1) Methodology

When considering longer time-scales, processes other than erosion start affecting the carbon and sediment distribution. One important process is carbon uptake. However, this is not mentioned in the paper. Please reconsider the potential effect of carbon uptake processes and include them, or elaborate on why leaving out is permitted.

It is correct that we do not considered C turnover in this study. We focus on the longer-term dynamics of sediment, clay and soil organic carbon delivery from micro-catchments. Although we agree that there is a coupling between erosion and carbon turnover processes, it is very difficult to parameterize a model to represent this accurately. Under the absence of observations and data to constrain such a coupled model at the scales under consideration here, we think that our assumption of a spatially and temporally constant C turnover is reasonable. In order to make this clear for the reader, we have added the following sentence to section 2.3:

"Hence, SOC is represented in different particle classes but it should be noted that the model assumes a constant C stock that is spatially and temporally constant and therefore does not account for C turnover processes."

In the discussion you mention that underestimation of the breakdown of aggregates might have led to underestimation of the clay enrichment ratio (P.9, l.31-33 P.10, l.1). However, the Methods section does not mention how this breakdown is modelled. Please add a section where you explain how the model handles (micro and macro-) aggregates, the ratio between loose sediments and aggregates, incorporation of OM in aggregates and how aggregate breakdown is organized.

The model does not explicitly represent the breakdown of aggregates during detachment and transport in a physical manner. There is no observational basis to accurately parameterize a model at the spatial and temporal scales considered in this study. Instead, the model uses two end-members when considering sediment particle size distributions, i.e. an aggregated state vs primary particles. These two end-members are derived from direct measurements: i.e. from laser diffraction measurements of Beuselinck et al. 1999. The laser diffraction measurements of Beuselinck et al. (1999) represent the grain size distribution difference between fully dispersed and non-dispersed soil. The erosion process controls whether primary particles or aggregated soil is transported by interrill or rill erosion, respectively. We take from the comment of the reviewer that this was not clear. We therefore added additional information on the representation of the grain size distributions (primary particle vs aggregates). We now highlight that SOC is associated to the clay fraction of both primary particles and aggregated soil (or POM) and removed the terms micro- and macro aggregates: Section 2.2 "To represent the amount of primary particles vs. soil aggregates of suspended sediments, the model interpolates the settling velocity for each particle class and grid cell according to the proportion of particles detached by interrill or rill erosion."

Furthermore, we have rewritten the scenario definition to make it easier and clearer for the reader.

Section 2.3 "Therefore, we considered the particle size distributions of both aggregated soil and primary particles in our simulations. We considered two erosion scenarios: Erosion scenario 1, in which both detachment by rill and interrill erosion leads to complete aggregate breakdown and soil is *transported and deposited following the settling velocity classes of primary particles (Fig. 2). Furthermore, POM is represented as a single particle class with a density lower than that of water. In erosion scenario 2, interrill erosion still breaks down aggregates and transports primary particles. In contrast, detachment by rill erosion does not lead to aggregate breakdown and entrains aggregated soil, following the settling velocity classes of aggregated soil (Fig. 2). Moreover, POM is assumed to be encapsulated in soil aggregates and is not treated as an individual class. Following detachment, the model simulates the transport and deposition of primary particles or aggregated soil based on the erosion type of detachment that they underwent."*

Section 2.3 "In terms of simplicity, and given the constraints imposed by the model structure, we considered two types of SOC for both primary particles and aggregate soil: (i) mineral-bound SOC, which represents 90% of the total and is associated with the finest sediment class (< 2 µm) and (ii) a POM fraction, which represents 10% of total SOC and is considered a separate class in the model, with a particle size of 250 µm and a density of 1000 kg m^{-3}. Hence, SOC is represented in different particle classes but the model does not account for C turnover processes."

The model erases the network of rills and ephemeral gullies by tillage operations (P.6, l.19-21). However, the paper doesn't mention that tillage also induces erosion, although that can be expected in an area with steep, convex slopes. Is this incorporated, and how does this influence the results and comparison with the other catchments?

We fully agree with the referee that tillage erosion is an important erosion process affecting soil and SOC redistribution. However, our paper focusses on the long-term effects of long series of water erosion events and the effect on sediment and carbon delivery. Tillage erosion is only taken into account as it directly effects the hydrological connectivity within the catchment by erasing a potential rill network. To make this clearer in the text, we introduced the sentence " Apart from removing rills, tillage erosion is not taken into account." to section 2.3.

Topographical properties like slope and catchment area have a major influence on erosion properties. Small differences in these parameters can result in different enrichment ratios and erosion patterns. Therefore, I would like to see more information on the topographical and soil properties (e.g. area, slope) of the two monitored catchments which were used for model evaluation (P.7, l.22-25). You can present them in a Table, which also includes the properties of the modelled catchment.

We will add the recommended information to the paper:

	Study area	Kinderveld	Ganspoel
Area [ha]	3	250	117
Elevation [m]	12	61	39
Mean slope [°]	4.4	3.8	3.4
Arable [%]	100	80.5	76.9
Forest & pasture [%]	0	16.7	9.0
Other [%]	0	2.8	14.1
Clay [%]	14	7-18	
Silt [%]	83	70-80	

You aggregate run-off events on a monthly basis and use these monthly recurrence intervals for the frequency analysis (Sect. 2.5). However, I'm concerned that with a monthly aggregation, you average out too much of the extremes and therefore too much of the erosion is attributed to smaller events. I would advise to use a smaller time-step, like 1 or 2 weeks, if the length of the longest events permit that, or add the reason why you use a monthly average. Next to that, in Sect. 3.4 you speak of events and event frequency, while you explicitly say that you will use the monthly recurrence intervals (P.8, l.1-2). Please correct this inconsistency.

We see the point and will follow the argument of the referee and calculate the event based recurrence interval. Therefore, we use the definition of a rainfall event also for erosion events. Hence, some events may have multiple hydrographs, which is pointed out in the text.

Section 2.1 "A rainfall-runoff event is identified as a period (i) in which rainfall depth exceeds 2 mm in 24 h (<1% of total runoff excluded) and (ii) which is separated by at least 72 h without rainfall. Accordingly, a rainfall-runoff event is not necessarily defined by a single hydrograph, but might contain multiple runoff peaks."

Furthermore, we will shorten the description of section 2.5:

"For an analysis of event based recurrence intervals, we follow the rainfall event definition given in section 2.1 (72 h window). Thereby, some events may contain multiple runoff peaks."

The values of section 3.5 will be updated but do not show substantial differences.

Fig. 3 is used to illustrate sediment and carbon fractionation. However, it is not clear which part of the Figure is used for this fractionation. This makes it difficult to understand what the Figure shows and how it is used in your paper. More explanation in the text or omitting an unused part of the Figure would increase its quality.

Yes, we agree. The Figure supports the basic setup but is only partially implemented to the model. After consideration, we think the Figure might confuse the reader and will therefore be removed.

2) Results and Discussion

The selective redistribution of carbon and sediments leads to depletion and enrichment at certain locations. These spatial patterns are in my mind one of the aspects of the objective to "improve our mechanistic understanding of sediment redistribution and carbon delivery" (P.3, l.29) and are useful for understanding variation in hydraulic properties and soil fertility. Therefore, I would like to see some maps of clay and carbon redistribution in the study area and a discussion on what the consequences are of this redistribution.

We introduced some methodological clarification in Section 2.2. "The MCST model keeps track of spatio-temporal changes in particle size distribution of the eroded and deposited topsoil sediment within 10 different size fractions. However, the particle size distribution of the soil surface is spatially homogenous and constant throughout the 100 yrs. modelling period."

We did not add maps of clay and carbon contents as we found this confusing for the following reasons: Changes in proportions of clay and carbon content need to be related to topsoil (or soil in plough *layer). Hence, any substantial change can only be recognized in depositional areas, where the proportion of deposited material is relatively large compared to the entire plough or topsoil layer. In contrast, areas with interrill erosion, which may undergo substantial depletion of fines due to selective erosion, would show only very minor effects when enrichment/depletion is related to the topsoil or plough layer. However, to illustrate the pattern of erosion and deposition in general we added a map of total erosion and deposition in section 3.2.*

Most references in this chapter only support your results or methodology. However, I am missing references supporting your interpretation of the results. Please complement those sections with more references. Examples are the role of event size (P.8, l.26-29) and selective uptake by interrill erosion and unselective uptake by rill erosion (P.9, l. 13-15).

The following paragraphs will be added:

Section 3.2 "This reflects the role of event size, whereby only larger events produce significant amounts of concentrated erosion once the hydraulic threshold for rill initiation is exceeded. This large contribution of rill erosion for sediment delivery was also observed by Wang et al. 2010 in the Kinderveld and Ganspoel catchment. In a modelling study, Wilken et al. (2016) tested the effect of different rill initiation chracteristics on carbon delivery in a catchment of similar loess derived soils. The results showed that rill erosion widely controls sediment and carbon delivery in catchments with high connectivity."

Section 3.3 "The concept of particle size-selective interrill and non-selective rill erosion which detaches and entrains the entire soil matrix was documented in numerous studies (Kuhn et al., 2010, Polyakov and Lal, 2004, Quinton et al., 2001, Schiettecatte et al. 2008a). Following non-selective splash erosion (Poesen and Savat, 1980, Poesen, 1985, Parsons et al., 1991), selectivity is caused by particle size specific deposition differences, where coarser and heavier particles settle out earlier than finer and lighter particles (Schiettecatte et al. 2008b)."

You mention that "the Hairsine-Rose model provides accurate physically based description of sediment transport and deposition for multiple sediment classes that differ in terms of settling velocities" (P.5, l.4-5). However, this contradicts with P.9, l.19-20: "The Hairsine-Rose theory does not appropriately predict the depositional behaviour of the fine fractions". Please consider this disadvantage in the introduction.

We have to thank referee #1 to point us this weak argument. After an extended discussion among the authors we came to the conclusion that our arguments, why the depletion of fines in the deposits are slightly overestimated, are all somewhat speculative. Therefore, we omitted the possible (but speculative) explanations from the text.

P.5, l.10-11: unclear what the α-parameter does. Can you provide a better explanation?

Yes, this was hard to understand. We hope reformulating this sentence helps to understand the α-parameter: "α_i is the ratio of the sediment class concentration of flow related to the local sediment class concentration of the parent material"

P.7, l.13-14: “the POM fraction was enriched in SOC relative to the bulk soil...”.
Unclear what you mean by that.
We will shorten this paragraph and remove this sentence in the text.

The definition of sediment delivery ratio is given on P.10, l.21. However, the term is used earlier in the paper (P.3, l.10;P.8, l.25-26). Please move the definition to one of those parts.
Thank you, we will move the explanation to P.8, l.25-26.

P.11, l.29-30: “Moreover, the episodic nature of soil organic carbon redistribution is particularly important when considering the effects of SOC input to surface water bodies”. Why is this so important and how do you conclude this from your research?
Yes, this opens a complete new debate. We will remove this sentence.

Table 1: Include a column with the symbols as they are used in the text. I also suggest to add all other model parameters and inputs (e.g. soil texture) for a complete overview.
We will extend the Table accordingly.

Symbol	Description	Unit	Range/value
Static parameters			
	Sediment density	kg m^{-3}	2600
δ	Aggregate density	kg m^{-3}	1300
	Particulate organic matter density	kg m^{-3}	1000
ρ	Water density	kg m^{-3}	1000
Ω_{cr}	Threshold of re-entrainment	W m^{-2}	0.6
g	gravity	m s^{-2}	9.81
v_{si}	settling velocity for class i	m s^{-1}	2.6 10^{-7} - 5.0 10^{-3}
Dynamic parameters			
R_i	excess rainfall at hyetograph at time step i	mm	
$P_{i,cum}$	cumulative excess rainfall during of past 24 h	mm	
$I_{a,cum}$	initial abstraction	mm	
$F_{a,cum}$	continuing abstraction	mm	
d_i	mass rate of deposition for class i	kg s^{-1} m^{-2}	
r_{ri}	rate of sediment re-entrainment for class i	kg s^{-1} m^{-2}	
C_i	mean sediment concentration for class i	kg m^{-3}	
Ω	stream power	W m^{-2}	
D	depth of water flow	m	
M_{di}	sediment mass of deposited layer for class i	kg m^{-2}	
M_{dt}	total sediment mass of deposited layer	kg m^{-2}	
D_r	rill detachment rate	kg m^{-2} s^{-1}	
D_{ir}	interrill sediment transport to the rill	kg m^{-2} s^{-1}	
Q	rill discharge	m^3 s^{-1}	
I	maximum 10 min rainfall intensity	mm h	
α_i	sediment:parent-material ratio for class i	-	
F	stream power fraction for re-entrainment	-	
H	shielding by deposits	-	
a	rill erodibility factor	-	
b	interrill erodibility factor	-	
S	local slope gradient	-	
Sf	slope factor	-	

Fig. 2 and Fig. 3: I understood that these Figures came from other research. Please add a reference in the caption to show that the work was not carried out by you.

Of course. Thank you for this comment.

Check the Methods section for past tense (e.g. P.7, l.22: “We evaluate the. . .”, but also P.9, l.2: “assumptions . . . are made” and P.1, l.18: “we apply an ...”)

Thank you! We will correct this.

Technical corrections

P.4, l.27: “in each raster cell”. Change to: “for each raster cell”

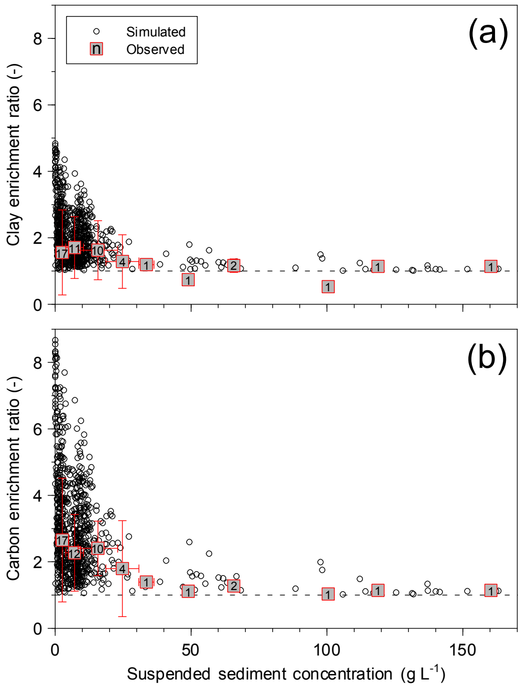
Thank you! We will correct this.

P.6, l.15: shouldn’t this be 1898-1997 in order to reach the 100 years?

This would have been a really unpleasant typing error. Thank you!

P.19, l.6: The carbon enrichment ratios displayed in Fig. 5 are between 1 and 8. Not between 1 and 9, as is mentioned in the text.

The text was correct. There was a mistake in the displayed y-axis and will be corrected (see below).



P.14, l.5. Reference is not in alphabetical order

We will correct this.