the KE of DT is proportional to the annual DT (Eq. 4). This error was already present in the original publication of RMMF model (Morgan, 2001) and has propagated to the MMMF model, equation 7 in Morgan and Duzant (2008) and other models including PSYCHIC (Davison et al., 2008) and SERT (López-Vicente et al., 2013). These KE equations were originally proposed by Brandt (1990) where they present two equations of kinetic energy per mm (J m−2 mm−1) of LD:

  E = 8.95 + 8.44 log I

  and of DT:

  E = 15.8 PH0.5 – 5.87

So, they need to be multiplied by the volume of rainfall (mm), LD and DT respectively, to obtain the KE (J m−2). Hopefully this correction won't change much the main results and conclusions. This error was previously corrected in some studies (Choi et al, 2017; Peñuela et al., 2018; Sterk 2021) however this error in the formulation of the RMMF and MMMF models was only pointed out by Peñuela et al (2018). To avoid further propagation of this error, I also encourage the authors to highlight in the manuscript the need to correct it when applying either RMMF or MMMF models and when developing new models based on them.

AR: We completely agree Eq. 6 should include the amount of annual leaf drainage, thanks for point out this error.

The change in Eq. 6 increased the simulated soil loss values due to an increase in the total rainfall kinetic energy, as expected. Trial runs also demonstrated the model was sensitive to the plant height (*PH*) parameter once the correct formulation was used, as you also pointed out in Peñuela et al. (2018). This led us to the realisation that the recommended value for *PH* for winter cereals (1.5 m) in the MMMF guidelines is somewhat excessive, considering, for instance, the height of current wheat varieties in the UK (mean = 0.61 m) (Berry et al., 2015). In addition, we looked into Brandt (1990) and noticed that *PH* in Eq. 6 does not refer to total plant height, but rather the fall height of leaf drips, which depended on the thickness of the canopy. So, on top of correcting Eq. 6 in the model code and in the manuscript, we changed the base value of *PH* to 0.4.

These corrections in model equations and model parameters are displayed in lines 150-157 and lines 245-257.

- How is the soil truncation calculated from SL? Can you further explain this? I think that while the formulation of the MMMF model is well described, it would be very helpful for other researchers interested in applying this method in their models to show explicitly the equations used to consider the effect of soil truncation.

AR: We agree including these equations would be beneficial to the paper and potentially useful to the readers. We inserted the equations to explain how we dealt with soil truncation and profile mixing to the manuscript (L269-300)

- Something that I missed in this study is an analysis of temporal evolution of the influence of soil truncation, in particular I would be very interested in knowing more about when this influence starts to be significant. It would be very useful for modellers to have an idea of under what circumstances, in particular number of years simulated, soil truncation should be taken into account or not. For example, if I do a 100-year simulation, should I include the effect soil truncation? While the results of this study cannot be generalized to other regions, I think that this can provide a first attempt to set recommendations, at least in UK, of when soil truncation should be considered depending on the number of years simulated.

AR: This is again a good point, thanks. The comments from the referees actually prompted a revision in our model code, as we noticed the temporal evolution of the soil properties and soil losses were insufficiently addressed in our equations. That is, our original model code failed to update soil properties until the mixing layer reached a different underlying horizon, which led to an underestimation of the effects of selective particle size removal during the initial simulation period (when the depth of the 20 cm plough layer mostly is lower than the thickness of the upmost soil horizon). Also there way a typo in equation 23 in part of the model code. These corrections, along with the changes in Eq. 6, had a large influence on the results, which meant that almost all soil profiles (98%) displayed a decelerating erosion trend. We could then characterise this general decelerating trend using the data from all profiles by fitting an exponential decay function, which allowed us to show how much (the median) soil losses decrease over specific time periods, as you requested. These results are displayed in lines 345-365.

- In the Discussion, please include a paragraph evaluating the performance of the model and the SL values simulate. Please include references of studies of measured annual soil loss in agricultural fields, in particular winter cereal, in UK, for instance Evans et al 2016 and Boardman 2013, and compare them to the simulated soil loss, are they similar or in the same order of magnitude?

AR: Apologies for not including this previously. We largely expanded the discussion, including a better comparison with observational data (L505-520)

Minor comments:

- Please justify in the manuscript the 20cm plough/mixing depth considered for the simulations.

AR: 20 cm is the average tillage depth in the UK (Townsend et al., 2016), as we now mention in the revised manuscript (L115).

- Can you please further develop the justification of using the MMMF model, for example, why is it important "its ability to simulate multiple erosion subprocesses" for this study? And that it is parsimonious?

AR: Thanks for this suggestion, we expanded reasoning for choosing the MMMF (L125-135).

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Reply to reviewer #3 (Joris Eekhout)

Dear Joris,

Thanks again for your comments and suggestions. As explained in the discussion phase of the preprint, the referee comments led to some substantial changes in the model and, consequently, in our results. Below we respond to your comments and explain the changes in the manuscript. Please note that your comments are displayed as bullet points followed by an author response (AR).

- In general, the manuscript is very well written and accompanied by clear figures. I think that the model experiment is very useful and gives some interesting insights on the feedback between soil thinning and soil erosion rates. However, I have the feeling that the implications for soil erosion modelling may be a bit overstated. The authors apply a 500-year simulation period, which generally speaking is much longer than normally applied in soil erosion modelling studies, which mostly apply a decadal time period. The results show that over this 500-year simulation period there are some notable changes in soil erosion rates, however, when focusing on the first few decades, the changes are negligible. Of course, as the authors point out in the Discussion section, there are some processes that are not accounted for by the MMF model, which may affect the results, also in the first few decades. But still, I doubt if the results of this study have significant implications for soil erosion modelling. The authors may agree or disagree with this, but I welcome to the authors to discuss this point somewhere in the manuscript.

AR: We are glad you generally appreciated the manuscript. Moreover, thanks for pointing out the issue with the time scales, which was also highlighted by another referee (Andres Peñuela). Both of your comments actually led us to revise our model code, as we realised the long time taken for erosion feedbacks systems to develop could be an indication that the model was not sufficiently addressing the effects of particle size selectivity. We then noticed our initial code only updated soil parameters once the plough layer reached a different underlying subsoil horizon (e.g., once the Ap is thinner than the plough layer and you start ploughing into an E, B, or C horizon). However, since the model simulates selective particle size removal, and because bulk density and soil moisture at field capacity are estimated via soil-texture-dependent PTFs, we should have been updating these properties for every time step – even when the plough layer depth is thinner than the soil upmost horizon. In this case, fresh material from the upmost soil horizon would be incorporated to the eroded plough layer, following a soil loss/truncation timestep. The changes in model equations are described in lines 145-155, 245-255, and 270-300.

These changes in the model code, alongside with the corrections regarding leaf drainage kinetic energy suggested by Andres Peñuela, led to some important changes in our results. For instance, the model now simulated a decelerating trend in 98 % of the soil profiles, due to a progressive decrease in silt contents and an increase in stone cover, even for the profiles with silty subsoil horizons (L340-370). That is, with the new and improved model formulation, the supply of erodible material from the substrata being incorporated into the plough layer was not sufficient to accelerate soil losses over time, but rather to keep it somewhat constant. Contrarily, the profiles with decreasing silt contents in their lower horizons had steeper decreases in erosion rates, even in a matter of decades.

With the new results we could characterise the temporal evolution of the soil losses (for all soil profiles combined) using an exponential decay function. Based on this function, we calculated that, on the median, erosion rates declined in 6 % over the first 50 years of the simulations, and 10 % over the first 100 years (Figure 5; L340-370).

From our understanding, these differences are relevant for erosion projections, which, for instance, look at the soil erosion responses to climate change in the next 50 or 70 years. The erosion feedback systems we simulated might also be important for modelers trying to hindcast the influence of land use change on erosion rates over the last century. Govers et al. (2006) also simulated an exponential decay in sediment production from arable hillslopes over a 50-year period following land use intensification, due to a rapid emergence of a soil with high stone cover, which further highlights the importance of considering erosion-induced changes in soil loss projections/hindcasts. It is also worth pointing out that our simulations provide a rather conservative scenario, as erosion rates in the UK are rather low compared to many other regions of the world (Benaud et al., 2020). We included some arguments regarding the relevance of these feedbacks for erosion modelling in the discussion (L529-534; L540-547). We also removed the sentence: "*To date, this soil erosion feedback system has been largely overlooked in soil erosion models, which might have led to spurious estimates of long-term erosion rates.*"

- Line 205: Table 2 shows the parameter values used in the Monte Carlo simulation. I'm not sure if this is really important, but if you would calculate the ground cover occupied by the number of stems (i.e. stem area times number of plants per unit area = (0.025**2

* pi) * 250) then I arrive at a value of around 0.49, while the ground cover is assumed 0.3.

AR: Thanks for mentioning this, we hadn’t noticed such discrepancy in the suggested parameter values. The model now calculates the ground cover using the number of plants per unit area and the diameter of plant stems, in order to avoid any inconsistencies (L252-255).

Benaud, P., Anderson, K., Evans, M., Farrow, L., Glendell, M., James, M., Quine, T., Quinton, J., Rawlins, B., Rickson, J. and Brazier, R.: National-scale geodata describe widespread accelerated soil erosion., Geoderma, 371(September 2017), 114378, doi:10.1016/j.geoderma.2020.114378, 2020.

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