

Biogeosciences Discuss., author comment AC2  
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## Reply on RC2

Eva Kanari et al.

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Author comment on "A robust initialization method for accurate soil organic carbon simulations" by Eva Kanari et al., Biogeosciences Discuss.,  
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**Anonymous Referee 2:** The article is very well written and presents an important piece of research on how Rock-Eval analysis of soil samples can be used (following AI-analysis of measured parameters) to efficiently calibrate a soil organic carbon model. Previous attempts at measuring the SOC pools used in models have not been very successful. By contrast, the present study makes a convincing case for the validity and success of the Rock-Eval & AI approach. I only have a series of minor comments:

**Reply:** We deeply thank anonymous referee 2 for the positive evaluation of our work and for their constructive comments.

**R2:** Line 230 Least square optimization of the AMG model I suppose.

**Reply:** Yes, that is correct. The phrase currently reads "the best fit on observed SOC time series was obtained". We suggest reformulating to: "the best fit **of the AMG model** on observed SOC time series was obtained" to clarify that the optimization was done with the AMG model.

**R2:** Line 232 Do you mean  $^{13}\text{C}$  monitoring data?

**Reply:** Yes, that is correct, we refer to natural abundance  $^{13}\text{C}$  data. We suggest including this clarification in the phrase (instead of its current position in a parenthesis at the end of the sentence) to avoid confusion. "In sites **with C3-C4 vegetation change chronosequences where  $\delta^{13}\text{C}$**  long-term monitoring data were available, the model was adapted to simultaneously match the observed evolution of C, C3 and C4 stocks (Clivot et al., 2019) for a given treatment."

**R2:** Line 265 to 267 Might just be easier to state that you compared  $C_S/C_0$  estimates for measurement based vs model optimized. The same is true in Figure 1. It seems that you

are comparing two different things "Rock-Eval predicted centennially stable SOC proportion" (also refer to as PARTY in other places) vs "AMG optimized  $C_S/C_0$  proportion", while actually you are comparing "measurement based  $C_S/C_0$ " vs. "AMG optimized  $C_S/C_0$ ". In short, to make it clearer, "Rock Eval + PARTY processing" would gain to be consistently referred to as "measurement based".

**Reply:** We appreciate the suggestion and we agree that changing all references from "PARTY<sub>SOC</sub> predicted" and "Rock-Eval-based predictions" to a uniform phrasing would make the manuscript easier to read. We propose using "PARTY<sub>SOC</sub> predicted stable SOC proportion" and "AMG optimized stable SOC proportion" throughout the manuscript.

**R2:** Line 366 Make it clear that you are referring to results from previous studies.

**Reply:** We thank the reviewer for their suggestion and we propose repeating the relevant references at the end of the sentence in line 367 to highlight that we are referring to results from previous studies: "is amongst the best available modelling frameworks of SOC dynamics in European arable land (**Martin et al., 2019; Farina et al., 2021**)."

**R2:** Line 371-373. Do the references at the end of the sentence agree with the statement or, to the contrary, argue in favour of more complex models?

**Reply:** The references used at the end of the sentence are participating in the debate for the most appropriate model structure. Some are in favour of more complex models (Lehmann et al., 2020; Crowther et al., 2019), some argue that a balance between explicit mechanisms and mathematical simplicity is necessary (Shi et al., 2018), while others discuss the power of simple models when novel initialization methods are used (Cécillon et al., 2021a, Dangal et al., 2021, Lee et al., 2020). We suggest expanding the current statement, since we believe this work should have a substantial voice in this debate, separating the references accordingly. "More generally, we recommend that the potential of multi-compartmental SOC dynamics models be fully explored and exploited by soil biogeochemists before a new generation of models of increased complexity becomes operational. **While new models including the diversity of microbial communities and related processes are emerging (Lehmann et al., 2020; Crowther et al., 2019), the uncertain structure and parametrization of more complex models is hindering their application as robust predictive tools (Shi et al., 2018). At the same time, simple conceptual models of SOC dynamics like AMG combined with novel initialization methods and data-based approaches such as PARTY<sub>SOC</sub> show promising improvements (Cécillon, 2021a; Dangal et al., 2021; Lee et al., 2020).**"

**R2:** Line 384-386 Would improved accuracy for wider pedo-climatic range be dependent on having long-term bare fallow experiments available in most regions of the world?

**Reply:** We thank the anonymous referee for this question. It is true that *in-situ* information on biogeochemical stability of SOC is not easy to access, yet it remains crucial for the calibration of the PARTY<sub>SOC</sub> model and for its expansion to larger scale. As briefly discussed in the manuscript (lines 382-384) apart from the currently used long-term bare fallows and C3-C4 vegetation change chronosequences, in this work we present how long-term agronomical experiments can be used instead for its calibration.

**R2:** Line 391 typo in word "conclusion"

**Reply:** We thank the reviewer for noticing and reporting the mistake.