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Biogeosciences Discussions

Interactive comment on "Global soil organic carbon removal by water erosion under climate change and land use change during 1850–2005 AD" *by* Victoria Naipal et al.

Anonymous Referee #1

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General Comments

Naipal et al., present an interesting study that quantified soil organic carbon erosion loss at the global scale from 1850 to 2005. The soil erosion processes are largely underrepresented in current generation earth system land models. I believe this study is making a good step forward, towards better modeling the global carbon cycle. Below are my comments including two major concerns.

Major Comments

1. I was not fully convinced by the vertical discretization approach that the emulator used. First of all, different soil layers have totally different biogeophysical and biogeo-

chemical features. Different layers are experiencing different amount of fresh carbon input (e.g., from fine roots exudates, fine root litter), different microbial community (e.g., fungi/bacteria with different carbon use efficiency), and have different soil structure (e.g., microagregate, macroagregate).

Secondly, even the idea of summarizing the above-mentioned vertical difference into one single factor (re) is believable, the value of re should be carefully inferred for this model, rather than taking from other studies.

Thirdly, and most importantly, the vertical discretization, artificially, increase total global SOC stock by 44%. This type of artifact should be removed. My suggestions is that, since ORCHIDEE has one single soil layer, k0 of ORCHIDEE is supposed to represent the mean turnover rate of the whole soil column. Therefore, the k0 (equation 5) in the emulator (here aims to represent top soil turnover) should not be k0 from ORCHIDEE. One approach is to change k0 in emulator to offset the total SOC stock artifact until it's removed.

2. Land use change map. The LUC is prescribed by PFT fractional change derived from Peng 2017. Wondered how this LUC dataset differs from Land-Use Harmonization (LUH2), the new CMIP6 land use change dataset. Given that LUC is a dominant factor of SOC erosion, I am curious about the uncertainty of SOC erosion, induced by using different LUC estimate (e.g., Peng 2017 vs LUH2).

Specific Comments

L16 The first sentence gives me an incorrect hint that the paper is going to talk about agriculture activity accelerates soil erosion.

L38 1.0 Pg, does it include fire emission?

L56 what is bookkeeping models?

L70 slow rate of carbon sequestration

L85 In order to better constrain

L96 be able to, remove

L179 in this version of ORCHIDEE model

L23 What's the meaning of randomly projected? A more reasonable way is to repeat 1990-1910 climates during 1850-1900.

L351 comapred to that without soil erosion

L421 "Also, intense soil erosion is typically found in mountainous areas where climate variability has significant impacts, while at the same time these regions are usually poor in SOC." It's not clear in the manuscript whether or not ORCHIDEE has topography information? In another word, if ORCHIDEE simulates a low SOC stock over the grid cells that have mountains, is that because of the topographical feature of this gridcell can not hold a lot of SOC in ORCHIDEE? Or because of other reasons such as climate constraints (e.g., colder in mountain area)?

L465 CO2 fertilization effects on NPP is not fully convincing here, because ORCHIDEE does not have nutrient constraints. OCN might be a better surrogate model to be able to say something about CO2 fertilization effect on NPP.

Figure 4. I do not fully understand why climate change either decrease or not change erosion?

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