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Comment

Interactive comment on “No change in topsoil carbon levels of Great Britain, 1978–2007” by P. M. Chamberlain et al.

Anonymous Referee #2

Received and published: 13 May 2010

The manuscript "No change in topsoil carbon levels of Great Britain, 1978-2007" will be a very important contribution to the discussion of the role of soil carbon for the terrestrial carbon balance and is in the scope of BG. It's a unique data set with repeated sampling in 1978, 1998 and 2007 on 635 plots (all three surveys) and more than 2500 additional sampling plots. It is written very well even though it needs some considerable shortening and pruning especially in the discussion section and for the tables and figures. The data analysis and statistical modeling seems to be done carefully using sophisticated mixed model approaches in combination with traditional statistics. The uncertainty of the dataset and the estimated soil C stock change in the UK has been discussed in his manuscript. However, some major uncertainties and unexplained results remain and should be further addressed.

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1.) Additional information on the soil sampling should be provided: Has there been only one sample per sampling plots? Has this sample been at least a composite sample? Has the forest floor or other organic layers been excluded or included? The increasing soil C densities from 1978 to 1998 (mainly in woodlands) and thereafter decreasing C densities are insufficiently explained, which puts a question mark to the whole data set. It should be estimated if this up and down or accelerated C loss maybe explained with sampling and analytical errors and biases (e.g. by using different methods at the different surveys). Especially in land use systems with steep C gradient with depth such as forests and grasslands, a small error in the sampling depth or not similar separation between mineral soil and forest floor may lead to a bias in the whole data set. If age class effects and harvesting are considered as reason for increasing densities (p.2286 l. 20) data on forest harvest should be shown to substantiate this explanation. I can only agree and repeat the comment 3 of reviewer 1 on the sampling procedure - it should be taken seriously.

2.) A chapter “Causes of change in topsoil C concentration and density” in a paper with the title “No change in topsoil carbon levels of Great Britain, 1978-2007” does not really fit. I agree with reviewer 1 that the title should be changed taking into account the uncertainty which is inevitable combined with the presented data set. Even though it is a unique dataset, the failure to detect significant soil C density changes can be also a result of the relatively weak sampling scheme (missing replications, missing bulk density for 1978 and 1998) than of missing regional scale C density changes.

3.) Chapter 3.6 should be completely omitted together with p.2268 l.22-25 and p. 2289 l. 23-25. The maximum C density of a land use class is a weak indicator for a potential to increase C densities. Maximum soil C densities are mainly constraint by environmental parameters such as the clay content, water availability and productivity of a site and only partly by human management. In order to estimate the management impact on soil C densities more data on the recent and historical management practice are needed. These data seem to be not available in this soil survey. Thus, any speculation

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on the manageability of soil C stocks lacks an adequate data source.

Further comments and suggestions:

p. 2272 l.6: How was the volume and plant material (roots?) determined?

p. 2272 l. 18 ff. and p. 2273 l. 2 ff: The function used to estimate C densities from C concentrations including its error should be displayed.

Equation 3: The unit on the left site of the equation does not fit to the unit on the right site (left site: mass per area; right site: mass per volume).

Chapter 2.6: The reporting categories (BH, AVC) are not in line with the stratification classes of the CS (ITE). Please explain this shortly.

p.2278l. 28 ff: You conclude that land use change does not play a role for soil C stocks. However, at p. 2281 l.11 ff an effect of land use change on soil C densities is assumed: You write that the areas that are converted from one land use class to another are expected to adjust to a new soil C density level. Maybe this needs some clarification.

Page 2282 l. 21-25 should be shifted to the introduction or omitted.

Chapter 4.2 and 4.3 should be pruned considerable or omitted. Most points raised here are repeated somewhere else.

All comments on the study of Bellamy et al. 2005 should be combined in the chapter 4.6 in order to structure the comparison with this study. The following party should be merged into this chapter: p. 2283 l. 17-20, 2284 l. 1-5, 2284 l. 12-28, p. 2285 l. 1-7, p. 2289 l.5-6.

Table 2 and 3 may be omitted, since very similar results are displayed in table 5 and 6. It seems to be enough to display densities since you used one function to derive C densities from C concentrations. The concentration data may be added in an appendix.

Table 6 and Figure 7 contain almost the same information and data. One of them

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should be omitted.

Large parts of Table 7 and 8 are redundant. Both tables should be merged.

In table 9 you may want to include additional national surveys as they are reported from Ciais et al., 2010 (Global Change Biology, Vol. 16/5, page 1409-1428) and the model derived soil C stock change estimated in this publication.

Figure 4 should be omitted since it is mainly a visualisation of the information that are already included in the tables.

Figure 7 should get the legend of the vegetation class into the graph like it was done already for “Crops and weeds” and for “Fertile grassland”. That makes figures much easier to read.

Figure 8 is already included in Table 8 and thus, can be omitted.

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