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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.

F. Conen et al.

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Thank you for opening the discussion on our manuscript. Your points raise important issues, which we will try to clarify.

1a) Your first concern is the fractionation procedure being too coarse. Our mOM fraction contains organic matter bound to reactive mineral surfaces (especially oxides) but also, on average much younger, particulate organic matter, which is present in micro-aggregates ($<$ 63 micrometer).

We completely agree with the idea of there being more than two different pools of soil organic matter. Given the innumerable forms of organic molecules and their possibly multiple interactions with each other, mineral surfaces and oxides, there is no limit to the number of pools that could be incorporated into a model of soil organic matter turnover.

Yet, the practical applicability of a model decreases as the number of parameters increases, especially when parameters are introduced that are difficult to measure on a sample. Alternatively, a concept may be deliberately reduced in complexity to a degree where it provides predictions of similar quality as more complex ones, but where its application is much less problematic or demanding (e.g. Fang et al., 2005). This is the route we were following in the development of our concept. Maybe, the description of the mOM fraction as being ’mineral-associated’ is not the best choice. It may be better, if we call it the ’protected’ fraction, whereby protection can be of chemical or of physical nature, which includes particulate organic matter occluded in the micro-aggregates.

1b) Another issue raised in your Point 1 are the quite varying ranges of epsilon values for the 15N fractionation in literature, which may suggest different processes involved.

Well, despite a range of fractionation factors repoted, our concept based on one single factor was able to satisfactorily predict relative ages in 11 of 13 samples. We may interpret this as an indication of the fractionation factor not necessarily being as variable as reported. The wide range of reported epsilons could also be a reflection of the difficulty to measure the fractionation factor of mineralisation directly in a natural soil (Högberg, 1997). Too small a proportion of organic matter is mineralised, even during an incubation over several months, to reliably determine a decline in organic N content and its change in delta 15N. The alternative, to determine the amount of NH4+ produced by mineralisation and compare its delta 15N to that of organic matter, is made difficult by rapid nitrification further altering the delta 15N of the mineralised N. We agree that our presumption of one specific fractionation factor is a simplification. An assessment of potentially resulting errors is indicated in Fig. 2 of the discussion paper. These errors are unlikely to explain the poor match between the relative ages predicted by our concept and the relative 14C ages for two of the samples at the highest altitudes. As mentioned above, we kept the concept deliberately simple. Some uncertainty is introduced by this, while other inaccuracies are avoided that are usually related to the necessity of more assumptions and a larger cumulative error arising from more parameters to be measured for more complex models. The two out of 13 points (Table 1) where our concept failed to reproduce the 14C estimate may indeed reflect its limitations. We think limits are reached where processes not reflected in the concept become important. This is most certainly the case were podsolisation occurs.

2) The idea to include the natural abundances of 13C and 12C for validation of the model seems appealing. Yet, we have not tried it for two reasons. First, the fractionation factor for organic matter decomposition is even more elusive than the one for N mineralisation. Second, the work by Kramer et al. (2003), cited in the discussion paper, shows little evidence for the correlation between delta 13C and the degree of organic matter transformation. It is well known that delta 13C is significantly altered by microbial turnover, however, the resulting signature of the processed material may be affected by too many different processes to produce a consistent picture.

Regarding Table 1: We have analysed bulked samples because we were not interested in the variance within the plots at different altitudes. Clearly it would have involved more work to get it and some of the measurement (e.g. 14C) are expensive. Therefore, we decided it was not worth analysing the sub-samples separately (Webster, 2007). Estimates of purely analytical uncertainty are mentioned in the discussion paper on page 2922, lines 11-14.

Fang, C., Smith, P., Smith, J.U.: A simple equation for simulating C decomposition in a multi-component pool of soil organic matter, Eur. J. Soil Sci., 56, 815-820, 2005.

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