Dear Andres,

Many thanks for taking the time to review our manuscript. We highly appreciated your corrections and suggestions, which, along with the comments from the other referees, had a large positive impact in our article.

Below we address to all your comments 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).

- 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. We already updated the model code (we will add a new version to Zenodo with the revised manuscript) and made new model runs. 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. We propose the following changes in the manuscript:

"The kinetic energy of direct throughfall KE_{DT} is calculated with the typical value of erosive rainfall intensity for a given location (I; mm hr^{-1}) and the amount of annual direct throughfall, whereas the kinetic energy of leaf drainage KE_{LD} is a function of the average plant height for the growing season (PH; m) and the amount of the annual leaf drainage. Total kinetic energy of the effective annual rainfall (KE; J m^{-2}) is then calculated as the sum of the throughfall and leaf drainage components.

$$KE_{DT} = DT \cdot (8.95 + 8.44 \cdot log_{10}I) \qquad (4)$$

$$if\ PH < 0.15; KE_{LD} = 0 \quad (5)$$

$$if\ PH \geq 0.15; KE_{LD} = LD * (15.8 \cdot PH^{0.5} - 5.87) \quad (6)$$

$$KE = KE_{DT} + KE_{LD} \quad (7)$$

Of note is that in the original MMMF publication (Morgan and Duzant, 2008), as well as in the Revised Morgan-Morgan-Finey paper (Morgan, 2001), equation 6 does not include the LD parameter. However, this would lead to the KE_{LD} *being expressed in J* mm^{-1} m^{-2}*, instead of J* m^{-2}*. Hence, we highlight that a correct application of equation 6 must include leaf drainage depth (see Brandt, 1990; Morgan et al., 1998; Peñuela et al., 2018 for further details)."*

"For the land cover parameters, we took the guide values recommended by Morgan and Duzant (2008) for conventionally tilled winter cereals, except for plant height (PH). In this case, we found the suggested value of 1.5 m excessive for winter cereals, especially considering that PH in equation 6 does not represent the height of the top of the canopy, but rather the height of the fall of drops from the plant, which depends on the thickness of the canopy (Brandt, 1990). Assuming an average absolute height of 0.61 m for wheat varieties in the UK (Berry et al., 2015), we adopted a baseline PH value of 0.4 m. This was also performed to constrain disproportionate estimates of leaf drainage kinetic energy, as trial model runs indicated a high sensitivity of the model outputs to parameter PH".

- 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 propose to insert the equations to explain how we dealt with soil truncation and profile mixing to the manuscript:

"As the MMMF model is applied with an annual resolution, we used the soil bulk density to convert the modelled soil losses from their native units to m yr^{-1} (SL_m)*, after each timestep:*

$$SL_m = \frac{SL}{BD} \cdot 10^{-3} \quad (23)$$

Next, the model reduced the depth of the upmost soil horizon considering the amount of eroded soil in the previous timestep. Since the model calculates soil losses separately for each particle size fraction, the soil texture of the 20 cm plough layer was updated after each timestep k using a mass-balance model based on the amount of fresh subsoil being incorporated into the plough layer. This requires estimating the mass of the original plough layer (M_k; kg m^{-2}) and the mass of the subsoil layer that will be incorporated by tillage (M_s; kg $^{-2}$):

$$M_k = 0.2 \cdot BD_k \cdot 10^3 \tag{24}$$

$$M_s = SL_m \cdot BD_s \cdot 10^3 \tag{25}$$

Where BD_k is the bulk density (Mg m^{-3}) of the original plough layer in timestep k and BD_s is the bulk density (Mg m^{-3}) of the subsoil layer s being incorporated by tillage.

The masses of each particle size fraction i are then calculated as:

$$M_{k_i} = \left(M_k \cdot \frac{T_{i_k}}{100}\right) - SL_{i_k} \tag{26}$$

$$M_{s_i} = \left(M_s \cdot \frac{T_{i_s}}{100}\right) \tag{27}$$

The masses $M_{k,s}$ then summed to calculate the mass and the percentage of each textural class i in the new plough layer for timestep k +1:

$$T_{i_{k+1}} = \left[\frac{\left(M_{k_i} + M_{s_i}\right)}{(M_k - SL_k) + M_s}\right] \cdot 100 \tag{28}$$

Rock fragments were assumed not to be removed from the soil matrix, and therefore the stone cover (if present) undergoes a residual increment, considering the soil losses from the previous timestep:

$$ST_{k+1} = \frac{(ST_k \cdot 200) + \left[ST_s \cdot (\frac{M_s}{BD_s})\right]}{\frac{(M_k - SL_k)}{BD_k} + (\frac{M_s}{BD_s})} \tag{29}$$

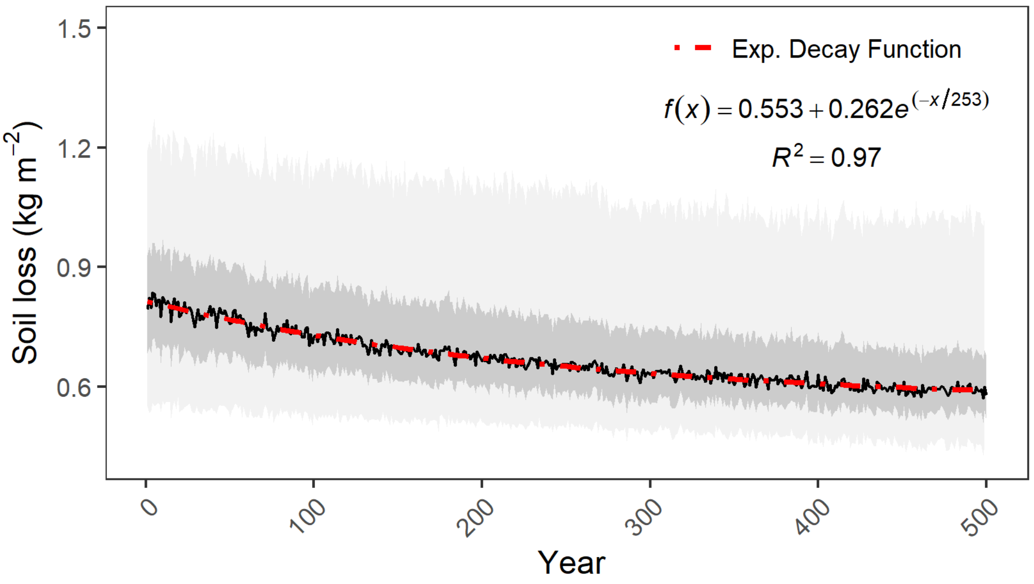
If the upper horizon depth was greater than the 20 cm plough depth, we mixed the eroded plough layer with fresh material from the upmost soil horizon using the mass balance model described above (Eqs. 24–29) to recalculate soil texture and the percentage of rock fragments (used as a proxy for the stone cover model parameter). Accordingly, soil organic carbon was assumed to remain stable as the selective removal associated to finer soil fractions was not simulated. However, if the upper horizon was thinner than 20 cm for any given timestep, the mass balance model (Eqs. 24–29) mixed the material in the plough layer with the underlying soil horizon. In this case, soil organic carbon values were also updated with a mass balance model:

$$OC_{k+1} = \left[OC_k \cdot \frac{(M_k - SL_k)}{(M_k - SL_k) + M_s}\right] + \left[OC_s \cdot \frac{(M_s)}{(M_k - SL_k) + M_s}\right] + \qquad (30)$$

- 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 will be further explained in the revised manuscript, but to answer your question: in the first 100 years, the median simulated soil losses decrease in 10 %.

The temporal evolution analysis you requested can be visualised in the figure below, which we will add to the revised manuscript:



Soil erosion trends over 500 years of model simulations for the profiles from the UK SOILPITS dataset. The dark solid line represents the median of all profiles and simulations, whereas the dark- and light-grey shaded areas are 50 % and 95 % prediction intervals, respectively.

- 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 will add a comparison of model outputs with the UK national-erosion data described in Benaud et al. (2020):

"Importantly, the soil losses estimated from our model outputs (median = 0.67 kg m^{-2} yr^{-1}; interquartile range = 0.58 – 0.80 kg m^{-2} yr^{-1}) are encompassed by the median and the upper quartile of measured erosion rates from arable land at plot scale in the UK (~ 0.1 – 1 kg m^{-2} yr^{-1}) (see Benaud et al., 2020). We compared model outputs to plot-scale measurements due to their similarity with our modelling spatial unit (i.e., 10 m eroding-hillslope segment with 6 ° or 10.5 % slope gradient). It is worth

highlighting that (i) the plot data described in Benaud et al. (2020) mostly derives from slopes below 10 %, (ii) net erosion rates at field or catchment scale in Britain are much lower than our model simulations (Benaud et al., 2020), which only represent an eroding, slopy, hillslope segment of arable land".

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 will mention in the revised manuscript.

- 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 will expand on the justifications for adopting the MMMF model:

"The model was chosen due to its ability to simulate multiple erosion subprocesses, which is desirable for understanding the specific mechanisms responsible for developing erosion feedback systems. That is, MMMF represents particle-size selectivity during erosion, transport, and deposition, incorporates the effects of stone cover on soil detachability, and simulates particle detachment by both raindrop impact and surface runoff. In addition, the MMMF has a parsimonious parameter set, which facilitates model application using national soil survey datasets. Moreover, the MMMF and its derivatives have provided acceptable predictions of annual soil losses for different soils, land covers, and testing sites in the UK (Morgan and Duzant, 2008; Peñuela et al., 2018; Smith et al., 2018)".

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