Biogeosciences Discuss., 7, C4398–C4402, 2010
www.biogeosciences-discuss.net/7/C4398/2010/

Biogeosciences Discussions

Interactive comment on "Quality or decomposer efficiency – which is most important in the temperature response of litter decomposition? A modelling study using the GLUE methodology" *by* J. Å. M. Wetterstedt and G. I. Ågren

J. Å. M. Wetterstedt and G. I. Ågren

goran.agren@ekol.slu.se

Received and published: 17 December 2010

We appreciate the comments from the reviewer as they identify points that might have been written to briefly or are unclear. Here follows a point-by-point response to his comments.

1. In response to the reviewer's first comment we suggest adding a paragraph just before section 2.3.1 to further explain the GLUE methodology in this context "The GLUE methodology is particularly useful in the field of environmental modelling in which the

errors involved in measurement data may be unclear and where the response surface of the "goal function" or likelihood measure (LM) is flat and likely to contain many local optima (cf. Hyvönen et al. 2005). The GLUE methodology also acknowledges that more optima will be found with a more extensive search in the parameter space. Since it is likely that these optima would move with already small differences in measurement data (measurement errors), it is not meaningful to only search for a global optimum.

The use GLUE includes the following steps (Beven, 2009)." The last sentence is moved from the previously last paragraph.

References to be added: Hyvönen, R., Ågren, G.I., and Dalias, P.: Analysing temperature response of decomposition of organic matter. Glob. Change Biol. 11,770-778, 2005.

2. We suggest rewriting the last paragraph of section 2.1 as follows: "Four versions of the model were run with combinations of one or two initial qualities combined with fixed or flexible decomposer efficiencies. When using two initial qualities, the high quality-value was taken from the best fit in the one quality model calibration process and the other quality-value was set to a lower value estimated to give a reasonable difference; the sensitivity of this choice was also tested. The initial amount of carbon was partitioned equally between the two qualities."

3. No, the likelihood measure on line 2, page 8705, was not chosen on the basis of the distribution of measurement error. The choice of likelihood measure is not particular sensitive as they are never used for any statistical purposes nor have their absolute values any deeper meaning.

4. Line 8, 8705: Yes, as it stands now LM is an average of the average l from each temperature, because we want to give each temperature series the same weight. For our application, for example extra measurement points in the 5+5 series would bias the results towards the behaviour at this temperature. Moreover, the numbers of measurements are almost equal at all temperatures, see section 2.2 for details. We suggest

adding a sentence to the end of the last paragraph of section 2.3.1 “Our choice of likelihood measure gives equal weight to the different temperature series.”

5. The choice of independent initial values for the parameters and an analysis of dependence between resulting parameters are two quite different issues. If we had know the correlation structure between the parameters we could have chosen initial parameter values more efficiently by excluding such combinations that deviate too much from the correlations. But that is all. The analysis of correlations in the resulting parameter sets contains, on the other hand, information from the experiments. The possible confusion here is hopefully avoided by the extension under point 1 above.

6 No, it should be weigh (it is a verb).

7. Yes, the choice of just using LM>0 is arbitrary just as any other choice would be. The advantage of using a fixed value of LM as cut-off point rather than, say, a fraction of the simulations is that LM>0 can be understood, which is in contrast to a cut-off point as a fraction.

8. Good suggestion. Will be incorporated in final version.

9. See point 2 above where this is clarified.

10 and 11. Thank you for drawing our attention to the Lipson reference and also the remark in the Schimel - Weintraub paper. We suggest rewriting the last paragraph in section 4.3.3 to incorporate these two references. “It should also be born in mind that the temperature response we find in e0 depends on the assumptions we have made about the temperature dependence of the other factors. For example, we are assuming that the dispersion function is temperature independent although the rate of decomposition is highly sensitive to the strength of dispersion (Hyvönen et al., 2005). This is a simplifying assumption but we are not aware of any experiments demonstrating temperature sensitivity of dispersion. Likewise, although there are theoretical arguments for the effects of quality on the temperature dependence of the rate of carbon utilisation

(Bosatta and Ågren, 1999), this has not been tested rigorously experimentally. Allison et al. (2010) point out another complication from temperature dependent decomposer efficiency. If decomposer efficiency goes down with temperature, decomposers assimilating the same amount of carbon will produce less biomass, which in turn should lead to a lower production of extracellular enzymes that can release assimilable carbon. In our terminology this should correspond a positive coupling between e0 and u0. The increased loss of carbon caused by a temperature increase resulting from lowered decomposer efficiency would then be counteracted by a lower use of carbon. Schimel and Weintraub (2003) suggest instead that lowered decomposer efficiency would not occur at the expense of enzyme production but rather lead to decreased microbial biomass. There is a possibility that different microbial populations are active at different temperatures and that cold-adaptation increases maintenance costs, i.e. decreases efficiency, which can lead to a negative coupling between growth rate and efficiency over changing temperatures (Lipson et al., 2009). In the laboratory experiment by Wetterstedt et al. (2010) the scope for changes in microbial populations was limited and the response should more reflect those of a fixed microbial composition although there were indications of changes in the microbial population (unpublished data). The question of the mechanisms behind the temperature response of decomposition is still far from being solved and it is likely that we need to consider additional couplings between processes.” References to be added: Schimel, J.P. and Weintraub, M.N.: The implications of exoenzyme activity on microbial carbon and nitrogen limitation in soil: a theoretical model, Soil Biol. Biochem., 35, 549-563, 2003. Lipson, D.A., Monson, R.K., Schmidt, S.K., and Weintraub, M.N.: The trade-off between growth rate and yield in microbial communities and the consequences for under-snow soil respiration in a high elevation coniferous forest, Biogeochem., 95, 23-35, 2008.

12. Thank you, we will incorporate your suggestion in the final text.

13. We suggest adding the following sentence to the end of the legend to Figure 6: “Note that the sums of the bar(s) and the areas under the corresponding temperature

curves are all equal, 88% of initial carbon."

Interactive comment on Biogeosciences Discuss., 7, 8699, 2010.