Biogeosciences Discuss., 11, C3683–C3685, 2014
www.biogeosciences-discuss.net/11/C3683/2014/

Biogeosciences
Discussions
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Interactive comment on "Implications of carbon saturation model structure for simulated nitrogen mineralization dynamics" *by* C. M. White et al.

Anonymous Referee #1

Received and published: 23 July 2014

The manuscript by White et al. compares four different models, leading or not to the saturation of the soil stock of stabilized C, and the impacts of these models on N dynamics. The subject is important and seems suitable for Biogeosciences. The general presentation of the paper is suitable and the writing in itself is overall clear. The results are interesting. My two main concerns remain that (1) somehow, the paper is too much focused on the models so that in some parts the underlying mechanisms or the "real world" are not enough emphasized, and that at the same time (2) some aspects of the models could be better presented for the readers to fully understand them.

1 I think the introduction should start by some general information on the mechanisms that could lead to saturation and the type of evidences we have for carbon saturation (there are already some information, but in my opinion not enough). I think the discus-

sion should be clearer about which of the four models is the most realistic according to the results or, more generally, about the impact of the results of the four models on what we think about the likelihood of the different mechanisms of saturation. The four models tested could be compared to other published models of C and N dynamics, at least in their formalism and structure. I think it would be interesting to refer for example to: Wutzler, T. & Reichstein, M. (2008) Colimitation of decomposition by substrate and decomposers – a comparison of model formulations. Biogeosciences, 5, 749–759. Perveen, N., Barot, S., Alvarez, G., Klumpp, K., Martin, R., Rapaport, A., Herfurth, D., Louault, F. & Fontaine, S. (2014) Priming effect and microbial diversity in ecosystem functioning and response to global change: a modeling approach using the Symphony model. Global change biology, 20, 1174-1190. Smith P, Smith JU, Powlson DS et al. (1997) A comparison of the performance of nine soil organic matter models using datasets from seven long-term experiments. Geoderma, 81, 153–225. Even if these papers address other issues than C saturation. Somehow, this would also help replacing C saturation among other theories about C dynamics.

2 The models are sometimes difficult to follow. It would be helpful: + to have a table defining all variables and constant parameters and giving the corresponding symbols + in my opinion the C/N ratios should be referred to in the equations by a single symbol and not something as "C:N". Just for the sake of clarity. + the introduction starts too quickly with equations and symbols that are not really defined. And I think Fig. 1 is not quoted. I think it would be useful either to leave the same information in the introduction but in a less mathematical way, with more explanations on the underlying mechanisms, or to move the information or its mathematical part in the Methods. In the same vein, the introduction could describe more clearly the type of carbon pools involved, to give the reader a clearer idea about the real pools modeled in Fig. 1. + I have difficulties understanding how the stoichiometric constraints are modeled. There is of course some information on this issue but I do not manage to make something consistent with it. Are the C/N ratios fixed (apparently not) or the results of the parameter determining fluxes? Potentially microbes could be N or C limited which should change the equations

and lead to different sets of equations. Maybe, Table 2 should also give the equations for the N compartments. These aspects (formalism of the stoichiometric constraint) are likely to be critical for the coupling of C and N dynamics and are not fully discussed in the Discussion. What is the C/N ratio of the inputs? Could it be useful to test the effect of this ratio on C and N dynamics? + As far as I understand there are constant inputs of organic matter so that C and N are constantly entering the ecosystem (but the equations for the Cr compartments are not given in Table 2). There are C output via respiration and CO2, but there is no N output (denitrification, leaching). This leads necessarily to an increase in the ecosystem content in N. . . and impacts C dynamics. Is that realistic? Could it not bias the results? + the different compartments of Figure 2 could be labeled / named

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Interactive comment on Biogeosciences Discuss., 11, 9667, 2014.