Biogeosciences Discuss.,
doi:10.5194/bg-2016-545-RC2, 2017

Biogeosciences Discussions

Interactive comment on "Modelling the genesis of equatorial podzols: age and implications on carbon fluxes" *by* Cédric Doupoux et al.

Anonymous Referee #2

Received and published: 13 March 2017

This study used the measurements of carbon stock and 14C of soil carbon at different soil layers to constrain the carbon fluxes into and out of the Bh layer and its carbon turnover rate. Even for a single –pool model of soil carbon in Bh layer at non-steady state, there are three unknowns: influx, efflux and turnover rate, with only two observations for each site (total amount of carbon and carbon age). Theoretically, the optimization problem is under-determined, there will be infinite number of solutions. However only a unique solution was found in this study. Therefore I must have missed additional data constraint used in the optimization by the authors. In general I found that the manuscript provides quite a lot of details and reasoning for the approach taken in this study. However the key message was somehow buried by detail as presented. Significant modifications should be made to distil wealth of information to highlight the key message. That is what are the magnitudes of carbon influx and efflux from the Bh

soil layer and its turnover rate. Estimates of carbon influxes by previous studies varied by one order of magnitude, and result from this study suggests that a lower estimate of the carbon influx is more likely. After presenting the modelling results of the one-pool model for the Bh layer, the section was unfortunately ended with one sentence "These observations are not consistent with very low ïĄćBh rates, suggesting that a single Bh C pool is incorrect and that two pools of Bh C are required to adequately represent Bh C dynamics". I find that quite disappointing. The authors went on to model the formation of the whole profiles with soil carbon Bh being represented using a two-pool model. My question is then how the results in Section 3.1 were used in Section 3.2, ie how the fluxes and turnover rates of the two-pool model for the Bh soil layers are estimated? This is quite unclear to me. In presenting optimization problem, you need to state clearly: observations, optimizing model parameters, the model and optimization method including cost function. This has not been done adequately in this manuscript. Therefore I recommend major revisions. Some additional comments. The results are quite specific to the sites you studied. What are more broad implications? L56 and L58. In L56, you stated that data from 11 test areas were used to constrain a model of C fluxes, but you actually only presented results of four profiles (see L58). Inconsistent! Section 2.3 Would it be simpler to assume that soil carbon pools at different layers were at steady state before 1950 and solve the model analytically at 1950, then integrate the model forward after 1950 to match both the observed carbon pool and age using optimization?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-545, 2017.