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Interactive comment on "Modelling dynamic interactions between soil structure and the storage and turnover of soil organic matter" *by* Katharina Hildegard Elisabeth Meurer et al.

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Summary The study contributes a new model on the dynamical between feedback soil organic matter (SOM) decomposition and soil aggregate structure. Like other models it employs the concept that the addition of low-density organic matter modifies both, the soil layer thickness, porosity, and the bulk density, but is the first study to my knowledge to explicitly discuss this feedback. It explicitly models retardation of SOM decomposition by aggregation and associated micropores. The approach is demonstrated using a simple parsimonious SOM model at pedon scale with a sensitivity analysis and a model calibration to a long-term field study. It will be a welcome contribution to the

SOM modeling community. I enjoyed reading the manuscript. It is well written and the logical flow is clear to me.

Response: We would like to thank Dr. Wutzler for his kind comments and for his constructive suggestions for improving the paper.

The study could be made stronger by including a simulation/calibration without the feedback and comparing the improvements between the two versions.

Response: See our response to the next comment below

General comments I missed a discussion on implications and results on whether the presented feedback is important for understanding or prediction of SOM dynamics or model structure. The authors showed that the relatively simple model could already predict differences in SOM and soil structure by different inputs. However, to what extent could this also be modeled without SOM influencing the soil structure? Although the paper holds enough new insights to be published, I encourage the authors to take the extra work to compare to a model version where the feedback is switched off. For example by calibrating time-constant bulk densities and parameters to the three input-scenarios.

Response: We have now run ICBM against the SOC data for the manure and bare fallow treatments and it performs almost as well as the model described in our paper (RMSE's are slightly larger than those shown in table 4), albeit with different parameter values: the retention efficiency is similar (0.35 vs. 0.37) but ko is much smaller (0.015 vs. 0.036 year-1), since physical protection is not modelled explicitly. However, even if a simpler OM model such as ICBM can be calibrated reasonably well to time-series of OM measurements at one site, our model that explicitly incorporates soil structure-OM feedbacks has many important advantages. This is because it enables simulations of the effects of soil structure and physical protection on OM turnover in contrasting soil types (e.g. sand vs. clay) explicitly and directly from measured particle size distributions, without having to resort to re-calibrating model parameters describing OM turnover for each soil, as was done, for example, by Poeplau et al. (Geoderma 237/238, 246-255). In principle, our model also has a much broader range of potential management applications. For example, it could be used to simulate the effects of contrasting tillage systems on SOC dynamics, as well as the effects of faunal bioturbation on OM stabilization. We would also like to emphasize here that in discussing the importance of accounting for soil structure effects on SOM storage in simulation models, we should not ignore "the other side of the coin", namely the importance of SOM for soil structure. We feel that the inclusion in our model of the effects of SOM on porosity, pore size distribution and soil water retention, is a very important advance compared to other models, because it enables straightforward links to models of soil hydrology, plant growth and therefore OM inputs to soil. This kind of dynamic soil-plant model would encompass, for the first time, a complete description of all the physical feedback mechanisms determining organic C sequestration in soil. We will expand our discussion of these important issues in the revised version of the paper.

The conclusions currently read more like a discussion. They could be sharpened to what readers should "take home" for their work from this study. What are the most important parameters and feedbacks that you think they need to consider in their experiments and studies?

Response: Yes, we will modify this section. In fact, in order to meet some of the other comments and suggestions from Dr. Wutzler and referee 2, we can see the need to include a short discussion section in the paper

There are already models that let SOM decomposition affect soil structure. For example in the model of Ahrens et al. 2015 (see also Yu 2020 eq. S28a) SOM dynamics affects bulk soil density and soil volume and this in turn affects modeled concentrations, changes in soil volume, and transport processes. They applied the same concept of Federer 1993 as in the current manuscript, but incorporated many more processes so that this feedback was not explicitly discussed. The present manuscript additionally partitions micro-and mesoporosity and models protection by aggregation. A little

comparison in the discussion or introduction would be nice.

Response: We did include a comparison of our model with several previous models in the introduction, but we had missed that Ahrens et al. and Yu et al. also model the effects SOM on bulk density (as Dr. Wutzler writes, this aspect of their model was not prominently discussed in the cited papers). We will include a reference to Ahrens et al. and Yu et al. in the revised version of our paper. Dr. Wutzler notes that in addition to the physical protection of SOM afforded by soil structure, we also model the effects of SOM on pore size distribution and water retention. As mentioned earlier, we consider that this is an important advance, because it enables subsequent links to models of soil water flow and plant growth. We will emphasize this aspect of the model in more detail.

P4L103: The authors argue that macropores probably are only a minor balance of SOM balance. Contrary, some researchers think, that macropores are a hot spot of SOM turnover and together with the rhizosphere are the most important places to study. Especially for systems with active earth worms this has been shown (e.g. Don et al. 2008).

Response: It would be possible to extend the model to deal with C inputs to the macropore region, for example by root in-growth or the exploitation of surface litter by earthworms, although this would increase model complexity and introduce new parameters. We agree that this is something that should be explored in the future. We will add some text on this in the final section.

eq. 7 and 11 seem to both add volume and additional pore space with addition of OM. In an alternative mind model putting dissolved organic matter or root exudates into soil would partly fill up existing pores. Please, add some explanation of assumptions to this part.

Response: Yes, this is an interesting question, which goes to the heart of the model concept. Adding a mass of OM must increase the volume of OM, but it could either increase or decrease the pore volume and thus total volume of the soil. The parameter that determines this is fagg (what we call the aggregation factor). If the addition of OM resulted in a net decrease of the pore volume, then fagg would take a negative value (the minimum value fagg could take is -1, if the added OM completely filled existing pore space, as Dr. Wutzler suggests it could). However, we can see from the data (see figure 4) that fagg = 2, in other words, a volume of OM creates twice its own volume of pores. There are several mechanisms and processes (both biological and physical) underlying the generation of aggregation pore space, which would be difficult to model separately, so our model makes no attempt to do so. The only assumption underlying the model is the linear relationship between aggregation pore space and OM, something which is strongly supported by experimental evidence (see text at lines 160-161). We will add some more explanatory text on this in the revised version of the paper.

I miss a paragraph how the model was integrated in time. I assume an explicit time (Euler forward) step much lower than the 5 years of distance between observations.

Response: Yes, it was an explicit numerical solution with Euler integration and an annual time step. We will add this missing information to the text.

How did you track the changes in soil depth (eq. 12) in the comparison to data?

Response: This was already explained in the paper. We simulated five soil layers, with variable thickness according to equation 12 (lines 328/329) and the difference in the total profile depth between the two treatments was compared with the difference in the soil surface elevation measured in 2009 (line 314).

Minor comments. The discussion at p3L55 argues about soil structure affecting SOM dynamics. If one could show that it is not only affecting fast pools, then this argument could be made even stronger to affecting SOM stocks and soil carbon sequestration.

Response: Yes, true. We feel that the experiments discussed at lines 62-65 (and other

similar experiments) give very strong evidence for the protective effect of soil structure on slow OM pools.

The font sizes in the figures are often very small, which makes it difficult to read the print-version.

Response: We will increase the font size of the figures.

eq.5 and 6: Why is there a factor of 1/2?

Response: It follows from the definition of kmix as the intensity of mixing of the stored OM in the two pore classes at an annual time scale. It gives perfect mixing for kmix = 1 year-1.

Please, check consistency of mathematical symbols. E.g. delta.z_min is sometimes written with min as subscript and sometimes with parenthesis (Table 1) denoting density gamma_o and gamma_m or gamma_org and gamma_min. F_text_mic or F_mic_text (fig. 6).

Response: Thanks for pointing this out. We will correct these inconsistencies.

p6L165: Parameter f_agg is introduced here. To my reading its quite an important parameter. I recommend explaining it (here or somewhere) in more detail. Does it correspond to the porosity of the volume occupied by organic matter?

Response: Not exactly, although fagg is related to the porosity of organic matter, see equation 24 (please note that there is a typo in equation 24: ïĄęmac should replace ïĄęmin. We will fix this in the revised paper). fagg is simply the slope of the linear relationship that is assumed between the volumes of aggregation pore space and OM. We will add some more explanation to the paper at line 165. We discussed the correspondence of the parameters of the Federer et al (1993) model with our more fundamental derivation of essentially the same model at lines 199-214.

eq 21-24: please, use a different symbols at the left hand side than in (19) and (20) to denote the quantities to use assumption of f_som = 0 r f_som = 1.

Response: Thanks for the hint – we will do so.

Sect 3.2: Given the 5 years interval of SOM measurements the non-identifyability of the fast turnover pool is expected. Could you think of additional observations or sub-experiments that could inform the shorter time scale?

Response: Incubation experiments would be needed to quantify the dynamics of the young pool at shorter time scales. However, these kinds of experiments are usually conducted under controlled conditions in terms of water content and temperature, which makes it difficult to transfer the results to the field. Another approach would be to study the degradation of organic matter using litterbags (containing, for example, above-ground harvest residues). However, in the treatments that do not receive organic material, it can be assumed that the young pool consists of roots and rhizodeposition. Since it is difficult to quantify the amount of C that enters the soil via roots, it is also difficult to quantify its degradation. We are not aware that these kinds of experiments have been carried out for the Ultuna frame trial.

Sect 3.2. The mixing ratio was quite influential in Table 1. I assume in the identifiability analysis it correlated strongly with other parameters -which ones?

Response: Yes, kmix correlated strongly with ky, ko, Fprot and Ftext(mic). We will add a table with this information in the revised version of the paper.

Could this lead to potential model simplifications?

Response: In some model applications, possibly yes, but not if the users are interested in the influence of tillage or earthworm bioturbation on C sequestration

P11L335: "root litter input was distributed uniformly across depth". What do you expect to be the effect of distribution root litter input with an exponentially decreasing profile? How do you treat partitioning of given total root input to the modeled top soil and the non-modeled lower depth?

Response: In the ploughed (and sampled) horizon relevant to this study, there would be no effect at all, because of tillage mixing. Input to the topsoil was distributed uniformly because we assume efficient mixing by tillage. The non-modelled lower depth is not relevant to this paper.

Fig 1: The dotted regions were not visible in my printout. Please adapt the pattern.

Response: We will replace the dotted pattern by blank regions.

Fig 2: The placing of the braces confused me. Vor micropores its at the maximum pore diameter for mesopores the lower boundary of the upper brace coincides with the blue line. To my understanding it should instead coincide with the red line at the upper diameter.

Response: We checked the figure and it is OK. We don't really understand this comment

Fig 3: Cannot read the subscripts in this figure. Please, adjust the font sizes. (Also in the other figures)

Response: We will adjust the font sizes of Figure 3 and the other figures as well.

Fig 8: I had to search for the difference between left and right panel. Please describe in the legend or make the font of the years 1997 or 2019 more prominent.

Response: We will add a title to both panels stating the respective year. We will also add the information to the figure caption.

Fig 9: Figure headings (bare fallow, manure) in addition to the legend would help the reader.

Response: Thanks for the comment – we will add headings to Figure 9 and also adjust the font size of the legend.

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