

I found this a generally well written and interesting manuscript on a timely topic. However I feel that the authors have to sharpen their arguments and they should remove several formal weaknesses (especially regarding mathematical notation and use of units), which make it difficult understanding the text.

The manuscript deals with the influence of accumulating erosion on yield and in turn on carbon sequestration. However, it ignores basic agronomic knowledge and agricultural concepts and thus has (presently) limited real-world relevance. From an agronomic point of view it is very clear that soil truncation has NO influence on yield in contrast to the basic assumption by the authors. My strong statement is easily proven because highest yields are possible without any soil (for extreme examples see the conceptual studies for a future Mars mission). What changes is not yield but the effort-yield relationship. The effort may increase to maintain yield. Some erosion effects can be changed with little or even no effort (e.g. nutrient losses in over-fertilized landscapes); other may require more effort (e.g., irrigation to compensate losses in water holding capacity). The authors may wish to argue that their relation holds true for a given and constant effort. Such behaviour may be found in controlled plot experiments but it is agronomically invalid because it would require that a farmer stops making decisions while in fact he has to decide and adjust his management every day. There is no other explanation why farmers accept soil losses that are above what soil scientist regard tolerable than that they regard the increase in effort to maintain yields smaller than the efforts needed to lower erosion. Note that usually it is assumed that erosion decreases productivity. This is something different than yield and switching from productivity to yield is not a trivial modification and would call for a discussion of its implications.

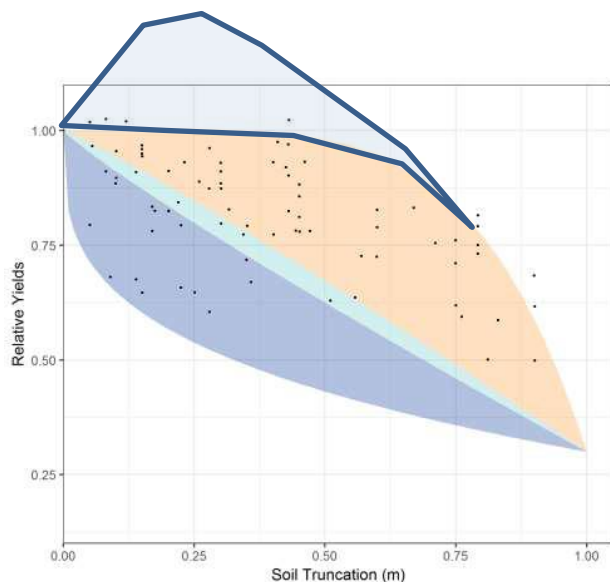
I wonder why EPIC was not used. Doesn't this do essentially the same job but allows a better control of agronomic practices and all other parameters that influence yields (which all are completely ignored in the manuscript). EPIC would allow deriving yields from productivity. This also leads to the next influence that the authors do not consider: some causes of productivity decline by soil truncation are difficult to remove while this is easy for others. For instance the authors expect the largest effect on SOC decline from a loss of nutrients due to erosion (although this is pure speculation). Such a loss of nutrients would be easy and cheap to replace in many countries. Reversibility of productivity again points to the importance of the effort-yield relationship.

The basic relation between soil depth and yield is given in figure 1. This figure suggests that the study used data but this is misleading. In fact only one conceptual relation was used although the authors suggest that this relation can be separated into three different cases. My main critique regarding this figure is twofold:

(i) It ignores a fourth rather common case, namely that productivity first increases with increasing soil truncation (often up to a truncation of 20 cm to 40 cm) and then starts to decrease. This behaviour can be found in many loessial landscapes and the effect is so strong that at least in former times without subsidies farmers paid higher prices for land where the clay depleted AE horizon had been lost and the better structured Bt horizon improved the properties of the Ap.

(ii) The interpretation of these three conceptual cases is brave. The authors explain a steep decrease in yield at little truncation by nutrient limitation. This is quite opposite to text book

knowledge of plant nutrition. Since the early times of Mitscherlich we know from the law of diminishing returns that a reduction in nutrient availability has little effect when starting at high availability. For the topic of the manuscript it is completely irrelevant whether the one curve is caused by nutrient loss and the other curve is caused by loss of water holding capacity. These interpretations, which are repeatedly treated in the manuscript like truth although any proof is missing, should entirely be removed.



I would suggest that the authors strictly follow the rule of notation in mathematics. E.g.: sometimes they ignore the multiplication sign and  $AB$  means  $A \times B$ , in other cases  $AB$  means one variable; sometimes variable are in italics, sometimes not; sometimes even mathematical signs are in italics (it should be  $dt$ ). Units are similarly ambiguous (e.g., the unit coulomb is reported but not meant). I suggest following the “Guide for the Use of the International System of Units (SI)” (<https://physics.nist.gov/cuu/pdf/sp811.pdf>).

The data that were used to calibrate the model come a bit out of the blue. “we used data from ten study sites” but I am not sure whether the five references distributed within this paragraph were the origin of the data. Without clear reference there is no information about their reliability and the boundary conditions under which they were carried out.

Some assumptions inherent in the model and some equations seem doubtful and would need better justification or modification:

- (i) The model treats organic manure and plant residues identical (eqn 2). I wonder whether this is true because digestibility of fresh plant material is around 75%. Hence only 25% is left after the passage of the digestive tract and it is likely to assume that the remaining 25% are more resistant to further degradation than the initial material. Furthermore, in solid manure often stabilization processes take place that do not occur with plant residues on the field.
- (ii) Surprisingly, the humification factor then distinguishes between manure and crop residues although this is not possible at this stage anymore because eqn2 has already mixed manure, crop residues and other young carbon into one young pool.

(iii) The model considers only temperature as climate and edaphic (!?) factor (which temperature is not said), while usually soil moisture is the most dominant influence on SOC stabilisation (see Jenny 1941).

(iv) The model does not consider any preferential loss of SOC or clay by erosion. The results may thus only be valid for tillage erosion

(v) Only roots incorporate SOC into subsoil. Bioturbation, leaching and other processes are omitted.

(vi) It is not clear, what follows in the model below 100 cm depth. I had the impression hard rock (i.e. the model does not shift the entire soil profile downward, when topsoil is lost). In this case, the model would be far too simple because hydrology then becomes tricky. Lateral water movement could not be ignored anymore when large parts of the soil were removed. Modelling would be easier and the results likely more realistic if soft rock would follow below.

(vii) Eqn (9) seems to be wrong because all carbon that leaves the young pool is delivered to the atmosphere although large part of this carbon (see humification factor) enters the old pool.

(viii) A value of 0.55 or 0.6 seems to be more appropriate for alpha than 0.7. This could have considerable influence on the results.

The authors decided to use RRMSE for optimization (eqn (10)). Why? Isn't this a bad decision because it puts larger weight on layers with low SOC content although those layers are rather unimportant and relative measuring error is larger there? The authors also seem to have forgotten that they used RRMSE because they frequently report units of RRMSE (e.g. in Fig. 2) although this parameter cannot have a unit.

Is a model error of 93% or even 121% acceptable (see Table 1b)? I would not be satisfied.

Table 2a+b: How can the contribution of all parameters sum up to more than 100%?

Table 2b: How can erosion rate have an influence on the result although erosion rate was set constant?

The Results chapter does not differ in style and content from the preceding chapters, which were assigned to Material and Methods. Most results are in fact reported in the preceding chapters. The manuscript requires better structuring

Fig. 4: Units of the left panel? Shouldn't be a time unit in the right panel? What do the black lines denote?

Fig. 5+6 are in poor quality. Use the same font size as in fig. 4

Fig. 7: the information about the treatments is repeated three times (twice in the figure and once in the caption). What do the boxes and whiskers show (there is no convention on this)?

I didn't like the Discussion. What I missed at the very beginning is a paragraph about the assumptions and simplifications of the model and which influence they can have on the results (a little bit on this can be found at the very end but this is not stringent enough). Be more critical regarding your work. This would increase its value. At the moment it is of little value for me because I do not know under which conditions the results would apply and under

which conditions nothing could be said. Studies are cited which seem to be in agreement with your results but this does not mean much. It only becomes meaningful if we know your assumptions and simplifications because then we also know that these assumptions and simplifications would not be important for the other study.

On the other hand there are parts in the discussion that could be written even without the preceding results (e.g. the last paragraph of chapter 4.1). They could be deleted in order not to increase the length of the discussion. Also all speculations about hindrance or nutrients should be deleted. They are all unsubstantiated and misleading.

Details:

In general, the use of blanks is strange. After semicolon the authors do not like blanks. Also periods are often omitted (e.g. in i.e.)

Figure 2 only allows for yield reduction. Yield increase would also be possible (as in the already mentioned case of alfisols or in the case where an acidified topsoil is lost; there may be more cases).

I wonder why the authors used different orientation of Table 1a and 1b. The same orientation in both parts would be possible. I suggest using the same orientation as in Table 1b also in Table 1a because this is the standard orientation (variables in columns, cases in rows). Table 1b shows the vertical C balance. In all other cases this is called vertical C flux (at least I assume that this is the same). Be consistent.

Fig. 3: Aren't the red profiles calibrated profiles (the word "simulated" would then be misleading). I thought the manuscript was about arable soils but apparently these soils do not have a plough horizon. Is the manuscript about grassland or woodland soils?

The manuscript frequently reports 1000 parameters. Fortunately the model has less. I guess the authors mean 1000 parameter sets.

There are many more technical details (e.g. inconsistent tenses, omitted periods and blanks, inconsistent formatting of references) but given that large changes are necessary it does not make sense reporting these details.