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Discussions

Interactive comment on "Variability in ^{14}C contents of soil organic matter at the plot and regional scale across climatic and geologic gradients" *by* T. S. van der Voort et al.

Anonymous Referee #1

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Review of Biogeo Disc / van der Voort Radiocarbon is a valuable tool in attempts to understand the formation and turnover of soil organic matter, but the difficulty and expense of making measurements have meant that the available data are relatively few. Therefore the authors' idea to assess the variability and representativeness of 14C measurements is welcome. Broadly, they conclude that there is not a great deal of spatial variation, so that depth variations in 14C are fairly consistent from site to site, and results already available have therefore been reasonably representative. I do not think this is at all a trivial conclusion – it is an important finding. My main criticism of the work is that the variable chosen to represent soil carbon was SOC concentration (%), which is not directly relevant to SOM turnover. Better would be SOC pool (g/m2),

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which is the natural relative of turnover rate. Therefore I suggest that either the authors justify the use of concentrations, or they reanalyse their results using C pools. More minor comments: Line 21 Here the results for “topsoils” are claimed to have been reported. As far as I understand it by topsoil they mean the top of the mineral soil beneath the O (LF) layer. I question whether this really is topsoil in the sense of containing organic matter undergoing turnover and being intimately connected to ecosystem processes – in other words I reckon that the LF material is functionally important and should be counted as soil. If the authors do not agree, then some discussion would be welcome. It would be of interest to know for example how much C (g/m2) is in the LF layer, and how much in the 0-5 cm at the top of the mineral soil. Moreover, the numbers quoted in the Abstract (Δ14C 159, sd 36.4) appear actually to refer to the LF layer (Table 2)! Line 26 This last line of the Abstract is weak, if there are “important consequences” you should say what they are. Materials and methods The dates of much of the soil sampling are stated to be “in the course of the 1990s” which could mean that some samples were collected 10 years apart. Other samples were collected in 2014. It therefore is not strictly correct to compare 14C values, since they are not constant with time in soil in situ – indeed that is why 14C is a useful variable, and why the data of sampling is an important qualifier of every 14C measurement. Maybe the analysis here would not be much affected by the assumption that the 14C values refer to the same point in time, but the issue should be acknowledged and the assumption justified – perhaps the MRT values are sufficiently long that a few years’ difference in sampling date is of no consequence? Equation (2) This doesn’t look right to me – the leading Δ14C shouldn’t be there. Also, is it really necessary to apply the equation only to samples with a value of R>1, which is what seems to be stated in lines 124-5? And after reading further, I realise that I do not understand the difference between R and Fm. Line 173 I don’t see why the expressions “worst case” and “best case” are used here – the facts are the facts, we should not judge them. Also, I do not fully understand what is learnt by showing that the variability of 14C correlates (or does not correlate) with variables like slope, MAP etc. This is not considered in the Discussion, yet the results for variation with clay and MAP appear as conclusions. I could not see any information about clay contents (e.g. why not in Table 1?). Line 234 The word “marked” here is used rather carelessly. The values of MAP and MAT admittedly vary, but within fairly small ranges in a global context. And since the soil types and geologies also vary it can hardly be claimed that variations in the site attributes have been sufficiently covered – it might be for example that a trend in MAT counters one in MAP, or in soil type or in geology, or indeed in vegetation type (as far as I can see no information on tree species is provided, certainly not in Table 1) or NPP. Although the results are certainly of considerable interest, the fact that definite trends cannot be found does not mean that there are no trends Line 288 Is it really necessary to incorporate “factors that drive small-scale variability” into larger-scale models of SOM turnover? Is it not possible that ecosystem complexity and the costs of analysis mean that the more complex models implied here are unachievable?

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-649/bg-2015-649-RC1-supplement.pdf

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