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Interactive comment on "Soil organic carbon stocks are systematically overestimated by misuse of the parameters bulk density and stone content" *by* Christopher Poeplau et al.

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Dear Editor, currently, scientists, politicians and environmental managers are desperately seeking for carbon sinks to improve the global carbon budget. Thus, it is important to use appropriate models or to improve models by identifying their shortcomings. Thus, the authors raise a very important issue dealing with the estimation of the soil organic carbon pools, which is the largest terrestrial carbon pool, and show the weakness of four SOC stock equations concerning the bias by comparing and evaluating them. I highly recommend publishing the manuscript. Nevertheless, I have some questions to the authors and some recommendations how the manuscript could be improved.

SPECIFIC COMMENTS Line 51: In line 44, you already mentioned that mineral frag-

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Discussion paper

ments > 2mm contain pieces of the size classes "gravel" and "stone". But it is not clear how you use the term “stone content” in your manuscript. For example in the Guidelines for soil description of the FAO, the size class of “Coarse gravel” is between 2 and 6 cm and “stones” are classified as mineral pieces between 6 and 20 cm. Can you clarify this? I confirm the comment of Referee #1, that the equations should not be repeated. Maybe they could be summarized in a table, which would allow to repeat the sub-equations in the columns (e.g. column 1: Method, C. 2: BD equation, C. 3: SOC stock equation) Line 70-71: Did these four authors which you cited develop the equations or did they only apply them? In the second case, please refer to the original reference. For example, Henkner et al. (2016) cited four publications concerning the SOC stock equation which was used by them. And during the description of the equations, could you refer to the original references in each section? Line 86: Do you have any references which underline that you use a stone density of 2.6? Or is it the average stone density of the dataset which you used? Line 107: What was the criterion for the classification of the data of the stone content? How did you choose the boundaries of the classes? Lines 118-119: I do not see this observation in Table 1. Line 124: This sentence should be part of the methods. Is BD fine soil of 1.2 the average of the whole dataset (German Agricultural Soil Inventory)? Line 174: Would it be difficult to recalculate SOC stocks which were done by other equations in former times by using Eq 7? Which modification of the raw data is needed? I ask you because such improvements may fail and do not find acceptance due to the bias which is produced by the change of the method. Therefore, it is the simplest way to apply a standardized and established method for dealing with time-series, for example, to keep the comparability at a high level. Could you therefore clarify how data of soil inventories like the inventory which you used must be re-organized first or could be used that the already taken samples and available data is applicable with Eq. 7 in the future? Table 1: In my opinion you show two different topics in this table. Is it therefore meaningful to show them in one table? Table 1 and Figure 1: Is the data of Tab. 1 and Fig. 1 in a way comparable? In my opinion, the highest differences and hence the relative deviation of M4 is found

in sampling depth 10-30 cm and not in 70-100cm (Figure 1). However, in Table 1 the highest value concerning the average relative deviation was found in 70-100 cm. Figure 1: Did you compute any significance test that you can show which method (M1-M3) differs significantly from M4 per depth and stone class? Figure 1: Clarify what kind of values you show. Average values? Figure 2: Would it be possible to show the 95%-confidence intervals of each of the functions? This would show the variability. Figure 2: How did you calculate the systematic deviation? Please, show the formulas at least in the Figure. Can you explain it in the method section? (see above comment line 124). Figure 2: What is the number of replicates per depth and stone class? Is the number of replicates reasonable for the different variabilities, to some extent? Further, could it be that samples of depth horizons 50-70 and 70-100 cm originate from deep soils like "Parabraunerde", "Böden auf Lössderivaten" or "Böden auf Sand", which can contain a low stone content in the upper sampling horizons and maybe also in the deeper sampling horizons? And soil from e.g the "Schwarzwald", "Mittelgebirge" and the Alps do not reach depths of 50, 70 or 100 cm. In these cases, the stone content is already high in the upper sampling layers like 0-10, 10-30 or 30-50 cm. Would this explain the higher variability in 10-30 cm besides the fact that the different depth intervals contributes also to the SOC stock (depthi)? Because during the single stratification by stone content, it seems that the different sampling horizons of all used test sites were mixed. Hence, the stone class (> 30 %) in depths 0-10, 10-30 and 30-50 is represented by sites from rugged terrain while the depth classes 50-70 and 70-100 cm contain samples of "deep soils" which show a stone content > 30 % only deeper than 50 cm soil depth. How does this issue influence your results? Therefore, I agree with Referee #1 comment to re-classify the dataset to avoid such an assumed mixture. For this, the parameter "depth" in the inventory could be the classification factor. Or it could be possible to apply a spatial classification by soil type or region. This would be the first step. After this, the sub-datasets can be stratified by the stone content in a second step. Figure 3: Where do you refer to this figure. In my opinion, it should be Figure 1 and placed in the method section.

TECHNICAL ERRORS Line 158: Do you mean SOC stocks instead of SC stocks?

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