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Biogeosciences Discussions

Interactive comment on "Relative stability of soil carbon revealed by shifts in δ^{15}N and C:N ratio" *by* F. Conen et al.

Anonymous Referee #1

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The present paper deals with the highly important question of stabilization of soil organic matter as influenced by possible climate variations. The authors use a climosequence approach, an approach which received considerable attention amongst researchers during the last decade. The major research question of the paper is to develop a concept to derive ages of soil organic matter fractions using the natural abundances of the 15N and 14N isotopes in soil organic matter. The relative enrichment of 15N during most microbial processes leads to an increase of delta 15N values. The different delta values and C/N ratios of particulate organic matter and mineral associated organic matter are used to estimate relative ages of these fractions. 14C data using recent 14C inputs into the system were used for validation purposes. In general I find the present paper very interesting and the concept worth while discussing.

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Interactive Discussion
Discussion Paper

The paper fits well into the scope of Biogeosciences and is excellently suitable for an open public discussion. The paper is generally well written, the English is correct, citations are well selected and an in-depth discussion of the results in provided.

General comments: Concerning the concept itself some questions arise, which should be discussed further.

1) The fractionation procedure and the concept: The authors divided SOM into two fractions. They removed particulate organic matter by ultrasonication and sieving (using sand-sized fraction). This brings me to the first major point of possible discussion: Using the 63 micrometer fraction excludes particulate organic matter present in micro-aggregates in the silt-sized fraction. These micro-aggregates contain to a larger extent the so called physically protected organic matter. This fraction is suggested to be on average much younger than OM bound to reactive mineral surfaces (especially oxides). My point concerning the concept is therefore that the mOM fraction contains at least two, but presumably more OM fractions of quite different long-term stability and presumably age. The large differences in age ratios found in the present work might partly be due to that. Another hint towards this direction is the finding of quite varying ranges of epsilon values for the 15N fractionation in literature, which suggest different processes involved, as stated by the authors on page 2922, top. The suggested model (Fig 1) just might be too simple to describe the N-stabilisation properly.

2) I wonder why the authors did not attempt to use additionally the natural abundances of 13C and 12C for validation of their model. It is correct that the variability on delta values of 13C introduced by microbial turnover and preservation of specific compounds are smaller than for 15N and most studies use the difference between C3 and C4 vegetation. However, (i) it is very well established that delta 13C is (as in the case of 15N) significantly altered by microbial turnover processes, (ii) in most IRMS the delta 13C measurements are much more accurate than the delta 15N measurements and (iii) it is well established in literature that delta 13C values increase with decreasing particle sizes, particulate organic matter in the sand sized fractions nearly always being

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Interactive Comment

more negative in delta 13C than the smaller fractions. I would find this approach quite interesting in addition, because it uses less assumption than the presented one.

Specific comments:

Table 1: Although the authors used composite soil samples and therefore do not have real replicates available, I strongly suggest to supply the readers with the analytical standard deviations of the replicate measurements (for all data in Table 1).

Interactive comment on Biogeosciences Discuss., 4, 2915, 2007.