Biogeosciences Discuss., 8, C390–C393, 2011
www.biogeosciences-discuss.net/8/C390/2011/

Biogeosciences Discussions

Interactive comment on "How to link soil C pools with CO_2 fluxes?" *by* Y. Kuzyakov

G. Ågren (Referee)

goran.agren@ekol.slu.se

Received and published: 31 March 2011

General comments

This is an interesting and provocative paper about the lack of cooperation within the scientific community. I think it can serve as a stimulating paper to make many people think more broadly about their investigations and to look at supplementary investigations to improve the validity of results. There is, however, one fundamental and contradictory aspect in the paper. The title refers to soil C pools and throughout the paper references are made to measuring pools in quite specific terms. Indeed, at the end of the paper, the author suggests that one should not look just for two pools but several (page 1971, line 15). At the same time the author dismisses the existence of pools ("so they do not really exist", page 1949, line 9, "As mentioned above, a clear physical separation of individual functional pools in soil by existing fractionation methods is not possible now

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

and probably will not be possible in the future", page 1969, line 17). I think the author has to make up his mind on this point. The idea of pools can useful for qualitative reasoning but I find it questionable how useful it is for quantitative analyses, except as a curve-fitting procedure.

Let me give an example to illustrate the problematic concept of pools in the context of this manuscript. Suppose we have a shift from a C3 to a C4 vegetation and for simplicity disregard quality differences in the litter of the two vegetation types. Let there also pass some time between the shift between the two vegetation types. The soil C from the C3 vegetation will then be old and recalcitrant whereas the carbon from the C4 vegetation will initially be young and labile. We can now follow the development of the soil carbon derived from the C3 and C4 vegetations based on the isotopic signature; we have two distinct pools. At the same time the C4 derived carbon will age and the old pool will consist of a mixture of C3 and C4 carbon but there is no way to separate the young and the old pool from each other; the two pools which should be the functionally important ones.

Specific comments

1. Page 1950, lines 25-26. I misunderstood this sentence when I first read it; it seems to imply an MRT of C in a flux. Maybe it could be rephrased as "the mean residence time (MRT) of C in the total soil is much longer than the MRT of the soil C that is emitted".

2. Page 1951, lines 2-3. I do not understand why there should be an error. If we know the MRT's and the pool sizes (given that they exist) there can be no error. In fact this is the idea behind the calculations in sections 4.1.1 and 4.1.2.

3. Page 1953, line 7. I think "approaches" should be preceded by "experimental". Theoretically it is possible to calculate both pools and fluxes, including isotopic signatures, at steady state.

BGD

8, C390–C393, 2011

4. Page 1956, point 3. If one instead of working with discrete pools uses a continuous quality distribution, the decomposition will automatically be non-exponential and slow down with time.

5. Page 1956, point 4. There are also other factors that may change, e.g. with depletion of C the water holding capacity of the soil can change.

6. Page 1956, lines 17-23. This reasoning requires the existence of discrete pools. If there rather is a continuous distribution of qualities, there is no unique decomposition rate of slowly decomposing carbon.

7. Sections 4.1.1 and 4.1.2. These two sections are very similar except that one focuses on long-term changes measured as stock changes and the other one focuses on short-term changes measured as fluxes. I think they could be merged into one section, or the differences must become clearer.

8. Page 1957, line 24. This is not precise. The fitted pools do correspond to the total soil C but the accuracy in their estimates may be low.

9. Page 1959, lines 2-4. If two pools have equal decomposition rates, why should they in this context be treated as different? The main characteristic of pools in this context is their decomposition rate.

10. Page 1959, line 6. This is well illustrated by Hyvönen et al. (2005).

11. Page 1962, line 11. I do not see the point with this paragraph; Figure 3 tells the same story but in more detail. The paragraph could be deleted.

12. Page 1963, Line 15. I think this sentence could be misunderstood, specific decomposition rate or absolute decomposition rate? A better formulation would be “and assuming that decomposition is a first order process”

13. Page 1964, lines 5-6. This is not correct. The continuous-quality theory has been tested extensively against empirical information. A better reference than Bosatta &

BGD

8, C390–C393, 2011

Ågren (1985) and with several examples is Ågren & Bosatta (1998). There are also several later publications.

14. Page 1964, line 8ff. This is not clear. I do not understand why there would be so different consequences of focusing on disappearance of old carbon versus increases in new carbon. The sum of the two is the total so knowing the total and one of the two, the other one can be estimated by difference.

15. Figure 2. Should there not be a thick line also for the bulk SOM in the abrupt permanent scenario? It is confusing to miss it.

16. Figure 3. The bottom part is not clear. What is meant by relative availability of new (C3) and old (C4), the green line? The legend to the right, should it not be C4/C3 in SOM? It cannot be in %.

References

Ågren GI, Bosatta E. 1998. Theoretical Ecosystem Ecology- Understanding Element Cycles.,Cambridge University Press, Cambridge., pp. 233

Hyvönen R, Ågren GI, Dalias P. 2005. Analysing temperature response of decomposition of organic matter. Global Change Biology 11:770-778.

Interactive comment on Biogeosciences Discuss., 8, 1947, 2011.