Biogeosciences Discussions

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Reply on RC3

Kristof Van Oost and Johan Six

Author comment on "Reconciling the paradox of soil organic carbon erosion by water" by Kristof Van Oost and Johan Six, Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-1-AC3, 2022

The manuscript submitted by Kristopf van Oost attempts to be a review of the state of the research on the role of soil erosion for the global Carbon cycle. Depending on the study, erosion is seen as either a source or a sink of organic Carbon. Kristof van Oost and Johan Six argue, as in most of their previous work in the past 20 years, that soil erosion moves Carbon from the atmosphere into long-term geologic sinks.

I have reviewed a manuscript by the two authors with the same title for another journal approximately a year ago. Apparently, the manuscript has been rejected by that journal. Comparing the two manuscripts reveals no major changes in both argument and literature.

// We strongly disagree with this statement. This study was indeed submitted to another journal, but the decision was a major revision, not a rejection. The editor of that journal also suggested to rework our paper as an original research paper. As also identified by reviewer 1 and 2, we see our work as a perspective that reconciles the opposing views and should serve as a starting point for future discussions on this topic. Hence, in the end, we felt that the BG Letters format was a much better outlet for our perspective than the original research paper format requested by the editor of the other journal. We also like to highlight that the paper has been substantially revised (based on the reviews received from the previous journal) for BG letters with much more observational data (colluvial and alluvial) and a conceptual framework that links the different space and time scales. We are of the opinion that these major changes have substantially improved the manuscript and are in that sense grateful for the reviewers comments received from the original journal.

The key conclusion of this manuscript, as in the other publications by the authors on the topic is that the uptake, or dynamic replacement, of atmospheric Carbon at sites of erosion compensates for a part of the Carbon loss caused by erosion. In additon, eroded Carbon is deposited in long-term permanent sinks, leaving a negative net balance for atmospheric Carbon caused by erosion. Since many field scale studies show a major negatie impact of erosion on soil Carbon, the sink caused by dynamic and deposition in long-term sinks has been questioned. Kristof van Oost and Johan Six argue that the negative impact observed in field-scale and process studies does is balanced when taking a large-scale, long-term perspectve. There are three key problems with this argument.

First, soil erosion rates are poorly constrained on a global scale. In their contribution to

Nature Communications, Borelli et al. (2017, DOI: 10.1038/s41467-017-02142-7) showed that an increase of the resolution in their global scale erosion model by reducing raster cell sizes to 250 m reduced the estimated global erosion rate approximately by half. This would imply that also only half of the soil Carbon is eroded than previous models suggested, which in turn significantly reduces the potential for Carbon uptake at the sites of erosion. The number of studies currently published on improving the representation of topography in erosion (e.g. Panagos et al. 2015 10.3390/geosciences5020117, Schmidt et al. 2019 doi.org/10.1016/j.mex.2019.01.004) supports the position that the quality of Carbon flux modelling for regional to global scales currently is still poor.

// The suggestion that initial estimates of global soil erosion are most likely overestimates has been around for more than a decade now (eg Quinton et al NGS 2010). Our paper focusses on processes and the reconciliation of the opposing views in the context of space-time scales. As such, the absolute magnitude of agricultural soil erosion is not the topic of our paper. When accepting the revised global estimates (of c. 20-40 Pg of soil), organic carbon fluxes associated with soil erosion are still very high and of relevance for the global C budget. Furthermore, the papers cited by the reviewer only consider interrill and to some extent rill erosion because they are based on the RUSLE model. This implies that other erosion processes such as gullying, tillage erosion, harvest erosion etc are not considered yet and thus most likely underestimate erosion rates.

A second problem arises from the lack of a geographically comprehensive data set on the actual impact of erosion on soil Carbon. The lack of reliable data on soil Carbon, especially from rangelands, has been acknowledged in many studies, including a 2014 paper in Nature that was co-authored by Johan Six (Pittelcow et al. doi.org/10.1038/nature13809) where the authors admit that the data on soil Carbon and from large parts of the planet are poor, mostly concentrated on European and American cropland, and thus the assessment of impacts of farming practices on soil organic matter are highly uncertain for most of Earth's agricultural land. The final major uncertainty in the argument for an erosion-iduced C sink is the lack of data on the past soil and sediment organic matter content. Kristof van Oost and Johan Six argue that over long periods of time and large spatial scales, the sink effect dominates. To my knowledge, there is no source-to-sink study on a higher order catchment scale that traces eroded soil organic from slope to ocean, nor has this been attempted for the past. Individual sink reconstructions exist, but they lack information on original soil C source which has been eroded or at non-eroding sites, been modified by land use. This leaves the balance Kristof van Oost and Johan Six want to solve with more than one unknown.

// 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. Furthermore, we will include, based on the comment of reviewer 1, an uncertainty analysis in our revised manuscript. At the end, this is the main objective of our work. Secondly, there are source-to-sink studies that also include higher order catchments and these are included in our study (eg. Stallard 1998, Dymond 2010, Worrall 2016, Wang 2017 …).

In the light of these uncertainties in the data on Carbon erosion and depostion in space and time, the conclusions drawn by Kristof van Oost and Johan Six appear biased towards the Carbon sink argument. It is also not new, Sandermann and Berhe already made a similar argument in 2017 in their paper on The soil carbon erosion paradox in Nature Geoscience (10.1038/nclimate3281), also referring to Wang et al. (2017) and Chapell et al. (2016). This leaves the key statements of the manuscript presented by Kristof van

Oost neither novel nor substantiated by new or more reliable data.

// 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 and is also emphasizeded by reviewer 1 and especially reviewer 2).

Furthermore, the small size of the potential C-sink induced by soil erosion has been accepted in the scientific literature for about 15 years (e.g. Berhe et al. 2007 doi.org/10.1641/B570408) and the IPCC has followed this argument in its reports on climate change. This leaves the discussion on the impact of erosion on the global Carbon cycle with a small effect, but a large uncertainty. A review should therefore in my mind point out the uncertainties and identify the research needs, rather than developing a conclusion.

// We agree, we will revise our conclusion by avoiding to identify erosion as a source or sink and focus on the implications of our reconciliation framework.

I therefore cannot help but think that this review, in particular the submission of a previously rejected manuscript to a different journal, is an attempt to preserve the legacy of the previous research of the authors rather than being open to the arguments made by reviewers. I therefore suggest to reject the paper.

// As indicated above, this assessment is ungrounded: Firstly, the manuscript was not rejected. Secondly, we have revised our manuscript based on the constructive comments of reviewer input (new data and a conceptual apporach that provides a framework to align past research across spatial and temporal scales). Again, we strongly feel that the BG letter format is much more appropriate for a perspective paper as opposed to an original research paper format that was suggested by the editor of that previous journal.