

Final Author Comments

We thank the two anonymous referees for taking the time to evaluate the manuscript and we are pleased about the favourable response.

Answers to referee 1:

Minor comments:

- It does not seem that you refer to data normality. Was it tested on the initial data and eventually accounted for in the statistical analysis? Usually soil-related parameters are non-normally distributed. There are two places in our manuscript where normality of data may be of relevance. Firstly, when modelling the relationship between OC concentration and bulk density with an exponential function, random deviations are usually assumed to be normal. We do not explicitly state this in the manuscript because given the large number of observations per site, the assumption is of minor importance since even for non-normal deviations, estimates are asymptotically unbiased. More important is the variance homogeneity of residuals, but Fig. 4 shows there is no indication of variance heterogeneity. Secondly, the approximation of the variance of calculated OC stock values could be influenced by normality of data. As this approximation relies on a linear Taylor series expansion, one could suspect that the symmetry of the normal distribution could improve the accuracy of the approximation but the reasoning itself remains valid also for non-normal distributions.
- Eq.(1): can you add a reference?

Equation 1 is one possible formulation for the transfer of element concentrations in a sample to element stocks for a defined soil volume, using bulk density and accounting for the stone content. This is general textbook knowledge and the formula - with some modification for different units applied - can be found in many publications (e.g., Lo Seen et al., GCB 2010, 1777-1792;...), so we do not think that it needs a specific reference.

- Page 732, line 12: can you add a reference for PTF?

We will add two references (from studies in Table 1) as examples for the application of PTFs where no BD measurements exist to a final version of the manuscript.

- Page 735, line 7: convert data in kg/m^2 since later it is so (page 740, line4). Check that all the reported values have consistent units: i.e. stocks in kg/m^2 and MDD in g/m^2

To avoid confusion with the units, we convert all data on carbon stocks and stock changes to g/m^2 , including Equation 1.

- Page 743, line3: be aware that most literature reported PTF were developed for agricultural soils

Yes, though their application was also tested for forest soils (e.g., de Vos et al. 2005). Due to the on average higher C contents of undisturbed soils than of soils under agricultural use, the predictive quality of OC concentration for BD estimation via PTF is supposed to be higher.

- Ellert et al. 2001: they state that the equivalent soil mass approach should be applied to agricultural soils only. Can you comment on that?

While Ellert et al. (2001) use changes in BD before and after tillage as an example to illustrate temporal changes in BD and the resulting biases of SOC changes when expressed for a defined soil depth, we did not find an argument in the paper promoting the exclusive application for agricultural soil. Since temporal variability of bulk density is not only restricted to agricultural systems but also relevant for natural sites, e.g., following changes in soil moisture or organic matter content, the equivalent soil mass approach seems to be reasonable for such situations as well. Also Grifford and Roderick (2003) state that they can see no situation in which spatial coordinates would be more appropriate to report changes in soil OC stocks than mass coordinates. We only see a limitation of the approach when spatial differences and not temporal changes of the same site are addressed.

- I would suggest to add these additional references that could enhance and strengthen the paper itself

We also refer to statements by Ellert et al. 2001, Rodeghiero et al 2010, and Saby et al. 2008 (also recommended by Referee2).

Answers to referee 2:

- Some parts of the discussion contain a lot of repetitions which can be removed to make discussion section more concise. This holds in particular for chapter 4.2, 4.3 and 4.4

We condense those chapters as far as possible.

- The paper is quite lengthy, and provides measurements very detailed. I recommend to skip figure 8, as this figure does not provide further important information

We agree, Figure 8 will be removed as it mainly visualized comparisons presented in the text.

- I suggest to link the result of this paper to some extent with the aspects raised in the Saby et al (2008) paper regarding the possible monitoring setup to detect changes in soil carbon

The Saby et al (2008) paper mainly deals with the potential use of already existing national European soil monitoring networks to detect changes in surface soil OC concentrations, and identifies gaps in the number of sampling sites in relation to spatial variability of OC concentrations in each country. It was tested if these monitoring networks are suitable and big enough to verify changes for C-sink accounting under the UNFCCC. The CarboEurope soil monitoring sites are plot studies. Observed future trends at those sites might or might not be representative for regional trends, but likely are affected by local conditions or site history. Their major purpose is to verify eddy covariance measurements, as the network of the eddy covariance towers has a larger spatial distribution and thus is more suitable to give general trends. We add a link to regional/national soil monitoring programs in the introduction. A direct comparison between the MDD results presented by Saby et al. and our study is not possible since Saby et al. only discuss concentration changes in topsoil layers but not stocks.

Details:

- P.1 line 25: is 10 cm also valid for grassland and forest?

Yes, sentence is to be changed accordingly

- P.5 line 1 Why this hypothesis? If you have some reasons from the papers cited please shortly mention them.

We add an explanation to the introduction why it can be assumed that variability of OC stocks based on equivalent soil masses could be higher than those for defined soil volumes to support the hypothesis.

- P.5 line 24 for comparison with other studies it helps if you provide the size of the monitoring plots in m² and ha.

The variability of OC stocks and thus the MDD depends on the size of the sampling area, so it was added to the methods part. The problem with direct comparisons of MDD between studies is that MDD is highly dependent not only on plot size and related variability of stocks but also on the sampling design. Besides the variability of OC stocks (which relate to plot size as indicated by the referee), especially the considered soil depth and the number of samples taken is important for absolute and relative measures of MDD, so they are seldom directly comparable. We are fully aware that the values presented are only valid for the chosen approach and not universally applicable – which is not indicated in the text and the reason why we do not compare our MDD results with those of other studies.

P.6 line 13ff: the soil types are provided in Table2, but in the interpretation of the results and discussion they don't appear anymore? I assume that the pedogenetic horizons of a Podzol or even Phaeozem contribute to the findings reported in the tables and figure.

Since we collected soil samples at defined soil depths, soil horizons were mixed so that direct relations of the results to soil genesis are difficult. Otherwise soil types certainly influence the amount and depth distribution of soil carbon. Still, it is difficult to separate variation induced by soil type from other possible sources. For the Podzols under the coniferous forest sites, for example, we cannot separate the contribution of carbon transport within the profile to the variation in SOC stocks from the variability of stone contents (at Wetzstein and Norunda) or the influence of deep plowing (at Le Bray). We discussed the special case of the Andosol in France but otherwise additional discussion on soil types would add too much detail since it is difficult to draw general conclusions from single sites.

- P.7 line 24: I do not understand that sentence, please rephrase

We re-phrase the sentence and split it to make it better understandable.

- P.8 Eq.4: beta0, beta1, and epsilon are not explained

These are variables of the exponential function. We add the explanation.

- P.8. Eq. 5: for the first term also Var(epsilon) instead of text can be used

No, epsilon is a part of the function, not an error.

- P.8. Eq. 6 & 7: index for ep, se, and j is not given

We add the respective information

- P.10 line 21: do soil type of Laqueuille and the past fertilization management explain this large C pool?

Yes, the soil type is important, see P.14 line 16ff. for reference in the manuscript. The site is under extensive use without application of inorganic fertilizer, only grazing (Soussana et al., 2007).

- P.11 line 8: referring to figure 5. The measured relative contribution is not explained in the method section, what is the unit? Why is the contribution of covariances negative? This would help the reader to understand Figure 5 better.

The unit for the relative contribution is %. We defined the total sum of all absolute values of the relative contribution terms as 100%. In the graph, we did not plot the absolute values but accounted for the algebraic sign, which was negative in the case of some co-variances, as explained in the text (p.11 line 19ff). We add a description of the calculation of relative contributions in the methods section.

- P.12 line 8: please add: ... using 100 soil cores at monitoring sites of ... m² ..

We add the size of the monitoring sites.

- P.13 line 9-11: Please check the unit of the last column in table 2.

The unit is g OC m⁻² as assigned in Table 2 and Figure 7. Still, the reader should note, Table 2 presents cumulative values for increasing soil volumes, while Figure 7 shows values for individual soil layers.

- P.14 section 4.1: for a comparison of the OC stocks of grassland or arable soils the past nutrient management and soil type is also quite relevant

The aim of this part was not to compare OC stocks between land use classes but to give the reader a general idea how representative the studied soils are with respect to their carbon contents in general, and which sites are exceptions. Otherwise, the reviewer is certainly right OC stocks depend on fertilization and management intensity at agricultural sites, and soil types are also relevant.

- P16. Line 28-32: you find in literature quite some papers dealing with PTF for BD, most of them include organic matter and clay content as predictor variables. The specific PTFs are better/less successful depending on the soil texture classes.

We agree, soil texture is probably more important in soils with low in organic matter than in soils with high organic matter contents. We tested the relation between clay content and BD at our study sites for the available 10 cores per site and did not find a strong relation. Therefore, and because data on only 10 cores were available, we did not include clay content in the PTF. After all, adding the clay content to the PTF would likely not have affected the general results of this paper.

- P.18 line9: I assume that the numbers given in Table 1 refer to regional studies ... In this case this is a pooled MDD for monitoring sites and not comparable with the MDD of one monitoring site

Yes, Table 1 refers to regional studies but we did not try to extract the MDD from these studies to compare them with our results. We report the detected changes in Table 1 as examples for observed rates of changes for different land use types across Europe, which – if valid across larger spatial scales - could apply also to the study sites, thus serve as references to estimate when changes could become detectable by the used sampling approach.

- Figure 7: please explain ESM in the figure caption

Explanation will be added.