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Comment on bg-2022-1

Adrian Chappell (Referee)

Referee comment on "Reconciling the paradox of soil organic carbon erosion by water" by
Kristof Van Oost and Johan Six, Biogeosciences Discuss.,
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Generally, a referee comment should be structured as follows: an initial paragraph or section evaluating the overall quality of the preprint ("general comments"), followed by a section addressing individual scientific questions/issues ("specific comments"), and by a compact listing of purely technical corrections at the very end ("technical corrections": typing errors, etc.).

As requested, I have followed the above requirements of the review in the following text.

General comments

The article attempts to do exactly as the title describes, to reconcile potentially competing perspectives on soil carbon erosion. The reconciliation is undertaken within the framework of scale which is used by the authors to demonstrate how these competing perspectives can exist at the same time and hence explain the paradox. I think this work contrasts markedly from the vast majority on this topic and many others in environmental science. The characteristics of that majority is typically atomised, perhaps even siloed, with a single perspective which is much easier to write, much easier for reviewers to understand and therefore readily published. Consequently, I congratulate the authors on this sophisticated integrated approach which is difficult to undertake and explain. The benefits of such a sophisticated approach are evident in the work, we have a proposal for understanding difference in perspective which enables the potential for the soil erosion community to re-gather momentum around the idea. I think the work is valid, straight-forward and effective which from my perspective equates to the work being incisive. On these bases I think the work should be published to act as a catalyst for further discussion on the topic. I have included below in the next section some specific comments which could form the basis for that discussion, would need clarification in the manuscript, but which I feel do not preclude the publication of this work.

Specific comments

There is an implicit assumption by many researchers working on soil erosion that the processes are dominated by water erosion. This is of course not the case in the vast nearly 50% of the Earth's land surface dominated by drylands where magnitude and frequency of wind erosion and dust emission very likely outweigh the influence of water erosion. Consequently, I would like to see improved clarification of the specific processes that are being considered throughout this manuscript. For example, starting with the title, should it read something like: "Reconciling the paradox of soil carbon erosion by water". The first sentence of the abstract perhaps should more precisely be "The acceleration of erosion, transport and burial of soil organic carbon (C) by water in response to....". Clarifications of this type throughout the manuscript, I think will serve to remind readers that much of the current thinking about SOC erosion is dominated by humid / temperate experience and measurements. Whilst the processes may be universal (notwithstanding a difference in fluid viscosity) the outcomes may be very different in relatively dryland regions. The authors might even like to include in their manuscript a statement that the paradox is only understood to occur in humid-temperate regions because there is far less work / understanding on this topic in dryland regions. The point I raise is perhaps best exemplified at Line 77 "On eroding hillslopes, soils are truncated, and C depleted subsoil material is brought to the surface layers." In drylands, I think soils may not be truncated and the subsoil may not be C depleted. The implication of this difference is that in drylands, soil erosion may be a limiting factor in the balance between SOC decomposition and SOC redistribution. This thinking is already included in the Section on C recovery and evident in the text around Lines 100-110. However, it is not clear how or indeed whether drylands are included in the universal nature of the description, whether wind erosion and dust emission are a special case, or are not included. I have no problem with the authors simply clarifying the scope of the manuscript and not extending in to these larger issues, unless of course they are already included and just not explicit. In which case, I think there is a need to clarify on that basis.

The points above about soil carbon erosion in drylands raise the need to consider an additional clarification. There is only one mention (in the abstract) of the word organic linked to the words carbon erosion. I think the focus on soil organic carbon (SOC) erosion should be made clear (like the point above about water erosion), in the title and throughout the manuscript as appropriate. I think this is important so that the focus on SOC erosion is distinguished from soil inorganic carbon (SIC) erosion. The SIC cycling and erosion processes are prevalent in dryland regions but not widely recognised / connected in the literature on soil erosion. Consequently, it is not clear from the manuscript whether / how SIC processes should be considered in the paradox.

The geography of SOC erosion demonstrates the overlap particularly in semi-arid regions of wind and water erosion processes. The significance of that interplay between wind and water erosion is its redistribution and difference in the sink of SOC. Wind erosion and particularly dust emission releases SOC in to the atmosphere and may transport SOC large distances from source, potentially influencing ocean carbon cycling. The main focus in the manuscript and the paradox, is the redistribution of SOC by water which is for a given erosion event relatively localised. Furthermore, there appears to be an implicit assumption that water erosion is dominant even in regions well-known to be influenced by wind erosion and dust emission. The question remains in my mind whether these differences influence the source-sink paradox. I recognise that this issue is beyond the

scope of this manuscript. As in the previous paragraphs, I think there is a need in this manuscript to clarify the scope of the SOC erosion paradox described and perhaps even include a statement that defines clearly the focus. The impact of these clarifications will I think be the broader recognition that the geomorphic conveyor is beyond water and consequently there may then be much broader recognition of the source-sink across domains. I note that some clarity already exists e.g., Section 2 is entitled Transport in runoff and rivers. However, the preceding section is written in a way which gives me the impression that the commentary is universally applicable. However, I think we are a little way from that knowledge and understanding from across wind and water erosion and from drylands being combined.

The next point I have to make is a little tricky since it is not directly evident in the literature. Nevertheless, it is relatively easy to appreciate even if one does not accept it. Some of the C recovery section in the manuscript is based on the relation between net primary productivity (NPP) and SOC erosion. Whilst NPP is an important concept, it is grounded / implemented by the use of leaf area index underpinned by reflectance-based vegetation indices. The vegetation indices describe greenness which is due but cannot readily be assigned to dual signals of plant health and / or plant coverage. Consequently, if e.g., plant coverage changes as is partially evident in satellite measurements of global 'greening', then it is very difficult to distinguish plant coverage from plant productivity. Incorrect attribution of greening to one or the other will introduce Type I and II errors increasing uncertainty about the relation between NPP and C erosion. Although the duality of information contained in NDVI is well known, it has not generally been troubling because of the endemic assumption of stationarity and in modelling which is intrinsically steady state. However, a changing climate or other underlying changes, now confound the ability to understand plant productivity. So the relevance to this manuscript is that over long time periods underlying change may cause a difference in the response between SOC erosion and plant productivity, where that productivity is assumed stationary by using a contemporary vegetation index framework.

I'm not a great fan of merging a Discussion and a Conclusion. I wonder if what is provided in the labelling of that section of the manuscript is strictly neither of those, but is something more akin to 'The implications of...'. I think many of the clarifications and issues raised here could usefully be included in that section to encourage workers to consider the implications from various perspectives.

Again, congratulations on putting together such a sophisticated and well-considered commentary. I believe and hope that it will act as an important catalyst for broad considerations of the C erosion paradox.

Best wishes,

Adrian Chappell