

Comment on soil-2022-28

Anonymous Referee #1

Referee comment on "Impact of agricultural management on soil aggregates and associated organic carbon fractions: analysis of long-term experiments in Europe" by Ioanna S. Panagea et al., SOIL Discuss., <https://doi.org/10.5194/soil-2022-28-RC1>, 2022

The study presented by Panagea and co-authors summarizes the outcomes of the analyses of topsoil structural stability as determined by soil aggregates, and related soil organic carbon under different soil managements that are proposed beneficial for soil (structural) quality. The authors made use of a European network of long-term field experiments, which allows them to cover a wide range of region-specific management practices, as well as pedo-climatic conditions. The approach is very good and the authors collected a valuable dataset. I would think that the authors should highlight even stronger the European gradient, which is both west-east and north south.

The manuscript is generally nicely written and understandable. Some sentences are long and should be split into two. For example, ll. 344 – 346 should be split into two sentences, one stating the outcomes of the study and the second one highlighting that this has also been observed in other studies. Please avoid long sentences containing too much information. The description of the methods is understandable and straightforward. I also very much like Figure 1, which nicely summarizes the methods described by the authors in subchapters 2.2 – 2.4.

Throughout the manuscript, the authors make some hints on long-term and short-term effects of the management practices, but do not mention the age of the respective experiments and for how long the respective practice has been applied to the individual field sites. Please add this information and include it in the interpretation of the data.

In the discussion, the authors put a lot of effort in discussing the impact of management strategies on SOC in general and on the soil profile. This is of importance, for sure, but it moves the attention away from the real intention of this study, i.e. the OC fractions associated with different soil aggregates (as stated in the title). This section could be shortened with only mentioning the most important studies (see below).

Please find some minor comments below:

Minor comments

I. 273: $h-1 = ha-1$

II. 288 – 290: not necessarily what has been proposed in the Introduction and hypotheses. Please add this to the text

I. 300: I suggest to remove “with these mechanisms” – it does not become clear if mechanisms means practices or what exactly they relate to

II. 300 – 303: this is true only for conservational managements – conventional tillage, for example, is not known to decrease mineralization as it leads to disturbances. Please re-phase this part and make clear which practices you refer to

I. 311: the choice of references is not clear to me: to my knowledge, neither Blanco-Canqui & Lal (2008) nor Haddaway et al. (2017) included SOM distribution in aggregate fractions in their analyses. However, the study of Haddaway et al. (2017) is built upon a methodological framework for meta-analyses on the impacts of soil management on SOC stocks in boreo-temperate regions. This framework was then further used in the meta-analyses performed by Meurer et al. (2018). So which point do the authors want to make here? From the choice of references it is not clear if “common methodological framework” relates to the SOC measurements taken in the field, or the compilation of studies involved in analyses (for this, please see Haddaway et al. (2015) and Söderström et al. (2014))

I. 313: I suggest to remove “alternative to inversion tillage”, as it is not fully clear what the authors intent to say with that. Just leave it with “alternative practices” or different practices”

II. 320 – 321 & 346 – 347 & 400: please mention the hypothesis – remind the reader. The same applies to I. 319 (s&c) and I. 322 (mM)

I. 323: please remove “especially in CZ and HU_2” or explain a bit better what is meant here.

II. 327 – 332: at this point, it would be interesting to discuss if this is a methodological issue, or if the other studies simply did not further study the composition of the macroaggregates, as has been done by the authors

II. 341 – 343: this is an important outcome and it should be further elaborated: what exactly is the benefit of large macroaggregates in relation to "good soil structure"?

I. 355: here, the humification coefficient should be explained in more detail. I assume that the authors mean the amount of C that remains in the soil?

II. 377 – 380: this is certainly true on average and depending on which studies are included in the analysis. Some studies shows that the effect of reduced tillage on SOC stocks in deeper depths might be even negative. See for example individual studies shown in Fig. 1 in Meurer et al. (2018).

II. 422 – 427: this is an important point! In addition to the impracticability at field scale, potential negative environmental impacts following the high doses of organic fertilizer application should briefly be discussed here

I. 450: requires

I. 472: please be more specific with what is meant by "more time"

Literature referred to in the text:

Meurer KHE, Haddaway NR, Bolinder MA, Kätterer T. Tillage intensity affects total SOC stocks in boreo-temperate regions only in the topsoil – A systematic review using an ESM approach. *Earth-Science Reviews* 2018;177:613-622.

Haddaway NR, Hedlund K, Jackson LE, Kätterer T, Lugato E, Thomsen IK, Jørgensen HB, Söderström B. What are the effects of agricultural management on soil organic carbon in boreo-temperate systems? *Environ Evid.* 2015;4(1):1.

Söderström B, Hedlund K, Jackson LE, Kätterer T, Lugato E, Thomsen IK, Jørgensen HB. What are the effects of agricultural management on soil organic carbon (SOC) stocks? *Environ Evid.* 2014;3(1):1.

