Biogeosciences Discuss., 9, C2858–C2861, 2012
www.biogeosciences-discuss.net/9/C2858/2012/

Biogeosciences Discussions

Interactive comment on "Stoichiometry constrains microbial response to root exudation – insights from a model and a field experiment in a temperate forest" *by* J. E. Drake et al.

Anonymous Referee #2

Received and published: 31 July 2012

The manuscript describes the results of a small field experiment and a model exercise that aimed at analysing the effect of variations in C:N of root exudates on microbial biomass and respiration rates. The topic itself, i.e. that exudate C:N may be an important driver for rhizosphere processes, is timely and interesting for a wider audience, but the manuscript less so. Here are my reservations and the reasons, why I am largely disappointed about the way the experiments were organised and the manuscript written:

(1) The two hypotheses that are put forward are not really inspiring. An inspiring hypothesis, to my opinion, should allow (once its supported or rejected) gaining some

insight into the mechanisms behind the relationship it describes. The two hypotheses here are both "if. . .then. . ." type of hypothesis, that are rather weak. The first one states: "if exudation alone is sufficient to stimulate microbial activity in rhizosphere soils, then the addition of exudate mimics will lead to higher microbial biomass, increased exoenzyme activities, and higher rates of C-mineralization relative to bulk soils". That's really very basic. It basically describes the well-known rhizosphere effect (that plants exude low-molecular-weight compounds to the rhizosphere supporting a certain microbial biomass and exoenzyme activities). Is there anything new that has not been tested extensively before? Despite the fact that the microbial biomass is a poor predictor of microbial activity and often the microbial biomass is lower (but more active) in times of highest plant C input into the soil. The second hypothesis then says that if microbes are N limited, exuding nitrogen-containing compounds will lead to a "higher rhizosphere response", presumably a higher microbial biomass and more investment in exoenzymes. Again, that's a very basic hypothesis. If an organism is N limited, then the addition of N per definition must lead to a higher biomass.

(2) The modelling approach used here is rather basic and straightforward and largely based on an older concept, already published in 2003 by Schimel & Weintraub. The framework is so easy to understand and so well explored already, that even without any math it is clear that a pulse of DOM with a C:N of 25 (at a soils C:N of 20) would lead to a greater increase in microbial biomass and a greater exoenzyme production, than one with a C:N of 100 (Fig.2). That more biomass is build when continued pulses of DOM with a C:N of 10 are simulated than with a DOM with C only (Fig.4c) is likewise not very surprising. I did not find any new aspects in the modelling approach that is presented here.

(3) The field experiments are interesting in their idea, but basic in their execution. The idea of pumping solutions through micro-lysimeters into soil to mimic root exudates, although not entirely new, is great. However, why the authors have chosen to do this in the field and then (after 50 days) taking the soil into the laboratory and measure the

respiration rate in closed flasks is difficult to understand. If the authors were really setting up the experiment after the model provided support for their hypotheses (as they state on page 6909), why did they not set up the experiment in the laboratory, where they could have measured microbial respiration continuously? There are several other aspects of the experiment that I cannot really understand. First, the reason given at page 6908 for using ammonium instead of amino acids (that are usually found in root exudates) as a N source is not comprehensible. Certainly, solutions can be prepared that contain the same amount of C at different amounts of N by using amino acids instead of ammonium. Second, the microbial carbon use efficiency, i.e. new biomass production over substrate uptake (biomass production plus respiration), is dependent, amongst other factors, on the degree of chemical reduction of the substrates. If the degree of reduction of a certain substrate is less than the mean degree of reduction of the microbial biomass (around 4.1), then this substrate does not contain enough energy to produce a unit of biomass, thereby lowering the carbon use efficiency and the biomass production. By choosing a solution with a very low degree of reduction (dominated by oxalic acid with the lowest possible degree of reduction of 1), the production of new biomass is not favoured. That's maybe the reason even with ammonium additions the increase in biomass production was relatively small and may have been mostly due to internal carbon reserves. Third, two controls are used, a water control and a disturbance control. Why? The water control includes the disturbance already. I have not found any part of the manuscript where these two controls were discussed or needed. In fact, only the water control is relevant to the experiment. I do not, in this respect, understand why the enzyme results in Figure 5 are first normalized to the disturbance and then the water treatment is shown as if it were a treatment, not a control. At least, it must be made clear in the legend, that this was not +C or +(C&N) but +(Water&C) and +(Water&C&N). Fourth and finally, a proper N control is missing. That would have allowed the authors to distinguish the effect of N alone (maybe N alone was sufficient to increase microbial biomass and exoenzyme production) from the effect of C and N together (maybe there was a co-limitation?) and therefore to allow dissecting the un-

derlying reasons for the observed pattern. It would have allowed to really address what the claimed in the abstract, namely to support a cause-and-effect relationship between root exudations and enhanced microbial biomass. As it stands it could also be that what has been found is solely an effect of fertilization.

(4) Another main point is that any description of the soils used (e.g., soil type, nutrient status, C and N content, and similar) is missing. That's certainly needed for such a paper.

Overall, I do think that the manuscript has its merits and that this is an important topic and an interesting idea. But I also think that the manuscript needs substantial re-writing along the lines shown above.

Interactive comment on Biogeosciences Discuss., 9, 6899, 2012.