

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2021-246

Anonymous Referee #2

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Referee comment on "A robust initialization method for accurate soil organic carbon simulations" by Eva Kanari et al., Biogeosciences Discuss.,  
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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:

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

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

Line 265 to 267 Might just be easier to state that you compared  $\text{Cs}/\text{C}_0$  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  $\text{Cs}/\text{C}_0$  proportion", while actually you are comparing "measurement based  $\text{Cs}/\text{C}_0$ " vs. "AMG optimized  $\text{Cs}/\text{C}_0$ ". In short, to make it clearer, "Rock Eval + PARTY processing" would gain to be consistently referred to as "measurement based".

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

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?

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?

Line 391 typo in word "conclusion"