Discussion letter

Report # 3 (Referee #1).

Thank you for the evaluation of our manuscript. In response to your suggestions, we have modified figure 4 and have used 1-SD for the input uncertainty.

Report #4 (Referee # 4)

a) We have modified the abstract and use the term "erosion-induced" sink or source.
b) Figure 3 and figure 4 are consistent, the only difference is that figure 4 uses a log scale to emphasize the dynamics in the early phases, while figure 3 uses a linear scale.
c) Line 110: thank you for spotting this type. This is now corrected
d) We fully agree with this statement and we discuss this in depth in the discussion (lines 167-170) *"…but large uncertainties remain. In particular, the outgassing of OC in burial sites is poorly constrained (Table 2 and Fig 4). It is also important to note that the available estimates are strongly biased towards high-input agricultural systems in humid/temperate settings with deep fertile soils developed on sedimentary substrates and thus more data on low-input systems on marginal lands and drylands are urgently needed.* In order to emphasize this important remark, we have added a cautionary statement on line 128: "*This is most likely due to a small fraction (i.e. < 10%) of NPP is removed by erosion (Berhe et al., 2008) and that the available observations are biased towards fertile soils in high-input systems (see discussion*). We hope that this addition addresses the concerns raised by the reviewer.

Report # 1 (Referee #3)

The authors suggest a set of interacting processes in soil erosion and transport that supposedly reconciles the conflicting results on the impact of soil erosion on the global Carbon cycle. Their attempt at such reconciliation confirms earlier studies they have published that came to the conclusion that soil erosion represents a small Carbon sink in the current global Carbon cycle. While I agree with the authors that the conflicting results of studies on soil erosion and atmospheric Carbon have to be reconciled, I find their conclusion very much forced into the direction of their earlier work. This leads to several major flaws in their arguments.

Thank you for this assessment. Our study does not report a strong conclusion in the direction of either a source or a sink, but rather presents a framework that reconciles the opposing views.

First and foremost, the current sink identified by the authors is a result of the combined C uptake at the site of erosion, the so-called dynamic replacement, and the burial of some of the eroded C in oceans after the sediment is associated with has moved from the site of erosion to the site of permanent burial, which is likely to take years to decades. This calculation has a major problem because it relates the C that is currently buried in permanent stable sinks, i.e. eroded some time ago, to current rates of soil C uptake. This assumption is problematic because the current C uptake could be greater than at the time of erosion some decades ago because of fertilisation and associated *increase of productivity, litter input and C sequestration. Further effects, such as CO2 fertilisation and prolonged growing seasons could also cause greater C-uptake than only decades ago.*

The framework presented in this study is based on a time-integrated analysis of observational data and presents the cumulative response since the start of the erosional disturbance. As a result, no assumptions have to be made regarding the timing.

A second problem arises from the qualitative nature of the proposed reconciliation. Most soil that is eroded is not moving in a continuous manner from site of erosion to an ocean deposition. In fact, sediment loads of major rivers with hillslope erosion rates in their catchments, they found that the amount of sediment moving from land to ocean through rivers is only 10% of the amount of soil that is eroded in their catchments. This leaves a large amount of eroded sediment stored in shallow sinks where the organic matter may be preserved or mineralised before the sediment may be remobilised again. Major gaps exist in our understanding of this C pool: current C concentrations in these shallow sinks are often lower than those of the soils that are supplying them with sediment and organic matter. This can be attributed to either lower source soil organic matter content in the past, such as on poorly-fertilised cropland before the advent of artificial fertilizer, or high rates of mineralisation during transport and after deposition, such as after land cover change from forest or pasture to cropland. Explaining the current content is therefore limited by an equation that consists of three variables (C content of original soil - loss during transport and after deposition = current C content of shallow sink) of which two are mostly not determined in the studies cited by van Oost and Six.

The mineralization during and after transport are explicitly represented in our study. We do agree that not all variables are known, but in most cases a space for time substitution (a widely used method in geomorphology and soil science) is used to provide quantitative estimates.

A third problem of the assumptions of van Oost and Six arises from an oversimplified perspective on the effects of erosion and transport on sediment C content. Erosion can occur by either non-concentrated or concentrated flow. Non-concentrated flow often generates sediment that is enriched in organic matter because it is associated with the smaller and lighter particles that such flow is capable of entraining. Likely being suspended sediment that moves to the ocean, or at least into a large floodplain, such organic matter enriched sediment potentially leads to an overestimation of the actual amount of eroded C and replaced at eroding sites, at least when soil organic matter and some general loss during transport assumptions are applied such as by van Oost and Six instead of a full mass balance that distinguishes between C and mineral sediment. Transport of eroded organic matter from sites of erosion to watercourses and eventually to oceans is further complicated by selective deposition in landscape sinks. This can work in two ways: an enrichment of Carbon in sediment moving through a catchment occurs when the sediment is poorly sorted and large, e.g. sand-sized particles containing little organic matter settle, while finer material with greater organic matter content moves on. This can lead to a similar overestimation of eroded soil C than selective erosion. However, the opposite can also happen: on well-structured soils sediment often consists of aggregates and the larger ones often have a greater organic matter content than the smaller ones, but are also more likely to be deposited in landscape sinks, leading to an underestimation of eroded soil organic matter when studying C-concentrations further downslope in a catchment. Ignoring the impact of transport on sediment C content when eroded soil moves along a catchment therefore leaves a high uncertainty in the assessment of the C flux from eroded sediment into the atmosphere.

We agree and we have added a section in the discussion to highlight this potential source of uncertainty: Line 170: *"In particular, the outgassing of OC in burial sites (Table 2 and Fig 4) and the effects of selective erosion and deposition are poorly constrained (Doetterl et al., 2019)."*

One could argue that over long-enough time and large-enough space the shortcomings of a quantitative analysis of the actual movement and fate of eroded soil organic matter even out, leading *to the reconciliation van Oost and Six argue, and an erosion-induced Carbon sink. However, the empiric evidence for such a reconciliation is simply missing from their manuscript. They actually concede that their argument is largely based on papers from temperate cropland. This means that the interaction of erosion, transport, sedimentation, C-sequestration and mineralisation they postulate to cause a small Carbon sink has been derived from only about a third of agricultural land of Earth and hardly includes rangelands. Important biogeochemical differences between cropland and rangeland Carbon cycles are therefore not considered in their letter, rendering their reconciliation empirically highly unfounded. The cropland-rangeland differences include the high degree of land degradation in rangelands, with possibly a past peak of Carbon erosion when overgrazing triggered erosion. As a result of limiting rainfall and/or low temperatures, soil formation and biomass production, dynamic replacement of soil organic matter, is much slower than on temperate, often heavily fertilised cropland. Therefore, the balance between C erosion and uptake observed on cropland does not apply to rangelands. With these differences in mind, one could also propose that the current rates of C-uptake and export of eroded C to oceans do not indicate a sink in a steady-state balance, but are the result of higher soil C in the past and the time it takes to get this sediment to a permanent ocean sink. Furthermore, because of a lower degree of hydrologic connectivity of drylands compared to temperate croplands, the deposition of C in shallow landscape sinks can be assumed to consist of a larger proportion of the eroded soil C than in temperate climates where the high connectivity moves more sediment faster through a catchment. The effect of such prolonged residence in shallow sinks remains unknown. In other words, if there were more studies from rangelands, an attempt at reconciling studies carried out at different scales would lead to the opposite conclusion, i.e. soil erosion causes a net flux of C into the atmosphere.*

We are caught between a rock and a hard place. Our paper is the first study to collate and synthesize all available data. In our discussion, we also identify that there is a bias with an underrepresentation of tropical and dryland regions. We strongly believe that our assessment and concepts, although it may not be fully representative due to the lack of data, is informative and inform future discussions. We report that the literature is biased towards temperate croplands on three occasions in our manuscript (line 170, line 128 and line 48). We highlight this shortcoming and point towards the need for a more representative study of erosion induced C fluxes. Although our study has limitations, this is clearly identified. Nevertheless, we would like to point to the fact that it is well established that croplands are the main contributor to global soil erosion by water (eg Borelli et al Nat com 2013).

In summary, the attempt at reconciling conflicting results on the impact of soil erosion and the Carbon cycle presented by van Oost and Six (i) ignores the temporal offset of C erosion and permanent sedimentation, in particular the effects of land degradation, the use of artificial fertiliser and climate change on land productivity and C-sequestration at eroded/eroding sites; (ii) does not consider that the overwhelming amount of eroded soil is deposited on terrestrial surfaces; our understanding of the stability of eroded Carbon in the shallow land sink is highly uncertain, which creates a potential for shifting the balance between sequestration, mineralisation and permanent ocean deposition in a way that erosion is actually a source and not a sink in the global Carbon cycle; and finally, (iii) large geographic regions are not represented in the analysis of van Oost and Six, which, by the acknowledgment of the authors, leaves the results of the study limited to temperate cropland; with the differences in the biogeochemistry between cropland and rangeland in mind, the applicability of their reconciliation is limited to less than 50% of the worlds agricultural land. Because of these uncertainties and gaps, the conclusion that soil erosion represents a C sink is scientifically not sound. Some elements of the framework that van Oost and Six present would be feasible to develop research to reconcile the different positions. However, van Oost have hardly reacted to this suggestion after an earlier review, therefore I therefore suggest to reject the paper.

The main point of our work is that the source vs sink behavior can be reconciled, and not that erosion represents a sink or a source. We feel that reconciliation, rather than reiterating the paradox as done in other studies, is novel. The latter is also emphasized by the three other reviewers. We do not conclude that erosion represents a C sink:

Lines 160-168: *"Considering all these processes reconciles the apparent soil OC erosion paradox by showing that both major source and sink terms for atmospheric OC are simultaneously induced by water erosion. The contrasting views that water erosion represents a large sink or a source originate from a partial analysis and an incomplete consideration of the underlying processes that occur at vastly different spatial and temporal scales. When a comprehensive analysis is done by considering the complete trajectory of eroded OC (i.e. the LOAC) at the appropriate timescales, the available evidence indicates that the sink and source terms are in the same order of magnitude. This implies that the assertations of a very large effect of agricultural erosion on the global OC budget, with a net OC flux of up to 1 to 2 Pg OC yr^{-1} (Berhe et al., 2007; Lal, 2004; Smith et al., 2005) are inconsistent with integrative assessments."*