Point-by-point response to the reviewers' comments

"A millennium of arable land use – the long-term impact of tillage and water erosion on landscape-scale carbon dynamics"

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We are grateful for the constructive and thorough feedback and thank the reviewers for their suggestions. Please find the answers to the reviewers' general and detailed comments in the text below. The original comments of the reviewers *are written in* blue *and our reply in black and italics.*

Reviewer #1 (Marijn van der Meij) – General comments:

Öttl and colleagues present a very comprehensive modelling study on the effects of soil redistribution on soil organic carbon stocks over millennial timescales. The manuscript provides detailed information on changes in agricultural practices, erosion intensities and the effect on SOC fluxes over this period.

The authors simulate a large variety of modelling scenarios, with varying erosion intensities and SOC dynamics. I think this is one of the first times that such a sensitivity analysis is performed in soil-landscape evolution studies. I'm impressed with the amount and quality of the work from the authors, but I have two main concerns which should be addressed before the paper can be published. These relate to the structure of the manuscript and missing information on certain data and definitions.

I detailed these concerns below and added more explanation and additional smaller comments in the attached PDF.

With best regards,

Marijn van der Meij

We thank the reviewer Dr. van der Meij for this encouraging feedback and for the substantial review, which heledp to improve the manuscript.

Structure of the manuscript

I feel that a clear structure is missing in parts of the manuscript, which makes it difficult to understand what is presented. With a revision of the structure, this should be easy to improve. Examples are

- the Methods section, where inputs, parameters, calibrations and justifications are presented in a mixed manner;
- The Results section, where a logical order of the results is missing;
- A missing overview of all the different scenarios that have been simulated. For example, only in the Results and Discussion it becomes clear that there are also simulations with and without deep C burial. A table with all scenarios would be helpful here.

We are grateful for the suggestions regarding the structure of our manuscript. We critically revised the structure of the methods and results sections to improve the understanding of the presented research (please see our answers to the specific comments). Moreover, we elaborated the different realisations and scenarios more clearly in the methods section to avoid any confusion in the results and discussion section. A table with the scenarios (Table 1 after line 179) and an explanation of the simulations with and without deep C burial were added (see lines 286ff of this point-by-point response).

Missing information

When reading the manuscript, I found several terms, data sources and modelling settings that were not introduced in the text. Examples are:

- The definition of a heavily eroded soil, both for the classification from remote sensing and in the model results (line 285, Section 3.1);
- The SOC-depth profile from forest soils that were used as initial condition (Fig. 3). This data is not introduced in the text;
- The absence of updating the topography after soil redistribution. This is mentioned for the first time in Section 4.4.1. I was surprised this was not included, as it has a big impact on erosion and SOC dynamics over 1000 years and should be relatively easy to include;
- A Discussion on how the size of the simulated landscape affects the model results. In other words, what are the benefits of simulating the entire Quillow catchment instead of the test sites?

We are grateful for the careful reading of the manuscript and finding missing information that is essential to understand our research. We revised the manuscript and incorporated the following details:

1) *In the methods section we explained the term "***heavily eroded soil***" (lines 282ff) as follows:*

   "The heavily eroded hilltops are visible in remote sensing data due to their brighter colours resulting from an exposure of the subsoil horizon partly consisting of glacial till (Figure 4)."

   We reformulated this sentence to make clear that brighter colours at eroded hilltops do not necessarily indicate exposed parent material but the incorporation of parent material into the topsoil (now in lines 287ff):

   "We qualitatively defined heavily eroded areas as the locations where bright subsoil material could be identified at the land surface by remote sensing images, which is indicative of the partial incorporation of glacial till into the plough layer due to extreme soil truncation. The exposure of such subsoil material implies that ca. 1 m of soil was removed by erosion (Van der Meij et al. 2017)."

2) *We are very grateful that the reviewer found this mistake regarding the missing data source of the* ***SOC-depth profile from forest soils****. We parameterised the forest soils according to Calitri et al. (2021). We apologise for the missing information, which unfortunately was lost during manuscript rewriting. A description of the dataset is now included in the methods section (lines 265ff):*

   "A mean SOC depth profile from three undisturbed soil profiles located in a forest in close proximity to the study area (Calitri et al., 2021) was used for the calibration of the first year of the model period (green line in Figure 3)."

3) *We now explain that we did not* ***update the topography after soil redistribution*** *in the methods section of the revised manuscript (lines 170f), so the reader is not confused in the discussion. We did not include an update of the digital elevation model (DEM) because compared to earlier studies dealing with DEM update, which mostly focused on bulk soil, we do not think that this can be easily implemented in an approach where SOC dynamics following soil redistribution are modelled. Modelling a millennium of soil redistribution by water and tillage in a yearly time step would require a DEM update every year. If the change in topography due to erosion and deposition is accounted for, this typically results in pits in the DEM. For models using flow or sediment routing, these pits have to be removed before the new DEM can be used in the next time step. The typical way handling this problem is filling the pits or redistributing soil to fill the pits with soil from their surroundings (Peeters et al. 2006). If SOC stocks as well as SOC sequestration and mineralisation are modelled, pit filling would strain the mass balance of SOC in the landscape or it is not clear what kind of soil and SOC is moved into the pits to fill them. Due to these difficulties, we decided not to account for a DEM update. To discuss the effect of a missing DEM update upon SOC balance we included the scenarios with and without deep carbon burial in the discussion section (already there in the current version of the manuscript). To clarify the use of these scenarios, we extended the methods section (see our answer to the comment in lines 286ff of this point-by-point response).*

4) *The main reason to* ***model the entire Quillow catchment*** *was to illustrate the effect of long-term soil redistribution on SOC stocks and dynamics in an entire landscape, while most small-scale erosion studies are often allocated at test sites which are specifically prone to erosion (Auerswald et al. 2009). This substantially biases the results and hence conclusions, because at larger landscapes, there is a tendency for slopes to be less steep and more deposition takes place. In general, the chosen approach and scale was used to analyse an average state of a landscape rather than to perfectly describe one single field.*

Reviewer #1 (Marijn van der Meij) – Detailed comments:

Title: Water is mentioned very dominant in the title, while the work mainly focuses on tillage erosion. Perhaps you can switch these two terms, or rewrite it as agricultural soil erosion?

We thank the reviewer for this suggestion and are happy to change the title as suggested to "A millennium of arable land use – the long-term impact of tillage and water erosion on landscape-scale carbon dynamics".

Line 2: Remove "associated"

This word was removed.

Line 2f: This sentence doesn't follow a logical structure. Please rewrite

The sentence was rewritten as follows (lines 2ff):

> *"Agricultural management has a strong potential to accelerate soil redistribution and therefore it is questioned if soil redistribution processes affect this potential CO_2 sink function."*

Line 6: Remove "the"

As recommended, "the perspective" was changed to "this perspective" (line 6).

Line 8: Remove "Therefore"

This word was removed.

Line 8: Can you indicate the modifications here?

As we elaborate the model modification in the methods, we changed the sentence accordingly (lines 8f):

> *"The spatially explicit soil redistribution and carbon (C) turnover model SPEROS-C was applied to simulate lateral soil and SOC redistribution and SOC turnover."*

Line 9: Remove "was applied" and "(spatial resolution 5 m x 5 m)"

We changed this sentence as stated in the answer to the last comment.

Line 16: Is this the current-day sink, or the sink over the entire 1000 years period? See also my comments in Section 4.4.3.

We realised that our current formulation was misleading and hence, slightly reformulated the statement in lines 15f. This number is the yearly, current-day gain in SOC stocks after 1000 years of soil redistribution. As explained in detail in lines 347ff and 356ff of this point-by-point response, we think that this number is an important outcome of our study and would like to keep it in the abstract. Lines 15f:

> *"Overall, it was estimated that after 1000 years of arable land use, SOC redistribution by tillage and water erosion results in a* ***current-day*** *landscape-scale C sink of up to 0.66‰ per year"*

Line 78: This paper mentions glacial retreat around ~20ka and landscape stabilization around ~15ka

We changed the sentence to (lines 75f):

> *"It represents a typical ground moraine landscape formed after the retreat of the Weichselian glaciers ca. 20,000 – 15,000 years ago (shaded area in Figure 1; Lüthgens et al., 2011)."*

Figure 1: The contour lines already show the elevation of the test sites. The colours don't add any extra information, especially in this low resolution. Perhaps you could visualize other terrain properties instead, such as hillshade, slope or TPI?

We have to admit that the information is redundant and appreciate the suggestion. We included the TPI in Figure 1 (instead of the elevation).

Line 101: Could you provide field names or place names of nearby towns?

We included the names of nearby towns in the methods section as follows (lines 99ff.):

> *"Test site A is located approximately in the centre of the study area belonging to the village of Christianenhof (53.3550° N, 13.6643° E), has a size of ca. 4.4 ha and a mean slope of 8.7% ± 3.9 %. Test site B is in the northeast of the study area close to the village of Holzendorf (53.3836° N, 13.7818° E), has an area of ca. 20.5 ha and a mean slope of 5.5% ± 2.9 %."*

Line 105: It's not clear to me what the modifications are from the original model. Could you elaborate on that?

Thank you for pointing us to the fact, that the sentence is misleading here. We did not modify the model but used the model as it was modified by (Van Oost et al. 2000). As this modification is explained in line 123f, we changed the sentence to (line 104):

> *"The spatially explicit soil redistribution and C turnover model SPEROS-C …"*

Line 108: The results show erosion of >1 m (Fig. 5). How is that possible with an initial soil thickness of 1 m? Does this soil thickness limit further erosion?

Although the model uses 10 soil layers with 0.1 m each (1 m in total), erosion is not limited because it is assumed that subsoil (or glacial till sediments) are uplifted as "fresh soil" in the deepest soil layer (0.9 – 1.0 m), if topsoil is lost. In case of deposition, the soil profile is accumulated, leading to soil burial below the 10^{th} layer. To make it clear that erosion and deposition are not limited, we added a sentence (bold text) to the paragraph in Section 2.2.1, subsection "Soil profile update" (lines 166ff) as follows:

> *"At eroding sites, a fraction of SOC from the first subsoil layer equal to the thickness of the eroded layer is incorporated into the plough layer.* ***Hence, erosion also leads to an uplift of soil into the deepest layer.*** *At depositional sites, a fraction of the SOC from the plough layer is shifted downwards into a buried plough layer. The underlying subsoil layers are further buried according to the depth of the soil deposition in that time step (Dlugoß et al. 2012, Van Oost et al. 2005)."*

Line 110: It's not clear to me what these fluxes are. Is it simply the SOC uptake and decomposition in every raster cell without any lateral redistribution?

Yes, this is correct. To make this clearer we extended the sentence accordingly (lines 107ff):

> *"To isolate C fluxes that occur solely due to total soil redistribution (TOT is the sum of TIL and WAT), a reference run simulating C fluxes without lateral soil redistribution was calculated, i.e. vertical C fluxes solely due to C input and decomposition."*

Line 166: So the plough layer retains a constant thickness, while additions and removals influence the layer below? Could you illustrate this for clarification? It is only mentioned in the Discussion (4.4.1) that topographic change is not included in the simulations. it would be better to mention it here as well, or, even better, include this feedback in the model

As already answered to an earlier comment, we added a sentence regarding the influence of additions and removals on the layer below (lines 167ff). We also added the information about the absence of topography update as follows (lines 170f; see also reply to general comments):

> *"Topographic change corresponding to soil redistribution was not taken into account to avoid blurring the mass balance of SOC."*

Line 172f: You mention the review here, but provide it later in the manuscript. Wouldn't the review be better placed at the study area descriptions, to keep the logical flow in the manuscript?

We highly appreciate this suggestion but in our opinion, it is important to mention changes in plough depth at this position and describe the results of the literature review of historical tillage implements in a later part of the manuscript.

Line 187: Could you provide this in a table or Figure?

We included a table specifying the composition of the nine realisations (Table 1 after line 179).

Line 200f: I find it difficult to believe that the farmers in the Uckermark still practiced manual hoeing, while the mouldboard plough was invented almost 1000 years earlier. Andersen et al. 2016 (DOI: 10.1016/j.jdeveco.2015.08.006) argue for a widespread adoption of the mouldboard plough in clayey regions of Europe in 1000 AD, so the start of your analysis period. Could you provide additional references supporting the use of the ard in the Uckermark from 1000-1100 AD, or change the plough type accordingly?

As stated in lines 200f, we know that the mouldboard plough was already invented much earlier and already widely used around 1000 CE. There is a book of Lüning (1997) entitled "Deutsche Agrargeschichte, Vor- und Frühgeschichte" in which it is stated that the simple ard plough was still a widely used tool until 900 CE. It was my assumption that not every farmer could afford using or had access to a mouldboard plough from 1000 CE onwards, as simple ard ploughs are still used today in developing countries. Moreover, it was our aim to show (i) a bit more variation than only 900 years of simple mouldboard and chisel plough and 100 years of mechanised agriculture, and (ii) to represent the gradual transition from simple ard ploughs to soil-inverting mouldboard ploughs. Of course, it is correct that the formulation "that the ***majority*** *of the farmers still practiced manual hoeing or used the simple ard plough in the first period" is somewhat speculative. As an improved k_{til} value that considers mouldboard ploughing for the first century would not change the median k_{til} value of 98 kg m^{-1} a lot (e.g. k_{til} value of a mouldboard plough before 1960 = 68 kg m^{-1}; see Table A1), we adapted the text as follows (lines 200f):*

> *"Although the medieval mouldboard plough was invented around 200-900 CE (Van der Meij et al., 2019), it was assumed that not every farmer practiced mouldboard ploughing and that manual hoeing or simple ard ploughs were still widely used in the first period (Behre, 2008, Herrmann, 1985)."*

Figure 3: Where do these forest soils come from? I can't find any explanation in the text. Are they based on experimental data that you use as reference, or is it a result from the modelling?

Here are some sources you could use for identifying old forest areas in this region:

-Wulf et al., 2021, https://doi.org/10.3832/ifor3774-014

-Calitri et al., 2021: https://doi.org/10.1007/s11368-021-03041-7

Please see our answer to the general comment in lines 68ff of this point-by-point response.

Figure 4: Could you include an example of the classification with the support vector machine in this Figure for comparison?

In the following, we show the classification for the two test sites A and B (Figure 1 of this point-by-point response). Although we think that the classification meets the erosional sites quite well, this independent data set is just used as a "soft"/ qualitative evaluation of the model results. We have decided against including it in the paper to avoid that this evaluation attends to much attention. The focus of our study should not be on the support vector machine approach or remote sensing data in general.

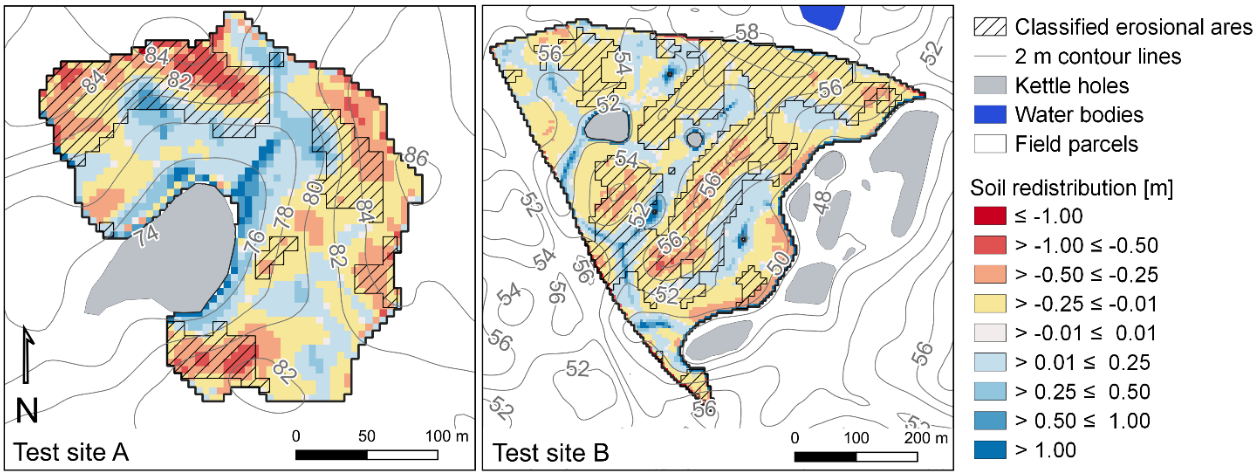

Figure 1. The dashed areas are erosional areas according to the classification of the Sentinel images, while the data in the background is the modelled total soil redistribution in m as shown in Figure 5 of the manuscript.

Line 300: I think this Chapter should be better structured. Usually, the model evaluation comes after the presentation of the model results, to give the reader some ideas about the types of output. The selection of the best model scenario is now hidden in Section 3.1, while that would also be relevant to start with.

Please consider to restructure this Chapter, and maybe write an introductory paragraph explaining its structure.

We changed the order of the results section so that the model evaluation follows the description of the model results. Moreover, an introductory paragraph explaining the use of scenario R4 was added (lines 307ff):

> *"The modelling approach resulted in a millennium of annual vertical C fluxes due to soil redistribution by tillage and water leading to a change in SOC stocks, as well as lateral C export by water erosion. According to the model evaluation (Section 3.2), realisation R4 (medium TIL, low WAT; Table 1) was used for the model analysis of the entire study area."*

Line 304: What is the definition of a heavily eroded soil?

If you compare this class to the exposed glacial till parent material in the aerial photographs, this means that the entire soil must be eroded. Natural soils can be (much) deeper than the 1 m soil column that you modelled (See Van der Meij et al., 2017). Also, in Fig. 5, there are barely any soils that have soil redistribution of 1 m or more.

Could you clarify in the text what it exactly is that you compare with each other, and, if you use different definitions for aerial photographs and model results, how that affects the evaluation?

As this was also a question in the general comments we refer to our answer in lines 57ff of this point-by-point response.

Line 307: Could you provide metrics of visualizations of these obvious relations? E.g. scatter plots, coefficients of determination or other statistics?

We realised that the term "visual comparison" is confusing or misleading. We wanted to show that the patterns fit, although the SOC contents are underestimated by the model results. By removing this statement, former Figure 5 (in the first version of the manuscript) does not have a function in describing the results anymore. We therefore removed the sentence with the "visual comparison" and former Figure 5 showing modelled spatial patterns of tillage-induced and total soil redistribution as well as modelled and observed topsoil SOC.

Line 309f: R2s only indicate correlations, but do not inform about the magnitude of the modelled and observed SOC stocks, or even the direction of the correlation. If I look at the SOC stocks, the modelled stocks are always underestimated.

If the orders of magnitude don't match, how can you make any reliable statements on the total SOC balance (lines 354, 472-474, 485)?

This is a fair point, which was also mentioned similarly by reviewer #2. The R^2 in Figure 7 show that the spatial patterns of the results are in agreement with the observations, i.e. the modelling approach results in low (high) topsoil SOC values at landscape positions where we also find low (high) observed topsoil SOC. We chose two agricultural fields for which recent SOC observation data was available and subsequently modelled 1000 years of soil redistribution and SOC dynamics. These two fields have an individual management and soil redistribution history that remains widely unknown as we just have general information about the history of land use in this region. Hence, a sound prediction of spatial patterns that we can observe in *the landscape is from our point of view very satisfying with respect to a modelling period of 1000 years. The relative error of the simulated SOC stocks for model realisation R4 is given in lines 380ff, where we mention the underestimation issue, though the predicted values are well within the range of the observed values (mean relative error of 40% and 20% for test sites A and B, respectively). To show the direction of the relation of modelled and observed topsoil SOC values, we added the Spearman's rank correlation coefficient for realisation R4 (lines 362f):*

> *"This realisation resulted in a Spearman's rank correlation coefficient of 0.83 and 0.82 for test site A and B, respectively."*

Figure 7(a) y-axis stating "C flux at erosional sites [g m^{-2}]": Not clear at first glance that these are only vertical fluxes. Could you adjust the label?

We changed the labels of Figure 7 accordingly:

> *"Annual vertical C fluxes at erosional sites [g m^{-2}]" / "Annual vertical C fluxes at depositional sites [g m^{-2}]"*

Figure 7(a) line showing data "with erosion": is this the same as soil redistribution? In that case, please use the same terminology

Figure 7(a) shows the temporal variation of annual C fluxes at erosional sites, while Figure 7(b) shows annual C fluxes at depositional sites. Therefore, the terms "with erosion" and "with deposition" are correct here.

Line 350: This is the first time you mention simulations with and without deep C burial. This should be introduced in the methodology.

We included an explanation of the simulations in the Methods Section (lines 171f):

> *"As this overstates the amount of buried C we created two model runs of vertical C fluxes taking deeply buried C (> 1 m soil depth) into account or not to show the effect of deep C burial on the C balance of the whole study region."*

Line 356: I'm missing a discussion on how the size of the simulated landscape affects the model results. You start with two test sites for model evaluation, before you apply it to the entire Quillow catchment. Do the results for the entire Quillow catchment differ from the simulations on the test sites, in terms of soil redistribution rates and fluxes? In other words, was it really necessary to simulate the entire Quillow catchment?

As this question was part of your general comments, please refer to our answer in lines 91ff of this point-by-point response.

Line 359: Remove "via the" and "The importance of"

The mentioned sentences were changed accordingly (lines 366ff):

> *"Understanding current agricultural soil-landscape relations requires to consider the long-term soil change, as today's soil and SOC patterns cannot be explained* ***by*** *the short-term soil redistribution history.* ***Our results demonstrated that long-term soil redistribution processes in agricultural landscapes are particularly important*** *in the Quillow catchment."*

Line 360: Remove "is particularly obvious"

Please see our answer to the last comment.

Line 376f: in Fig. 6 you mention that the observed SOC stocks are from the top 10 cm. Which depths were used for the comparison?

For the comparison of observed and modelled topsoil SOC stocks, we referred to the first layer of the model output, i.e. the first 10 cm.

Line 394: In Van der Meij et al., 2020 (https://doi.org/10.5194/soil-6-337-2020) we did incorporate this, and indeed show that C losses due to deforestation are much larger than current-day source or sink functions of agricultural landscapes

We incorporated this citation by pointing to the difference to our study as follows (lines 401ff):

> *"This was not included in previous long-term modelling studies of larger areas (e.g. Bouchoms et al., 2017; Wang et al., 2017) but only applied to an artificial topographic setting (2.25 ha; Van der Meij et al., 2020)."*

Line 427: This is 200 times more

Of course, we are happy to change the number to "200 times" (line 436).

Line 430: Interesting Section, but I'm not sure it is relevant for this study

This section is important for us to stress the dominance of soil redistribution by tillage in this region and to state that the C balance depends on farmer's decisions. Although we appreciate your thoughtful comment, we would like to keep Section 4.3.2 in the manuscript (lines 439ff).

Line 446: I assumed your model did include topographic change caused by soil redistribution, but this is the first time you mention that this is not the case. For simulations over such long timescales, topographic change would be considerable, with significant effects on erosion processes (reduced gradients in the landscape) and SOC dynamics (deep burial), as you also argue in this Section.

What is the reason this isn't included in the simulations? I would say it is pretty straightforward to update the DEM with simulated erosion and deposition after each calculation step.

There are models that do incorporate elevation changes caused by soil redistribution. (E.g. Van der Meij et al., https://doi.org/10.5194/soil-6-337-2020), and Kwang et al., https://doi.org/10.1029/2021JG006616). Wouldn't such model be more suitable for these types of studies, because they incorporate topographic changes?

Please see our answer to the general comment regarding the missing DEM update in lines 76ff of this point-by-point response.

Line 472: This would be a very unlikely assumption

We wanted to give a range of possible values, but of course, you are right that this scenario is very unlikely. Hence, we removed this assumption and reformulated the sentence as follows (lines 480ff):

> *"In our modelling approach the assumptions regarding the stability of SOC buried below 1 m are of tremendous importance in the range of soil-redistribution induced C fluxes (Figure 5 d).* ***Assuming that all SOC allocated below 1 m is stabilised, the overall soil-redistribution induced current-day C sequestration potential would lead to an increase in SOC stocks of 0.66‰ per year.*** *However, long-term modelling of SOC turnover in these deep layers is challenging …"*

Line 474: How does this sink compare with measured carbon fluxes in these agricultural landscapes?

Depending on crop rotations, weather conditions etc. the yearly fluxes of C into or from arable soils will vary and this variation might lead to larger SOC changes between years than 0.66% (see eddy-flux-measurements from other arable regions as e.g. in Schmidt et al. (2012)). However, a yearly mean increase of the SOC stocks of 0.66‰ per year is 16.5% of the SOC stock increase targeted by the 4‰-initiative (https://sdgs.un.org/partnerships/4-1000-initiative-and-its-implementation). As our formulation was probably not very clear, we changed this (see earlier comments). In our opinion, this is an important information for the reader.

Line 485: I would be hesitant to mention quantitative results here, as the simulated SOC stocks are systematically lower than the observed stocks (Fig. 6), so the simulated numbers are probably also off. I would use more qualitative conclusions

We do not agree here. The manuscript clearly states several times that the aim is to gain system understanding while modelling erosion-induced changes in landscape-scale SOC stocks over 1000 years. In the absence of data to properly validate the model (which would require, e.g. detailed management data for all the different fields in the test region), we use measured spatial patterns of topsoil SOC stocks from two fields to test the plausibility of the model in reproducing these spatial patterns (Figure 6 of the manuscript). By comparing SOC patterns rather than absolute SOC stocks, we can see that the modelled redistribution explains the patterns quite well, whereas we do not expect the measured absolute SOC stocks to match (it might even be suspicious if they would match). There is one main reason for this: The model is parameterised and fed with data based on general assumptions/estimates about soil management, crop rotations, crop yields, etc. that represent the whole catchment for the last 1000 years, whereas the test fields are likely to have a different soil management and cropping history. This means that the model is neither parameterised nor fed with data to represent the two small test sites, so any over- or underestimation could be due to model problems, or simply to the fact that these two out of hundreds of fields in the region cannot be represented by a generic model parameterisation and input data set. Apart from this general problem, one could also discuss the different measurement approaches used to estimate topsoil SOC stocks in the different test fields (soil sampling in a grid vs. remote sensing based topsoil SOC estimates assuming that the surface represents the top 10 cm) and the much higher sensitivity of topsoil to recent (changes in) management.

As a consequence of the above, it cannot be concluded that the model systematically underestimates topsoil SOC stocks. By being transparent in the manuscript about all model limitations, but also keeping in mind that the presented 0.66 ‰ per year is only the additional C flux resulting from soil redistribution (which should be only slightly affected by potential errors in absolute SOC stocks), we still believe that mentioning quantitative results for the whole catchment is reasonable and informative for the reader.

Reviewer #2 (Anonymous):

Öttl et al. explored the long-term impact of water and tillage erosion on landscape-scale carbon dynamics based on a modified version of the spatially explicit soil redistribution and carbon (C) turnover model SPEROS-C, Moreover, the model parameterisation uncertainty was estimated. The results indicate that in young moraine areas, SOC patterns and dynamics are substantially affected by tillage-induced soil redistribution processes, and it was estimated that after 1000 years of arable land use, SOC redistribution by tillage and water erosion results in a landscape-scale C sink of up to 0.66‰ per year. The MS is well-written and should be of interest to readers of SOIL. I have two suggestions that may improve the manuscript.

We thank the reviewer for these supportive comments.

First, more details should be provided in Methods, so the readers are easily able to understand the paper, e.g., how the data was obtained and the models constructed.

We carefully revised the methods and added more details where needed, e.g. a table with all model realisations and scenarios was added (Table 1 after line 179), the explanation of carbon turnover was extended (lines 166ff), and the absence of topography update was included in the methods section (lines 170f).

Second, in Figure 6, while a significant correlation was observed, however, there are still spaces to improve prediction accuracy (R2). Moreover, if the data is suitable for regression analysis? e.g., if the residuals of the regression line are normally distributed?

[In the revised manuscript, Figure 6 is now Figure 7.]

It is not our goal to improve the prediction accuracy, but to check the plausibility of the model results that is illustrated by Figure 7. The R^2 in Figure 7 show that spatial patterns of the results are in agreement with observations, i.e. the modelling approach results in low (high) topsoil SOC values at landscape positions where we also find low (high) observed topsoil SOC. We chose two agricultural fields for which recent SOC observation data was available and subsequently modelled 1000 years of soil redistribution and SOC dynamics. These two fields have an individual management and soil redistribution history that remains widely unknown as we just have general information about the history of land use in this region. Hence, a sound prediction of spatial patterns that we can observe in the landscape is from our point of view very satisfying with respect to a modelling period of 1000 years. The relative error of the simulated SOC stocks for model realisation R4 is given in lines 380ff, where we mention the underestimation issue, though the predicted values are well within the range of the observed values (mean relative error of 40% and 20% for test sites A and B, respectively). To show the direction of the relation of modelled and observed topsoil SOC values, we added the Spearman's rank correlation coefficient for realisation R4 (lines 362f):

> *"This realisation resulted in a Spearman's rank correlation coefficient of 0.83 and 0.82 for test site A and B, respectively."*

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