

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 #2

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General comments:

Naipal and coauthors present an interesting study in which they estimated the potential magnitude of carbon loss from soil erosion globally from 1850 to 2005. Soil erosion is often absent from land-use change assessments and earth system models. However, as the authors show, soil erosion can play a significant role in SOC dynamics at the global scale and must be represented more thoroughly in large-scale models.

Major / Specific comments:

The emulator used in this study seems to have various limitations that make the numbers presented quite uncertain – further discussion on, and quantification of, these

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uncertainties is warranted and would greatly improve this manuscript. Specifically, I would have liked to see additional support for the SOC model formulation, parameters, and built-in feedbacks chosen for the emulator, as well as support for its vertical discretization and parameterization.

The carbon emulator is supposed to describe the carbon pools and fluxes exactly as in ORCHIDEE, yet the total global SOC stocks from the emulator are 44% higher than that of the original ORCHIDEE model. This is a big difference. What does this tell us about the accuracy and applicability of the emulator, and how do the SOC stocks of the two models compare to the Harmonized World Soil Database (HWSD) and other global SOC databases? Additional major comments/questions, especially those regarding the assumptions and methods used, are detailed below.

* L131: What are the limitations of not including these processes in the emulator? Can it capture all feedbacks and dynamics?

* L141-142: "although originally calculated by complex equations, the dynamic evolution of each pool can be described using the first-order model" – why were the complex equations needed initially then? Again, what are the limitations of this first-order model?

* L180: What does the passive pool correspond to (as a measureable pool)? Why is there no transfer from p to s (k_{ps})? Why no input to this pool?

* L190: Does this allow for emergent differences in the relative distribution of the three pools with depth? (e.g., relatively more passive C than active C with depth, etc.)

* L196: "The SOC respiration rates for the topsoil layers are equal to those from ORCHIDEE" but how about subsoil respiration? Does the emulator have more respiration overall then? Please clarify how the models compare.

* L207-208: "total global SOC stock is approximately 44% larger than that from the original ORCHIDEE model" – what does this tell us about the accuracy and applicability

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of the emulator? This seems to be a big difference. How do the SOC stocks of the two compare to the HWSD and other global SOC databases?

* L209-210: How are these fractions determined? What are the implications of the uncertainty in this partitioning?

* L269: Why "randomly projected"? Please explain how and why.

* L290: But you used CRU-NCEP for ORCHIDEE... what are the caveats of using different climate datasets for each model?

* L297: Why this dataset? How does it compare to the HWSD and SoilGrids (Hengl et al. PLoS ONE 2014, 2017) datasets?

* L342: How uncertain are these numbers given the model formulation assumptions, land-use maps, and methods used? It would help to see a sensitivity analysis and some uncertainty ranges.

* L517: (Section 4.4) with all of these model limitations, it would be nice to have a rough quantification of uncertainties.

Minor comments / Technical corrections:

L24: Add abbreviation "(RUSLE)"

L25: "soil removal only" as opposed to what here?

L27: 100 PgC +/- ? This is a potential, but it would be helpful to know the uncertainty of this number based on your methods and datasets used.

L31: 5 PgC +/- ?

L45: This paragraph discusses erosion as a C sink, while the following paragraph discusses C source mechanisms. There could be a more clear transition here or a sentence that sets this up before the two contrasting paragraphs.

L57: Please elaborate a little on "bookkeeping models"

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L59: Although trivial, state "Net Primary Productivity (NPP)" before using just "NPP" in the text.

L68: Drop "to be able"

L75: Change "been applying" to "applied"

L76: Change "different" to "a range of"

L79: Change "While..." to "In contrast, ..." or combine this sentence with the previous one.

L80: Add "Revised Universal Soil Loss Equation (RUSLE)" to this first use of RUSLE in the text.

L112: Change to "(hereafter simply referred to as)"

L119: Be more consistent with PFTs (instead of PFT's) throughout the manuscript.

L144: "based on the stock and output fluxes"

L149: "harvest index (HI)" state this here before the first use of the abbreviation.

L168: "leaves" instead of "leafs" and be consistent with "below-ground" vs. "below-ground"

L204: How does this compare with Hicks Pries et al. Science, 2017 soil respiration with depth?

L202: max, etc. should be defined earlier where it is used in Eqns. 6 and 7.

L243: "such as those covered by"

L257-258: "to preserve the mass balance... and that new substrate..."

L277: Remove extra "the"

L314: Change to "steady-state"

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L318: "CO₂" with a subscript

L322: Change to "In the first simulation, we allowed the climate and land cover to vary through..."

L329: "descriptions"

L331: "changes"

L335: "steady-state condition"

L349: "despite the" remove "of"

L355: Add "100%" or exact % change to the "almost doubled" to allow easier comparison of the numbers.

L388: "slightly improves" but because poor agreement with global SOC stocks to begin with?

L408: Remove "in his study"

L409-410: Ok, there were limitations in that study, but this makes it sound like you didn't have limitations with your methods.

L468: Replace "only" with "alone" (?)

L475: "between a factor of" add "a"

L479: "significant, making" add a comma

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-527>, 2018.